TRANSACTIONS

OF THE

AMERICAN INSTITUTE

OF

ELECTRICAL ENGINEERS

JANUARY TO MAY, 1913

VOL. XXXII, PART I

PUBLISHED BY THE
AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS
33 WEST THIRTY-NINTH STREET
NEW YORK, N. Y., U. S. A.
1913

Press of McIlroy & Emmet, 22 Thames Street, New York

OFFICERS AND BOARD OF DIRECTORS

1912-1913

PRESIDENT.

RALPH D. MERSHON.

JUNIOR PAST-PRESIDENTS.

DUGALD C. JACKSON.
GANO DUNN.

VICE-PRESIDENTS.

DAVID B. RUSHMORE.
W. G. CARLTON.
CHARLES W. STONE.
A. W. BERRESFORD.
WILLIAM S. MURRAY.
SEVERN D. SPRONG.

MANAGERS.

H. H. BARNES. JR.
R. G. BLACK.
W. S. RUGG.
CHARLES E. SCRIBNER.
F. S. HUNTING.
NORMAN W. STORER.
WILLIAM S. LEE.
FARLEY OSGOOD.
COMFORT A. ADAMS.
J. FRANKLIN STEVENS.
WILLIAM B. JACKSON.
WILLIAM McCLELLAN.

TREASURER.
GEORGE A. HAMILTON.

SECRETARY.
F. L. HUTCHINSON.

HONORARY SECRETARY.
RALPH W. POPE,
33 West 39th Street, New York.

GENERAL COUNSEL.
PARKER and AARON,
52 Broadway, New York

PAST-PRESIDENTS.—1884-1912.

*NORVIN GREEN, 1884-5-6.
*FRANKLIN L. POPE, 1886-7.
T. COMMERFORD MARTIN, 1887-8.
EDWARD WESTON, 1889-9.
ELIHU THOMSON, 1889-90.
*WILLIAM A. ANTHONY, 1890-91.
ALEXANDER GRAHAM BELL, 1891-2.
FRANK JULIAN SPRAGUE, 1892-3.
EDWIN J. HOUSTON, 1893-4-5.
LOUIS DUNCAN, 1895-6-7.
FRANCIS BACON CROCKER, 1897-8.
A. E. KENNELLY, 1898-1900.
CARL HERING, 1900-1.
CHARLES P. STEINMETZ, 1901-2.
CHARLES F. SCOTT, 1902-3.
BION J. ARNOLD, 1903-4.
JOHN W. LIEB, Jr., 1904-5.
SCHUYLER SKAATS WHEELER, 1905-6.
SAMUEL SHELDON, 1906-7.
HENRY G. STOTT, 1907-8.
LOUIS A. FERGUSON, 1908-09.
LEWIS B. STILLWELL, 1909-10.
DUGALD C. JACKSON, 1910-11.
GANO DUNN, 1911-12.

*Deceased.

LOCAL HONORARY SECRETARIES.

JAMES S. FITZMAURICE,
G. P. O. Perth, Australia.
HORACE FIELD PARSHALL,
Salisbury House, London Wall, E. C., London.
L. A. HERDT, McGill University, Montreal Que.
CLARE F. BEAMES,
Bangalore, Mysore Province, India.
WILLIAM G. T. GOODMAN,
Adelaide, South Australia.
ROBERT JULIAN SCOTT.
Christ Church, New Zealand.
HENRY GRAFTIO, St. Petersburg, Russia.
RICHARD O. HEINRICH,
Genest-str. 5 Schoeneberg, Berlin, Germany
A. S. GARFIELD, 67 Avenue de Malakoff, Paris, France.

STANDING COMMITTEES

EXECUTIVE COMMITTEE.

RALPH D. MERSHON, Chairman.
COMFORT A. ADAMS.
GEORGE A. HAMILTON.
WILLIAM S. MURRAY.
W. S. RUGG.
CHARLES W. STONE.

Finance Committee.
CHARLES W. STONE, Chairman.
A. W. BERRESFORD.
W. S. RUGG.

Code Committee.
FARLEY OSGOOD, Chairman.
J. C. FORSYTHE.
H. B. GEAR.
H. N. MULLER.
A. M. SCHOEN.
GEORGE F. SEVER.
JOHN B. TAYLOR.

Meetings and Papers Committee.
W. S. RUGG, Chairman.
H. W. BUCK.
A. F. GANZ.
W. C. L. EGLIN.
JOHN M. HIPPLE.
H. A. HORNOR.
S. G. McMEEN.
H. H. NORRIS.
E. W. RICE, JR.
F. J. SPRAGUE.
H. G. STOTT.
PERCY H. THOMAS.
JOHN B. WHITEHEAD.
GEORGE R. WOOD.

Library Committee.
SAMUEL SHELDON, Chairman.
FREDERICK BEDELL.
PHILANDER BETTS.
DUGALD C. JACKSON.
MALCOLM MAC LAREN.

Editing Committee.
LEWIS T. ROBINSON, Chairman.
ALBERT F. GANZ.
CARY T. HUTCHINSON.
A. S. McALLISTER.
WALTER I. SLICHTER.
NORMAN W. STORER.

Law Committee.
CHARLES A. TERRY, Chairman
CLIFTON V. EDWARDS.
FRANCIS BLOSSOM.
W. G. CARLTON.
GEORGE E. CRUSE.

Board of Examiners.
PERCY H. THOMAS, Chairman.
MAURICE COSTER.
A. F. GANZ.
A. S. McALLISTER
WILLIAM McCLELLAN

Standards Committee.
A. E. KENNELLY, Chairman.
COMFORT A. ADAMS. Secretary
W. C. L. EGLIN.
H. W. FISHER.
E. R. HILL.
PETER JUNKERSFELD.
B. G. LAMME.
W. L. MERRILL.
W. S. MOODY.
W. H. POWELL.
CHARLES ROBBINS.
CHARLES F. SCOTT.
J. FRANKLIN STEVENS.
CHARLES P. STEINMETZ.
SAMUEL W. STRATTON.

Sections Committee.
PAUL M. LINCOLN, Chairman.
H. W. CROZIER.
S. G. McMEEN.
GEORGE F. SEVER.
J. FRANKLIN STEVENS.

Chairman of Sections. Ex-officio Members.

A. M. SCHOEN.
J. B. WHITEHEAD.
F. P. VALENTINE.
RALPH H. RICE.
E. J. EDWARDS.
T. W. BEHAN.
O. S. MORE.
E. L. NICHOLS.
G. A. DAMON.
W. A. HALL.
E. H. KIFER.
H. S. FOLEY.
T. E. BARNUM.
A. L. ABBOTT.
H. A. HORNOR.
E. L. FARRAR.
W. C. SMITH.
H. R. WAKEMAN.
H. W. CROZIER.
JOHN B. TAYLOR.
J. D. ROSS.
JOSEPH A. OSBORN.
JOHN B. FISKEN.
GEORGE E. KIRK.
F. A. GABY.
A. M. BUCK.
F. D. NIMS.
JOHN H. FINNEY.

SPECIAL COMMITTEES

Railway Committee.
FRANK J. SPRAGUE, Chairman.
A. H. BABCOCK.
FRED. DARLINGTON
C. E. EVELETH.
GEORGE GIBBS.
CARY T. HUTCHINSON.
DUGALD C. JACKSON.
EDWIN B. KATTE.
RICHARD McCULLOCH.
WILLIAM S. MURRAY.
LEWIS B. STILLWELL.
B. F. WOOD.

Telegraphy and Telephony Committee.
S. G. McMEEN, Chairman.
F. F. FOWLE.
H. M. FRIENDLY.
BANCROFT GHERARDI.
A. H. GRISWOLD.
F. J. MAYER.
H. MOURADIAN.
L. M. POTTS.
HENRY L. REBER.
ALLARD SMITH.
J. L. WAYNE.
GEORGE J. YUNDT.

High Tension Transmission Committee.
PERCY H. THOMAS. Chairman.
H. E. BUSSEY.
MAX COLLBOHM.
G. FACCIOLI.
P. T. HANSCOM.
JOHN HARISBERGER.
R. F. HAYWARD.
HAROLD PENDER.
NORMAN ROWE.
C. S. RUFFNER.
DAVID B. RUSHMORE.
HARRIS J. RYAN.
P. W. SOTHMAN.

Electric Lighting Committee.
W. C. L. EGLIN, Chairman.
R. G. BLACK.
K. H. HANSEN.
SIDNEY HOSMER.
PETER JUNKERSFELD.
J. A. LIGHTHIPE.
S. J. LISBERGER.
H. W. PECK.
T. S. PERKINS.
D. W. ROPER.
L. E. SINCLAIR.
W. F. WELLS.

Industrial Power Committee.
JOHN M. HIPPLE. Chairman.
H. B. EMERSON.
R. S. FEICHT.
E. FRIEDLANDER.
E. H. KIFER.
C. D. KNIGHT.
J. C. LINCOLN.
R. S. MASSON.
W. H. POWELL.
BARTON R. SHOVER.
R. H. TILLMAN.

Educational Committee.
HENRY H. NORRIS, Chairman.
C. R. DOOLEY.
W. A. HILLEBRAND.
JOHN PRICE JACKSON.
G. W. LAMKE.
C. L. MEES.
A. J. ROWLAND.
ROBERT SIBLEY.
WALTER I. SLICHTER.
CHARLES P. STEINMETZ.

SPECIAL COMMITTEES—Continued

Electrochemical Committee.
ALBERT F. GANZ, Chairman.
E. R. BERRY.
CHARLES E. BONINE.
C. P. BURGESS.
C. F. ELWELL.
A. McK. GIFFORD.
CARL HERING.
W. R. WITHNEY.

Power Station Committee.
HENRY G. STOTT, Chairman.
C. L. DE MURALT.
GLENDOWER DUNBAR.
J. H. HANNA.
HENRY A. LARDNER.
H. ST. CLAIR PUTNAM.
SEVERN D. SPRONG.
F. A. VAUGHIN.
W. F. WELLS.

Committee on Use of Electricity in Mines.
GEORGE R. WOOD, Chairman.
C. W. BEERS.
H. H. CLARK.
C. T. HENDERSON.
W. E. MOORE.
A. J. NICHT.
K. A. PAULY.
W. A. THOMAS.
H. M. WARREN.

Electrophysics Committee.
JOHN B. WHITEHEAD, Chairman.
L. W. CHUBB.
W. S. FRANKLIN.
EDWARD P. HYDE.
CARL KINSLEY.
EDWARD L. NICHOLS.
E. F. NORTHRUP.
G. W. PIERCE.
M. I. PUPIN.
EDWARD B. ROSA.
HARRIS J. RYAN.
H. CLYDE SNOOK.
CHARLES P. STEINMETZ

Committee on the Use of Electricity in Marine Work.
H. A. HORNOR, Chairman.
FRANK R. BACON.
J. J. CRAIN.
MAXWELL W. DAY.
W. L. R. EMMET.
F. C. HANKER.
O. P. LOOMIS.
C. S. McDOWELL.
DAVID M. MAHOOD.
J. P. MALLETT.
JULIUS MARTIN.
ELMER A. SPERRY.

Public Policy Committee.
CALVERT TOWNLEY, Chairman.
JOHN J. CARTY.
C. C. CHESNEY.
JOHN H. FINNEY.
HENRY FLOY.
W. W. FREEMAN.
C. F. LACOMBE.
L. A. OSBORNE.
E. W. RICE, Jr.

Code of Principles of Professional Conduct.
B. A. BEHREND, Chairman.
JOHN F. KELLY.
H. ST. CLAIR PUTNAM.
LEWIS T. ROBINSON.
GEORGE F. SEVER.

Patent Committee.
BION J. ARNOLD, Chairman.
C. S. BRADLEY.
F. F. FOWLE.
PETER COOPER HEWITT.
JOHN F. KELLY.
H. WARD LEONARD.
M. I. PUPIN.
W. E. WINSHIP.
B. F. WOOD.

Committee on Sections Participation in Conduct of Institute Affairs.
E. A. BALDWIN, Chairman.
DUGALD C. JACKSON.
W. S. LEE.
PAUL M. LINCOLN.
S. G. McMEEN.
CHARLES F. SCOTT.
CHARLES P. STEINMETZ.
PERCY H. THOMAS.

Relations of Consulting Engineers.
LEWIS B. STILLWELL, Chairman.
FRANCIS BLOSSOM.
W. K. DUNLAP.
FRANK R. FORD.
E. W. RICE, Jr.
FRANK J. SPRAGUE.

Membership Committee.
H. CLYDE SNOOK, Chairman.
E. A. BALDWIN.
F. J. BULLIVANT.
S. K. COLBY.
MAURICE COSTER.
HENRY FLOY.
ROBERT T. LOZIER.
C. E. MAGNUSSON.
K. C. RANDALL.
E. P. ROBERTS.
A. M. SCHOEN.
C. E. SCRIBNER.

INTERNATIONAL ELECTRICAL CONGRESS, SAN FRANCISCO, 1915.
Executive Committee of Committee on Organization.
CHARLES P. STEINMETZ, President.
CHARLES F. SCOTT, First Vice-President.
A. E. KENNELLY, Vice-President on Program.
C. O. MAILLOUX, Vice-President on International Relations.
HENRY A. LARDNER, Vice President on Pacific Coast Relations.
EDWARD B. ROSA, Secretary.
PRESTON S. MILLAR, Treasurer and Business Manager.

Historical Museum Committee.
T. C. MARTIN, Chairman.
JOHN J. CARTY.
CHARLES L. CLARKE.
LOUIS DUNCAN.
E. W. RICE, Jr.
CHARLES F. SCOTT.
FRANK J. SPRAGUE.

Index Transactions Committee.
GEORGE I. RHODES.

International Electrotechnical Commission.
United States National Committee.
C. O. MAILLOUX, President.
A. E. KENNELLY, Secretary
COMFORT A. ADAMS.
B. A. BEHREND.
LOUIS BELL.
FRANCIS B. CROCKER.
GANO DUNN.
W. C. L. EGLIN.
H. W. FISHER.
BANCROFT GHERARDI.
JOHN W. HOWELL.
PETER JUNKERSFELD.
B. G. LAMME.
W. S. MOODY.
EDWARD B. ROSA.
CHARLES F. SCOTT.
SAMUEL SHELDON.
C. E. SKINNER.
CHARLES P. STEINMETZ.

Constitutional Revision Committee.
WILLIAM S. MURRAY, Chairman.
W. G. CARLTON.
F. L. HUTCHINSON.
DUGALD C. JACKSON.
PAUL M. LINCOLN.
H. ST. CLAIR PUTNAM.
LEWIS T. ROBINSON.
CHARLES F. SCOTT.
CHARLES E. SKINNER.
LEWIS B. STILLWELL.
CHARLES W. STONE.
HENRY G. STOTT.
JOHN B. TAYLOR.
PERCY H. THOMAS.
CALVERT TOWNLEY.

New York Reception Committee.
A. H. LAWTON, Chairman.
H. H. BARNES, Jr.
F. C. BATES.
H. M. BRINCKERHOFF.
W. G. CARLTON.
MAURICE COSTER.
H. W. FLASHMAN.
HENRY FLOY.
J. W. LIEB, Jr.
R. T. LOZIER.
O. S. LYFORD, Jr.
C. O. MAILLOUX.
WILLIAM McCLELLAN.
W. E. McCOY.
F. A. MUSCHENHEIM.
FARLEY OSGOOD.
C. E. SCRIBNER.
GEORGE F. SEVER.
SAMUEL SHELDON.
S. D. SPRONG.
P. H. THOMAS.
CALVERT TOWNLEY.
W. F. WELLS.

SPECIAL COMMITTEES—Continued

Special Committee on Organization of Technical Committees.

H. G. STOTT, Chairman.
COMFORT A. ADAMS.
H. W. BUCK.
P. M. LINCOLN.
H. S. PUTNAM.

Joubert Memorial Committee.

C. O. MAILLOUX, Chairman.
COMFORT A. ADAMS.
CARL HERING.
C. E. SCRIBNER.
W. D. WEAVER.

Edison Medal Committee.

Appointed by the President for terms of five years.

Term expires July 31, 1917.
A. E. KENNELLY.
H. WARD LEONARD.
ROBERT T. LOZIER.

Term expires July 31, 1916.
FRANK J. SPRAGUE.
SCHUYLER S. WHEELER.
W. D. WEAVER.

Term expires July 31, 1915.
ELIHU THOMSON, Chairman.
JOHN W. LIEB, Jr.
EDWARD L. NICHOLS.

Term expires July 31, 1914.
PHILIP P. BARTON.
JOHN J. CARTY.
JAMES G. WHITE.

Term expires July 31, 1913.
COMFORT A. ADAMS.
C. C. CHESNEY.
RICHARD N. DYER.

Elected by the Board of Directors from its own membership for terms of two years.

Term expires July 31, 1914.
FARLEY OSGOOD.
W. S. RUGG.
CHARLES E. SCRIBNER.

Term expires July 31, 1913.
LEWIS B. STILLWELL.
H. H. BARNES, Jr.
SEVERN D. SPRONG.

Ex-Officio Members.
RALPH D. MERSHON, President.
GEO. A. HAMILTON, Treasurer.
F. L. HUTCHINSON, Secretary.

Sections and their Secretaries

ATLANTA
H. M. Keys, So. Bell Tel. & Tel. Co., Atlanta, Ga.

BALTIMORE
L. M. Potts, Industrial Building, Baltimore, Md.

BOSTON
Leavitt L. Edgar, 39 Boylston St., Boston, Mass.

CHICAGO
E. W. Allen, 1028 Monadnock Building, Chicago, Ill.

CLEVELAND
R. E. Scovel, American Steel and Wire Company, Cleveland, Ohio.

DETROIT ANN ARBOR
Ray K. Holland, Cornwall Building, Ann Arbor, Mich.

FORT WAYNE
P. H. Haselton, Fort Wayne Electric Works. Ft. Wayne, Ind.

INDIANAPOLIS-LAFAYETTE
Charles A. Tripp, 710 Majestic Building, Indianapolis, Ind

ITHACA
George S. Macomber, Cornell University, Ithaca, N. Y.

LOS ANGELES
E. R. Northmore, Los Angeles G. & E. Co., Los Angeles, Cal.

LYNN
E. R. Berry, General Electric Co., Lynn, Mass.

MADISON
F. A. Kartak, Univ. of Wisconsin, Madison, Wis.

MEXICO
James Carson, Mexican L. & P. Co., Mexico City, Mexico

MILWAUKEE
L. F. Reinhard, Mech. Appliance Co., Milwaukee, Wis.

MINNESOTA
Fred G. Dustin, 9 South Fifth St., Minneapolis, Minn

PHILADELPHIA
H. F. Sanville, 1326 Chestnut St., Philadelphia, Pa.

PITTSBURGH
M. C. Turpin, Dept. of Publicity, W. E. and M. Company, Pittsburgh, Pa.

PITTSFIELD
W. W. Lewis, General Electric Co., Pittsfield, Mass.

PORTLAND, ORE.
G. P. Nock, Pacific Tel. and Tel. Co. Portland, Ore.

SAN FRANCISCO
A. G. Jones, 819 Rialto Bldg. San Francisco, Cal.

SCHENECTADY
J. A. Dewhurst, Gen. Elec. Co., Schenectady, N. Y.

SEATTLE
M. T. Crawford, 608 Electric Bldg., Seattle, Wash.

ST. LOUIS
A. McR. Harrelson, Emerson Elec. Mfg. Co., St. Louis, Mo.

TOLEDO
Max Neuber, Care of Cohen, Freidlander & Martin, Toledo, O.

SPOKANE
H. B. Peirce. Box 1436, Spokane, Wash

TORONTO
H. T. Case, Continental Life Bldg., Toronto, Ont.

URBANA
F. G. Wilson, Univ. of Illinois, Urbana, Ill.

VANCOUVER
L. G. Robinson, British Columbia Elec. Railway Company, Vancouver, B. C.

WASHINGTON, D. C.
C. B. Mirick, 1330 New York Ave., N. W., Washington, D. C.

Branches and their Secretaries

AGRICULTURAL AND MECHANICAL COLLEGE OF TEXAS
E. S. Lammers, Jr.,
College Station, Texas.

ARKANSAS, UNIV. OF
G. W. Watkins,
Room 25, Buchanan Hall.
Fayetteville Ark,

ARMOUR INSTITUTE
T. C. Bolton,
Armour Inst. Tech.,
Chicago, Ill.

BUCKNELL UNIVERSITY
Robert L. Rooke.
Bucknell University,
Lewisburg, Pa.

CALIFORNIA, UNIV. OF
L. E. Rushton,
University of California,
Berkeley, Cal.

CINCINNATI, UNIV. OF
A. H. Davis,
Univ. of Cincinnati,
Cincinnati. O.

CLEMSON AGRICULTURAL COLLEGE
H. J. Bomar,
Clemson College, S. C.

COLORADO STATE AGRICULTURAL COLLEGE
R. K. Havighorst,
Colorado State Agricultural College,
Fort Collins, Colo.

COLORADO, UNIV. OF
Frank A. Redding,
Univ. of Colorado,
Boulder, Colo.

HIGHLAND PARK COL.
Ralph R. Chatterton,
Highland Park College,
Des Moines, Iowa.

IOWA STATE COLLEGE
F. A. Robbins,
Iowa State College,
Ames, Iowa.

IOWA, UNIV. OF
A. H. Ford,
Univ. of Iowa,
Iowa City, Ia.

KANSAS STATE AGR. COL.
W. C. Lane,
Kansas State Agric. Col.,
Manhattan, Kan.

KANSAS, UNIV. OF
A. J. Pecht,
Univ. of Kansas,
Lawrence, Can.

KENTUCKY, STATE UNIV. OF
W. M. Lane,
216 Rose Street,
Lexington, Ky.

LAFAYETTE COLLEGE
V. A. Davison,
Lafayette College,
Easton, Pa.

LEHIGH UNIVERSITY
E. F. Weaver,
Lehigh University,
S. Bethlehem, Pa.

LEWIS INSTITUTE
A. H. Fensholt,
Lewis Institute,
Chicago, Ill.

MAINE, UNIV. OF
J. Larcom Ober,
S. A. E. House,
Orono, Maine.

MICHIGAN, UNIV. OF
Edward A. Roeser.
Univ. of Michigan,
Ann Arbor, Mich.

MISSOURI, UNIV. OF
E. W. Kellogg,
9 Engineering Building,
Columbia, Mo.

MONTANA STATE COL.
J. A. Thaler,
Montana State College,
Bozeman, Mont.

NEBRASKA, UNIV. OF
V. L. Hollister,
Station A,
Lincoln, Nebraska.

NEW HAMPSHIRE COL.
Clayton W. Work,
New Hampshire College,
Durham, N. H.

NORTH CAROLINA COL. OF AGR. AND MECH. ARTS
J. W. Johnson,
N. C. Col. of A. and M. Arts
West Raleigh, N.C.

OHIO NORTHERN UNIV.
D. W. Yambert,
Ohio Northern Univ.,
Ada, Ohio.

OHIO STATE UNIV.
T. O. Farmer,
Ohio State Univ.,
Columbus, Ohio.

OKLAHOMA AGRICULTURAL AND MECH. COL.
J. W. Harvey,
416 Hester Street,
Stillwater, Okla.

OKLAHOMA, UNIV. OF
Leo H. Gorton,
526 Univ. Boulevard,
Norman, Okla.

OREGON AGR. COL.
Charles E. Oakes,
Oregon Agric. Col.,
Corvallis, Ore.

OREGON, UNIV. OF
C. R. Reid,
Univ. of Oregon,
Eugene, Oregon.

PENN STATE COLLEGE
I. S. Nippes,
Pennsylvania State Col.,
State College. Pa.

PURDUE UNIV.
A. N. Topping,
Purdue University,
Lafayette, Ind.

RENSSELAER POLY. INST.
W. J. Williams,
Rensselaer Poly. Inst.,
Troy, N. Y.

ROSE POLY. INST.
Joseph E. O'Connell,
457 N. 8th Street,
Terre Haute, Ind.

RHODE ISLAND STATE COLL.
L. A. Whittaker.

STANFORD UNIV.
L. M. Bussert,
Stanford University,
California.

SYRACUSE UNIV.
R. A. Porter,
Syracuse University,
Syracuse, N. Y.

TEXAS, UNIV. OF
Joseph W. Ramsey,
University of Texas,
Austin, Tex.

THROOP COLLEGE OF TECHNOLOGY
R. W. Parkinson,
Throop Poly. Institute,
Pasadena, Cal.

UNIV. OF WASHINGTON
S. R. Shave,
Univ. of Washington,
Seattle, Wash.

VERMONT, UNIV. OF
O. Krupp,
65 North Bend St.,
Burlington, Vt.

VIRGINIA, UNIV. OF
Henry Woodman Clark,
A. X. P. House,
University, Virginia.

WASH., STATE COL. OF
H. V. Carpenter,
State Col. of Wash.,
Pullman, Wash.

WASHINGTON UNIV.
A. S. Blatterman,
45 Lewis Place,
St. Louis, Mo.

WORCESTER POLY. INST.
George I. Gilchrest,
Worcester Poly. Inst.,
Worcester, Mass.

YALE UNIVERSITY
M. R. Wibberley,
136 Vanderbilt-Scientific,
New Haven, Conn.

INSTITUTE REPRESENTATIVES

On Board of Award, John Fritz Medal.
LEWIS B. STILLWELL. GANO DUNN.
DUGALD C. JACKSON. RALPH D. MERSHON.

On Board of Trustees, United Engineering Society.
H. H. BARNES, JR. GANO DUNN.
CHARLES E. SCRIBNER.

On Library Board of United Engineering Society.
SAMUEL SHELDON. DUGALD C. JACKSON.
FREDERICK BEDELL. MALCOLM MACLAREN.
F. L. HUTCHINSON.

On Resuscitation Commission.
A. E. KENNELLY. ELIHU THOMSON.

On Electrical Committee of National Fire Protection Association.
The Chairman of the Institute's Code Committee, FARLEY OSGOOD.

On Advisory Board of American Year-Book.
EDWARD CALDWELL.

On Advisory Board, National Conservation Congress.
CALVERT TOWNLEY.

On Council of American Association for the Advancement of Science.
W. S. FRANKLIN. G. W. PIERCE.

On Conference Committee of National Engineering Societies.
CALVERT TOWNLEY. W. W. FREEMAN.

On Joint Committee on Engineering Education.
CHARLES F. SCOTT. SAMUEL SHELDON.

On American Electric Railway Association's Committee on Joint Use of Poles.
FARLEY OSGOOD. F. B. H. PAINE.
PERCY H. THOMAS.

On National Joint Committee on Electrolysis.
BION J. ARNOLD. F. N. WATERMAN.
PAUL WINSOR.

On Board of Managers, Panama-Pacific International Engineering Congress, 1915.
A. M. HUNT. J. G. DEREMER.
And the PRESIDENT and SECRETARY of the Institute.

CONTENTS.

MEETING AT SAN FRANCISCO, FEBRUARY 28, 1913.

MEETING AT NEW YORK, MARCH 14, 1913.

MEETINGS AT NEW YORK, MARCH 19, 26, APRIL 2, 9, 16, 1913.

MEETING AT PITTSBURGH, APRIL 18-19, 1913.

[redacted]

A paper presented at the 279th Meeting of the American Institute of Electrical Engineers, New York, January 10, 1913.

HIGH-SPEED TURBO-ALTERNATORS—DESIGNS AND LIMITATIONS

BY B. G. LAMME

The real problems in the design of turbo-alternators did not really develop until the high-speed, large capacity units came into demand. In the earlier work, the difficulties in design were mostly those due to lack of experience and to insufficient knowledge of the possibilities of materials, etc. As more data were obtained, the speeds and capacities were gradually increased, until with the present large capacities and high speeds a number of conditions are encountered which may be considered as true physical limitations.

The principal difficulties in the design of the earlier machines were found in the permissible weight which could be carried by bearings, undue noise due to the open construction of the machines, and the troubles incident to the through-shaft construction of the rotor.

The bearing problem was eliminated by securing more complete data, which showed that the possibilities in this feature had hardly been touched upon.

The solution of the noise problem was largely one of enclosing the machine without interfering with the ventilation. In doing this, the noise problem was practically eliminated, but the greater problem of ventilation then developed.

In overcoming the difficulties of the through-shaft construction, the first great advance was made in the direction of larger outputs at higher speeds. In very high-speed machines, the diameter of the shaft in the rotor core is necessarily small. As the over-all diameter of the core is comparatively small, it follows that, after allowing for the slot depth, and the metal in the core necessary to withstand the high rotative stresses, there is left but little available space for the shaft. About 600 kv-a. capacity at 3600 rev. per min. was the limit with this construction.

The first great advance in this problem was made by the introduction of rotors without the through-shaft. By this means, the parts of the shaft adjacent to the rotor core proper could be very much heavier than with the through-shaft type, and this, combined with the solid rotor core, gave great stiffness or rigidity compared with the former through-shaft type. This allowed much larger cores, with correspondingly increased outputs. The two-pole parallel slot type of rotor with bolted-on shaft construction, as described later, was apparently a leader in this respect, due to mechanical, rather than electrical, characteristics. When this type had proved to be a successful one, the possible capacities of two-pole, 3600-revolution, 60-cycle machines at once jumped from 600 to 1000 kv-a., and this was

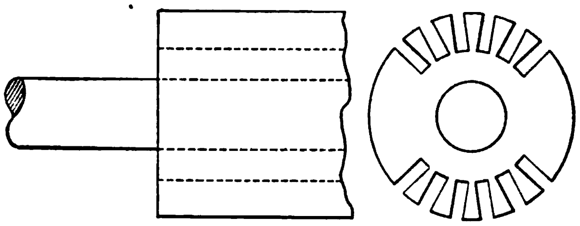


FIG. 1

quickly followed by 1500, 2000 and 3000-kv-a. units at 3600 revolutions. Since then, the increase in capacity at this speed has been more gradual, but has been carried up to 5000 kv-a. at present, with possibilities of a 6250-kv-a. unit.

The radial slot type of rotor, also described later, when constructed with its core and shaft in one piece, quickly followed the parallel-slot type in the above growth, and may eventually catch up with its only rival in the two-pole, 60-cycle field of construction.

About the same time that the through-shaft type was superseded in the two-pole, 60-cycle machine, a corresponding change was made in the two-pole, 25-cycle, and in four-pole rotors for both frequencies, so that, at the present time, practically no designs for the highest-speed machines use the through-shaft type of construction. This latter, however, has been retained in some of the more moderate speed large-capacity units.

On account of the high rotative and peripheral speeds, the general design of large capacity turbo-generators turns upon the type and construction of the rotor, rather than the stator. Various designs and types of rotors have been developed, but, with rare exceptions, only two general types are now built in this country. These may be designated as the radial-slot and the parallel-slot types. Each has a number of advantages over its rival and each has given good results in practise.

Radial Slot Type of Rotor

In the radial slot type, as usually constructed for high-speed machines, the core and shaft are forged in one piece in the smaller and more moderate sizes, but may be built up of a number of separate plates or disks bolted rigidly together in the larger sizes. In this type, the core is cylindrical in all cases, and in the outside surfaces are radial slots, usually arranged in groups, in which the

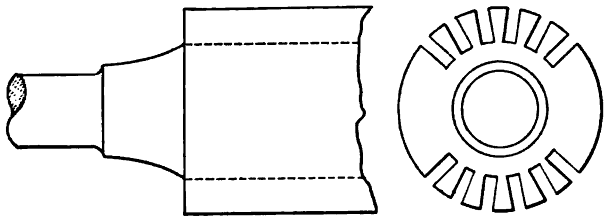


Fig. 2

exciting windings are placed. While all radial slot types of rotors bear a general resemblance to each other, yet there are marked differences in the method of forming the slots and teeth which constitute the outer surface. In some types the solid rotor core has radial slots milled or slotted in the main body of the core. In other cases the slots are formed outside the main core by inserted teeth, usually with overhanging tips, between which the exciting coils lie. These two general constructions are illustrated in Fig. 3. Examples of the inserted-tooth construction are found in the large moderate-speed rotors of one American company, and in somewhat higher speed machines of a German company. However, with the advent of the high-speed, high capacity machines, the milled-in construction of the radial slots appears to be taking the lead, due to certain mechanical limitations in the inserted-tooth types.

On account of the radial slots and the usual concentric arrangement of the exciting coils, the field or exciting turns cannot be assembled and insulated before placing on the core, except in the inserted-tooth type of construction. With the milled-in slot type, the field conductors, usually of flat strap, are dropped into the slot one at a time, with insulation between individual turns. For ease of winding, the ends are usually allowed to overhang the core, and require a very ample outside support in the very high-speed machines. This is illustrated in Fig. 4. The completed coils are usually held in place by strong non-magnetic wedges in the tops of the slots. These wedges are usually carried by overhanging pole tips, in the inserted-tooth type, or by grooves in the sides of the slots in the milled-slot type. The design of the supports for the overhanging end windings has furnished one of the difficult problems in this type of construction. Examples

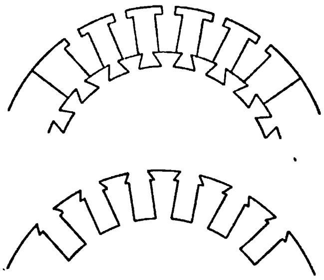


FIG. 3

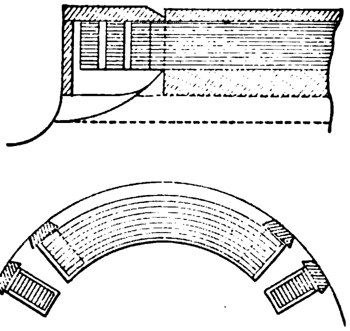


FIG. 4

of radial-slot end windings, and of the rotor complete, are shown in Figs. 5 and 6.

This general construction of the radial slot type of rotor is obviously applicable to machines of any number of poles. With a two-pole machine there will be only two groups of coil slots and two groups of concentric coils, while with four poles or six poles there will be four or six groups respectively. It is evident that, with this construction, a cylindrical rotor is obtained, regardless of the number of poles. It is also evident that the problem of supporting the end windings becomes an increasingly difficult one, as the number of poles is decreased and the span of the end windings is correspondingly increased.

The support over the end windings usually consists of a heavy ring which, in very high-speed machines, must consist of material

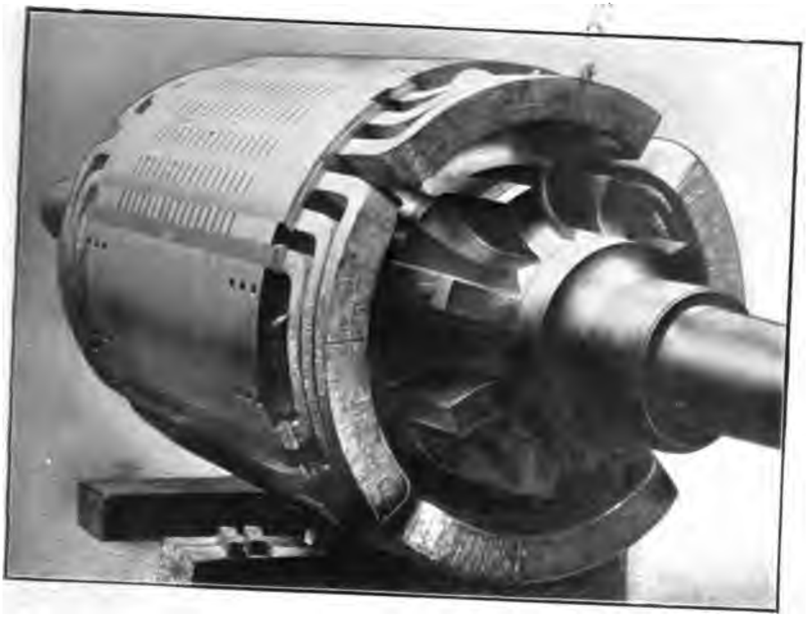
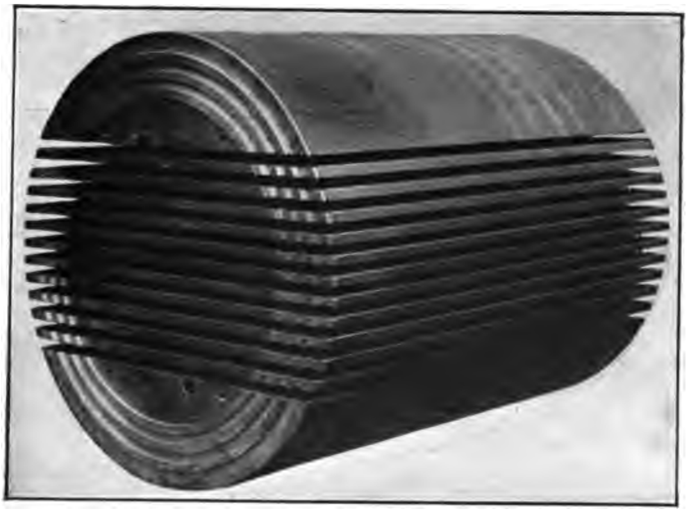


FIG. 5 [LAMME]

FIG. 6 [LAMME]

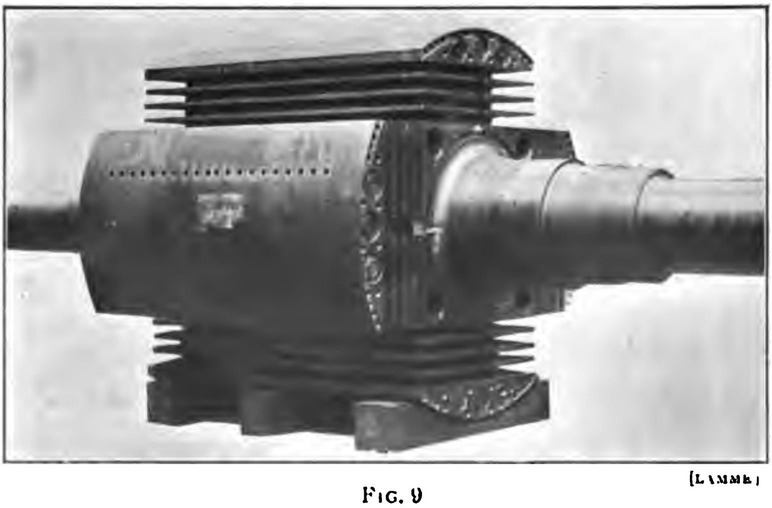


[LAMME]

FIG. 7

[LAMME]

FIG. 8



[LAMME]

FIG. 9

having extra good physical characteristics, for this ring must not only be able to carry itself, but must also carry the weight of the underlying end windings which it supports. In the German inserted-tooth rotor, the end windings are supported by steel bands of many layers, instead of the solid steel ring. In some of the lower speed radial slot machines, such as one American type with inserted teeth, the end supports are of ring form, usually made in a number of sections, which are bolted to an inner shelf by numerous bolts extending from the outer ring between the coils of the end windings to the inner shelf. While this construction is satisfactory for the more moderate peripheral speeds, yet with the much higher speeds in some of the later practise, this construction has been superseded by a solid ring type of support.

Parallel-Slot Type of Rotor

In the parallel-slot type of rotor, the slots for the exciting coils, for any number of field poles, lie in planes parallel to one another and to the rotor axis. The arrangement is illustrated by Fig. 7. As usually constructed, the slots are cut across the ends of the poles, as well as in the sides, so that the exciting coils are embedded in metal throughout their length. The object of this general arrangement of parallel slots is to facilitate the winding of the exciting coils. The rotor can be placed upon a turn-table, or similar device, and rotated, to wind the coils in place under tension. Two or more coils can be wound at the same time, as is actually done in practise. As the coils can be wound under tension, and as the conductors usually consist of thin flat strap, which can be wound in very tightly, the resultant winding is a very substantial piece of work. The finished winding is supported by metal wedges over the coils.

It is obvious that, with this construction, no external support is required for the end windings, as the field core proper furnishes the necessary support. It was largely on account of this feature of well supported end windings that the parallel-slot type took a leading position during the growth of the larger two-pole, 60-cycle alternators. With the radial-slot type, the support of the end windings presented a more difficult problem in the large capacity, high-speed, two-pole machines, which, however, is being gradually solved.

In the two-pole, parallel-slot construction, in order to utilize the available winding space to advantage, it is necessary for the windings to cover the central portion of the core end where the shaft is usually attached, as shown before in Figs. 7 and 8. Therefore, with this construction, a separate " head " or driving flange must be bolted to the core at each end, this head carrying the shaft, as shown in Fig. 8. To avoid magnetic shunting of the field flux, this driving head must be made of non-magnetic material, usually of some high grade bronze, to which the shaft is attached in such a way as to keep the magnetic leakage as low as possible. This makes a good strong construction, but is necessarily rather expensive, due to the bronze driving heads. As these cost but little more for a long rotor than for a short one, the construction therefore tends toward relatively long, small-diameter cores in order to lessen the relative dimensions of the bronze heads.

In two-pole, *single-phase* machines of this construction, the copper cage damper for suppressing the armature pulsating reaction on the field is comprised partly of these bronze heads, which form the " end rings " for the copper bars embedded in the slots in the rotor face.

In the four-pole, parallel-slot machine, no bolted-on driving heads are necessary, for the core proper and the shaft may be cast, or forged, in one piece, or in two or more pieces, which are bolted or " linked " together to form a solid core. The principal difference between the two-pole and the four-pole parallel slot constructions, is that the latter must have salient or projecting poles, in order to utilize the parallel construction for the slots, while the two-pole machine is preferably made cylindrical. Fig. 9 illustrates this feature.

It is evident that there is considerable available space lost by the openings between the projecting poles, while the sections of the poles themselves are cut down very materially by the slots for the exciting winding. The limitations, therefore, in such a rotor are in the magnetic section of the field poles and in the available copper space, and in these features the four-pole parallel slot rotor is inferior to the radial slot type. In the two-pole machine, however, the difference between the radial slot and the parallel slot is not nearly so pronounced, as is indicated in Fig. 10 where the two arrangements are shown on one core for comparison. It may be seen from this that, in the two-pole form, the two constructions approach each other, to a certain extent, some of the slots in the parallel construction being radial, while others depart but little from the radial. One disadvantage in the two-pole, parallel-slot type, however, lies in the smaller amount of copper space which is obtained, for the slot space must necessarily cover a less proportion of the total circumference than is permissible with the radial slot type. This winding space is limited by the physical requirements as regards bending and breaking strains in the overhanging tip *a* in Fig. 10. In the radial slot type, the slot space has no such limitation. Also, on account of the grouping of the field copper into a narrower zone in the parallel slot type, the heat conduction from the copper presents a more difficult problem than in the radial type.

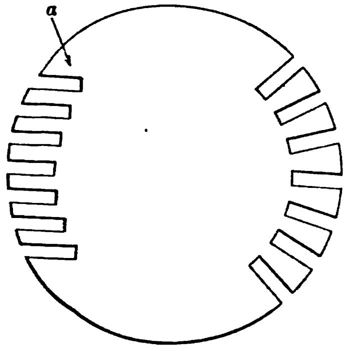


FIG. 10

At first glance, it would appear that the effective length of the field core in the parallel slot type is very considerably diminished by the slots across the ends of the core. However, this is only an apparent effect, for the true length of the core should be taken as that inside of the winding slots, and it should be considered that the additional

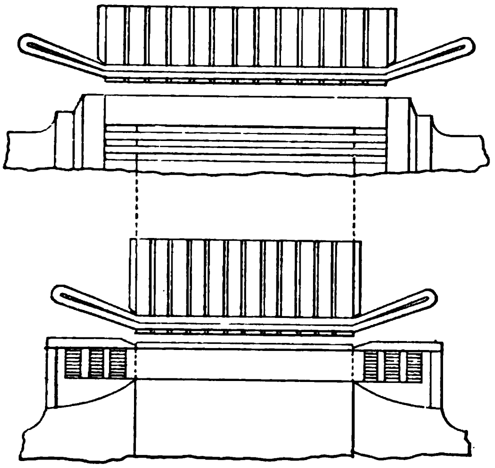


FIG. 11

length of the core at the pole face is in the nature of a coil support which takes the place of the separate support in the radial slot type. Therefore, if over-all lengths, including rotor coil supports, are compared in the two types, there is but little difference, as indicated by Fig. 11. However, if the armature core is made of the same width as the pole face, in both types of rotors, then in the parallel-slot type it will be materially greater than in the radial, for the overhanging pole tips of the parallel-slot machine are also effective magnetically in furnishing flux to the armature. Therefore, as regards the stator, this tends toward a wider core in the axial direction, and a shallower depth of iron back of the armature slots, as indicated in Fig. 12. Also, on account of the relatively larger polar surface, in the parallel slot type of rotor, the magnetic flux density in the air gap is usually relatively smaller than in the radial slot type, which conduces towards a larger depth of air gap. Also, on account of the larger polar surface, the available space for armature slots and teeth is correspondingly increased. Therefore, this type of construction is better adapted for the straight air gap method of ventilation, as will be described later. The greater section available for slots and teeth at the stator pole face permits a large number of ventilating ducts. The relatively large depth of gap allows a large amount of air to be fed through the air gap to the ducts. Therefore, the " radial " type of stator core ventilation has been used very largely with this type of rotor construction. In the parallel-slot type of rotor, it is obvious that, due to the large polar surface compared with the minimum section of the field core, a limit in design is found in the magnetic saturation in the field core itself.

In the four-pole parallel-slot rotor, the field section is more limited than in the two-pole machine, due to the fact that considerable magnetic space is lost by the notches between the projecting poles. However, in this type of construction, the air gap method of ventilation is relatively easy, due to the fact that these interpolar spaces furnish easy access of the ventilating air to the stator ventilating ducts. In consequence, the problem of ventilation is usually not a serious one in this type of rotor. Due to the polar projections, however, the tendency to noise is obviously greater than in either the radial-slot type or the two-pole parallel type, which are always cylindrical.

Nothing has yet been said as to the peripheral speeds obtained in some of the actual designs of the higher speed generators. These, in themselves, indicate some of the limitations which now confront the designer.

In the 5000-kv-a., two-pole, 3600-rev. per min., 60-cycle generator already referred to, which is of the parallel-slot rotor construction, the rotor diameter is 26 in. (66 cm.). This gives a

FIG. 12 [LAMME]

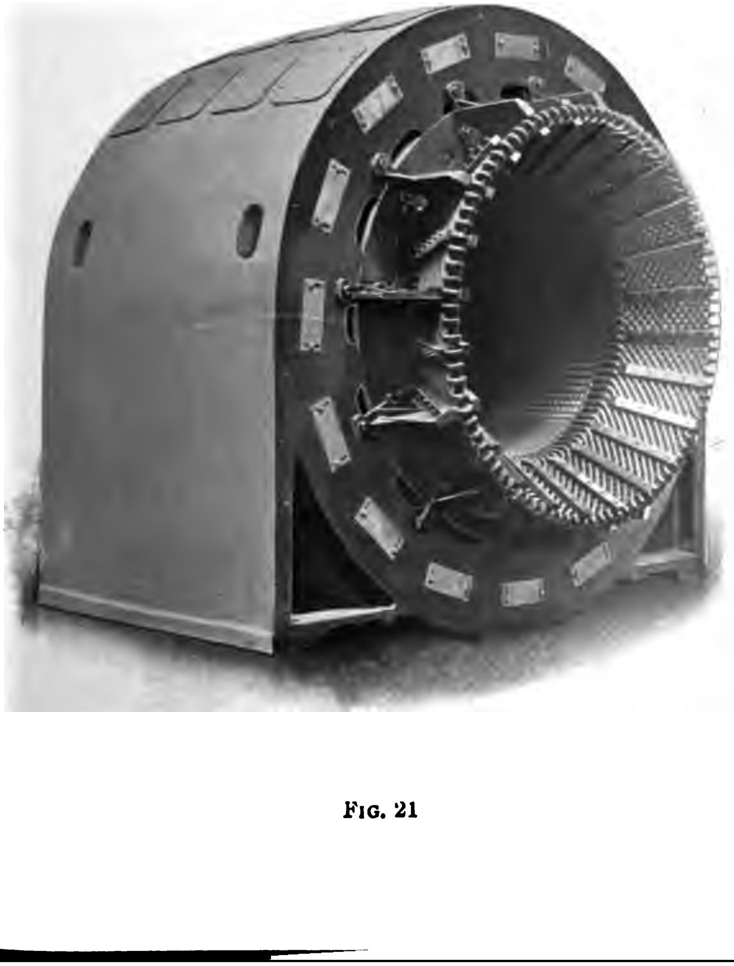


FIG. 21 [LAMME]

peripheral speed of 408 ft. (124.3 m.) per second, or approximately 24,500 ft. (7468 m.) per minute. The core is designed for a very considerable margin of safety, and is actually tested at overspeeds which give practically 30,000 ft. (9144 m.) peripheral speed at the surface of the core.

In certain 19,000-kv-a., 62½-cycle, four-pole, 1875-rev. per min. machines now being built, which are of the radial-slot rotor construction, the rotor diameter is 49 in. (124.4 cm.). This gives a peripheral speed of 24,000 ft. (7315 m.) per minute. This compares with a speed of 21,600 ft. (6583 m.) in a 21,000-kv-a. two-pole, 1500-rev. per min., 25-cycle, radial-slot machine also being built, the rotor core of which is shown in Fig. 12. Obviously the mechanical limitations are being more closely approached in the 60-cycle machines, up to the present capacities.

If a comparison is made between the above 5000 and 19,-000-kv-a. rotors, with their parallel and radial type constructions, it is found that their limitations lie in quite different features. In the radial-slot type, the core stresses are much lower than in the other, but the supporting end ring is an important problem, requiring for its solution a very high grade of steel for the material of the ring. In the parallel-slot rotor, the maximum stresses are in the core itself, principally in the parts which overhang the slots at the sides and ends of the core. In the radial slot core, there are no such overhanging masses. In both constructions, the core material is purposely made of relatively soft steel, having a high percentage elongation, the object being to obtain a material which can yield sufficiently to transfer the strains from local higher points, to adjacent lower parts, and thus equalize them, to a great extent.

The smaller diameter rotor cores are made of steel forgings, in one piece. The larger cores are made up of thick steel plates assembled and bolted together to form a solid mass comprising the core and shaft extensions. By this disk construction, commercial material is used which is of uniform quality clear to the center of the disks. The fiber of the material is in a direction best suited to the directions of stress. With corresponding size cores made in one piece, the outside, to a certain depth, can be given fair physical characteristics, but the center is liable to be glass-hard, as found by experience. However, this may not be a prohibitive condition in machines of more moderate peripheral speeds. Herein lies one great difference between American and European limitations. In American practise, 60 cycles, calling for 3600 and 1800-rev. per min. machines, is the standard frequency, while in Europe, 50 cycles is standard, giving 3000 and 1500-rev. per min. machines. These lower speeds make an enormous difference in the possibilities of design and construction.

Present Limitations in Design

On account of the very great capacities, at high speeds, now being obtained in turbo-generator practise, a number of problems are being encountered, the solutions of which are producing more or less radical changes, both in design and in practise. Some of the limitations now encountered are in the relatively high temperatures in certain parts, high losses in a relatively small space, the difficulty of ventilation, due to the requirement of enormous volumes of cooling air through limited openings or passages, the type of insulation, fire risks, regulation and short-circuit conditions, etc.

A number of these limiting conditions, such as the temperature, ventilation, losses, and insulation, are so closely related to each other that it is difficult to describe any one of them in detail, without including the others to a considerable extent.

The Problem of Ventilation

In the general problem of ventilation, four conditions must be considered, namely, the total loss, or heat, developed, the surface exposed for dissipating this heat to the air, the quantity of air required to carry away the heat, and the temperature of the cooling air.

In the conduction of heat from the surface of a body into the air, the quantity of heat per unit area which can be dissipated depends upon the difference in temperature maintained between the surface of the body and the body of air to which the heat is conducted. The heat dissipated raises the temperature of the adjacent air a certain amount, and thus tends to reduce the temperature difference, unless the air is renewed with sufficient rapidity. On the other hand, if the quantity of air is so great, in proportion to the heat dissipated, that there is but little rise in the air temperature, then any increased amount of air over the surface will represent practically no gain in ventilation. In other words, when the amount of air passed over a surface is sufficient to take up the heat dissipated from the surface without an undue rise, then a further quantity of air is wasteful, and it may even be considered as indirectly harmful, in those cases where the total quantity of air is limited. This has a direct bearing on the size of ventilating ducts or passages in a machine. If the air path through a duct is relatively long, then a considerable width of duct may be required in order to get the necessary quantity of air through it. On the other hand, if the air path is very short, then a very narrow duct may be most effective, for a wider duct may allow more air to pass through than can be utilized in taking up the heat.

No matter how thoroughly the ventilating air is distributed through the heat-generating body, or however effective the heat-dissipating surfaces may be, the total air supplied must be ample in quantity, or its temperature will be raised an undue amount. As the surfaces to be cooled must always have a higher temperature than the cooling air, any considerable rise in the latter will have a direct influence on the ultimate temperature which may be attained by the body to be cooled. Conversely, if an ample quantity of cooling air is supplied, but the heat-dissipating surfaces are insufficient, the ultimate temperature of the body will also be affected.

In large capacity, high-speed turbo-generators, the problem of ventilation is one of the most difficult ones encountered. The trouble lies principally in the large total loss expended in a very limited space. The difficulties of the problem may be illustrated by the following example:

Assume, in a 1500-rev. per min., 25-cycle, 15,000-kv-a. machine, a total efficiency of 96.5 per cent, including air friction loss inside of the machine. This means a total loss in the machine of 545 kw., which is not excessive for this capacity, but is very large for the limited space in which it is developed. A very large volume of cooling air is required for carrying away the heat due to this loss. A simple approximate rule for determining the quantity of air required is that an expenditure of one kw. in one minute will raise the temperature of 100 cu. ft. (2.8 cu. m.) of air 18 deg. cent. Therefore, 545 kw. loss would require a supply of ventilating air of approximately 50,000 cu. ft. (1416 cu. m.) per minute for a rise of the outgoing air of 20 deg. above that of the incoming air. Assuming a velocity of 3000 ft. (914 m.) per minute, this would mean, with a cylindrical ventilating channel, a diameter of 56 in. (142.2 cm.), which is greater than the rotor diameter itself. However, as the cooling air ordinarily would be supplied to both sides of the machine, the ventilating passage need only be half the above section for each side.

Obviously, such passages are prohibitively large, and much greater air velocities through the machine proper are necessary. Velocities as high as 5000 to 6000 ft. (1524 to 1828 m.) per minute are common, while, in some cases, more than 10,000 ft. (3048 m.) per minute has been required in certain constricted sections of the air path inside the machines. Therefore, no matter how the problem is considered, it may be seen that the above condition of the enormous volume of air required, makes the problem of ventilation a difficult one.

There are several methods of ventilating large turbo-generators, depending upon the system of applying the air. There is, first, the radial system, in which practically all the cooling air passes out radially through ventilating ducts in the stator core. This radial system of ventilating can be subdivided into two alternative methods, depending upon whether the air is partly or wholly supplied through passages in the rotor, or through the air gap alone. These two methods are illustrated in Fig. 13. The straight air gap arrangement may require a relatively large air gap, combined with very high velocity of the air along the gap, while the other method permits a considerably shorter gap. The straight air gap method of ventilation is used, to a considerable extent, in all 60-cycle machines of two-pole construction, while it is practically the only one that has been used with the parallel-slot type of machine with either two or four poles. In this parallel-slot type of rotor, however, the air gap can be relatively larger than the radial-slot type of rotor, as explained before, which compensates, to some extent, for the necessity of depending upon this method entirely. In the four-pole parallel-slot rotors, the interpolar spaces are also effective. Moreover, with parallel-slot rotors in general, the openings from the air gap into the stator ventilating ducts can usually be somewhat larger in total section than with the radial type of rotor, as also described before. However, the relatively greater axial length of the core of the parallel-slot type of rotor increases the length of the constricted air passages along the air gap in the two-pole machines, which is a material disadvantage.

The straight air gap type of ventilation has proved astonishingly effective in cooling the rotor in both the radial and parallel-slot types of rotors, and with either type there is usually no great difficulty in forcing through enough air to cool the rotor core in a fairly effective manner. It must be considered, however, that the total rotor loss in large turbo-generators is possibly only 10 per cent of the total loss which must be taken care of, and a relatively small proportion of the total ventilating air may suffice to cool it. According to actual measurements, corroborated by general experience, the cylindrical surface of the rotor core can give off four or five watts per square inch (6.45 sq. cm.) to the cooling air, with a temperature rise of the rotor surface of about 35 to 40 deg. cent. above the cooling air. To those who have had experience with dissipating heat from electric apparatus, this result will appear to be extremely good.

The real difficulty with the air gap method of ventilation is not so much in getting enough air through for cooling the

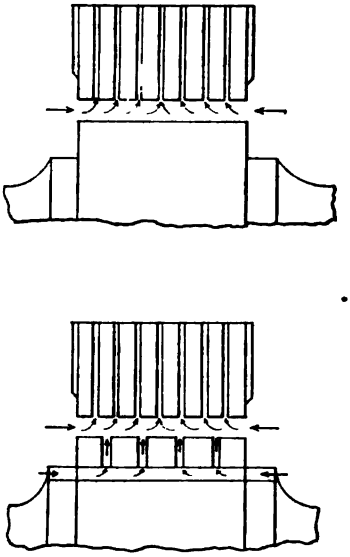


FIG. 13

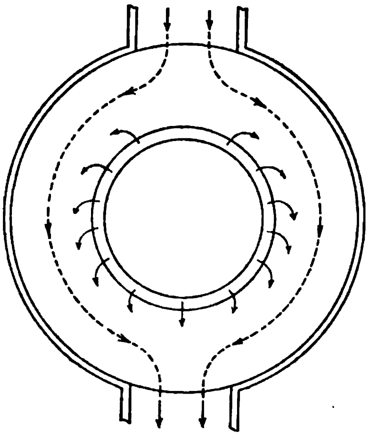


FIG. 14

rotor itself, but it is in the much larger quantity required for the stator. For instance, a one-inch (2.54-cm.) depth of gap (from iron to iron) with a 50-in. (127-cm.) diameter of rotor, means a total section of air path into the gap (counting both ends of rotor) of 314 sq. in. or 2.18 sq. ft. (0.19 sq. m.). At a velocity of 10,000 ft. (3048 m.) per minute, this allows a flow of only 21,800 cu. ft. (617 cu. m.) per minute, which will not take care of a large machine, from the present standpoint of possible capacities with the above diameter of rotor. By additional openings in the rotor core, this might be increased to 30,000 cu. ft. (849 cu. m.) per minute, but even this is still much less than a machine with a 50-in. (127-cm.) diameter of rotor would require if built for capacities otherwise possible. Therefore, on account of this limitation in the amount of cooling air, other means of ventilation have received much consideration. Two other general systems of ventilation, in addition to the gap method, have been used, namely, the circumferential method, and the axial. The former has been developed and applied more extensively in the past, but the latter contains possibilities which are bringing it rapidly to the front.

In the circumferential method of ventilation, air is supplied to one or more points on the outside circumference of the stator, and is forced circumferentially around through the air ducts to suitable outlets, also on the outside surface. Air gap ventilation is usually combined with this circumferential method, partly to cool the rotor. The general arrangement is indicated diagrammatically in Fig. 14, in its simplest form, namely, with one inlet and one outlet diametrically opposite. A serious objection to this method of ventilation is found in the limited section of the ventilating path. Assuming, for example, a depth of stator core of 20 in. (50.8 cm.) outside the armature slots and a total of 40 $\frac{3}{8}$-in. (9.5-mm.) ventilating ducts, or a total effective duct space of 15 in. (37.1 cm.) width, then this gives a total section of ventilating path of $20 \times 15 \times 2 = 600$ sq. in., or 4.16 sq. ft. (0.386 sq. m.). On account of the relatively great length of the ventilating path, air velocities of more than 6000 to 7000 ft. (1828 to 2133 m.) are not desirable or economical, but even with 10,000 ft. (3048 m.) velocity, the total quantity of air would be only 41,600 cu. ft. (1166 cu. m.) per minute. Furthermore, this method is handicapped in machines with very high-speed rotors, by interference between the radial and the circumferential systems of ventilation, so that the full benefit of either is not obtained. Below a certain rotor velocity, apparently the circumferential action can predominate, and the method is fairly effective up to the permissible air capacity of the stator ducts; but at very high speeds the radial ventilation may very seriously interfere with the other, so much so, that the radial ventilation alone, even with its very restricted gap section, may give as good results as the two methods acting together.

To avoid this interference, various methods have been devised, such as closing part, or all, of the radial ventilating ducts at the air gap to keep the radial effect from interfering with the other. One arrangement which has been used in Europe to a considerable extent is indicated in Fig. 15. In this, the alternate radial air ducts are closed at the outside surface, while all are closed at the air gap. The air enters by the ducts open at the back of the machine, flows both circumferentially and toward the gap, and crosses over to the intermediate ducts by axial openings back of the armature teeth, and then along these ducts to the outlet. This scheme is effective in principle, but is uneconomical in the sense that less than the total section of stator ducts is useful, as regards the quantity of air which can be carried. There is usually one large central duct to allow an outlet for the rotor ventilating air. This particular arrangement of the stator also uses axial ventilation in crossing over from one set of ducts to the other, which is an effective arrangement.

A modification of the simple circumferential method of

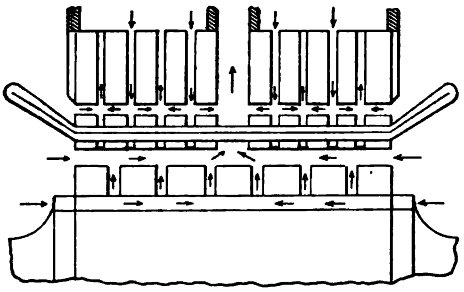


FIG. 15

ventilation is to admit air to the back of the stator at two opposite sides of the machine, and deliver it at two outlets at intermediate points on the surface, as shown diagrammatically in Fig. 16. By this means, the cross-section of the ventilating path is doubled and the length is halved. Also, the interference of the radial ventilation with the circumferential will be less harmful. A serious disadvantage in the circumferential ventilation in general is that the ventilating path is relatively long, especially where there is but one inlet and outlet, and therefore the cooling air at the outlet of the channel may be considerably hotter than at the inlet, with consequently less effective cooling action. This means points of local higher temperature in the core, due to the method of ventilation. In the radial type of ventilation, the coolest air is applied near the seat of the highest losses, namely, at the armature teeth, and immediately back of them, and the air, as it becomes heated, passes over the outer part of the iron which has a diminished loss, and therefore normally less heat to dissipate. Therefore, the effect of the increased temperature of the cooling medium is offset by the lower loss, and consequent less necessity for ventilation, in the part where the air is hottest. The radial system of cooling is therefore theoretically the most effective, but practically, the difficulty is in applying it, due to the limited air passages available.

Both the circumferential and the radial methods of cooling are subject to one serious defect, namely, most of the generated heat in the stator iron must be conducted across the laminations to the air ducts. The rate of conduction across the lamina-

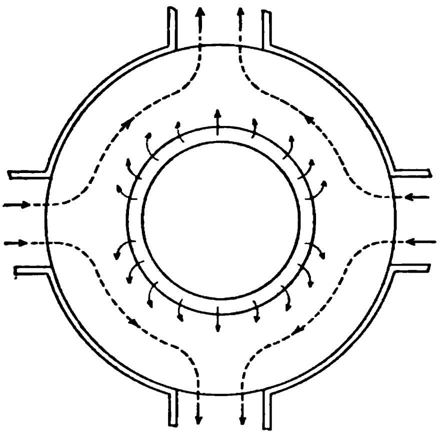


Fig. 16

tions is only from 1 per cent to 10 per cent as great as along the laminations themselves, according to various authorities. Therefore, if the heat could all be conducted along the laminations to the ventilating surfaces, apparently much more effective heat dissipation could be obtained, provided sufficient surface is exposed to the air, and an ample quantity of air supplied. This has led to the development of the axial system of ventilation, as distinguished from the radial and circumferential. In this method, numerous axial holes are provided in the stator core which may extend uninterruptedly from one side of the core to the other, or they may extend from each side to one or more large central radial channels which form the outlet. The usual numerous radial ducts are omitted, or may be considered as combined in one central channel. This general arrangement is illustrated in Fig. 17. The rotor cooling is accomplished by air along the air gap, and through the rotor core to the large central duct. In this method of ventilation, therefore, there is a combination of two types, namely, the axial and the air gap, but there is not the interference between the two that is sometimes found where the circumferential method is used.

From the preceding, it may be seen that the problem of putting a sufficient quantity of air through the machine is an extremely difficult one. In addition, in very large machines, the problem of supplying the required quantity of air from a suitable blower forms another serious problem. In smaller capacities, and in slower speed machines, it has been the usual practise to attach blowing fans to the rotor shaft or core, as

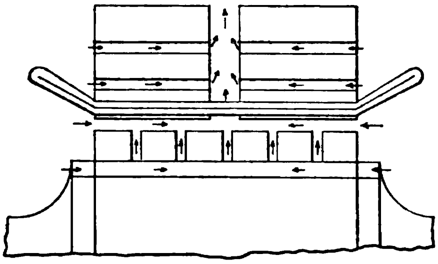


FIG. 17

part of the outfit. There is no particular difficulty in this arrangement, except, possibly, in the high-speed construction of the fans required for 60-cycle, two-pole machines. Such fans can supply an amount of air which is limited by the diameter and other dimensions of the fan itself.

Assume, for example, that by lengthening the rotor core, or by other modifications in the construction, the capacity of the machine can be doubled, and therefore double the quantity of air is required for cooling. If the limit of the fan design or operation was reached before, then obviously some radical change is required with the new capacity of the machine. This condition apparently has been reached in some of the later practise in large, high-speed turbo-alternators. One obvious solution of this difficulty lies in the use of separate lower-speed, large diameter, fans or blowers. This may appear to be a step backward, but when the above conditions and limitations are taken into account, it is not so. The "tail" must not be allowed to "wag the dog;" the blower, which is an adjunct, must not be allowed to dominate the construction of the machine itself. Moreover, there are a number of meritorious features in the use of a separate blower. In the first place, it can be made somewhat more efficient than the high-speed, rotor-driven fans. Again, with a suitable means to drive, variable speeds, and therefore different air pressures, can be obtained. This feature may prove to be very desirable or advantageous under peak, or overload, or emergency conditions.

One further condition keeps cropping out in the general problem of ventilation, namely, that of filtering or washing, or otherwise cleaning the ventilating air. With 50,000 to 75,000 cu. ft. (1415 to 2122 cu. m.) of air per minute passing through a large machine, obviously in a year's time an enormous quantity of foreign matter is carried through the machine with the ventilating air. A deposit of a very small per cent of this in the machine will probably be disastrous. In fact, however, the high velocity of the air through the machine serves to keep the air passages clear if no oil or moisture is allowed to enter. That a large amount of foreign matter does go through the machine is very soon shown in case a little oil is allowed to get into the ventilating passages. This oil catches the dirt and in a short time the ventilating passages may be very materially obstructed.

On account of the deposit of dust, etc., in the ventilating passages, it is necessary to clean certain types of machines at more or less frequent intervals, and it is advisable to clean all types occasionally. With some systems of ventilation, where such cleaning is difficult, or almost impossible, such as that shown in Fig. 16, provision must be made for cleaning the air before it enters the machine. With the particular construction shown in Fig. 16, air filters are almost always supplied. In the American types of construction, however, such filters have not yet been used, except in a more or less experimental manner, due probably to the greater accessibility of these machines as regards cleaning. But such filtering processes possess considerable merit in general. One modification which is being agitated at present is that of washing, instead of filtering, the air. This serves the double purpose of cleaning and cooling the air, and in very hot weather, when the available capacity of the machine is at its minimum, this cooling effect may mean a reduction of 6 to 10 deg. in the temperature of the machine.

The Temperature Problem

In the general problem of temperatures in electrical apparatus, it is not the rises, but rather the ultimate or limiting temperatures which are of first importance. Furthermore, the real limitation in ultimate temperature does not lie in the copper and iron, but in insulating materials used; and only insofar as the temperatures of the former affect the latter do they concern the general problem. However, as insulating materials in themselves are not usually sources of heat, but as they receive most of their heat from adjacent media, such as iron or copper which may be generating loss, the real temperature problem, as regards insulation, resolves itself into the consideration of that of the adjacent materials. Therefore, it is one which, for its full analysis, requires a knowledge of the sources and amounts of heat generated, and its conduction and distribution to other parts.

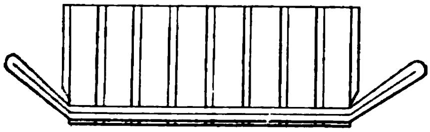
Broadly speaking, there is always a flow of heat from points of higher to those of lower heat potential and the amount of flow is also a function of the quantity of heat generated, the section and length of the paths through which it can flow, and the specific heat resistance of the various materials which conduct the heat. In an electric generator, for example, heat is generated in large quantities in the armature teeth and in the armature core. It is also generated in the armature coils when the machine is carrying load. Part of the armature copper is buried in the armature slots where it is almost surrounded by iron, which, in itself, develops a loss, while another part, such as the end windings, may be surrounded by, and thoroughly exposed to, the ventilating or cooling air. In such end portions, the flow of heat will usually be from the inside copper, directly through the insulation to the cooling air. The amount of heat which will flow from the copper through the insulation, depends upon the temperature differences between the copper and the outside surface of the insulation, upon the cross-section of the path of flow, upon the thickness and " make-up " of the material, and upon the heat-conducting properties of the insulation itself. There is also a considerable temperature gradient from the outside surface to the air. If the surrounding air is not renewed with sufficient rapidity, the flow of heat from the insulation to the air may raise the temperature of the adjacent air, so that the total temperature drop is decreased, and the amount of heat dissipated is correspondingly reduced.

In the armature core, the problem is much more complex.

In the copper buried in the armature slots, there are usually three paths along which the heat can flow: First, it may flow from the copper directly through the insulation to the iron, provided the adjacent iron temperature is lower than that of the copper. Second, it may flow lengthwise of the copper to the end windings to be dissipated directly into the air from that portion of the winding, as described above. Third, in the case of open-slot machines, one edge of the coil may be exposed to the air in the air gap, and there may thus be a direct conduction of the heat through the insulation to the air in the air gap. This latter case, however, only holds for the upper coil, or that next to the gap, in the case of two coils per slot, which is the most common construction. In the bottom coil, the only means of conduction in the buried portion of the coil, are to the adjacent iron or lengthwise to the end windings, or to the adjacent upper coil, which, however, would normally have at least as high temperature as the lower coil. Therefore, the two effective paths should be considered as through to the iron and thence to the air, and lengthwise of the copper to the end windings and to the air. It is the relation of the various factors of these two paths that control the actual temperatures.

It has usually been considered that, in the buried copper, the greater portion of the heat is conducted directly into the surrounding iron. This, however, is only partially true, depending upon many features in the construction and type of apparatus. The heat conductivity of copper is, roughly, about six times that ofl aminated iron lengthwise of the sheet, which is possibly ten to twenty times as great as across the laminations. In an armature which is comparatively narrow and which has very open, well ventilated end windings, a relatively small difference in temperature between the copper at the center of the core and that in the end windings, may cause a relatively large flow of heat from the buried copper to the end copper. Therefore, in certain designs, a great part of the armature copper heat may be dissipated through the end windings, and not through the armature core, especially in those cases where the armature core in itself has a considerable temperature rise. There might even be no conduction of heat from the copper to the iron, or there may be conduction from the iron to the copper; for if the copper is at the same temperature as the iron at the center of the core, for instance, then at each side of the center, or as the edges of the core are approached, the copper temperatures will be relatively lower than at the center, and therefore lower than the adjacent iron, on the assumption that the iron temperatures would be practically constant over the full width of the core. The conditions would therefore be as represented in Fig. 18. The solid line *a* in this figure represents the iron temperature at uniformly 40 deg. cent. rise, and the dotted line *b* represents the copper temperatures from the center of the core to the edges. The temperatures at the center being assumed the same for copper and iron, obviously there will be a flow of heat from the iron to the copper near the edges of the core. The effect of this additional heat carried out by the copper would be such as to tend to increase the temperature of the copper at the center of the core by "banking up" the copper heat.

Again, if the temperature of the copper at the center is materi-



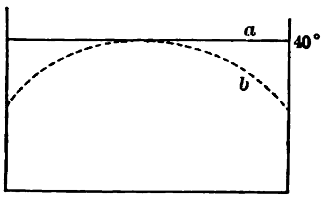


FIG. 18

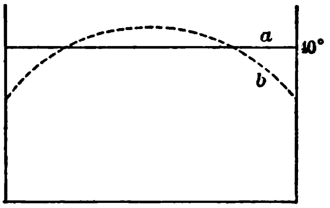


FIG. 19

ally higher than that of the surrounding core, the conditions may be as represented in Fig. 19. In this case, assuming the core at constant temperature, there will be heat flow from the copper to the iron at the center of the core, and from the iron to the copper at the edges.

This study of the problem leads to certain very curious conditions which are sometimes found in large machines. At no-load, for instance, with practically no copper loss present, and with high iron loss, there may be a very considerable flow of heat from the armature teeth through the insulation into the copper, and thence to the end windings and to the air. In this way the temperature of the armature teeth at no-load, and with normal voltage generated, may be considerably reduced by conduction of the iron heat into the copper, while the copper itself may show a very considerable temperature rise. When load is placed upon such a machine, sufficient to raise the temperature of the copper up to that of the iron in the armature teeth, the latter is actually increased in temperature, due to the prevention of the heat conduction into the copper. In this way, therefore, the copper may apparently heat the iron, although there is no direct flow of heat from the copper to the iron, but the reverse flow is prevented.

In high-voltage windings requiring thick insulation, the temperature drop from the copper to the outside may be relatively large; that is, with a given difference of temperature between the copper and the surrounding air, a relatively small amount of heat may be conducted through the insulation. Experience shows that the amount which can be conducted is a function of the quality of the material, the way it is built up, its thickness, and also the pressure upon it. It is almost impossible, in a machine in service, to calculate exactly the flow of heat, even if all the temperature conditions are known, for the insulating material itself is one of the variables in the problem. The ability of the insulation to conduct heat will change with operating conditions, to some extent, as, for instance, it may tend to expand somewhat under heat, and thus change its heat conducting qualities.

In the armature iron, the problem of heat conduction is just as complicated as in the armature conductor. The principal sources of heat lie in the armature teeth and in the armature core back of the teeth. As a rule, the loss in the portion of the core immediately back of the teeth is relatively greater than at a greater depth, for the magnetic fluxes, which cause the temperature rise, generally crowd close to the teeth, so that the density is higher at such parts.

The heat from the armature teeth can be dissipated along several paths. It can flow lengthwise of the laminations to the end of the tooth and into the air gap, where the ventilation is usually fairly good, but the tooth surface exposed is relatively small. In the second place, it can flow back along the laminations to the armature core where it can spread out through a path of much greater cross-section and be conducted partly to the back part of the laminations, and partly transversely to the ventilating ducts. A third path from the armature teeth is across the laminations of the teeth, to the neighboring ventilating ducts. This latter path, however, must necessarily be relatively poor in conductivity per unit section of path, compared with the others, but offsetting this, it is frequently of much greater cross-section and of relatively small length. In passing from plate to plate, the heat must pass through the insulating varnish, or other material used, which is of relatively high heat resistance compared with the iron itself. Nevertheless, in machines with radial ventilation, a very considerable portion of the heat due to the tooth loss is carried transversely through the plates to the air in the ventilating ducts, simply because that is the path of lowest total heat resistance, everything considered. In many cases, the temperature in the core back of the teeth may be as high as that of the teeth themselves, so that the only flow possible is across the laminations to the air ducts, or lengthwise to the tip of the teeth in the air gap. Therefore, the question whether the armature teeth may be hotter than the armature core, or whether the flow of heat is from the teeth to the core, or from the core to the teeth, is a very involved one; and yet upon this question depends, to a great extent, the temperature rise in the buried armature copper. If the armature core is normally hotter than the teeth, and a considerable amount of heat in the teeth is carried away by the buried copper at no load, then it may happen that when carrying heavy load, the heat in the teeth will rise very considerably above the no-load condition, and it may actually so " bank up " that there is still more or less flow from the iron to the copper, even with load. With such a condition, therefore, the outside of the insulation may reach a higher temperature than the inside, while in those cases where the temperature of the copper rises above that of the iron of the armature teeth, the inside of the insulation will be hotter. Therefore, the temperature to which the insulation is liable to be subjected appears to be largely a problem for the designer to determine from his calculations, based upon accumulated data and experience. This is especially the case with very wide armature cores and large, heavily insulated armature coils, such as found in large capacity, high-speed turbo-generators. In such machines, experience has shown that various temperature conditions may be found, depending upon the location and relative values of the losses in the different parts and the means for conducting away the heat. Tests have shown that, in some cases, the armature iron at the center of the core is considerably warmer than the armature copper, while in other cases the opposite is found to be true.

In such apparatus, the temperatures actually obtained are liable to be materially higher than the usual methods of measurement will indicate. These temperatures are inherent in the conditions of design and cannot be avoided economically, in certain types of apparatus, such as turbo-generators. In such machines, the limitations in speed, strength of material, etc., force the designer to certain proportions which preclude larger dimensions, or lower inductions in the iron, or lower densities in the copper, or increased ventilation. In such apparatus, therefore, the development apparently lies in the direction of insulations which will stand the higher temperatures which may be obtained.

These conditions of higher temperatures in some parts of the machine than indicated by the usual tests, have been recognized for years by designers and manufacturers of large electric machinery. A rough indication of these temperatures can be obtained by exploring coils or thermocouples suitably located. However, it is evident that such coils, if located next to the copper, will not give the correct temperature measurement if the flow of heat is from the iron to the copper, while a coil next to the iron will not give the correct result with the flow from the copper to the iron. Experience has shown that the temperatures, in corresponding positions around the core, may not be uniform, due to local conditions. In consequence, it is not practicable to actually determine the true temperatures of all parts of the insulation on commercial machines, except by measurements of a laboratory nature, which would involve such a number of separate readings as to be commercially prohibitive.

On account of the higher temperatures which may be found in such apparatus, and the difficulty of making exact measurements, except by laboratory methods, manufacturers very generally have adopted the use of mica as an insulating material on the buried part of the coils. Experience has shown that such material, when properly applied, can safely stand temperatures of at least 125 deg. cent. How much more has not yet been determined.

Of such machines it may be said that the manufacturer, with his guarantee of 40 deg. cent. by thermometer, actually builds for possible temperatures of 70 to 90 deg. cent. in some parts of the machine, for he expects to find fairly high temperatures in some cases with exploring devices. The usual guarantee of 40 deg. cent. therefore should be considered as only a relative indication of a safe temperature in such apparatus.

If, for instance, the exploring coils should show 70 deg. cent. maximum rise under running conditions, and the permissible ultimate temperature of fibrous or tape insulation is assumed as 90 deg. cent. for continuous operation, then obviously, with air at 40 deg. cent. the insulation would be considered as insufficient from point of durability, except for intermittent service, such as overloads, and such limited conditions. Plainly, the insulation, for such temperatures, should be of mica, or equivalent material, for which 125 deg. cent. has been found to be safe.

Furthermore, it may be stated that with such mica insulation, a turbo-generator which shows 75 deg. cent. rise by exploring coils, or thermocouples, has, in fact, more margin of safety than the ordinary varnished-tape-insulated low-voltage machines of any type, which show 40 deg. cent. rise by thermometer or 50 deg. cent. rise by resistance.

The foregoing aims to bring out clearly that the temperature problem is a most complex one, in all electrical apparatus, and especially so in turbo-generators. It indicates that no simple temperature test can show all the facts, and that all commercial methods must be considered as approximations. It also shows the absurdity of classifying a piece of apparatus as *good* or *bad*, respectively, according to whether it tests possibly one or two degrees below or above a specified thermometer guarantee. Also, following out the above principles on heat flow, various fallacies in temperature measurements might be noted. For example, it is usually assumed that, after shut-down, if a gradually rising temperature is shown, this is a more accurate indication of the true temperature. But this may be entirely wrong as regards windings. If, for instance, the core back of the armature slots is materially hotter than the armature teeth while carrying load, then, upon shut-down, with the air circulation stopped, the teeth will rise to approximately the same temperature as the core back of the teeth, and there may be a flow of heat into the coils, which condition may not have existed while carrying load. A thermocouple on the coil or in the teeth would thus indicate a false temperature rise after shut-down. This is cited simply as one of many instances, to show the possibilities of entirely wrong conclusions which may be reached in the problem of temperature.

The Insulation Problem

The one fundamental condition which must be considered in the insulation problem is the durability of the material itself, and this must be viewed from two standpoints—the mechanical, and the electrical. From the mechanical standpoint, the material may have its insulating qualities impaired by the action of mechanical forces which tend to crack, or crush, or disrupt the material itself, or it may be affected by being permeated by foreign materials or substances, or it may be injured by such overheating as will partially or wholly carbonize it, or render it brittle or otherwise unsuitable for the desired purpose.

From the electrical standpoint, it may be weakened by deterioration of the quality of the insulating material itself or some of its component parts, which may be due to heating, or oxidation, or many other causes.

The effect of mechanical injury, such as cracking, crushing or overheating, on the insulating qualities, will depend upon many conditions. In some cases, with relatively low voltage, any effective mechanical separation of the parts is sufficient for electrical purposes. For higher voltages, continuity of the separating insulating medium is necessary.

Experience has shown that, for moderate voltages, temperatures which may injure, or even ruin, the insulating material, from a mechanical standpoint, may not seriously affect its insulating qualities. Many insulating materials of a cellulose nature will still retain good insulating qualities if maintained at temperatures as high as 150 deg. cent. for such long periods that the material itself semi-carbonizes. Under such high temperature conditions, however, it becomes structurally bad—that is, it may become so brittle that it tends to crumble, or powder, or flake off, and thus its value as an insulation is impaired by displacement of the material itself. In low voltages, therefore, it is not a deterioration in the insulating qualities, but rather a mechanical breakdown of the material itself, which is liable to cause trouble. With high voltages, however, the conditions may be quite different. With some insulating materials, the dielectric strength may be so affected by long continued high temperatures that the insulating quality becomes insufficient. This has a direct bearing on large capacity, high-voltage tubo-generators.

In the problem of insulation, certain difficulties have been encountered in large turbo-generators, which, while they would have developed eventually in other large machines, yet became apparent more quickly and prominently in the turbo type, due to the abnormal conditions in its design. The two most prominent difficulties were, first, that of relatively high temperature in the buried copper, already described, and second, the destruction of the insulation by reason of static discharges between the coils and the armature iron.

Due to the fact that the ultimate temperature reached in such machines not infrequently exceeds the safe limits for insulation of the fibrous or cellulose type, such insulations will show deterioration eventually in their insulating qualities and their durability. In consequence, with the advent of the larger machines, it became necessary to return to the use of mica for insulating purposes on the buried part of the coil. This type of insulation, in the form of mica wrappers, had been used extensively on some of the earlier large capacity, low-speed generators, but it had not been adopted on large turbo-generators, due principally to the difficulty in applying the very long wrappers for the straight part ot the coil. However, when the gradual deterioration of the fibrous type of insulation was noted in large turbo-generators, the mica wrapper type of insulation was again taken up and, after considerable experiment, was applied successfully for the outside insulation on the straight parts of the coils. This use of mica overcame the deterioration in the insulating qualities of the outside insulation; but for a while it was considered that a fibrous type of insulation was still effective between turns in those coils where there were two or more turns in series per coil. As stated before, the insulating qualities of many fibrous materials will stand up astonishingly well under low voltages, when the material is apparently so greatly heated that it is practically carbonized. Therefore, temperatures which did not carbonize, but simply browned, or darkened, the material, had not been considered dangerous, and undoubtedly many thousands of electrical machines of all kinds are today in operation, in which the insulation is in this condition, and in which no trouble need be expected. For this reason, little or no trouble was expected between turns on the turbo-generators. However, a new condition was encountered in large capacity machines, namely, the insulation between turns, when it became dry and brittle at the higher temperatures, was liable to be injured by the terrific shocks to which the coils were subjected in such machines, in case of a short circuit across the terminals. The insulation would be cracked, or so disturbed that short circuits would occur later, without apparent cause. These short circuits on large machines most often appeared as breakdowns to ground, even with the mica wrapper insulation on the outside of the coil. Incidentally, several cases were discovered where arcs had occurred inside the coils between adjacent turns, and where they had not yet broken through the outer insulation to ground. For many months the writer, with his associates, followed up this matter, examining all available coils and windings. Eventually the conclusion was reached that many of the breakdowns to ground had actually started between turns on the inside of the coil. Moreover, as a corroboration, it was noted that in machines with one conductor per coil the breakdowns were practically negligible. This investigation led to the practise of insulating the individual turns, in each coil, from end to end, with mica tape. After the adoption of this practise, it is noteworthy that the breakdowns to ground practically disappeared, although the outside insulation to ground had not been changed in type or thickness.

Many improvements have been made in recent times in the application of this mica insulation. One of these is the Haefely process, developed in Europe, but now being used extensively in this country. By this process, the mica wrappers are so tightly rolled on the coil that practically a solid mass of insulation, of minimum thickness and greatest heat conductivity, is obtained.

By means of the mica insulation, the temperature difficulties in general have been entirely overcome, and a durable and non-deteriorating construction, from an insulation standpoint, has been obtained with the temperatures which appear to be more or less inherent in the large, high-speed turbo-generators.

The second trouble, namely, that due to static discharges between the armature copper and the iron, was also encountered to a certain extent on some of the earlier machines. It was found that these discharges were apparently "eating" holes, or even grooves, through the outside insulation of the armature coils. This effect was most pronounced at the edges of the air ducts and at the ends of the armature core, where edges were presented by the iron. After a long period, the holes or grooves would become so deep that the insulation was weakened or ruined.

This was a very disturbing condition, when it was once fully recognized and appreciated. Again, a comprehensive investigation was made to discover a cure for this difficulty. Various types of machines and windings were examined. It was noted that the action was a function of the voltage of the machine, but was noticeable, in some cases, at relatively low voltages. During the course of the investigations, it was noted that where mica wrappers were used with an outside layer of tape, the " eating away " extended only through the outside wrapping in as far as the mica, and that no apparent effect at the mica was visible. Even when examined with a very powerful microscope, no evidence of any puncture of the mica was found, in any case. These investigations naturally led to the conclusion that the most suitable remedy for the trouble was the use of mica insulation, which was also a remedy for the temperature conditions. This is one of the rare cases in large turbo-generators where two desirable conditions do not conflict with each other. The mica insulation on the buried part of the coil has now been very generally adopted in this country on high-voltage machines, whether of the turbine-driven, or any other type.

This static trouble was considered so serious at one time that low-voltage practise with step-up transformers was adopted by some manufacturers as the safest course, until something positive in the way of a remedy was proved out. This trouble promised to be one of the most serious encountered in high-voltage generator work, and even threatened to revolutionize practise in winding generators for the higher voltages. However, as consistently advocated by the writer, the use of mica, suitably applied, appears to have entirely overcome this trouble, as evidenced by several years' experience, and all indications now are that there need be no fear of static discharges on windings of 11,000 and 13,000 volts. Even in the 11,000-volt New Haven generators with one terminal grounded, which gives the equivalent of a 19,000-volt, three-phase, winding with the neutral grounded, the mica insulation appears to be successful and durable.

Rotor Insulation

In most of the early turbo-generators, the rotor winding in the slots was insulated with fibrous material, " fish paper " and " horn " fiber having the preference. One of the difficulties in the rotor is that the insulation between the winding and the slot is liable to be crushed or cracked by the high centrifugal forces. In the earlier insulations, before fish paper was used, it was found that even at very moderate temperatures the insulation got dry and brittle, and cracked readily. Fish paper, or horn fiber, was then adopted pretty generally. Such material apparently stood much higher temperatures than the ordinary fibrous insulations. However, experience also showed that eventually this also became brittle, and was liable to be cracked, and then displaced, due to the centrifugal forces. There is always the possibility of a small amount of movement in the field coils when a machine is being brought up to speed, and this movement, in itself, may eventually damage the insulation if it is at all brittle.

As the capacities and speeds of turbo-generators were increased and the space limitations for the rotor windings became more pronounced, the resulting higher normal temperatures led to the adoption of mica for the insulating material in the slots with either mica or asbestos for the insulation between turns. As the voltage between adjacent turns is always extremely low, what is needed is really a durable separating medium, rather than an insulation, this medium being one which will not become crisp or brittle at fairly high temperatures. Asbestos has served for this purpose very effectively, and even has some advantages over mica, as the latter must be applied in relatively small pieces in the form of strap or tape, and the individual pieces are more readily displaced or shifted than is the case with asbestos. Some very severe tests have been made in order to determine the possibilities of such rotor insulation. In one case, a turbo rotor thus insulated was run at full speed for over 40 hours, with such a current that the rise by resistance in the rotor copper was about 250 deg. cent. It was the intention to continue this test very much longer, but the conduction of heat from the winding to the core, and thence through the shaft to the bearings, was such that finally the bearings became overheated and gave out. After this test, the winding was carefully dismantled, and no evidence of any injury to the insulation could be discovered. Of course, such temperatures are not recommended in turbo rotor practise, but this was simply an attempt to find a temperature limitation. If a designer wants to find the facts in any apparatus, he will obtain the most valuable information if he operates the apparatus up to the point of destruction. He thus fixes a limit which he must keep below.

The use of mica, or mica and asbestos, on turbo rotors has been very generally adopted in this country at the present time, and it may be said that, within the writer's experience, no case of destruction of one of these windings through heating has come to his notice, although a great number of them have been in service for a relatively long time. In many of the older machines with fish paper insulation in the rotors, the conditions of ventilation and the normal ratings of the machines were such that the maximum temperatures in the rotor windings were relatively much less than in present practise. It may therefore be said that the use of mica in the rotor has been largely due to the introduction of the larger capacities and higher speeds.

Losses in Turbo-Alternators

The total iron and copper losses in a large, high-speed turbo-alternator are in general no higher than in a corresponding capacity low-speed machine.

As far as the iron losses are concerned, no further comment need be made than that the magnetic flux densities in general are somewhat lower than in lower speed machines of the same frequency, and therefore the losses per unit volume of material are no larger.

The total armature copper losses in turbo-alternators, as a rule, are considerably smaller than in corresponding capacity machines of the moderate or low-speed types. This is due partly to the use of a smaller total number of conductors, and partly to a lower current density in the armature conductors. As brought out before, in a narrow core machine, a considerable portion of the buried copper heat may be conducted lengthwise of the conductor into the end winding, and there dissipated into the air. In the turbo-generator, with its much wider core and greater distance from the buried copper to the end windings, a smaller percentage of the buried copper heat will be conducted into the end windings. To partly compensate for this, it is usual to work the armature copper in the turbo-generators at a lower current density, and therefore at a relatively lower total copper loss. This is somewhat of a handicap in the economical design of the generator, as extra space is thus required for the armature winding. In some of the earlier machines, the armature conductors were made of solid copper bars of relatively large section, partly for stiffening or bracing the end windings, as will be referred to later. With these solid conductors there was a very considerable loss in the buried copper due to eddy currents. To compensate for this, the armature conductors were made very large in section, so that the current density, due to the work current alone, was very low compared with practise in other types of machines. On account of the comparatively large section of armature conductors, the conduction of heat from the buried copper to the end windings was relatively large. In some of these earlier, large capacity machines, the nominal current density in the armature conductors was so low, and the section of conductors so great, that the total buried copper loss, due to the work current, could be carried from the buried part of the coils into the end windings with a comparatively small drop in temperature, so that, if there had been no eddy currents present, the buried copper would have shown less rise than the iron. Any considerable rise which occurred was thus chargeable to eddy currents in the buried conductors, rather than to the work current. While such construction was fairly effective for the purpose, yet it was decidedly uneconomical in design, as indicated before. In fact, with later proportions and methods of design, the safe outputs of some of the earlier machines could easily be 50 to 75 per cent greater, largely on account of elimination of eddy currents and improvement in methods of dissipating heat from the end windings. In many of the older machines, the ventilation of the end windings was not nearly as effective as in modern types, due principally to the form and arrangement of the end connectors. Usually air spaces were allowed between adjacent coils, although, in some instances, these were so small as to give but little benefit. Moreover, in many cases, the type of end winding employed rendered these air spaces between coils rather ineffective, unless special means were taken to deflect the air between the coils. With later constructions, the end windings lie more or less across the path of the ventilating air, and there are ample openings between the coils, so that a very considerable part of the ventilating air will actually pass between the coils of the end windings in such a way as to give the maximum possible ventilation. When it is considered that the total armature copper loss may be only 20 per cent of the total stator loss it will be seen that an excessive amount of air is not required when the end windings are properly arranged for most effective ventilation.

Much effort has been expended in eliminating or reducing the eddy current losses in the buried copper of large turbogenerators, as well as in other types of large capacity alternators. These eddy currents are due to two sources, namely, the alternating magnetic flux across the slots due to the armature ampere turns per slot, and secondly, the magnetic fringing from the rotor pole face into the open armature slots. In some instances, tests have indicated that the local e.m.fs. set up in the armature conductors by the flux through the slot opening are very considerably greater than those due to the flux across the slot. Obviously, with partially closed slots, this fringing into the top of the slot should be practically absent.

The simplest remedy for the eddy currents set up by these local e.m.fs. is to subdivide the conductors into a number of wires or conductors in parallel, so arranged or connected that the local e.m.fs. oppose and to a great extent balance each other. This opposition may be obtained by special arrangement of the conductors in each individual slot, or parallel conductors in the two halves of a complete coil may be connected in opposition to each other. Some of these arrangements do not completely balance the opposing e.m.fs., but they include the resistance of the complete coil in the eddy current circuit, so that the eddy losses are not only very materially reduced, but they are distributed over the entire coil, including the end windings, which condition, in itself, represents a very material improvement.

Protection Against Fire

An important problem connected with the insulation of large turbo-generators is found in the fire risk, or danger of destruction of the end windings due to starting an arc at some point. On account of the tremendous ventilation in such machines, a fire, if once started, may quickly ruin the entire end winding. An extended investigation was made, with a view to providing an insulation which would not burn rapidly. Among other tests, the end windings were finished on the outside with an asbestos covering or tape. However, such tape requires some sort of sealing varnish, or material to fill its pores, to keep it from absorbing moisture or oil. The tests showed that if a fire was once started, combustion would be maintained by the gases liberated by the " gasification " of the varnishes and other material in the end windings, whether the coil was covered with asbestos or not. No covering which was tested appeared to be very effective. Although some outside covering might be found which would be slightly effective in preventing fire from starting so readily, yet, if once started, it appears that a fire can very easily maintain itself in such machines. Eventually, the conclusion was reached that the safest course would be to provide suitable closing doors or valves in the air inlets to completely shut off the incoming air to the machine. In addition, suitable doors on the air outlets, where they can be applied, should also be helpful, by retaining the smoke and burnt gases inside the machine, which thus assist in smothering the flames. The use of fire extinguishers of the gaseous type will usually be rather ineffective, unless the incoming air and ventilation is practically cut off. For instance, with 60,000 cu. ft. (1698 cu. m.) of air per minute passing through a large machine, the addition of a little gas for extinguishing the fire would hardly make any impression. In one instance, in attempting to extinguish a fire, an effort was made to feed the gas in against the ventilating pressure of the fans. Obviously, this would not work, and then a hose was used in order to get enough pressure to counteract the fan action. Although the fire was extinguished, the resultant effect of fire and the high pressure water was that new insulation was required.

Regulation and Short-Circuit Characteristics

It has been known for many years to designers, that alternating-current generators can give, at the instant of short circuit, a much greater current than that which they will give on continued short circuit. The first emphatic evidence of this, in the writer's experience, was in connection with the first Niagara generators in 1894. Upon short-circuiting one of these machines at full speed and normal voltage, the results indicated a current rush so great that it was apparent that it was limited only by the armature self-induction, and not by the so-called synchronous reactance. Later, after being put into actual commercial service, it was found necessary to brace the end windings on these machines. However, at that time, no suitable instrument, such as the oscillograph, was available for determining the conditions on short circuit, and the phenomena did not permit of much experimental investigation.

Similar evidence was found from time to time, as in the first Manhattan Elevated engine type generators, which bent their end windings out of shape on a dead short circuit. But the real possibilities for trouble in this matter did not develop until the large capacity turbo-generators came into use. In these machines, the armature ampere-turns per pole are so high, compared with moderate speed alternators, that the stresses due to the stray magnetic fields on short circuit are much greater than the natural rigidity of the end windings will withstand. The manufacturer of such apparatus, without data of any quantitative value at hand, did not fully recognize the real weakness in the end windings until disaster overtook them. Even then it was a long and difficult undertaking to overcome the trouble. All kinds of designs of end supports and various arrangements of end windings were tried, with more or less success. But each new step in the increase in capacity opened up the problem again. It was soon noted that those armature windings which were made up of cable or small wires, suffered most on short circuit, and for a while there was a tendency on the part of some manufacturers to use heavy, solid conductors to give rigidity in the end windings. This was effective within certain limits, but was very expensive from the design standpoint, as, on account of eddy currents in the buried copper, it was necessary to work at a very low current density, which was not economical in winding space.

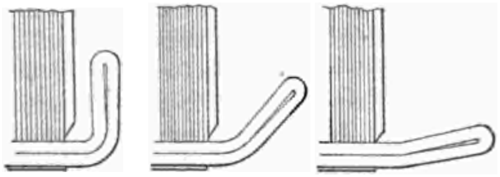


Fig. 20

In this country, the types of armature windings finally narrowed down to the open-slot construction, usually with an upper and lower coil per slot, with the end winding arranged in two layers, similar to d-c. armature windings, or the common induction motor primary windings. This turbo end winding was extended at various angles to the axis of the machine from almost parallel up to 90 deg., as shown in Fig. 20. The principal survivor of these types is one which extends at some angle between 30 and 60 deg. to the axis. There are several reasons for this—first, it allows a very substantial bracing to be applied to the end windings. Second, the stray fields around the end windings do not, to any extent, cut the adjacent solid parts, such as the end housings, stator and end-plates, etc. An angular position of approximately 45 deg. seems to be a good compromise on these points. Ample supports, as shown in Fig. 21, can be applied for bracing the windings against movement in any direction. Such end windings are usually braced against metal supports attached to the stator end-plates. The coils are so clamped to the racks, and are so braced against each other that the windings will sustain a dead short circuit across the terminals, even in the largest capacity machines, without injury.

On some recent large turbo-generators, the end windings have been further strengthened by double metal racks between the two layers of windings, so arranged as to key these two layers securely to one another at certain points. Moulded mica troughs are placed around the coils as an extra insulation from the metal racks. By this keying of the two layers to one another, the winding as a whole is stiffened, quite irrespective of any other clamping arrangement. In fact, this is practically equivalent to putting the end windings in rigidly held slots, thus approaching the conditions which obtain in the buried part of the coil.

In order to limit the momentary short-circuit current, the armature reactance is now usually made as large as the condition of the design will permit. This naturally means high ampere-turns per pole, which in turn means high synchronous reactance, and consequently poor inherent regulation of the machine, especially on inductive loads. This can be illustrated by the following example: Assume a 5000-kw. unit of an earlier design, which can give 25 times full load current on momentary short circuit. By certain improvements in the design of the armature coils, such as the use of deeper slots, better subdivision of the copper to eliminate eddy currents, improved ventilation and conduction of heat, etc., the capacity of the machine is assumed to be increased to 10,000 kv-a., the number of armature turns remaining the same as before. It is evident that when short-circuited, the revised machine will give the same total current as on the former rating, which, however, is only 12½ times the rated current on the new capacity basis. Obviously, the end winding stresses are no greater than before, although the nominal capacity has been doubled, and if it were possible to brace the end windings satisfactorily with the former rating, the same bracing should be effective on the new rating. This illustrates, roughly, what is taking place in later designs, although the steps in the change may not be just those mentioned. Again, in the above example, it is obvious that, with the new rating, the inherent regulation at full load is the same as at 100 per cent overload on the old rating, which means that it is relatively poor. Another way to express this is, that the old rating might give 2½ times full load current on steady short circuit, while the new rating gives 1½ times.

This condition of poorer regulation is inherent in the newer practise, but is apparently acceptable to the users of such apparatus, for a variety of reasons which do not come within the province of this paper.

Conclusion

The foregoing covers, in a general way, many of the problems encountered in large turbo-generators, and defines the situation as it stands at present.

It may be suggested, in connection with the temperature problem, that the high temperatures obtained are due to forcing the construction too far; but, in answer, it may be stated that it is forced no further in this feature than in many others. The whole design has been carried far beyond the most economical construction, from the generator standpoint alone. In fact, the whole machine is more or less a compromise between desirable conditions as a generator, and most economical conditions as part of a combined turbine and generator unit. It may be added that the ultimate limits in construction and capacity will be obtained only when the steam turbine conditions are satisfied, and there are indications that possibly this result is being approached now with the present high speeds.

There is one small consolation in all the confusion of development which has attended the turbo-generator work, in the few years it has been with us, namely, the question of choice of speed has been practically eliminated. For 25 cycles, there remains only one speed, namely 1500 revolutions, with two poles, from the smallest unit up to 25,000 kv-a. as a possible upper limit. For 60 cycles, up to 5000 kv-a., two-pole machines at 3600 revolutions are being furnished, while from this capacity up to 20,000 kv-a. four poles may be used.

It will be evident to any reader of this paper that the designers of large turbo-alternators have had a strenuous time during the past few years—very much more so than is indicated herein, for their successes rather than their failures have been discussed. In fact, much of the time they have been working ahead of their data and experience. In presenting this situation from the design point of view, it is hoped that a better and clearer understanding of the turbo-generator problem will be obtained by all who are interested in such apparatus.

Discussion on "High-Speed Turbo-Alternators—Designs and Limitations" (Lamme), New York, January 10, 1913.

Henry G. Reist: The author has treated this subject so fully that it leaves even those of us who have spent a good part of our lives on this work very little to add. He has weighed the different methods of construction so carefully, accurately and fairly that there is very little room for controversy, so what I have to say will be very brief and a part of it, at any rate, will be in the way of a slight further explanation of some of the methods of construction. The author limits the size of a two-pole machine of 3600 rev. per min. with a through shaft to about 600 kv-a. That is probably near the limit, if a solid shaft is used; but by the use of a flexible shaft, that is, a shaft which allows the rotor to run above the critical speed, the limits of output may be very materially increased. This is similar to the operation of the de Laval turbine, which in starting runs through a preliminary stage where it is not quite as steady as afterward, and then gets down to a perfectly quiet state. This construction does not require so large a shaft as if the machine were operated below the critical speed, and it is possible considerably to increase the limit of size. There are today a great many machines running, constructed on this plan, of from 1000 to 2000 kw. in capacity, at 3600 rev. per min.

The virtue of radial slots in rotors is well brought out in the paper. As the author points out, the stresses in the steel part of the rotor are all in tension, because the parts are radial, and there is no bending stress in the teeth; the radial slot design also allows the copper to be placed with the broad side against the next turn, so that no stresses come on the edges or corners of the copper. This is particularly advantageous where the ends of the coils are carried by a steel end ring. By having the ends of the coils radial, there is no side strain and comparatively little blocking is required to hold the coils securely in place.

Another point I might mention is that while a high grade of steel is used for making the weldless rings for supporting the windings, there does not yet seem to be any opportunity of making use of the higher grades of steel available, for the reason that the modulus of elasticity of the various grades of steel is about the same, and with a given stress in the ring the elongation will be approximately the same, whatever grade of steel is used. In order to avoid the ring loosening at high speed due to elongating, it should be placed on the rotor with a stress equal to, or greater than, that to which it will be subject while it is running. This is usually accomplished by heating the ring and shrinking it on. To enlarge it to the diameter it will have while in operation, requires heating to a temperature likely to injure the insulation while placing it into position. For this reason it is not desirable to go to the extreme in the stress which might be carried on these parts with the materials at hand.

As the author points out, one of the big problems in connection with the construction of turbo-generators is ventilation, and this is generally accomplished by subdividing the parts, and getting the air to move as rapidly over as large a surface of as many of the parts as possible. I agree with the author that it is really surprising what good results are obtained in this way.

The static discharge on coils is a trouble that is more prevalent than we generally appreciate—this corona appears at much lower potentials than we ordinarily suppose, with thick as well as with thin insulation. But, fortunately, it usually does not do much damage, for the process of oxidation is so slow that it extends over a very long time. As far as my experience goes, I do not recall any machine that was badly affected, that gave serious trouble, in a period of, say, less than six to ten years—and this with high-potential coils, 12,000 and 13,000 volts, and in some cases with fabric and varnish insulation. Probably if the coils were hot, the injury from this source would be very rapidly increased, since the corona produces an active form of oxygen which attacks the insulation chemically, and we know that chemical action is much more active when parts are heated than when they are cool.

Mica insulation, as it is made up for use for electrical purposes, is a composite material. It is built up of paper, usually, and a varnish, generally shellac, and mica. A good part of the thickness of the insulation consists of other material than mica, in many cases as much as 50 per cent. These other materials, as we use them at present, are similar in behavior to cotton cloth or varnish. That is, they do not resist high temperature and they will char. So that even a mica insulation, as built up, will have parts of it, that is, the interstices between the pieces of mica, char in subjecting it to high temperature, and we may expect that the corona will, even with mica coils (since they have the other combined materials in them) attack the mica insulation very much more rapidly if it is hot than if cool, because the other insulations will char. I think that some time we shall overcome this, because we shall probably obtain a varnish, perhaps a synthetic varnish, or wax, or gum, that will not be affected by the temperature at which our ordinary organic materials char. Then probably it will be advisable to go to higher temperatures, but until that time arrives I believe it is well to confine our heating, so far as possible, to points below 100 deg. cent.

In conclusion, I want to say just one word in regard to regulation, and that is, there does not seem to be any great advantage in good inherent regulation. I think I may say that this applies to almost all classes of alternating-current generators, because the best regulation that we can produce in any machine will not be good enough for commercial use in lighting, so that it is desirable to maintain constant potential either by hand or automatic regulation; the latter, of course, being ordinarily much better.

Now, if we have to regulate, we might just as well have poor inherent regulation of the generator, to obtain the various advantages, such as lower cost, higher efficiency, and particularly on the machines that we are discussing, much better protection against injury from short-circuit, so that we should practically eliminate the idea that we care about any special degree of regulation in this class of machine.

R. B. Williamson: As Mr. Lamme has pointed out, the design of a turbo-alternator is essentially an abnormal one from the electrical point of view, and the high speed combined with large output is a necessity imposed by the steam end of the unit. It is generally recognized that for a given output there is a certain speed at or near which the most economical design is obtained. Such conditions are frequently approached in water-wheel generators of large output. In turbo-generators the speeds are far beyond the point best suited to economical design and these excessive speeds are indirectly responsible for the difficulties encountered in the way of heating, ventilation, etc.

The coils in the radial slot type of rotor do not interfere with the shaft in either the two-pole or four-pole machines. So far as the coils are concerned, there is nothing to prevent the use of a through shaft, whereas in the two-pole parallel slot type a through shaft cannot be used unless some of the copper space on each side of the rotor is sacrificed (see Fig. 10 of the paper). Bolted-on stub-shafts therefore have a special advantage in this type of rotor. However, the possibilities of the radial slot rotor with a through shaft lie much beyond the limits stated on the second page of the paper. In the case of 3600-rev. per min., two-pole machines, generators of 1250 kv-a. output with through shaft have been built and successfully operated.

In large 25-cycle two-pole machines, a generator of 12,000 kv-a. output having a through shaft has recently been built and tested. A number of 60-cycle 1800-rev. per min. generators ranging from 5000 to 10,000 kv-a. have been built with through shafts and are in successful operation. Of course these machines operate above their critical speed, and it has been found that such rotors are easier to balance and, if anything, run more smoothly than rotors of similar design that operate below their critical speed.

As regards peripheral speeds, these are abnormally high at best, and the lower they can be kept without interfering with the design in other respects, the better will be the result. In the past, peripheral speeds have been pushed higher than necessary in some machines, and better results have been obtained by improving the ventilation and dropping back to smaller diameters and greater axial length. A marked reduction in windage loss with corresponding increased efficiency has thus been obtained.

As Mr. Lamme has clearly pointed out, the problem of ventilation is by no means an easy one, since the volume of air to be handled is so large and the space in which the heat is liberated is so limited. So far as the rotor is concerned, the statement on page 13 regarding heat dissipation from the rotor surface is fully borne out by numerous tests. If the rotor winding is designed so that the heat can readily pass from the copper to the iron and thence to the outer surface, it can be removed by the scrubbing action of the air, which is very effective at these high peripheral speeds. Air ducts in the rotor, particularly radial ducts, are of doubtful advantage. They are usually much restricted at the roots of the rotor teeth and as some oil vapor always gets into a machine sooner or later, the dirt sticks in these small openings and shuts them off. The duct thus becomes a dead air space, which is worse than the solid metal so far as getting rid of heat is concerned, although when the machine is new it may be fairly effective. The loss in the rotor is not large, and if an ample supply of air is provided at the surface the scrubbing action will remove the heat, and the ducts are not necessary so far as the rotor itself is concerned.

The disadvantages of the circumferential method of ventilation mentioned near the bottom of page 10 have been entirely overcome by providing a sufficient number of multiple paths for the air currents in the stator. This shortens the paths and decreases the amount of air to be passed through any one section back of the teeth, thus reducing the velocities in the ducts. The plan shown in Fig. 14, which may be termed a two-path scheme, was satisfactory for machines of moderate output, and by carrying Fig. 16 still further and making say a 6-, 8- or even 12-path arrangement, very even and effective cooling is obtained, which enables long machines to be built which will be well ventilated at the central part. In this plan there is practically no interference of air currents, particularly when the rotor is made without ducts, and as the air blows radially inwards at several points against the surface of the rotor, the latter is well cooled.

In some cases the use of a separate blower may be desirable for large units, but by careful design a fairly good efficiency can be obtained in turbo fans, and it is a question if much is to be gained on the score of efficiency, especially when it is considered that separate blowers would have to be driven by a separate motor or engine, whereas the turbo fan derives its power directly from the turbine spindle. Moreover, the large air ducts and piping system required for separate blowers might prove objectionable in some cases.

It is pleasing to see the importance of air filters or air washers emphasized. Cloth air filters have been installed to some extent in the past, but they have often been worse than useless, as they have had insufficient area of cloth and the filters have soon become very dirty and clogged up and shut out the supply of air from the machine. A filter of this kind must have a very large area and consequently low air velocity. By arranging the cloth in zigzag fashion on suitable frames, an efficient filter can be put into small space and built up in connection with the turbine foundation so as to form a very compact arrangement. A large filter made in this way has been in operation for a year and a half, and has such large area that it has not required cleaning during this time. In this installation there are two filters, each handling between 30,000 and 40,000 cu. ft. per min.

The matter of insulation is closely connected with the internal temperatures attained in the coils. Unquestionably most of the breakdowns in the stators of turbo-alternators have been between turns rather than to ground, and mica insulation between turns is desirable. Mica should also be used liberally in the slot portion of the coil, but, unless the machine is operated at temperatures in excess of 90 deg. cent., there is no objection to using varnished fabric in combination with the mica to give the necessary insulation in case the mica wrapper should become accidentally cracked.

The trouble due to static discharge perforating the outer layers of insulation next the iron is not peculiar to turbo-generators. The same effect has been noted on coils from a 6600-volt engine-type generator. In this case the outer wrapper or trough of fishpaper put on the coil for mechanical protection was literally riddled with pin-holes, while the varnished cloth insulation immediately underneath was in perfect condition, as was also the mica wrapper next to the copper. In a paper read in 1911 before the British Institution of Electrical Engineers by Mr. F. P. Fleming and Mr. R. Johnson, this effect is described and shown to be due to the fact that in a composite insulation of materials having different specific inductive capacities, it is possible to have a condition where the potential gradient across part of the insulation may exceed its disruptive strength, therefore perforating this part without affecting the rest of the insulation. It was also shown that this perforation might be caused by the double-voltage puncture test usually applied to machines.

So far as losses in turbo-generators are concerned, they are, with the exception of the windage loss, no higher than in other generators, and in some cases they are less. With a well-distributed rotor winding and with moderate peripheral speed, the core loss, assuming equal working densities, will be a lower percentage than in salient pole alternators. The stator and rotor copper losses will also be smaller, while the stray loss will be higher, the latter being due partly to the enclosed construction which allows stray flux to get into parts that are unlaminated.

Philip Torchio: I am asked to make some remarks from the standpoint of the user, and I may state that this is a point of view that sometimes escapes the designer. For instance, on page 6 Mr. Lamme says: "To avoid magnetic shunting of the field flux, this driving head must be made of non-magnetic material, usually of some high grade bronze...this makes a good strong construction, but is necessarily rather expensive, due to the bronze driving heads. As these cost but little more for a long rotor than for a short one, the construction therefore tends toward relatively long, small diameter cores in order to lessen the relative dimensions of the bronze heads." That makes the machine longer, it may be one foot or three feet longer, and consequently the engine room one foot or three feet longer. It saves a few dollars on the bronze heads and necessitates our spending a few thousand in having a larger station. In many cases, space requirements are very important to us, and oftentimes it means that we have to sacrifice many thousand kilowatts simply for the reason that a larger machine cannot be put in a certain space because it is four or five feet too long.

As to the question of having outside blowers for ventilation, one of our allied companies is about to install some of these machines that Mr. Lamme has mentioned, and we were satisfied, because of the reasons given by Mr. Lamme and his associates, that outside blowers would be satisfactory; to facilitate their operation we have provided means through which, when the machine field switch is closed, the blower will be started automatically by a motor fed from some proper supply. I do not think that the cost or the efficiency of the system will be materially different; if anything, it will be a little better than having the blowers and the ventilating apparatus in the rotor itself, as Mr. Lamme states. If by adopting outside blowers you can reduce the size of the machine, I would emphasize to the manufacturers the desirability of doing that for large units.

At the bottom of page 24, Mr. Lamme states: "Of such machines it may be said that the manufacturer, with his guarantee of 40 deg. cent. by thermometer, actually builds for temperatures of from 70 to 90 deg. cent. in some parts of the machine, for he expects to find fairly high temperatures in some cases with exploring devices." I do not like that statement. We have been trying and failed to get the manufacturer to put exploring coils in the windings of the machine so that we could read them while we are operating the machine and see where the temperature goes. Now, if the safety of the insulation is limited within ranges below 100 deg. cent., and the room temperature may be 40 deg. or more, one cannot exceed 50 deg. maximum at any one point; it would not be safe at the present time to do it. In fact, if we do not have more troubles it must be that we do not carry the overloads or obtain the output that we should get from the machines, but which we may require at any moment.

The author says on page 25: "It also shows the absurdity of classifying a piece of apparatus as *good* or *bad*, respectively, according to whether it tests possibly one or two degrees below or above a specified thermometer guarantee." If I have a contract to pay $1,000, and give $900, the $900 may be perfectly good, but I will be short $100 of the amount which I must pay. The user has been buying in accordance with the thermometer readings, incorporated in the printed specifications of manufacturers. The standard should evidently be changed.

I am very much interested in the subject of the mica insulation, and I was a little disturbed by the previous speakers and Mr. Reist, particularly, who suggested that there may come another type of insulation to try upon us, as a better substitute for mica. My experience with mica insulation is too limited now to have any definite report to make, though the reason given by Mr. Lamme, that mica, being an inorganic substance, is not attacked by the oxygen, seems plausible. It is of general importance to know how to screen or to avoid ozonizing. I have had some experience in this matter, having had rather serious troubles on braided cable laid on imperfect ground. We found, for instance, that rubber insulation, which is more easily affected by chemical action, would stand the least, and we substituted paper or fabric, although I think it is undesirable—I do not speak of the interior of the machine, but the outside— to use a braided cable without a definite ground around it, like a lead sheath, because ozonizing is likely to take place at any time and under uncertain conditions

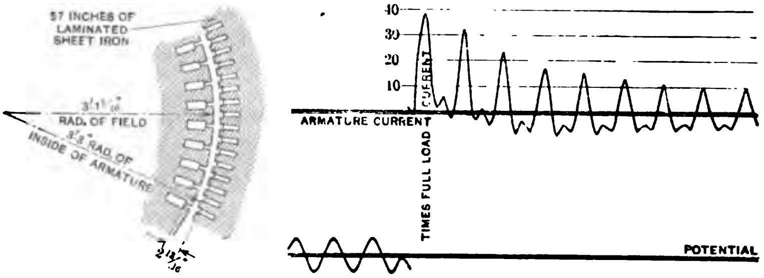


Fig. 8

over which one has no control, and by so doing one jeopardizes the protection of the apparatus. If the mica acts as a perfect screen, it might help in the cable manufacture and possibly in taping cable ends and splices.

I present seven illustrations giving a photographic reproduction of the very rapid deterioration of rubber produced by ozonization at moderately high voltage stresses upon the dielectric, having air insulation in series.

The experiment was made upon a sample of three-conductor cable with 5/32-in. (3.97-mm.) rubber insulation around each conductor. Ends were opened out about four inches between centers, leaving an air space between each conductor varying from zero at the apex to about 4 in. (102 mm.) at the ends. Voltage was applied between two conductors only. Fig. 1 shows conditions before voltage was applied, and Figs. 2 to 7 show successive stages after voltage was applied 40, 50, 60 and 70 minutes. Smaller cracks appeared after 20 and 30 minutes, but could not be photographed. Breakdown occurred after

FIG. 1—BEFORE TEST TORCHIO]

FIG. 2—18,000 VOLTS FOR 40 MINUTES [TORCHIO]

PLATE V
A. I. E. E.
VOL. XXXII, 1913

Fig. 3—18,000 Volts for 50 minutes [Torchio

Fig. 4—18,000 Volts for 60 minutes] [Torchio]

[TORCHIO]

FIG. 5—18,000 VOLTS FOR 70 MINUTES

[TORCHIO]

FIG. 6—PUNCTURED AFTER 70 MIN. 30 SEC. AT 18,000 VOLTS

[TORCHIO]

FIG. 7—CONDUCTORS SPREAD TO SHOW FAULT

70 min. 30 sec. It was shown that the rubber deteriorated at a point where there was about 0.4 in. (10.16 mm.) of air between the rubber surfaces, extending in each direction to points including 0.25 in. (6.35 mm.) and 0.5 in. (12.7 mm.) of air.

Similar results occur at considerably lower voltages, as low as 4000 volts, at correspondingly longer periods of electrification, when braided cables are not properly supported by insulators, but are laid on imperfect or intermittently imperfect insulators, like ducts, concrete, etc. Under the latter circumstances, which one should always try to avoid, the usual remedy, whenever the induced sheath currents are not too great, is to cover the cables with lead sheath, in which case the metallic sheath completely protects against ozonization.

I would like to ask Mr. Lamme if a similar solution might not be possible for high-voltage armature windings, making them metal-covered, so as to use fabric insulation while positively eliminating the trouble of ozonization affecting it, and at the

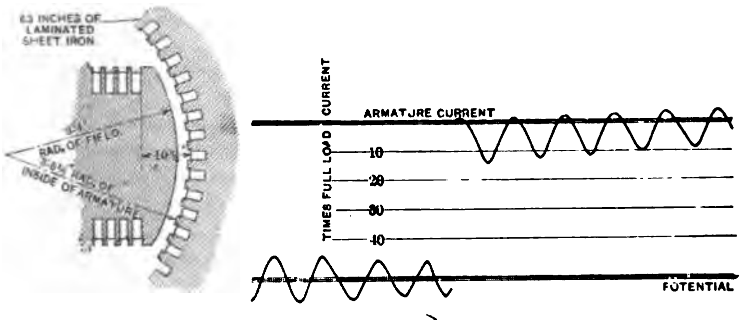


FIG. 9

same time improving dissipation of heat from the windings through the metallic sheath.

As to regulation of machines, designers might like to have definite information of what the results of radial slots and parallel slots would be. Figs. 8 and 9 refer to two 25-cycle, 8000-kw., 750-rev. per min. turbo-generators. Fig. 8 shows a radial slot machine with a distance between the armature and the field copper of 2 13/16 in. (71.4 mm.), and its corresponding oscillogram under short-circuit, indicating a maximum current 38 times the full-load current of the machine. With the parallel slot machine, of the same size, same frequency, same speed, but the distance between the copper of the armature and field 10¾ in. (273 mm.), the maximum current is about 14 times the full-load current of the machine. The short circuit took place at a voltage a little higher than zero; if it had been zero the current might have been a little higher, 15 or 16 times. These results show a great difference in the reactance of the two machines, due to the difference in the construction of the field.

C. J. Fechheimer: With few exceptions the limitations imposed by the rotor of the turbo prevent our reducing the size of the machine. In designing a turbo-alternator we first proportion the rotor and then place (on paper) the stator around it; whereas in designing low-speed alternators, we usually design the stator and then place a suitable rotor within it. It is of course essential to choose between the possible types of construction.

The limitations appearing in the rotor are:

(1) Stresses. (2) Temperature. (3) Cost. (4) Regulation. (5) Ability to maintain voltage.

A good design is necessarily a good compromise between these five factors.

Modifying any one of these factors will usually affect the remaining four. For example, we can, by increasing the stresses, reduce the cost of machine, lower the temperatures and possibly cause the machine to give better regulation and enable it better to hold up its voltage. Just how far any one of these factors should be carried at the sacrifice of the others is what the designer must decide.

From the standpoint of safety, stresses should be treated most conservatively.

Second in importance is the matter of temperature, for if the critical charring temperatures of fibrous insulation are exceeded, the machine will burn out. Even though, as Mr. Lamme points out, we were to adopt mica and asbestos in preference to fibrous material, there is still a question as to how much the life of the machine would be impaired by excessive heating. For example, we all know that iron will age if allowed to heat and cool alternately (this operation being repeated numerous times), the effect being that the efficiency would be reduced and the machine would heat still more. Furthermore, as stated by Mr. Lamme, it is advisable to fill the pores of asbestos material with inflammable varnish which, if subjected to excessive temperatures, will deteriorate with most undesirable effects.

We cannot afford, in commercial designs, to ignore the cost of the machine; and while for certain ratings we are so limited as to have very little choice, for the majority of conditions we have a larger selection, and therefore can materially affect the cost by wise or unwise proportions. In this connection it is essential for the designer to use as much foresight as is possible, for he must usually so proportion alternators that the principal parts can be used frequently in future machines without great modifications in patterns or dies.

Whereas regulation was considered to be of considerable importance about eight years ago, when machines were of smaller capacity than at present, we are feeling more and more that regulation, especially in large generators, can easily be sacrificed to gain in other respects. However, we must always bear in mind that even though we employ good regulators to main tain constant voltage on one of the phases, there is a probability of having the voltages badly unbalanced if the load be different in magnitude or power factor on the various phases. This will be more especially the case if the inherent regulation of the alternator be coarse.

Mr. Lamme implies on page 36 that in some cases the rating of the alternator can be doubled, provided we can keep the stator cool enough, and have $1\frac{1}{4}$ instead of $2\frac{1}{2}$ times normal short-circuit current. We must remember that if we keep the number of turns in the stator the same, and increase the rating, the effective drop in the stator increases in proportion to the current, so that if the generator just maintains voltage for a given excitation at a certain power factor at the low rating, it would not be able to maintain its voltage at the higher rating, other conditions being the same. Hence, it may be essential to use more turns in the stator for the higher rating, and this will increase the drop still more. We can go so far that an increase in capacity for a given number of rotor ampere-turns will be impossible if the machine is to maintain its voltage at a certain load and power factor. The service to which many alternators are subjected is such as to require them to maintain their voltage at 80 per cent power factor, and since we have been accustomed to rate machines for normal and for 25 per cent overload, the full field conditions should be such that the alternator may maintain its voltage at 25 per cent overload, 80 per cent power factor. This is nearly equivalent to the maintenance of its voltage at normal kilovolt-ampere load, zero power factor. We may then calculate what will be the maximum capacity for which an alternator can be rated, if the full field corresponds to zero power factor, and normal kilovolt-amperes.

We shall endeavor to show what the maximum output is, using the following symbols to indicate the various quantities:

$A\,T$ = full field ampere-turns.
a = number of active conductors per slot.
E_i = internal voltage.
E_e = external voltage.
I = current.
n = number of phases.
P = kilovolt-ampere output.
p = number of poles.
R = reluctance of magnetic circuit.
S = number of slots per pole, per phase.
$\Sigma\,\lambda$ = equivalent of stator leakage paths.
ϕ = magnetic flux per pole.
X = stator leakage reactance per phase.
$\sim$ = frequency.

The internal voltage, if a sine wave be assumed, neglecting the pitch and distribution factors, is

$$E_i = 2.22\,\phi \sim S\,a\,p \times 10^{-8}$$

The flux per pole is

$$\phi = \frac{(A\ T) - \dfrac{a\ S\ n\ I}{2\sqrt{2}}}{R},$$

where the second term in the numerator is the armature reaction per pole. R includes $\frac{4\pi}{10}$ (3.19 for inch system).

At zero power factor the armature resistance drop is negligible, and we may therefore write

$$E_s = E_i - I\ X$$

And for simplicity,

$$X = 2\pi \sim a^2\ S\ p\ (\Sigma\ \lambda) \times 10^{-8}$$

The kilovolt-ampere output is

$$P = n\ E_s\ I$$

Combining the above equations, we obtain

$$P = n\ I \left\{ \frac{2.22}{R} \left[A\ T - \frac{a\ S\ n\ I}{2\sqrt{2}} \right] \sim S\ a\ p \times 10^{-8} - 2\pi \sim a^2\ S\ p\ I\ \Sigma\ \lambda \times 10^{-8} \right\}$$

Assume constant magnetic reluctance (true with straight-line saturation curve) and that output is increased by augmenting number of conductors (equivalent to increasing electromotive force; same results are obtained by considering number of conductors constant and current variable). To determine maximum output obtainable with given number of field ampere-turns, we differentiate the above equation with respect to a, and equate to zero, and obtain after simplifying:

$$a = \frac{\sqrt{2}\,(A\ T)}{n\ I\ S + 8\ R\ I\ \Sigma\ \lambda}\ *$$

The first term in the denominator in this expression is dependent upon armature reaction; the second term upon armature leakage fields (reactance).

*The form of the equation, before solving for a, was of first degree; hence only one solution was possible. The physical interpretation of this is that if the number of conductors is indefinitely *decreased*, an output of zero is approached; whereas, if the number of conductors is *increased* without limit, the reactance, being proportional to square of conductors, causes a greater drop in the armature than the value of terminal voltage.

In most turbo-alternators the number of field ampere-turns required to compensate for the drop in voltage due to armature leakage fields is small compared with the ampere-turns needed to compensate for armature reaction (counter magnetomotive force at zero power factor). To assist toward obtaining a simple physical interpretation of the above equation, we shall disregard for the moment the second term in the denominator, and then find

$$A\,T = \frac{a\,S\,n\,I}{\sqrt{2}}$$

That is, the full field ampere-turns are just double the armature reaction $\left(\frac{a\,S\,n\,I}{2\sqrt{2}}\right)$. This condition obtains if armature resistance and reactance are negligible: that reluctance of magnetic circuit is constant, and that generator just requires $A\,T$ ampere-turns on the field to maintain normal voltage, with normal kilovolt-ampere output at zero power factor.

To further interpret our results, we see that with the straight-line saturation curve assumed, we shall have 100 per cent regulation at zero power factor, and that normal current will flow if the alternator be gradually short-circuited with the excitation required for no-load normal voltage. In this latter case the ampere-turns in the field are just one-half the full field ampere-turns.

If we take into consideration the saturation of the magnetic circuit (variable reluctance), and the effect of local armature impedance, we see at once that, due to the former, the regulation at zero factor will be somewhat finer than 100 per cent; and the ampere-turns in the stator, for maximum kilovolt-amperes, will be somewhat less than one-half of the full field ampere-turns. In other words, somewhat more than normal current will flow when short-circuited with no-load field. The result which Mr. Lamme speaks of (" 1¼ times short-circuit current ") is about the condition for maximum output.

The question arises: would it not be possible to employ a smaller air gap and secure a larger output? This undoubtedly could be done if the steel in the magnetic circuit did not become prohibitively saturated. If the air gap and parts of the magnetic circuit were reduced to have zero reluctance, we should only require sufficient ampere-turns to overcome armature reaction and impedance, and the regulation would be infinite. To secure maximum kilovolt-amperes with diminished air gap, we should most advantageously use a larger value of flux, provided the iron did not saturate too highly, and we would still have the relation that full field ampere-turns would be twice the armature ampere-turns, and normal current would flow on short-circuit with no-load excitation were the reactance and resistance of negligible value. The rating would then be increased in proportion to the increase in flux.

Mr. Lamme speaks of air velocities as high as 10,000 ft. (3048 m.) per minute. When we remember that losses in air are proportional to the cube of the velocity, we shall readily appreciate that the losses with such velocity may be excessive. I ask Mr. Lamme how such velocities are measured in the machine; whether he attempted to measure air velocities directly in any parts not easily accessible.

William LeRoy Emmet: I am glad this paper has been written, because it gives everyone who reads it a comprehensive idea of all the work which has had to be done in the design of these high-speed alternators and the limitations of these designs. We know from experience that the difficulties of designing are too often overlooked by engineers who desire to purchase or install alternators, but all these limitations should be very carefully studied and considered, and the manufacturer should not be forced into conditions which are undesirable or entail difficulty. There is one thing which I think should be considered in connection with the design of large alternators, and that is, that the value of the product which they handle is enormous, in proportion to the cost of the machinery, and consequently we can afford to use only the best, and even if the machine costs a great deal more, if it is better or simpler or more reliable, it ought to be used. We should not, in other words, incur risk or inefficiency or difficulty of any kind on the score of cost in apparatus of this type. A very simple glance at the figures involved in the fuel consumption of such machines in a year's service will show that to be true.

As in other things, purchasers should not limit manufacturers unless that limitation is in the direction of value, and I am very sympathetic with Mr. Lamme in what he has said about temperature, that it would be highly desirable if we could build apparatus for higher temperatures, and if we could do so we could make it more efficient and make many improvements, provided the temperature could be run higher. The question has been in my mind as to what temperatures we should use in large apparatus. I have worked with a view to recommending to our customers what I believed to be the very best engineering solution of the problem, whether we get the job or not, in these cases, and I think that in so doing it is a nice question as to what type of insulation we should use. Insulation of all types which we have is very imperfect, and if we could find some better means of insulating high-voltage machines we would be very much better off, but I am a little disposed to differ with Mr. Lamme's implications that we could use temperatures, safely, as high as 125 deg. cent. Mica, of course, is indestructible at such temperatures, but as Mr. Reist has said, a great deal of other material is used with mica, and this is not only subject to destruction by heat, but subject to a sort of destructive distillation which causes fumes and other constituent parts of these substances to percolate through the insulation and seek out paths of discharge, and this destructive distillation of insulating materials is a very important cause, I think, of trouble. I have seen cases of burn-outs in mica-insulated machines of the most approved designs which rather clearly showed the danger in this direction, and with our best work in heat-proof insulation, I believe that we do not quite reach the state of safety that we do with cool machines. In some of these large machines the question of getting coolness means making the machine larger. That, of course, increases its cost, and also increases the mechanical difficulties—difficulties of shipment, difficulties of getting the forgings, difficulties incident to windage, which are very great at high velocities. However, with the best methods of cooling, it is astonishing what an amount of heat can be removed by virtue of the fact that rapidly moving air scours away the heat from the surfaces. We have worked very cautiously in the use of high temperatures. We are trying hard to build machines to run hot, but have not yet reached the point where we dare to do so.

Paul M. Lincoln: Perhaps the most significant feature about Mr. Lamme's paper is that about two-thirds of it is discussion of the closely related subjects of ventilation, temperature and insulation, and such things as inspection and cleaning of air and discussion of the mechanical details, make up the other third. That is a good indication of just how serious a problem the ventilating and cooling of a turbo-generator has become.

Another thing which this paper brings out plainly is that turbo-generators must be very efficient machines. All of the losses in the generators, of course, turn into heat, and the problem of carrying away that heat is the greatest one to be solved, and consequently it is almost essential to build turbine generators of comparatively high efficiencies.

There is one additional matter which has not been touched on, except slightly by Mr. Fechheimer, and that is the fact that the high velocities of the air which are necessary in order to carry off the heat from the restricted surface of the turbo-generators, may of themselves, if carried too far, generate undue heat. In fact, the windage losses in our turbo-generators are quite a large proportion of the total losses, and these windage losses come on account of the inherent difficulties in the problem so plainly set forth in Mr. Lamme's paper; that is, they come on account of the comparatively large amount of heat to be carried away from a restricted surface. If the air velocities are carried too high the method defeats its own object, because of the amount of heat that is put into stirring up the air.

I notice the expression which was used by Mr. Reist—"good inherent regulation," which he named as a bad thing. Now that is a nomenclature which is certainly unfortunate, because when we say "good inherent regulation" we mean one which is objectionable. It seems to me we might find a little better term to describe the kind of regulation which we would say is not objectionable.

Referring to Mr. Williamson's discussion, he described, as I understood him, an air filter which ran for over a year and did not get dirty. I must say that I am unable to conceive of an air filter which takes the dirt out of the air without itself becoming dirty. If it does not become dirty, it seems to me self-evident that it is not a good air filter.

Mr. Williamson also stated one thing which deserves some further discussion, when he mentioned the fact that if we had an insulation which would carry the heat readily from iron to copper, or copper to iron, our difficulties would be largely overcome. Now, that is an impossibility. We cannot design an insulation which has a good heat conductivity. The heat resistance of the insulations which we have in turbo-generators is about 1000 to 3000 times that of copper, and it is not possible by any means we know of to get a heat conductivity very much better in any known insulations. We must put up with those thermal drops in the insulation. There is no way to avoid them.

Peter Junkersfeld: The most suitable voltage for a given generator is, of course, largely a question of the insulation material and other limitations of design that exist at that particular time. In Chicago we have installed two 20,000-kw. 25-cycle units in a new station within the last year in which the voltage was half the busbar voltage, stepping up through auto transformers, and in so doing securing the necessary reactance at the same time. On the other hand, more recently we have, with better insulating material available, ordered a unit of the same total maximum kw. output in which the generator voltage will be the same as the busbar voltage. That happened to be a 60-cycle machine in which the problem of securing sufficient reactance was not difficult.

I feel, however, on this whole matter of generator design, that there is one point that perhaps has not been given as much attention as it should have been given, and that is the arrangement and smoothness of the air passages. Mr. Lamme, in Figs. 15 and 17, gives diagrams showing how air passes through certain designs of machine, but air has a habit of not turning right-angle corners, and not always behaving the way you want it to; moreover, air, notwithstanding what Mr. Lincoln has said about the filtering of air, is always dirty to a greater or less extent, and I do not believe that is confined solely to Chicago air, either. Air filters are not only very expensive, but, so far as I know, they have never come quite up to expectations, and, further, they are difficult to build in any fireproof form, or even semi-fireproof form. The requirement should be to get along without air filters if it is possible to do so. Some installations are becoming so large that it will probably pay, in large installations, to put in air filters and make the best job we can, but in small and moderate size stations I doubt if it will pay. The arrangement and smoothness of air passages should, therefore, be an important factor in selecting a generator design. I do not mean it should be the controlling feature, but when you have taken into account all the other limitations you should make a special effort to get a machine which is least likely to clog up with dirt.

That brings up another point. As soon as a machine begins to clog up with dirt, whether it occurs in one month, or six months, or twelve months, windings in the slots will necessarily become hotter, and for that reason temperature coils are very desirable in the machine, because they give an indication as to when the machine has reached, or is approaching, the absolute temperature for which it was designed. Notwithstanding the fact that temperature coils do not give absolutely accurate data, as pointed out by Mr. Lamme in his paper, they give the best and most positive indication you can have that it is time to go to the expense of taking down that machine and cleaning it thoroughly.

I am glad to hear both Mr. Lamme and Mr. Emmet speak of higher temperatures, and sincerely hope that their expectation may be realized, but before purchasers will agree to very much higher temperatures they naturally must feel reasonably certain that the machines will be at least as reliable as they are now.

The experience of the past ten years, as most of you have known, and as the author of the paper points out, has been a strenuous one and not at all times satisfactory. With the fibrous insulation 85 deg. cent. seems to be about the safe ultimate temperature. With the mica insulation it, of course, should be and probably will be very much higher. A slight difference in cost is not a serious thing with a very large machine, and while higher temperatures are very desirable we should not go to them if we are going to have a lesser factor of safety than we have at the present time with fiber insulation and only 85 deg. cent.; in other words there is still need for considerably more reliable generators than there are in existence today.

H. M. Hobart: There is one method of filtering, or, at any rate, cleaning air, which certainly would not involve any fire risk. The method to which I allude consists in washing it by passing it through sprays of water. It has the additional advantage of imparting to the air a certain amount of humidity and this is associated with a decrease in the initial temperature, which goes part way toward modifying the limitations imposed by the maximum temperature that the machine can endure. Five degrees decrease of temperature at the inlet is certainly well worth while. I am glad that the present trend of engineering opinion is in the direction of employing a central plant for the provision of the air. It permits not only of conditioning the air in the way I have mentioned but it also frees the designer from one set of considerations with which there is no need that he should be embarrassed. Any one who has read Mr. Lamme's paper will have considerable respect for the task of designing an extra-high-speed turbo-alternator, and will be disposed to agree that, other things being equal, if you can relieve the machine of all other duties than that of turning out electricity, it is in the interests of obtaining the best result. Large modern stations often require a total amount of air of some 200,000 cu. ft. per min., or even more, and the provision of this large amount of air is in itself a fairly elaborate undertaking. It can be worked out much better by putting in a separate plant with motor-driven blowers. Mr. Lamme has reasoned out very clearly that if we are determined to have these high-speed machines we must put up with higher temperatures than would be associated with machines of less speed.

Mr. Lamme goes on to develop the point that with these extra-high temperatures, the point of weakness is not in the copper; there will not be any trouble there because of any heating of the copper itself; nor in the iron, but in the insulation. In these extra-high-speed machines it is necessary to have a considerable pressure between adjacent turns. The higher the speed the less the turns and the higher the pressure per turn, and thus the question is not simply that of the copper and iron, but also of the insulation between the turns, as well as of the main insulation between the copper and iron, which bound this hot copper and this hot iron. We must admit that insulation appropriate for use in the slots has not the heat-resisting character possessed by metallic materials. Nevertheless, very great progress has been made in the matter of developing insulation in recent years. For a long time questions relating to insulation were apt to be ignored. Designers would deal carefully with the copper part and with the iron part of the structure, but the insulation has usually been sadly neglected.

Now, however, a very great advance has been made, and we have arrived either, as Mr. Lamme is inclined to believe, at a stage where we can safely use these high temperatures (and personally I will say frankly I agree with him) or, even if he and I are a little too sanguine, we are at any rate just on the point of arriving there. Every few months records further progress in these directions, and personally I feel that we should consider the temperatures named in the paper as those which can safely be used, with ample regard for the customer's side of the case.

What does the customer want? He does not necessarily want a machine that will last twenty years. None of us have seen even *ten* years go by, without the machinery which was produced at the beginning of the ten years having become so inferior to machines which could be produced in accordance with up-to-date knowledge that it would pay to scrap that machinery. The factor of obsolescence is thus one of great importance. Is it really good engineering in figuring on depreciation in the case of electrical machinery of this kind to spread it over so long a term as twenty years? My own opinion is that ten years will be a liberal provision. As has been stated by one or two of the speakers, these large high-speed machines are enormously expensive. The initial outlay for a 10,000-kw. turbo-generator may run into a matter of $100,000, and if that is spread over ten years, adding the capital charges, interest, allowance for insurance, taxes, etc., the total capital investment would probably average $200,000, or something like $20,000 a year. That is merely the line of argument; the precise values I have assigned have no special significance, though their order of magnitude is correct. $20,000 per annum seems a large figure, but it is small compared with the cost of the fuel for which that generating set is responsible per annum. I have not figured it out closely, but I believe that fuel consumption runs to something like $50,000 or $60,000 a year, as against $20,000, representing these capital costs of the turbo-generator. Then there is also the outlay for labor and attendance, etc. Therefore, anything that will conduce to decreasing the large fuel cost can quite properly be associated with a shorter life. It is for the purpose of decreasing the fuel cost that we resort to the very uttermost speed. You may say that having got that fuel cost away down, we want the machine to last a long time, but let me remind you that when ten years have elapsed that fuel cost will not represent the limits of economy to which we have attained. We will then be able to build machines for considerably lower fuel costs. But the point is that we want a good, sound machine, useful throughout its ten years' life. It should be a fine engineering product, on the basis that ten years is approximately the estimated life of the insulation. It is not undergoing deterioration throughout every hour of the ten years. It would be rare in central station practise for a 10,000-kw. machine to be in service more than 2500 hours out of the 8750 hours that make up the year. Let us take 25,000 hours as the aggregate time in service in ten years, and the problem is to provide insulation for a life of something like 25,000 hours' exposure to well on towards 125 deg. cent.

That is the problem engineers must consider in designing such machines, and they are rapidly getting where they can tackle it on that basis and provide the appropriate insulating material. This seems to me to be the correct proposition for cases where the insulation is the limiting feature. The fact is brought out in the paper that since the question of insulation is the one point which constitutes the limitation, we should devote our energies to that purpose and get the right stuff for it. The resources of modern engineering have never before failed us and are not going to fail us at this juncture, and if we determine upon this high temperature limit we shall learn to meet the new conditions imposed and meet them with safety. We should also follow up the related problem of getting a maximum amount of air through the machine. The more air we get through the machine the lower the temperature, or, for constant temperature, the more air we get through, the larger output we can go to, and we may not be limited to a 6000-kv-a. turbo-generator for a speed of 3600 rev. per min., or to a 25,000-kv-a. generator for a speed of 1500 rev. per min. We may be able to go to higher outputs for these speeds. It is in these directions, the methods of cooling and the methods of designing insulations which will stand higher temperatures, that our efforts should be put forth, so far as the kind of machinery we are discussing is concerned, namely, extra-high-speed machinery for large outputs.

W. L. Waters: Mr. Lamme's paper is especially interesting at the present time when the design of the modern turbo-alternator appears to be settling within the well-defined limits which have been decided by the materials commercially available. As pointed out, the design centers around the field magnet—the limitations being more serious on this than on any other part of the unit. The parallel slot type of magnet as developed by Mr. B. G. Lamme, and the radial slot as developed by Mr. C. E. L. Brown, are the only two commercial possibilities at the present time for high-speed generators. The former is a much simpler manufacturing proposition and the insulation is subject to less severe operating conditions, but it suffers from the difficulty of obtaining suitable commercial material in the required form and of determining the actual distribution of stresses. These limitations have recently brought the radial slot type into prominence for large high-speed units; the question of reliable and commercial high-grade materials being solved by the adoption either of a solid rotor built up of rolled plates, or of a rotor in which loose teeth of laminated steel are dovetailed into a hollow forged cylindrical drum.

The design of the stationary armature is comparatively simple, except for the question of ventilation, which, as stated, frequently becomes quite a serious problem in the case of high-speed units of large capacity. The system of ventilation first worked out in Germany, which consists of axial ventilation of the armature, combined with a ventilated rotor, seems to offer the greatest possibilities for such units. But, as pointed out, complicated systems of ventilation are rarely successful, as the various currents of air always interfere with one another. The problem of ventilation is not only to force a certain quantity of air through the machine, but also to do it with as little expenditure of energy as possible. Most of the energy required to force the air is wasted in eddies and churning, and this wasted energy raises the temperature of the air, thus decreasing its cooling effect. I remember one case in which a generator provided with an inefficient blower absorbing 100 h.p. actually operated with a lower temperature rise when the fan was removed; and there are a number of turbo-generators on the market in which, when rotating without load or excitation, the temperature of the air is increased 15 deg., due to eddies and churning while passing through the machine.

The cooling system of most turbo-generators is undoubtedly inefficient, but personally, I doubt the accuracy of the very high local temperatures sometimes obtained from resistance coil or thermocouple measurements. It required ten years' experience of the manufacturers and a considerable amount of work by the Standards Committee before any reliability was obtainable in temperature measurements by thermometer, and I think it will be necessary to duplicate this work before any reliance can be placed on the usual resistance coil or thermocouple tests. Such measurements must at the present time be considered as laboratory tests requiring an observer experienced in the use of such methods, and are worse than useless in the hands of the average tester. The most reliable deductions are those drawn from examination of the condition of machines that have been in operation, and as Mr. Lamme has pointed out, these results emphasize the advisability of employing some high-temperature insulation such as mica in both the field magnets and armature of any generator subject to the severe operating conditions of the modern large high-speed turbo-alternator. Mica as an insulating material for electrical power machinery has been in disrepute for the past fifteen years on account of its poor mechanical properties, and it is only during the last few years that methods have been devised for using this material under conditions which do not allow it to be subjected to mechanical abuse either during the manufacturing processes or in practical operation. These developments in regard to the use of mica as an insulator have had an important influence on the development of the modern high-speed turbo-alternator of large capacity.

The point in Mr. Lamme's paper which probably affects operating engineers most is that it is clearly shown that the carefully worded specifications and elaborate detailed guarantees usually required, are not only of little value, but inadvisable, as tending to give a false idea of security. The selection of satisfactory materials, the limiting of stresses to safe values, the choice of insulating materials and the adoption of an adequate system of ventilation which will avoid the presence of dangerous temperatures in inaccessible parts of a machine, are all questions which can be passed upon only by an engineer who has had wide experience in the design, manufacture and operation of these units; and on such questions, the detailed guarantees usually specified have practically no application. Large high-speed units are coming into increasing use every year and it will be well for prospective purchasers to realize that they must necessarily depend on the ability and standing of the manufacturer when buying such units, rather than on some specification containing a number of more or less unimportant guarantees.

Comfort A. Adams: Mr. Lamme has made such a thorough job of the subject in hand that I will confine my discussion to a more quantitative treatment or explanation of a few of the points made in the paper.

First, consider the relation of armature copper and core losses as between engine-driven and turbine-driven alternators. For a given gap density and coil pitch, the volts per foot of active conductor will be proportional to the peripheral velocity, or the total length of active conductor inversely as the peripheral velocity. Thus if the current density in the copper is unchanged, the armature copper loss is inversely as the peripheral velocity. A change in the ratio of idle to active wire or in the coil pitch may modify this proportionality somewhat when applied to the total armature copper loss, but not seriously. Thus a jump from a peripheral velocity of 100 ft. per second in an engine-driven alternator to 400 ft. per second in a turbo-alternator means a reduction of armature copper loss to approximately one-fourth.

With the core loss it is quite different. For a given frequency and magnetic density, the pole pitch and therefore the depth of core back of slots is proportional to the peripheral velocity; thus any reduction of cylindrical core section is neutralized by the increased core depth; moreover, the ratio of outer core diameter to gap diameter is so much larger in the two-pole and four-pole machines that the core volume and thus the core losses are considerably greater for the same magnetic densities. The actual ratios are as follows: a two-pole machine has about 80 per cent more core volume and core loss than a 60-pole machine, other things being equal, a four-pole machine 40 per cent more, an eight-pole machine 20 per cent more, and so on. This increase of core loss back of the slots is, however, slightly neutralized by less tooth volume and tooth loss. Thus the ratio of core to copper loss is several times as great as in the engine-driven machine. This has an important bearing on the method of ventilation, as indicated in Mr. Lamme's paper.

A similar change in distribution takes place between the various elements of leakage reactance, the slot leakage decreasing as the length of active conductor decreases and the peripheral velocity increases, while the coil end reactance increases materially.

Coming now to the question of heating, it may be interesting to apply a simple calculation to a condition which is sufficiently near the facts in some cases to make the results significant and instructive. Consider a copper conductor completely heat-insulated laterally so that the heat generated therein must flow to the ends—to find the difference of temperature between the center and the ends.

Let l = embedded length of conductor in core (cm.).
a = current density in amperes per sq. cm.
p = resistivity of conductor in ohms per cm. per sq. cm.
$p' = a^2 \rho$ = watts per cubic cm. of conductor.
θ = thermal resistivity of conductor in deg. cent. per watt per cm. per sq. cm.
x = distance of any point from center of conductor.
$p' x$ = watts flow of heat through the conductor at x.

Then the difference in temperature between the ends of the element dx is

$$dT = a^2 \rho \, \theta \, x \, dx$$

and the difference of temperature between the center and end is

$$T = a^2 \rho \, \theta \int_0^{\frac{l}{2}} x \, dx = \frac{a^2 \rho \, \theta}{8} \, l^2 \qquad (1)$$

Taking $\rho = 2.20 \times 10^{-6}$ (at 90 deg. cent.) and $\theta = 0.29$,

$$T = 8 \left(\frac{a}{100}\right)^2 \left(\frac{l}{100}\right)^2 \qquad (2)$$

Table I gives values of T for various values of a and l

TABLE I

Amperes per sq. cm.	Amperes per sq. in.	Cir. mils per ampere	l			
			20 in. 50.8 cm.	40 in. 101.6 cm.	60 in. 152.4 cm.	80 in. 203.2cm.
150	970	1310	4.65 deg.	18.6 deg.	42 deg.	74.5 deg.
200	1290	985	8.3 deg.	33.2 deg.	75 deg.	133 deg.
250	1610	790	13.0 deg.	52 deg.	117 deg.	208 deg.
300	1940	655	18.6 deg.	74.4 deg.	168 deg.	297 deg.
400	2580	493	33.2 deg.	133 deg.	300 deg.	530 deg.

There is thus a pretty definite limit to the safe length of embedded conductor unless considerable heat escapes through the slot insulation, or, for a very long core it is pretty certain that most of the heat generated near the center of the embedded conductor must flow through the slot insulation.

In such a case the temperature difference between slot copper and core may be approximately determined as follows:

Let S = area of section of coil (or double coil in a two-layer winding) inside of coil- and slot-insulation. The perimeter of this section, or the cross-section (per unit of slot length) of the path through which the heat must flow, will be, for an average slot shape, $4.5 \sqrt{S}$.

Let f_s = copper space factor within the coil insulation.

Then the copper watts per cm. length of slot will be

$$p'_s = (S f_s \, a)^2 \rho \frac{1}{S f_s} = \rho \, S f_s \, a^2$$

Let θ = the thermal resistivity in deg. cent. per watt per cm. per sq. cm., and t = (cm.) thickness of insulation, iron to copper. Then the temperature difference between copper and iron will be

$$T = \frac{p_s'}{4.5\sqrt{S}}\theta t = \frac{\rho \theta f_s}{4.5} a^2 t \sqrt{S}$$

$$T = (\rho \times 10^6)\left(\frac{\theta}{10^3}\right)\left(\frac{a}{100}\right)^2 (10t)\sqrt{S} f_s \quad (3)$$

Take $\rho \times 10^6 = 2.2$ (90 deg. cent.), $\frac{\theta}{10^3} = 0.7$, and $f_s = 0.8$ (large machine, bar-wound).

This value of θ is an average for the ordinary slot insulations. Then

$$T = 2.2 \times 0.7 \times 0.8 \left(\frac{a}{100}\right)^2 (10t)\sqrt{S}$$

$$= 1.23\left(\frac{a}{100}\right)^2 (10t)\sqrt{S} \quad (4)$$

or the temperature gradient in deg. cent. per mm. is

$$T_m = 1.23\left(\frac{a}{100}\right)^2 \sqrt{S} \quad (5)$$

Values of T_m are given in Table II for various values of a and S.

TABLE II

Deg. cent. per mm. thickness of insulation.

Amperes per sq. cm.	S						
	0.25	0.50	0.75	1.00	1.50	2.00	2.5
	deg.	deg.	deg.	deg.	deg.	deg.	deg.
150	1.39	1.95	2.4	2.77	3.4	3.92	4.38
200	2.46	3.48	4.26	4.92	6.03	6.95	7.8
250	3.85	5.45	6.67	7.7	9.43	10.9	12.2
300	5.55	7.85	9.6	11.1	13		
400	9.84	13.9	17				

For wire-wound machines f_s will be less, but there may be a considerable temperature gradient between the center of the coil section and the inside of the coil insulation, so that equation (5) will give too small values if used to determine the maximum internal temperature of wire-wound machines, particularly where there are many turns of fine wire.

When considering the flow of heat from copper to iron, it is obviously desirable to know what happens in the iron itself.

Consider a large mass of laminations in which heat is being generated by core loss at the rate of p' watts per cu. cm.

Assume first that the heat flows only parallel to the laminations, and let l be the total depth of laminations. Then, from equation (1),

$$T = \frac{p' \theta}{8} l^2 \qquad (6)$$

Take $\theta = 2.4$. Then $T = 0.3\, p'\, l^2$ (7)

Table III gives values of T for various values of p' and l.

TABLE III

p'	Watts per cu. in.	Watts per lb.	l					
			4 in. 10.2 cm.	8 in. 20.3 cm.	12 in. 30.5 cm.	16 in. 40.6 cm.	20 in. 50.8 cm.	30 in. 76 cm.
			deg.	deg.	deg.	deg.	deg.	deg.
0.01	0.164	0.58	0.31	1.25	2.8	5	7.7	11.2
0.02	0.328	1.16	0.62	2.50	5.6	10	15.5	22.3
0.03	0.492	1.74	0.94	3.75	8.4	15	23.2	34.0
0.04	0.656	2.33	1.24	5.00	11.2	20	31	44.7
0.05	0.82	2.92	1.55	6.25	14.00	25	37.7	56

This assumes that the two exposed edges are at the same temperature. If this is not the case, the temperature difference will be slightly less with respect to the hotter edge and greater with respect to the cooler edge or surface.

Next assume that the flow of heat in the core is entirely across the laminations. From Table III it is obvious that in the case of deep cores without transverse or axial ducts, the above assumption will approximately represent the facts, at least so far as the radially central part of the core is concerned.

For this case take $\theta = 30$. This varies considerably with the tightness of the laminations, and is frequently larger than the value here assumed.

Equation (6) thus becomes

$$T = 3.75\, p'\, l^2 \qquad (7)$$

Table IV gives values of T for various values of p' and l.

TABLE IV

Watts per cu. cm. p'	l			
	2.0 in. 5.08 cm.	2.5 in. 6.34 cm.	3.0 in. 7.62 cm.	3.5 in. 8.9 cm.
0.01	0.97 deg.	1.5 deg.	2.17 deg.	3 deg.
0.02	1.94 deg.	3.0 deg.	4.35 deg.	6 deg.
0.03	2.9 deg.	4.5 deg.	6.52 deg.	9 deg.
0.04	3.88 deg.	6.0 deg.	8.7 deg.	12 deg.
0.05	4.85 deg.	7.5 deg.	10.9 deg.	15 deg.

These are merely suggestions of what may be done with simple calculations, the results of which, though somewhat crude, are very significant.

Mr. Lamme's paper illustrates what the writer has so often urged, the importance of careful analysis of each problem from the groundwork of fundamental principles, the habit of thinking rather than simply remembering, and particularly the habit of thinking *first* before being forced to do so by the failure of some plan or design.

Allan B. Field: Mr. Lamme's description of the general turbo situation carries us so easily through the history that we are apt to overlook the great difficulties that were encountered in connection with the early big machines, five or six years ago, the difficulties, for instance, in obtaining suitable large steel forgings and castings. Mr. Lamme and his associates, in conjunction with the steel foundries and mills, carried out a lengthy investigation, to determine the best means of producing this material, the compositions to be used for the steels, and the method of casting and heat-treating. Large castings and forgings were cut up and test pieces taken out in many directions and positions, to determine the effects of various factors.

These difficulties in obtaining large masses of steel having the desired physical properties, increase rapidly as the sizes go up. If the steel mills are asked whether they can provide a rotor forging, say, 55 or 60 in. in diameter, and weighing some 60,000 or 80,000 lb., they will assent at once. If we begin to inquire about tests, they are quite willing to accept fairly rigid specifications, but will want to locate the test bars at the ends of the forging, where there has been a considerable amount of forging work done; when we insist on locating these test bars in the large diameter of the rotor, the steel mills require an easier material specification. If we go further, and wish to take the test bars out in the direction in which they tell us most, viz., in the radial direction, the steel mill will refuse, or give such specifications as are of very little value. It is such considerations as these that have forced the rotor construction along new lines in recent years. The use of heavy steel plates for turbo rotors is comparatively old, having been adopted for a number of years both in Europe and in this country, in cases where a through-shaft can be employed. In the largest machines under discussion, where rotor ventilation becomes a necessity, where the slots are deep, and where ventilation slots below the winding slots are required, the stresses in the center of the disk, with a hole through for a shaft, run up to figures which render the use of special steel advisable. Such material is somewhat hard to obtain, particularly when the mills are busy. By using a plate without a hole in the center, the stresses in this region are kept down comparatively low, and a commercial material can be used.

This construction, which is illustrated in Figs. 5, 6 and 12 of of the paper and which was first proposed by Mr. B. A. Behrend, appears a very bold one at first sight. The solid plates are rabetted together, assembled between two flanged shaft ends, and the whole clamped together by means of large bolts located in the poles. There is no through-shaft, and the resistance to twisting of the rotor is entirely one of friction, aided by the shear resistance of the bolts. However, a little investigation showed that this was quite a feasible form of construction, a conclusion entirely born out by subsequent results. Plate steel is a commercial form in which this material can be most relied upon, and the flanges on the two stub shaft ends are thin enough to be formed by upsetting, instead of forging down, thus obtaining excellent properties in a radial direction. For the smaller radial-slot rotors, solid forgings can be satisfactorily used, and where ventilation is required this is generally obtained by grooving down the rotor in a lathe in several places, the grooves extending all the way around and communicating with axial air passages. In the solid plate constructions, these grooves are formed by milling the flat face of the plate in the region of the slots only, leaving the full thickness of the plate in the polar region. In this way the rigidity of the rotor is not impaired by the means for ventilation and an exceedingly stiff construction can be obtained.

There have been several references in the discussion to the critical speed. I am of the opinion that there are many more machines running above their critical speed than is generally believed. Machines can be made to run satisfactorily above the critical speed, but there is a considerable advantage in running below, where this is commercially feasible. This is particularly so in the case of the four-pole machine, as distinguished from the two-pole machine. In the case of the four-pole machine a short-circuited turn on one pole, for instance, will considerably unbalance a machine that is mechanically balanced, if it is running above its critical speed; on the other hand, a large percentage of the winding on one pole can be short-circuited without causing vibration, if the machine is running below its critical speed. There are also advantages in a stiff rotor for the two-pole case.

The constructions discussed here provide some very large machines running below their critical speed, and this feature is believed to be of considerable importance, and one for which it would be worth while to sacrifice to some extent electrical considerations, when necessary.

Mr. Junkersfeld has referred to the question of the collection of dirt in generators, and has drawn attention to the fact that exploring coils might be put in and the temperature rise used as a criterion for the periodical cleaning. With the more common methods of stator ventilation, the vent entrances, being limited by tooth size and duct width, are small and rather easily clogged, and the air passages are intricate; further, to clean them out the rotor must be removed and even then a thorough cleaning is difficult. In the case of the axial arrangement of ventilation described in Mr. Lamme's paper, the vents are straight and large, and of uniform section throughout their length, and it is generally possible to clean them out without removing the rotor, by merely passing through them a wire scratch brush similar to a rifle cleaning brush.

W. J. Foster (by letter): Limitations in design are less oppressive with the selection of proper voltage. For three-phase generators of over 20,000 kv-a., that have only two poles, potential of 10,000 or 11,000 volts is probably preferable to 6600, since the problems of insulating are not so serious as the difficulties involved in a design with too small a number of slots and with conductors carrying excessive current.

The limitations in design can undoubtedly be reduced in the case of the largest two-pole generators by increasing the number of phases to six or twelve. It would probably be found in connection with a 40,000-kv-a., 1500-rev. per min., 25-cycle generator that decided advantages would result from the use of twelve phases and about 4000 volts.

The fields of the largest generators should never be wound for less than 250-volt excitation.

Mr. Lamme rightly dwells upon the desirability of an insulation that will stand temperatures as high as 125 deg. cent. It is to be hoped that the particular type of mica insulation described has raised the safe limit to that figure, and that further improvement in the use of mica or the development of some other type of insulation will raise it to a much higher figure with safe internal temperatures.

In the matter of the limitation due to the ventilation problem, the designer often has it in his power, in connection with very large (possibly not the largest) machines, to exaggerate certain features, such as the use of an extremely large air gap and stator slots left open at the gap. This can be done by deliberately increasing the axial length of the machine, thus reducing the pole face density and not increasing at all the magnetic reluctance in the air gap. Of course, such a design involves a decided increase in the amount of material used.

K. E. Czeija (by letter): Mr. Lamme has considered all points of interest in regard to modern turbo-alternator design, and after this logical separation and analysis of the different problems, a discussion of the details seems unnecessary. Nevertheless, I would like to mention a few points that seem to be worth while considering in regard to the axial ventilation problem.

Evidently, the most effective ventilating scheme will be obtained when for a minimum amount of energy required for circulating the air through the machine, a maximum amount of heat will be absorbed by this air. We will come nearest to this ideal condition when the path of the air through the machine has the least possible number of changes in the direction in which it is flowing, and moreover, when it comes in contact with those surfaces to which the heat from the inner part is conducted with the smallest possible resistance.

[redacted]

If the cooling air enters the machine at one side in order to be blown in an axial direction through the stator, the air gap, and the rotor, the only important question will be the length of the paths along which the air may be moved without the temperature difference between the air and the surrounding metal parts becoming too small to be effective. For the case in which the limit set by this principle will be exceeded, it will be necessary to divide the cooling air paths in two or more parts.

It has been proved that the introduction of radial ventilating ducts wider than the usual ½ to ¾ in. (12.7 to 19 mm.) will produce local losses due to unequal flux distribution in axial direction along the stator core, unequal dielectric stresses in the insulation material, whirling of the air, and cause noise, in addition to which dirt may collect at some places.

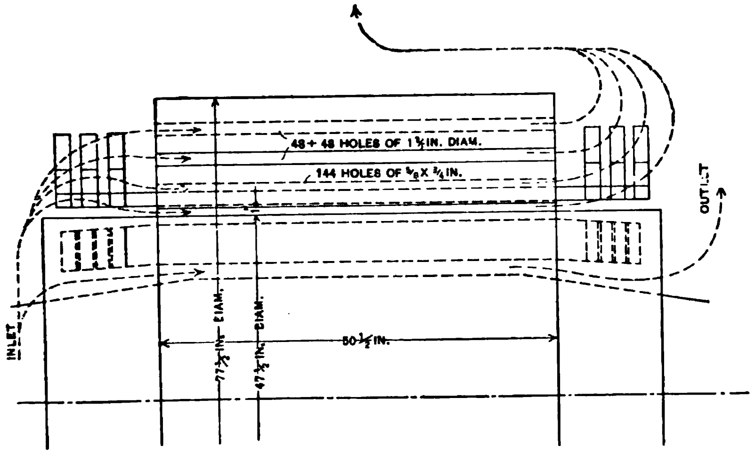


Fig. 10

In order not to subdivide the stator iron more than absolutely necessary, and in order to get the smallest distance between bearings, where it is advisable to adhere to the axial ventilation principle, the number of radial air ducts should be kept as small as possible.

The sketch herewith (Fig. 10) represents a very interesting example of the straight axial ventilation principle applied on a 7500-kv-a. three-phase turbo-alternator* for 2200 volts, 70 per cent power factor, 50 cycles and 1500 rev. per min. In addition to the values shown in the sketch, the following data will be of interest on account of the length of this machine, representing probably the maximum obtainable length of undivided axial air paths.

*Built in Germany.

Running without armature current and a field current corresponding to full load zero power factor, the following losses and temperatures were tested:

Iron loss 190 kw.

$I^2 R$ field 30 kw.

Windage 42 kw.

At the total air quantity of approximately 19,000 cu. ft. (538 cu. m.) per minute, the incoming air is 20 deg. cent., the outgoing air 50 deg. cent.

Stator iron at the inlet side 27 deg. cent.

" " " " outlet " 83 deg. cent.

Alexander Gray (by letter): A clear conception of the subject of ventilation is of the utmost importance to the designer of electrical machinery and particularly to the designer of turbo-alternators, and for that reason I have considered it advisable to enlarge on the subject of axial ventilation.

The statement is often made that, since the conductivity of iron along the laminations is much greater than that across the laminations and layers of varnish, it is advisable to cool an iron core by means of axial ducts, so that the cooling air can be blown across the ends of the laminations. This statement is misleading. That of Mr. Lamme is more guarded; he states that "if all the heat could be conducted along the laminations to the ventilating surfaces, apparently much more effective heat dissipation could be obtained, provided sufficient surface be exposed to the air."

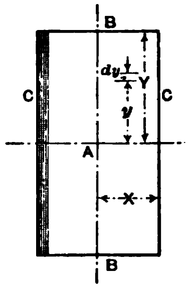


FIG. 11

The turbo-alternator is a large and expensive machine. There is therefore little chance of such machines being specially built for experimental purposes, and the designer has to depend largely on the intuition gained from a wide experience with other types of electrical machinery.

The following investigation in which the conductivity of the iron core is compared with that of the surface between the iron and the adjoining air is of considerable interest. Fig. 11 shows an iron core built up of laminations which are separated from one another by layers of varnish; the loss in this core is supposed to be uniform throughout its volume. The heat generated in the core has to be conducted to and dissipated by the surfaces B and C.

If all the heat passes in the direction Y then the watts crossing each square inch of the core at y = (watts per cu. in.) y and the difference in temperature between two surfaces a distance dy apart

$$= \frac{(\text{watts per cu. in.})\, y\, dy}{1.5} \text{ deg. cent.,}$$

where 1.5 is the thermal conductivity of iron in watts per inch cube per deg. cent. difference in temperature. The difference in temperature between surfaces A and B for the assumed conditions is therefore

$$= \frac{(\text{watts per cu. in.})\ Y^2}{3} \text{ deg. cent.}$$

$$= T_{ab}$$

When air is blown across the surface of an iron core with a velocity of V ft. per min., the watts dissipated per square inch of the surface for 1 deg. cent. rise of the surface temperature = $0.0245\ (1 + 0.00127\ V)$*. If then the heat is all dissipated by the surfaces B, the difference in temperature between surface B and the air = T_b

$$= \frac{(\text{watts per cu. in.})\ Y}{0.0245\ (1 + 0.00127\ V)} \text{ deg. cent.}$$

In the same way, if it is assumed that all the heat generated in the iron core passes in the direction X, then, since the conductivity across the laminations may be taken as 1/50 of that along the laminations for an iron core built up with varnish and paper between the laminations,

$$T_{ac} = (\text{watts per cu. in.}) \frac{50\ X^2}{3} \text{ deg. cent.}$$

$$T_c = \frac{(\text{watts per cu. in.})\ X}{0.0245\ (1 + 0.00127\ V)} \text{ deg. cent.}$$

For a 60-cycle turbo-alternator assume the following figures:

pole pitch = 40 in. (1016 mm.)
core depth behind teeth = 14 in. (355.6 mm.) = $2\ Y$
space between vent ducts = 2 in. (50.8 mm.) = $2\ X$
air velocity across the surfaces = 6000 ft. (1829 m.) per min.

Then T_{ab} = 17 deg. cent.
T_{ac} = 17 deg. cent.
T_b = 33 deg. cent.
T_c = 4.7 deg. cent.
$T_{ab} + T_b$ = 50 deg. cent.
$T_{ac} + T_c$ = 22 deg. cent.

That is to say, for the assumed conditions the thermal conductivity along the laminations and across the surface is only 43 per cent of that across the laminations and surface; there is evidently still a strong case for radial vent ducts.

In the actual case there is of course a larger loss per unit volume in the teeth than in the core behind the teeth, and in some other respects the case discussed does not correspond exactly with that of the turbo-alternator core, but the argument has been worked

*Ott; *Electrician*, March 7, 1907.

up to show that, compared with the surface resistance between the iron and the air, the thermal resistance across the laminations is relatively not very large.

Since it is necessary to keep the insulation cool, it would seem that axial ventilation along the air gap is desirable, but such ventilation should be combined with circumferential ventilation in order to keep the bulk of the core cool. Fig. 12 shows a method of ventilation which has advantages for machines that are not too large; for long machines the air for circumferential ventilation should be sent in from both ends of the machine; that for axial ventilation along the air gap should also be sent in from the ends and then led out through a wide duct in the center of the core. In the diagram shown there are ten short paths through the core, and with ducts spaced so that the blocks of iron are 2 in. (50.8 mm.) thick and with a core density of 60,000 lines per sq. in (9300 lines per sq. cm.) at 60 cycles, it is possible to get 100 cu. ft. (2.83 cu. m.) of air per min. through the machine per

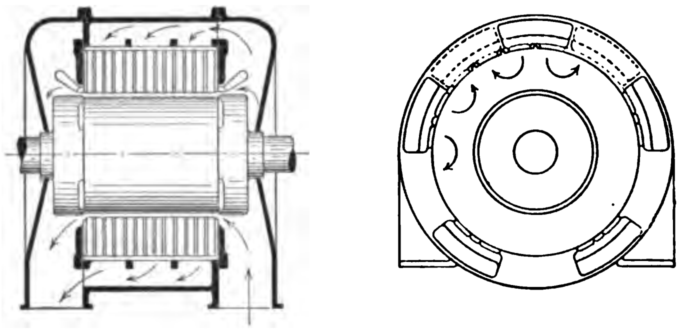


FIG. 12

kilowatt loss without the velocity in the ducts exceeding 4000 ft. (1219 m.) per min.

The statement in Mr. Lamme's paper that the temperature of the iron does not limit the machine, but rather that of the insulation, requires some modification. The above investigation shows that at certain points in the body of the core the temperature may be 20 deg. cent. higher than that of the surfaces of the vent ducts, and we are asked to approve of surface temperatures of the order of 100 deg. cent. Such temperatures are not safe unless non-aging iron is used for the core, and the writer would like to know if time tests are made on the iron used for turbo-alternators at the temperatures which may be expected in the body of the core when the machine is in operation, in order to determine whether or not the iron loss increases with time.

It has been evident for some time that the measurement of temperature rise by thermometer, and a guarantee of 40 deg. cent. rise, have become obsolete, and yet a temperature rise of 80 or

100 deg. cent., the temperature to be measured by resistance or by a resistance thermometer, is unsafe unless mica insulation is used. It would therefore seem that the purchaser of a turbo-alternator has to depend largely on the reputation of the manufacturing company, rather than on a written specification, to ensure satisfaction, and also that ordinary witness tests are of little value compared with tests run over a long period of time. That being the case, the writer would like to have an opinion as to what kind of clause should be inserted in specifications in order to protect both the purchaser and the manufacturer and also what tests should be considered as satisfactory for acceptance of the machine. The purchaser should be protected against deterioration of the insulation and increase of the core loss due to high operating temperatures and yet the manufacturer should not be required to wait indefinitely for his money.

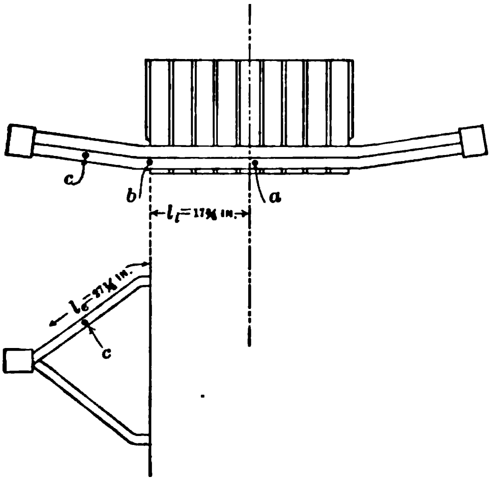


Fig. 13

Bradley T. McCormick (by letter): Under the subject of temperature rise, Mr. Lamme refers to machines in which the stator teeth are hotter than the copper. Such a condition might exist at low loads, but I am inclined to believe that on turbos of usual design, the hottest spot in the stator at full load is the point *a* (see Fig. 13), the copper in the top of the slot in the center of the machine; and that the most difficult problem in stator design is to keep this point at a temperature low enough to prevent injury to the insulation. On machines of the turbo type it is hardly probable that the teeth can ever be hotter than the copper in the center of the machine, unless the iron is so poorly ventilated that the teeth and core are much hotter than they should be, in which case the machine will not meet the guaranteed temperature rise.

Suppose, for the sake of argument, that the teeth are hotter than the copper. Then no heat flows from the windings into the iron, but all the heat generated in the coils must flow along the copper to the coil ends, and there be carried away by the air. It can be shown that in order to force the heat to travel along this path, the difference in temperature between the copper at the center of the machine and the outside surface of the coil ends, will be so great that the resulting temperature of the copper at the center of the machine, and the teeth, (which we have assumed to be hotter still,) will be quite beyond the limit of safety.

Take an example from a machine which is operating successfully. We have a total embedded length of stator copper of 35½ in. (901.7 mm.) working at a density of 820 cir. mils per ampere. We will first assume that there are no eddy currents. If all the heat flows along the copper and out at the ends, we have, as the difference in temperature between the copper in the center of the machine at *a*, and at the point *b*, where the coil leaves the slot,

$$T_i = \frac{0.0625 \times 10^6 \, l_i^2}{(\text{cir. mils per ampere})^2}$$

$$= \frac{0.0625 \times 10^6 \times (17\tfrac{3}{4})^2}{820^2} = 29 \text{ deg. cent.}$$

where 0.0625 is a constant involving the heat conductivity of copper.

Practically no heat can find its way out from the coil to the air gap through the wooden or fiber wedge, on account of its poor heat conductivity. Some of the heat will be dissipated at that part of the coil ends near the core, while the remainder will flow along the coil ends and be dissipated near the clips or U bends. A further difference in temperature is therefore necessary to pass the heat from the point *b* to points further along on the ends of the coils, where it can be carried away by the blast of air. This temperature difference it is extremely difficult to calculate, but it can be roughly estimated. If we assume for simplicity that the end connection temperature drops uniformly from the iron to the clips, then the temperature of the point *c* will be the mean temperature of the coil ends, and we can consider the problem as if all the heat generated inside the slots were conducted to the point *c* midway between the iron and the clips, and dissipated at this point. We shall then have

$$T_e = \tfrac{1}{2}\, T_i \frac{l_e}{l_i} = \tfrac{1}{2}\,(29)\, \frac{27\tfrac{1}{4}}{17\tfrac{3}{4}} = 22.3 \text{ deg. cent.}$$

where T_e is the difference in temperature between the points *b* and *c*.

A still further temperature difference is necessary to pass the entire stator copper loss through the insulation on the ends of the coils. From the thickness and quality of insulation, and the surface exposed to the air, the temperature drop across the coil end insulation is estimated at 13 deg. cent. for this machine.

Assume now that the air blowing on the coil ends is at 25 deg. cent. and that the blast is sufficient to limit the running temperature of the surface of the coils at *c*, to 40 deg. cent. This assumption of only 15 deg. cent. rise of coil surface exposed to air is certainly as low as could be expected.

We can now calculate the temperature of the copper at the center of the machine, which will be as follows:

29 deg.	temperature drop between *a* and *b*
22 "	" " " *b* and *c*
13 "	" " across coil end insulation
40 "	" of surface of coil ends while running
—	
104 deg. cent.,	temperature of copper in center of machine.

This temperature of 104 deg. cent. is quite up to the limit of safety for cotton insulation, and allows of no overload. Furthermore, this figure is based upon the assumption that eddy currents are absent, a condition never fully realized in practise. A great many machines are operating successfully with a total stator copper loss, including eddy current losses, of two or three times the normal I^2R, or even higher. Assuming that in our example we have a total stator copper loss of only 1½ times the normal I^2R, and that the running temperature of the coil ends is still 40 deg. cent. as before, then the first three items in the sum will be increased 50 per cent, and a resulting temperature of 136 deg. cent. will be obtained for the point *a* in the copper at the end of the machine.

If, as Mr. Lamme claims, the teeth are hotter than the copper, then the teeth of this machine must be at a temperature greater than 136 deg. cent. Such could only be the case if the machine were insufficiently ventilated, and the excessive iron temperature would show up on test, and the machine would be rejected.

The turbo chosen in this example is quite typical, and the results obtained from these calculations are so extreme that one may infer that any other turbo treated in a similar manner will also show a temperature unreasonably high.

It would therefore appear hardly possible that the teeth of a turbo-generator can run hotter than the copper in the center of the machine, and the machine still be at all acceptable, although it is of course possible that those portions of the winding near the ends of the core may be cooler than the teeth. With sufficient ventilation to limit the tooth temperature to 40 or 50 deg. cent. rise by thermometer, the extremely high copper temperatures, which have been calculated above, are prevented by the flow of heat from the copper through the insulation into the iron, so that although the hottest point *a* may still be over 40 deg. cent. above the air, it will, however, be at a safe temperature that will not injure the insulation.

The above discussion has a very important bearing on the question of insulation, for if most of the heat from the windings is conducted to the coil ends, and there carried away by the air, the designer is justified in resorting to very thick slot insulation; but if the cooling of the winding is largely dependent upon the ability of the heat to pass through the slot insulation into the iron, as seems to be the case, then an increase in slot insulation involves higher temperatures in the stator copper, which may induce the designer to reduce the factor of safety of his insulation, with invariably disastrous results. These conditions are especially true in connection with machines of high voltage, and form one of the limitations to the continued increase in size and voltage of turbo-generators.

These difficulties naturally lead to the discussion of the advantages of mica insulation. Mica, on account of its ability to resist heat, is largely employed in turbo-generators, but as a conductor of heat it is only from 50 per cent to 70 per cent as good as varnished cambric, depending upon the way in which the mica is built up. Due to this fact, and also to the fact that mica is very expensive, the designer may be tempted to use a thin mica insulation, and feel that he is justified in so doing by the high dielectric strength of mica. Such a practise only invites the insulation difficulty to which Mr. Lamme refers, static discharges between the coils and the iron.

It is extremely difficult to apply mica insulation in such a way as to exclude air pockets. The pressure gradient at which air breaks down is quite variable, depending upon whether a large or small quantity of air is under stress, but for small air pockets in the insulation and dense air films lying in contact with surfaces, corona will form at a pressure gradient somewhere in the neighborhood of 200 volts per mil. With a specific inductive capacity of about 6 for mica, it therefore requires an insulation distance between copper and slot sufficient to give a mean pressure gradient, in round numbers, of 35 volts per mil, based upon the Y voltage, that is, the line voltage divided by $\sqrt{3}$.

Any attempt to decrease the insulation beyond this point will result in the formation of static in the enclosed air pockets, as well as at the points where the coils emerge from the slots, both inside the ducts and at the ends of the machine. In machines which have not been properly insulated the presence of static can sometimes be detected, when running on full voltage, by a distinct odor of ozone near the terminals where the voltage to ground is highest. This of course only applies to engine type machines or waterwheel machines whose speed is low enough to prevent the windage from blowing away the ozone as fast as it is formed, and thus making it impossible to detect its presence.

Jens Bache-Wiig (by letter): Mr. Lamme's paper shows, I think, that a large high-speed turbo-alternator can now be built sufficiently safe electrically to put it on a par with, say, a low-speed alternating-current generator with mica-insulated armature coils and asbestos-insulated field coils. As stated in the paper, this is due principally to the improved method of ventilation, to the insulating material adopted and to the electrical design as a whole, especially as regards short-circuits.

Concluding, Mr. Lamme asks if the temperature problem inherent with these machines may be considered due to the design being too much forced.

It seems to me that this problem is solved very satisfactorily by the adoption of mica insulation. I would like to add as my opinion that the development of low-speed electrical machinery will not be completed until the same kind of insulating material has been adopted for these machines as well.

In one point, however, I do think the construction is stretched too far, and that is with regard to the mechanical design.

As an example is given a 5000-kv-a. two-pole 3600-rev. per min. 60-cycle generator with a rotor diameter of 66 cm. This gives a peripheral speed of 124.3 m. per sec. It is stated that the core is designed for a very considerable margin of safety, and is actually tested at overspeeds which give about 152.3 m. per sec. peripheral speed. This means that the rotating part has been tested at about 22.5 per cent overspeed.

Compared with general practise in turbo-alternator design this may be called "a very considerable margin of safety," but why is it that such machines are designed so very close to the bursting point, as compared with other electrical machinery? The answer that it can not be done any other way does not seem satisfactory.

Take for instance a waterwheel-driven alternating-current generator. The design of such a machine is not considered safe, unless it will withstand successfully the runaway speed of the turbine. Now, if this is of such importance with regard to one type of turbine-driven alternators, why does it not hold true for the other type?

If anything, the case seems to be slightly worse for the steam-driven turbine than for the water-driven turbine, as the inertia of the rotating element is usually smaller for the former and it will therefore more quickly attain a high speed.

The reliability of the regulating devices cannot be any greater for the steam turbine than for the water turbine.

Furthermore, a waterwheel-driven generator is often specified to withstand this runaway speed of the turbine, starting with maximum excitation, at no load.

As compared with this, many a steam turbine-driven generator has been designed which at normal speed and no load would not safely withstand the voltage obtained at maximum excitation.

It seems to me that there ought not to be any such great discrepancy between the designs of two such closely related types of machines. Either the steam turbine-driven generator is not designed with enough margin of safety, or the design of the water-turbine-generator is being unnecessarily handicapped. Experience has shown that the latter is not the case. What about the safety of the other?

F. H. Clough (by letter): Mr. Lamme has given a very able and comprehensive review of the design of large turbo-generators, and I should like to add my appreciation of the way in which he has discussed the difficulties that have been encountered, and described the methods that have been adopted to overcome these difficulties. In most points I can thoroughly endorse his conclusions.

In discussing the paper I should like to suggest that the author has not given sufficient prominence to the possibility of running generators above the critical speeds of their shafts, as in the first portion of his paper several forms of rotor construction are discussed and all these lead up to the solid forged rotor with slots cut out in the periphery.

The company with which I am associated has for some years past been building high-speed machinery (mostly for 3000 revolutions) in which the critical speed is about half the running speed, and this practise has proved itself to be entirely satisfactory.

It was necessary in the first case to design a bearing which would give a slight amount of freedom at one end of the shaft to avoid danger when passing through the critical speed, and, with this precaution, no trouble has been experienced, and I am inclined to think that the running of a machine with a low critical speed is better than a stiff shaft machine, provided the same amount of care be taken in balancing in both cases. When machines are designed for the highest possible outputs and speeds, the critical speed of even a solid rotor tends to become uncomfortably close to the normal running speed—and in some extreme cases may be even below it.

It has been suggested that the shock caused by an accidental short-circuit might cause trouble to a machine with a low critical speed, but no such effect has been noticed in practise.

The use of a small diameter shaft with a low critical speed makes a much more consistent design of the rotor, as on account of the comparatively small torsional forces and the high rubbing speeds of the bearings the diameter of the shaft is usually small in the journals. Further than this, the use of a small shaft gives opportunities for ventilation of the rotor which cannot be obtained with a solid forging, and also allows a suitable depth of rotor punching to withstand the centrifugal forces which occur.

B. G. Lamme: There seems to be an impression among some of those who have discussed the paper that I am advocating new and higher temperature limits than we have at present. I had no intention of giving such an impression, but simply meant to bring out that we have, in some cases, temperatures of 125 deg. cent. at present and that we must therefore consider such temperature limits from the commercial standpoint. We have had relatively high temperatures in practise for a good deal longer than many people think. Numerous machines which have been running for years attain temperatures of 100 deg. cent. or over at the hottest parts, although insulated with fibrous materials, and in many cases these machines have had a comparatively long life. However, where the actual temperatures have been materially higher than 100 deg. cent. we have had to protect them by the use of the mica class of insulating materials.

There has been great misunderstanding regarding the actual highest temperature obtained in commercial machines. When we say that 90 deg. is the limiting temperature for fibrous insulation, we should really say that it is the limiting temperature, by certain specified methods of measurement. In addition to this measured temperature, we must consider the internal drop which brings the actual temperature of the hottest part to 100 deg. or even higher.

I have had very considerable experience with mica insulations, both in high-voltage armatures and in field coils, in generators and in other kinds of apparatus; and, based on my experience, I am willing to state that mica insulation, well put on, is as safe at 125 deg. cent. as ordinary fibrous insulations at 90 deg. cent., on the basis of the same methods of measurements that we ordinarily use, such as thermometer, resistance, and exploring coil or thermocouple methods.

Turbo-generator fields furnish one fairly accurate means for determining permissible limiting temperatures. In the parallel type of slot rotor described in the paper, the field windings are completely embedded in iron and, in some cases, all parts of the winding have about equal temperature rise. In such cases, therefore, the resistance measurement of the field gives a fairly reliable measure of the true temperature obtained. In such fields I have seen fibrous insulations worked at 125 deg. cent for several months before being mechanically ruined. This gives an idea of what some fibrous insulations will stand. In other cases, a temperature of 100 deg. cent. or slightly higher has been found in machines which have stood up for many years. In the case just mentioned, where 125 deg. with fibrous insulation was attained, the field was eventually rewound, with mica in the slots and asbestos between turns, and has stood up without injury, as far as has been determined, up to the present time. In fact, it has run considerably above the 125 deg. cent. temperature at times, as the load conditions were increased after the field was rewound. A number of cases are known where, with mica insulation, 150 deg. cent. has been attained for long periods without any apparent injury.

Available data thus indicate that, where reasonably accurate measurements of the *hottest* part of the winding have been made, we have actually encountered much higher temperatures than usually supposed. Our general ideas of 80 to 90 deg. cent. limits are therefore based upon relatively crude methods of measurement. It has long been known by designers that, in designing certain windings, we must actually insulate for considerably higher temperatures than the ordinary methods of measurement will indicate.

Referring briefly to some of the features brought out in the discussion, Mr. Torchio mentioned the use of a metallic sheath over the armature winding. I will say that this has been considered at various times, but one difficulty lies in the fact that e.m.fs. will be generated in this sheath, necessitating that it be carefully insulated. Moreover, in general, it is necessary to laminate the conductors in the coil to eliminate eddy currents. A continuous sheath would be subject to such eddy currents, and, as this sheath would be considerably wider than the conductors in the coil, in many cases it would be subject to excessive losses due to such eddies.

In reference to the short-circuit tests referred to by Mr. Torchio, I do not think that all the difference shown is accounted for by the difference between the parallel and radial slot rotors. The arrangement of the end windings in the machines was quite different in the two cases. The number of conductors per slot in one case was only two-thirds that of the other, while the number of conductors in series was also considerably smaller. All these differences, combined, should account for a considerable difference between the two machines, but not nearly as much as Mr. Torchio has shown. However, I am not prepared to account for all the difference, as I do not know all of the conditions.

Mr. Fechheimer evidently did not understand my remarks in regard to making the regulation poorer by doubling the rating. I do not believe that I really implied that we could double the rating on a given machine. I simply assumed a double rating to indicate, in a general way, how the regulation and the short-circuit current would be affected. This was simply an illustration, not a statement of general practise.

Mr. Fechheimer raised the question as to how I measured an air velocity of 10,000 ft. (3048 m.) per minute in some parts of the air paths of turbo-generators. I will say that this was not measured directly, but was determined by measurement of the total quantity of air per minute fed into the machine, and the cross-section of the smallest openings through which this air had to flow. This, in some instances, indicated an air velocity in certain restricted parts of the path which exceeded 10,000 ft. per minute.

Mr. Junkersfeld suggests that 85 deg. should be the limit for fibrous insulations. This temperature is probably based upon his experience with measurements at the hottest part which could be found, by exploring coil or otherwise, on the outside of the insulation of high-voltage coils. If this is the case, I agree with him in general, except that possibly his figure is too high for very high voltage machines, in which there is possibly an internal drop of 20 deg. cent. from the outside surface of the insulation to the hottest part inside. This would bring the temperature up to 105 deg. cent. on my assumptions, which would be right on the ragged edge.

Mr. McCormick criticises my statement that the stator teeth can be hotter than the copper. In the general discussion of flow of heat and temperature rise in my paper, I was dealing largely with the general problem. What I wished to bring out was that, in the turbo-generator, the temperature was likely to be much higher at the center of the machine than in ordinary machines. In some cases, with ordinary machines, it was found that, even at full load, the temperature of the armature teeth was higher than that of the inside copper. However, this has rarely been found true in turbo-generators, although a few instances of this sort have been noted. In the example cited by Mr. McCormick, where the temperature of 104 deg. would be attained in the copper at the hottest part, he then adds a very considerable amount, in addition, for rise due to eddy currents. However, if the winding were so completely laminated, or so arranged that eddy currents were practically absent, then the result indicated in his example could be a possible one, although mica insulation would be required. Such cases, however, are unusual, and were brought into my paper simply as one extreme condition. I did not intend to give the impression that tooth temperatures higher than copper temperatures were common at heavy loads.

Mr. McCormick states that mica is a much poorer conductor of heat than varnished cambric. I will take some exception to this point. I will admit that mica insulation, as usually built up, is liable to be poorer than varnished cambric and such materials, as the mica laminae may not be in contact with each other. But when mica insulation is built up by some of the new processes, as referred to in my paper, where it is made almost bone hard, it is practically as good a heat conductor as varnished cambric, or other materials.

Mr. Bache-Wiig brings out the question that the overspeeds allowed in turbo-generators are very small compared with those in waterwheel practise, and thinks that we have not placed the limit high enough.

In answer to this, I will say that in the waterwheel-driven generator, the overspeed is placed at the runaway speed which could be attained by the waterwheel if the load were thrown off suddenly. In the case of the turbo-generator units, the overspeed attainable by the engine, in many cases, is so high that it is impracticable to build either a turbine or a generator which can stand such speed. This being the case, and as automatic cut-offs therefore must be relied upon, it then becomes a question as to what overspeed should be allowed before the cut-off can act positively. It is generally conceded by turbo manufacturers that the automatic cut-off must work under 15 per cent overspeed, and on this basis, 22½ per cent overspeed, as referred to by Mr. Bache-Wiig, is ample margin. In general, if it will not work at this speed, it will not work at all.

A paper presented at the Midwinter Convention of the American Institute of Electrical Engineers, New York, February 26, 1913.

TEMPERATURE AND ELECTRICAL INSULATION

BY C. P. STEINMETZ AND B. G. LAMME

The problem of permissible temperature limits in electric apparatus is largely that of the durability of the insulation used. As this may consist of materials of widely varying heat-resisting qualities, the problem resolves itself into one of consideration of the properties of the materials themselves.

The durability of insulation may be considered from two standpoints, the mechanical and the electrical. Tests and experience have shown that temperatures which may ruin the insulation, from a mechanical standpoint, may not radically affect its dielectric strength. This is particularly true with moderate voltages, where the insulation serves largely as a separating medium. The purpose of the insulation usually is two-fold: First, it must serve to separate, mechanically, the electric conductors from each other, and from other conducting structures, and second, it must withstand the voltage between the electric conductors, and between the electric circuits and other conducting parts. In lower voltage apparatus, usually only the former function applies, as the mechanical separation is more than sufficient to withstand the voltage used. The dielectric strength of the material is, however, of first importance in high-voltage apparatus.

A great majority of the electrical "breakdowns" on low-voltage apparatus is due to mechanical weaknesses, as far as the temperature problem is concerned; that is, high temperatures may make the insulation brittle, or crisp, so that it may flake off, or powder, or crack, or be crushed by mechanical action, thus allowing the conductors to make contact with each other or with adjacent conducting material.

The " life of insulation " is an indefinite term and must be defined in time, mechanical strength, absence of foreign materials of a conducting nature, etc. Almost all insulating materials will be somewhat affected in time, and many of them tend to become dry and brittle. The rate at which deterioration occurs with any given material, is some complex function of the temperature and of other conditions.

Classes of Insulations

Insulations may be classified under three headings, depending upon their heat-resisting properties. However, all such classifications must be relative, for no absolute limit can be fixed, as there is no definite point at which injury or destruction can be said to take place.

The usual insulating materials can be considered as included in three general classes:

Class A. This includes most of the fibrous materials, as paper, cotton, etc., most of the natural oil resins and gums, etc. As a rule, such materials become dry and brittle, or lose their fibrous strength, under long continued moderately high temperature, or under very high temperature for a short time.

Class B. This includes what may be designated as heat-resisting materials, which consist of mica, asbestos, or equivalent refractory materials, frequently used in combination with other supporting or binding materials, the deterioration of which, by heat, will not interfere with the insulating properties of the final product. However, where such supporting or binding materials are in such quantity, or of such nature, that their deterioration by heat will greatly impair the final product, the material should be considered as belonging to class A.

Class C. This is represented by fireproof, or heat-proof materials, such as mica, so assembled that very high temperatures do not produce rapid deterioration. Such materials are used in rheostats and in the heating elements of heating appliances, etc.

All the above are relative terms. The first class, for instance, represents materials which are really more or less heat-resisting, but which deteriorate at lower temperatures than those in the second class, which are defined as heat-resisting. Also, the fireproof materials of the third class are not strictly heat-proof or fireproof, but will simply withstand very high temperatures for relatively long periods without undue deterioration.

In class A, the materials appear to have a very long life (or an almost indefinitely long life, aside from mechanical conditions) if subjected to ultimate temperatures which never exceed 90 deg. cent. Also, they appear to have a comparatively long life even at ultimate temperatures as high as 100 deg. cent. At materially higher temperatures than 100 deg. cent., the life is very greatly shortened, and temperatures of 125 deg. cent. will apparently ruin the insulation, from a mechanical standpoint, in possibly a few weeks, if such temperature is maintained steadily. However, for low voltages, the insulating qualities may still be very satisfactory, even at this temperature, and therefore the destruction of the insulation is purely one of injury or breakdown from the mechanical standpoint, as stated before. Tempera-

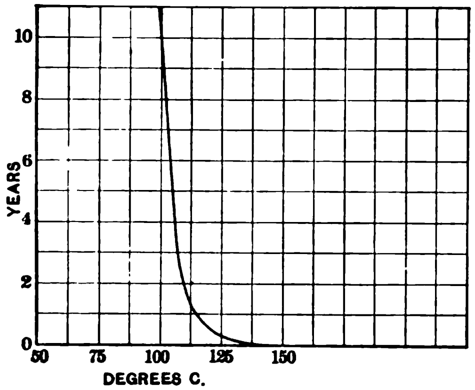


FIG. 1

tures as high as 160 deg. cent. on such insulations for a considerable period may not entirely destroy their insulating qualities, although, mechanically, such temperatures appear to be impracticable, except for very short periods.

In order to illustrate the relation between the possible life and temperature of class A insulation, Fig. 1 is shown. This must not be taken as representing actual results, but is simply intended to illustrate, in a merely approximate manner, the very great shortening of the life of insulation by increase in temperature.

It may be assumed that at very high temperatures, the insulation will have practically the same life, in actual hours of high temperature operation, whether the temperature is applied continuously or intermittently. For example, if an insulation has 10,000 hours life with a certain high temperature continuously applied, it is assumed that it will also stand the same temperature for 10,000 hours in short periods, provided the intermediate temperatures are low enough to represent an indefinitely long life. It is probable that under the intermittent condition, the life will really be slightly greater, due to the fact that depreciation will be largely mechanical, and the insulation may " recover," in some of its mechanical characteristics, after each period of high heating.

If, therefore, high temperatures are reached intermittently, with intermediate periods of lower value but still high enough to shorten the life of the insulation, it may be assumed that the total life of the insulation is the resultant of the life under the two temperature conditions.

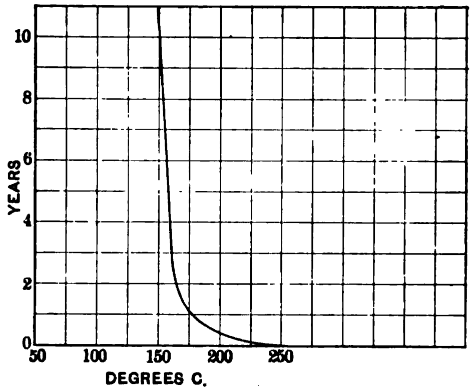


FIG. 2

In heat-resisting materials, such as those of class B, temperatures of 125 deg. cent. are comparable with 85 deg. cent. or 90 deg. cent. in class A, and 150 deg. cent. in the former is comparable with 100 deg. cent. in the latter. Fig.2 illustrates merely approximately the life-temperature curve of such insulations. As in Fig. 1, this should not be taken as an exact representation of the actual life. Due to the greater heat-resisting qualities of such materials, it appears that relatively higher temperatures are not as quickly harmful as in the first class.

In class C materials, it is difficult to give any reasonable indication as to the limits of temperature, except that very high temperatures (practically up to the point of incandescence) are found in some heating appliances.

Temperatures and Flow of Heat

As the insulation, in itself, is not usually the seat of generation of loss or heat, it is the temperature of adjacent materials which must be considered in defining the conditions in the insulation. The temperatures of the adjacent materials should therefore be considered only in so far as they affect the insulation itself, and where such temperatures do not affect the insulation, or the life of the apparatus, or its normal perfomance, they are immaterial.

Considering the influence of the temperatures of the adjacent media, the direction and amount of heat flow must be taken into account, as the maximum temperature in the insulation is dependent upon these. In the case of armature windings, for instance, the heat flow may be from the buried portion of the

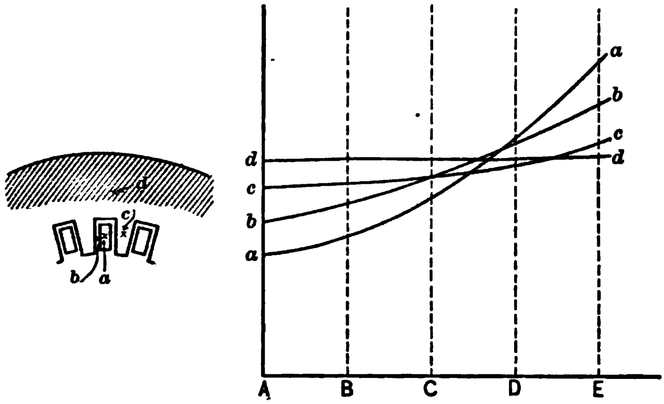


Fig. 3

coils toward the end windings. It also may be from the buried copper through the insulation to the armature teeth, or there may be a reverse heat flow from the iron to the copper, depending upon the various factors of construction, heat conductivity of the materials, amount of heat generated in the various parts, ventilation, heat dissipation, etc.

Depending upon conditions of heat flow and distribution, various methods of temperature determination may be used. No method is accurate, unless all the conditions of heat flow are accurately known, which is never the case in commercial machines.

The difficulties in the problem of commercial temperature determination are illustrated by Fig. 3.

In the figure, *a* represents the temperature inside an armature coil, *b* the temperature between the insulation and the iron of an armature tooth, *c* that in the body of the tooth, and *d* that in the body of the core at some point back of the coils and teeth. Let the temperatures at no load be represented on the ordinate *A*. Then, at some load, represented by ordinate *B*, the relations of the various temperatures have changed. At *C*, *D* and *E*, there are still greater changes, depending upon the heat generation and distribution. If the rated capacity of the machine is at *E*, for instance, then the armature copper is hotter than the iron, while if rated at *B*, the reverse would be true. Obviously, no rule can be formulated to cover these various conditions in different machines, nor even in a given machine, unless all the heat generation, distribution, and dissipation characteristics are known. Obviously, as far as the insulation is concerned, the temperatures of *a* and *b* are the only ones which need be considered.

All temperature determinations of a commercial nature are necessarily approximations, or relative indications, upon which proper margins must be allowed for the ultimate temperature possibly attained. Therefore, in apparatus where there are liable to be discrepancies of 10 deg. between the measurable and the actual ultimate temperatures, a limit of 90 deg. cent. should be allowed by conventional temperature measurement on insulations in which 100 deg. is set as the maximum temperature with a reasonable length of life.

The conventional methods of temperature measurement, as by resistance, and by thermometer, do not usually give the maximum temperature, but give either the average, or the outside surface, values, and, when measuring the temperature by these methods, which are the only ones generally applicable, an allowance must be made in windings for possible local higher temperatures. These methods apply especially to those machines of moderate or low voltages in which the insulation is relatively thin, so that the heat gradient from the inside copper to the outside surface is small. Also, they apply particularly to those machines in which the conditions of ventilation are not normally difficult, and in which a fairly thorough distribution and dissipation of heat occurs among the various parts, such as in ordinary direct-current armatures, induction motor primaries, stators and rotors of moderate speed alternators in which the width is relatively small compared with the diameter, etc.

As the ultimate temperatures obtained by the apparatus depend upon its rise above the room temperature, or that of the cooling medium, and as such temperatures may vary over a wide range, it is not practicable to specify or guarantee ultimate temperature of apparatus without also specifying the elements upon which it depends. This, therefore, results in specifying the temperature rise in relation to that of the cooling medium.

While most apparatus operates at materially lower cooling temperature than 35 deg. cent. to 40 deg. cent., yet such temperatures are sometimes reached for considerable periods of time in steam stations, and it appears therefore as justifiable to choose the permissible temperature rise, such that, at room temperature of 35 deg. cent. to 40 deg. cent., an ultimate temperature of 85 deg. cent. to 90 deg. cent. by conventional methods of measurement is not exceeded. This means, therefore, a temperature rise of 50 deg. cent. with conventional methods of testing, such as by increase of resistance, or by thermometer, in those insulations which can stand a continuous ultimate temperature of 100 deg. cent. with a comparatively long life. This allows an excess of 10 deg. cent. to 15 deg. cent. for local spots, or for the temperature gradient through the insulation. A less allowance should be made for this difference when methods of temperature measurement other than the conventional are used, and which approach more closely to the highest temperature actually attained.

When the above temperatures are liable to be materially exceeded for long periods, heat-resisting insulation of class B is recommended. With such materials, a temperature of 125 deg. cent. is comparable with 85 deg. cent. to 90 deg. cent. in the materials of class A. Therefore, on this basis of a room temperature at 40 deg. cent. or 45 deg. cent., rises of 85 deg. cent or 80 deg. cent. should not be considered harmful. However, in those special cases where the conventional methods may not sufficiently approximate local high temperatures, as may be the case in large turbo-generators, or in wide core alternators of large capacity, the rises of 80 deg. cent. or 85 deg. cent. should not be specified by resistance or thermometer, but preferably some lower temperature such as 50 deg. cent., thus allowing a very considerable margin for local higher temperatures. In such apparatus with the higher temperatures, which require class B insulation, there is liable to be less uniformity of heat distribution.

If special methods of temperature measurement, such as exploring coils or thermocouples, are used in such apparatus, the temperature limit of 125 deg. cent. should be considered, and not the conventional 50 deg. cent. rise. In those machines of this class which have relatively thick insulation, and consequently may have a high heat gradient between the copper and the iron (depending upon how much heat is flowing from the copper to the iron), an ultimate temperature of the inside insulation of 150 deg. cent. is considered as the limit, this being comparable with 100 deg. cent. with insulations of class A.

In certain classes of apparatus which are artificially cooled by air from outside the room, the cooling is accomplished partly by dissipating heat to the artificial air supply, and partly by dissipation into the surrounding room. If the temperatures of the cooling air and of the room are widely different, the resultant of the two temperatures should really be taken as that of the cooling medium.

The variation of the temperature rise has heretofore been considered as having a definite relation to the temperature of the cooling medium. However, it appears that it does not follow any definite simple law, but it is sometimes positive and sometimes negative, so that no satisfactory correction for room temperature is possible at present. It is therefore desirable to make the temperature tests at a room temperature as near as possible to some specified reference temperature, so as to make any temperature correction negligible. The reference temperature in the guarantees should therefore be such as can easily be secured; that is, it should be the average temperature of the places at which the apparatus may be operated. This is from 20 deg. cent. to 25 deg. cent., and as it is easier to raise than to lower the room temperature, the upper figure is advisable as a reference value. This reference temperature therefore should be chosen as 25 deg. cent., which is in accordance with the previous A.I.E.E. standard.

Measurement of Temperature

In the conventional methods of temperature measurement, by thermometer, and by resistance, many conditions should be taken into account, and good judgment is required, in all cases, or fallacious conclusions may be obtained.

There are many conditions which affect the accuracy of both the resistance and the thermometer methods of measuring temperature. The resistance method measures only the average temperature rise, and not that of local hot spots. However, it measures the internal temperature of windings, and therefore no correction is required for the temperature gradient through the outside insulation. The proposed margin between the result by the conventional method, and the actual temperature, can therefore be allowed, in the resistance measurement, as the difference between the warmer and the average temperatures in the windings. In the resistance method of measurement, the rate of transfer of heat from one part of the winding to another will not greatly affect the result, as the measurement indicates an average temperature, which would be obtained if the heat were equalized throughout the winding. However, the rate of flow of heat from the windings through the outer insulation to other parts will affect the temperature measurement by resistance, and preferably the measurements by this method should be taken during operation, in those parts where this is practicable, as in field coils, and some other instances. In those parts where the resistance cannot be measured during operation, this should be done as quickly as possible after shut-down, and the time taken to shut down the apparatus should not be unduly long. Preferably, during shut-down of rotating apparatus the normal current should be maintained on the apparatus until at least a relatively low speed is obtained. This would represent only an average condition, as the ventilation at lower speed is very greatly decreased, while the losses in the windings will remain normal, thus tending to give an increased temperature in the windings. It would be difficult to fix any definite rule which would give the exact temperature conditions during shut-down.

In the measurement of temperature by thermometer, considerable judgment is required. Wherever possible, the temperature should be taken during operation, but the thermometer with its pad should be so placed that it does not interfere with the normal air circulation. In thermometer readings, as usually obtained on windings, the heat gradient through the insulation must usually be allowed for, this being 10 deg. to 15 deg. as previously defined. However, depending upon the method of taking the temperatures, this allowance should vary over a considerable range, depending upon whether or not the method of measurement approximates the actual internal temperature. For instance, the total heat gradient from the inside copper to the outside air will be that through the coil insulation, plus the thick covering pad over the temperature bulb. If the gradient through the covering pad is very large compared with that through the insulation, the thermometer may indicate almost exactly the internal temperature of the copper; that is, the heat gradient through the insulation to the thermometer may be relatively small compared with the total gradient to the air. This is particularly true where the thermometer rests on a metallic seat which covers a considerable portion of the coil surface. In this case, the heat which affects the thermometer bulb will pass through a relatively large section of surface, with a correspondingly small drop in temperature, so that the bulb more closely approximates the temperature of the inside copper.

Where there is local heating in the windings, and a consequent liability of rapid transference of heat to other parts, the results obtained by the thermometer method will vary to some extent with the rapidity with which the actual measurement is made; that is, the more quickly the thermometer can be brought up to the full temperature, the more accurately the temperature of the hottest part is determined. With a very rapid method of measurement, it may be possible to measure practically the internal temperature of the copper of the winding before any great heat transference or dissipation has occurred. In such cases, obviously, the full allowance for the usual temperature margin should not hold. It should be fully understood that it is the ultimate temperature, and not the temperature rise, which should be considered as the limiting condition, and that the measured rise, plus the allowances for temperature gradient, plus the measured room temperature, is simply an indication of the possible ultimate temperature. By whatever method the temperature measurement is made, in all cases the results may be considered as more or less approximate, and in the end, it is the manufacturer who must supply the necessary margin over the approximate measurement, in order to make the machine safe.

A blind adherence to some particular rule or method of taking temperatures may lead to fallacious results in some instances. In armature windings, in particular, incorrect readings may be obtained after shut-down. For example, if the armature iron back of the armature teeth were hotter than the armature teeth and coils during operation, then the temperature to which the insulation is subject during operation may be considerably lower than that in the hottest part of the machine, due to the ventilation conditions when running. However, upon shut-down, the temperature at the insulation may rise to that of the hottest part of the machine, and therefore a false temperature, by any method of measurement, might be indicated.

Recommendations

That with class A insulation, 90 deg. cent. be taken as the ultimate temperature limit, as indicated by conventional methods of measurement, or those which give similar results, and that 100 deg. cent. be considered as the maximum ultimate temperature permissible in the insulation, where a comparatively long life is a requirement.

That 40 deg. cent. be taken as the limiting temperature of the cooling medium, or room, and that, therefore, 50 deg. cent. be the permissible rise by conventional methods of measurement, with class A insulation.

That 25 deg. cent. be taken as the reference air temperature. With the permissible 50 deg. cent. rise, this gives 75 deg. cent. as the average operating condition, by conventional methods of measurement, or 85 deg. cent. actual temperature, when the usual margin represented by the temperature gradient is added.

An exception to the rise of 50 deg. cent. can be made in those cases where space or weight limitations are such that higher temperatures, with consequent reduced life, are commercially economical, such as in railway motors. In such cases, with class A insulation, a rise of 65 deg. cent. with reference air at 25 deg cent. is at present accepted as good practise.

That with class B insulations, 125 deg. cent. be taken as the ultimate temperature limit, as indicated by conventional methods of measurement, or by equivalent methods, and 150 deg. cent. be considered as the maximum ultimate temperature permissible in the insulation. It follows therefore that 80 deg. cent. to 85 deg. cent. rise is allowable, with such insulations, by the usual methods of measurement.

No temperature correction should be made for variation of the cooling temperatures from the reference temperature of 25 deg. cent.

When the method of temperature measurement shows the highest temperature actually obtained in the insulation, the maximum temperatures specified for the given type of insulation should hold.

In the final decision on questions of temperature rise, the ultimate temperature should be the basis, rather than the rise.

A paper presented at the Midwinter Convention of the American Institute of Electrical Engineers, New York, February 26, 1913.

METHOD OF RATING ELECTRICAL APPARATUS

BY W. L. MERRILL, W. H. POWELL AND CHARLES ROBBINS

An investigation, covering the past three years, by the sub-committee on the subject of Rating Electrical Machinery, has shown conclusively that the present Standardization Rules of the American Institute of Electrical Engineers have, in many respects, become inconsistent as well as inadequate to meet the present service conditions which electrical apparatus is called upon to fulfill.

A comprehensive study of the subject has led us to believe that the rules representing fundamental methods of rating could be formulated. Such rules should not only meet existing conditions but also provide such flexibility as is necessary when limitations, due to existing materials, are changed by the future progress of the art.

It is highly desirable that the rating of electrical apparatus should be accurately defined and placed upon such a basis that it can be adopted internationally, since it is obvious that the service requirements in other countries are substantially the same as those in the United States.

This subject has been considered at great length by the International Electrotechnical Commission and an analysis of the work done by it leads us to believe that an international standard of rating is not only possible but essential. This is very desirable in view of the fact that the influence of the A. I. E. E. is recognized in the United States, Canada and Mexico.

The suggestions herewith submitted are based upon the accumulated data prepared after an analysis of service conditions, and represent the fundamental limits which should be considered in the rating of electrical apparatus, and it is believed that further investigation and the discussions on service conditions will substantiate the conclusions herein presented.

Capacity Rating

Item 1. *All electrical apparatus should be rated by output.*

Item 2. *Apparatus which delivers electrical power*:

Apparatus of this class should be rated in kv-a., or in kw. where the kv-a. and kw. are equal:

a. Therefore, direct-current generating apparatus should be rated in kw.

b. Therefore, alternating-current generating apparatus should be rated in kv-a. at a definite power factor which must always be specified, if not inherent in the apparatus (as for instance is the case in the induction generator.)

Item 3. *Apparatus which transforms from electrical power to electrical power*:

Apparatus of this class should be rated in kv-a. or kw. in exactly the same manner as in item 2.

Item 4. *Apparatus transforming from electrical to both electrical and mechanical power*:

Apparatus of this class should be rated in kv-a. or kw., which represents the resultant of mechanical and electrical outputs, the two components being specified.

Item 5. *Apparatus which transforms electrical to mechanical power.*

Since the input in apparatus of this class is measured in electrical units, and the output has a direct relation thereto, it is logical and desirable to measure the delivered power in the same units. Therefore the output of motors should be rated in kilowatts.

On account of the predominant practise of rating mechanical power in h.p. it is suggested that apparatus of this class for the present be given a double rating, *i.e.*,

kw.................... h.p..................

Service Ratings

An analysis of the service conditions that electrical apparatus is called upon to meet, leads us to recommend that ratings be divided into two general classifications, based upon the length of time of service the apparatus is in operation.

These two general classifications will again divide into three sub-classifications depending upon the character of the load conditions.

Continuous Rating. (A) In which, under specified conditions of operation, there is an attainment of approximately stationary temperatures, and no other limit of capacity is exceeded. This may be subdivided into the following classes:

(A 1) Constant service—where the load is continuously applied.

(A 2) Continued periodic service—in which the service consists of alternate conditions of load and rest, the periods of which must be specified.

(A 3) Continued pulsating service—in which the service consists of conditions of load and partial load, the magnitude and duration of which must be specified.

Short Time Rating. (B) In which, under specified conditions of operation, stationary temperature is not reached, and no other limit of capacity is exceeded.

(B 1) Short time constant—where the load is continuously applied for a specified period.

(B 2) Short time periodic—in which the service consists of alternate conditions of load and rest, the periods of which must be specified.

(B 3) Short time pulsating—in which the service consists of conditions of load and partial load, the magnitude and conditions of which must be specified.

Temperature of Electrical Apparatus

As the ultimate temperature is the sum of the temperature of the surrounding cooling medium and the rise of temperature due to the load conditions imposed upon the apparatus, it is necessary to take into account the variations encountered in the cooling medium. The cooling medium will vary in different places, depending upon geographical location of the apparatus, upon the season of the year, and the housing of the apparatus. Under this latter condition, it is found that the cooling medium may vary very widely in temperature, and may, in poorly ventilated places, and in locations influenced by other conditions, run as high as 40 deg. cent. even though the outdoor temperature may be lower.

It is believed that the average temperature of the cooling medium in which electrical apparatus is operated in practise and in which apparatus should be tested, is about 25 deg. cent.

It is therefore recommended that the standard temperature of the cooling medium be chosen as 25 deg. cent., and that all measurements and tests of electrical apparatus be based on a room temperature of 25 deg. cent.

Since, however, the life of the insulating material depends on the ultimate temperature attained, and this depends on the temperature of the cooling medium, it is necessary to recognize the fact that apparatus is frequently operated in locations where the cooling medium may have a range of temperature much higher than 25 deg. cent. It is recommended that 40 deg. be recognized as the upper normal limit of the cooling medium and that temperatures higher than 40 deg. should be accepted as abnormal conditions that require special consideration.

Temperature of Apparatus

1. Ultimate Temperature:

A. Cotton, treated cloth, paper, and similar substances which may fall in this general classification, a maximum ultimate temperature of 90 deg. cent.

B. Heat resisting materials such as asbestos and mica compounds, etc., a maximum ultimate temperature of 125 deg. cent. Insulating material should be considered as belonging to class B even if containing material of class A, provided that class A material is used only as a means of facilitating construction and may be entirely destroyed without interfering with the functions of the insulation. In this case the ultimate temperature is that of class B.

C. Fireproof materials such as pure mica, porcelain, etc. No temperature limit can be specified.

These recommendations do not apply to apparatus, or parts thereof, the principal function of which is the generation of or dissipation of heat, (such as rheostats and heating devices) and to structures and materials which are not damaged by heat.

2. Temperature to be determined by approved method as may be specified in A. I. E. E. Rules.

3. Under the stipulated load conditions, the following temperature rises on windings are recommended:

CONTINUOUS SERVICE	
Class A insulation	50 deg.
Class B insulation	85 deg.
SHORT TIME SERVICE	
Class A insulation	55 deg.
Railway motors, class A insulation	65 deg.
Class B insulation	90 deg.

Short time temperature rises may be on a slightly higher basis, *i.e.* five deg. as indicated in the foregoing table, because the fluctuating load will permit of substantially the same life, in years, when operating intermittently under slightly higher temperature.

It is recognized that railway motors require special consideration, owing to the relatively larger power required in a confined space, and that therefore a higher temperature rise must be permitted, and that the life of the insulation under operating conditions will be materially less than for the other classes of service.

As railway motors operate under good conditions of external ventilation when carrying load, the temperature rise of 65 deg. is permissible.

Commutators. The heating of a commutator is due to several causes:

a. Conduction of current in the bars.

b. Conduction of heat from the armature structure.

c. Conduction of heat from the bearings.

d. Heating due to brush friction.

e. I^2R loss between the commutator and the brush.

This heating has no material effect upon the life of the insulation between the commutator bars, except that mechanical distortion may take place. The effect of the heating may, when excessive, cause deterioration of the commutator due to the distortion of the bars, but if the structure is so designed that *harmful* distortion cannot take place, the heating limits of the commutator should be that of the winding to which it is connected, *i.e.*, 50 deg. cent. for class A insulation; except in those instances where class A insulation may have a permissible rise of 55 or 65 deg.; or 85 or 90 deg. for class B insulation.

Other Material. The temperature rise of any part of the apparatus which is in contact with insulation must not be greater than that allowed for the insulation with which it comes in contact.

A higher temperature than that specified is permissible for such parts of the apparatus as do not come in contact with the insulation and therefore cannot, by their higher temperature, deteriorate the insulation.

Bearings. It is recommended that bearings should fall into the general classification of " Other Material " of the machine not in contact with insulation, and may have any temperature that will permit of safe and successful operation.

SERVICE RATINGS

It is recommended that continuously rated electrical apparatus falling within class (A 1) be rated upon a basis of ultimate temperature without other limits of capacity being exceeded, and it is further recommended that no overloads be specified for apparatus except momentary overloads. Momentary overloads are defined as not exceeding sixty seconds, and should be as follows:

The maximum running torque of motors should not be less than 150 per cent normal rated torque.

Commutating apparatus should commutate not less than 150 per cent of the rated current.

Alternating-current machines should carry momentarily not less than 150 per cent rated current without regard to the rated voltage.

Motor-Generators. Generators should have the same overload requirements as designated under "Generators." The motor should have sufficient capacity and overload reserve to drive the generator.

Mechanical Service Conditions. All types of rotating machines should be so constructed that they will operate with safety at an overspeed of 25 per cent above the maximum rated speed, except in the case of steam turbines which, when equipped with emergency governors, should be constructed for 20 per cent over the maximum rated speed, and in the case of series motors no overspeed can be recommended on account of the varying service conditions.

Waterwheel generators should be constructed for the maximum runaway speed which can be reached by the combined unit under service conditions.

For a large percentage of apparatus which falls within classes (A 2) and (A 3,) and (B 1), (B 2) and (B 3), other limitations prevail. These types of electrical apparatus are designed to meet definite service conditions demanding selection of apparatus having a rating which will meet the exact operating specifications. These limitations are fundamental to the design, and it is therefore desirable that a definite commutation limit, a prescribed starting torque, a prescribed pull-out torque for a-c. motors, with a definite factor of safety for mechanical construction, be the limiting factors of the rating.

APPENDIX

Capacity Rating

Item 1. All electrical apparatus should be rated by output.

Item 2. In the case of alternating-current generators, the kilovolt-ampere is a much more logical unit than the kilowatt, since the capacity of such machines when expressed in kilovolt-amperes is generally independent of the power factor, and in any case is much more nearly independent of power factor than when the rating is given in kilowatts. However, in the case of machines in which the field limits the capacity, the allowable kilovolt-ampere rating varies considerably with the power factor and hence power factor should be specified in all cases.

Item 3. In the case of stationary transformers, the rating in kilovolt-amperes is almost entirely independent of power factor, hence this method is the only logical one in this case. For synchronous converters, the rating depends both upon the power factor at the a-c. end and the direct-current load. Hence, both should be specified. For frequency changers and motor-generator sets, the same conditions obtain as in Item 2.

Item 4. Thus a synchronous converter which delivers both mechanical power and electrical power should be rated as kilowatt mechanical power and kilowatt (or kilovolt-ampere at a particular power factor) electrical output. If it is used partly as a synchronous condenser as well, for power factor correction, the a-c. power factor should be specified.

Synchronous condensers, for power factor correction only, should be rated in kilovolt-amperes at zero power factor. When a machine of this type is operating as a synchronous condenser, the power factor will not be quite zero, since losses are supplied electrically, but the difference will generally be too small to consider.

Item 5. From the pure science standpoint, the kilowatt is a better unit of power than the horse power since it is based directly upon the absolute c.g.s. system, the watt being equal to 10^7 dyne-centimeters per sec., while if we accept the definition of the horse power as being 550 ft-lb. per sec., it is a gravitational unit and so is not strictly constant, but varies slightly with the latitude.

Also the kilowatt is the practically universal unit for the measurement of electrical power throughout the civilized world, while the "horse power" beside varying on account of its being a gravitational unit is defined differently in different countries In several European countries it is defined as 75 kg-m. per sec., while here it is 550 ft-lb. per sec. The difference is about 1½ per cent.

Then too there seems to be no real reason why both electrical and mechanical power should not be expressed in the same units, and since the kilowatt is much more desirable as a universal unit than the horse power for the reasons enumerated, it is recommended that the output of motors be expressed in kilowatts instead of horse power.

In the case of synchronous motors which are also used for power factor correction, the power factor at the motor terminals should also be given (leading power factor being understood) unless otherwise specified.

Name-Plate Stamping

It is evident for ratings (A-2) and (A-3), (B-1), (B-2) and (B-3), that certain stand tests or compromise runs of machinery, and particularly motors, are desirable. These stand tests in addition to determining various characteristics of the apparatus, should also determine as a basis of comparison the heating of the machines, and as the machinery is selected for various and sundry cycles of operation, two methods can be recognized for stand tests.

One is, in the case of class A machinery where the machine is run until it reaches a constant temperature under conditions where the losses and windage would be the same as the normal rating of the machine. This, however, in many cases might prove difficult, and a stand test made in accordance with a (B-1) rating could properly be substituted, as is now done in case of crane motors, machine tool motors, railway motors, etc., where the stand test may or may not be the same as the rating of the machine.

The present method of rating machinery on the so-called intermittent basis, is somewhat confusing, as for example; a motor rated " 10 h.p.—two hours intermittent," really means that the motor will perform certain cycles of operation, the equivalent stand test of which is two hours of continuous running at 10 h.p. This is often interpreted to mean that the motor when installed will have to be shut down after two hours of operation or the motor will be injured.

It is suggested that a system of name plate stamping be adopted which will designate the apparatus according to the various classifications set forth in this paper, and that in addition to the ordinary information given on the name plates, the equivalent stand test be included.

The following load and temperature curves indicate the nature of the six ratings as recommended by the sub-committee on rating.

temperature at the insulation may rise to that of the hottest part of the machine, and therefore a false temperature, by any method of measurement, might be indicated.

Recommendations

That with class A insulation, 90 deg. cent. be taken as the ultimate temperature limit, as indicated by conventional methods of measurement, or those which give similar results, and that 100 deg. cent. be considered as the maximum ultimate temperature permissible in the insulation, where a comparatively long life is a requirement.

That 40 deg. cent. be taken as the limiting temperature of the cooling medium, or room, and that, therefore, 50 deg. cent. be the permissible rise by conventional methods of measurement, with class A insulation.

That 25 deg. cent. be taken as the reference air temperature. With the permissible 50 deg. cent. rise, this gives 75 deg. cent. as the average operating condition, by conventional methods of measurement, or 85 deg. cent. actual temperature, when the usual margin represented by the temperature gradient is added.

An exception to the rise of 50 deg. cent. can be made in those cases where space or weight limitations are such that higher temperatures, with consequent reduced life, are commercially economical, such as in railway motors. In such cases, with class A insulation, a rise of 65 deg. cent. with reference air at 25 deg cent. is at present accepted as good practise.

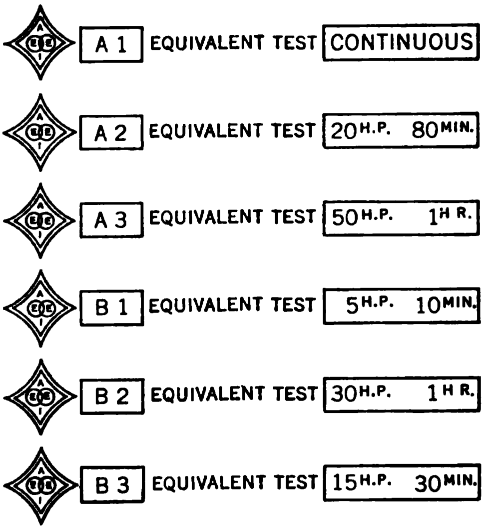
That with class B insulations, 125 deg. cent. be taken as the ultimate temperature limit, as indicated by conventional methods of measurement, or by equivalent methods, and 150 deg. cent. be considered as the maximum ultimate temperature permissible in the insulation. It follows therefore that 80 deg. cent. to 85 deg. cent. rise is allowable, with such insulations, by the usual methods of measurement.

No temperature correction should be made for variation of the cooling temperatures from the reference temperature of 25 deg. cent.

When the method of temperature measurement shows the highest temperature actually obtained in the insulation, the maximum temperatures specified for the given type of insulation should hold.

In the final decision on questions of temperature rise, the ultimate temperature should be the basis, rather than the rise.

It is also recommended that the name plates of all machinery rated in accordance with the future rules of the Institute bear the inscription of the A. I. E. E., as suggested in the following examples.



DISCUSSION ON "TEMPERATURE AND ELECTRICAL INSULATION" (STEINMETZ AND LAMME) AND "METHOD OF RATING ELECTRICAL APPARATUS" (MERRILL, POWELL AND ROBBINS). NEW YORK, FEBRUARY 26, 1913.

F. B. Crocker (by letter): The paper on "Method of Rating Electrical Apparatus" by Messrs. Merrill, Powell and Robbins, Sub-committee on Rating, states that it is "recommended that no overloads be specified for apparatus except momentary overloads. Momentary overloads are defined as not exceeding 60 seconds" and are 150 per cent of normal rated torque or current.

This method is known as "Maximum Rating" or "Single Rating" in contradistinction to the present A. I. E. E. Standardization Rules, which require a definite overload capacity. In the case of a gas engine, maximum rating is not objectionable, in fact it is practically inevitable. Most electrical apparatus, however, is self-destructive as compared with a gas engine, which protects itself by stopping if its load is excessive. An electrical machine does not protect itself when overloaded; it exerts itself to carry increased loads until it reaches the point of self-destruction. The difference between electrical machinery and almost all other kinds is like the difference between children who want to study too hard and those who do not want to study enough. The former must be protected against themselves and require totally different treatment.

In practise it is impossible to predetermine the load of an electrical machine because it varies considerably and continually, with changes always taking place even when supposed to be constant. If a machine with no overload capacity is used to drive its rated load, the usual variations that occur are likely to be sufficient to injure it. If supplied with protective apparatus to safeguard it against overload, that same safeguard will prevent it from carrying the frequent and considerable variations in load occurring in practise. Those variations would make it necessary to adjust the safeguard at a point so much above the rating of the machine that it will be heated considerably beyond its normal temperature if it should carry, except momentarily, the load corresponding to this adjustment.

The basis assumed for maximum or single rating is briefly as follows: Fibrous (organic) insulating materials, as cotton, paper, etc., have a very long life provided that their ultimate temperatures never exceed 90 deg. cent. If, then, the temperature of the windings of a machine insulated with such a material does not exceed 90 deg. cent. when operating continuously, the maximum load at which it can so operate is taken as its rated load.

If, however, the machine has been running a sufficient time to bring its temperature up to 90 deg., any overload applied to it would heat the insulation beyond the safe temperature and injure it. As already stated, no overloads are guaranteed on machines rated in this way. Beyond this point there is a disagreement between two groups of the advocates of single rating. One recommends that machines with such insulation should be rated to give an ultimate temperature at full load of 90 deg. The other group maintains that temperature rise, and not ultimate temperature, should be the basis for rating, so that a room temperature is assumed and deducted from 90 deg., the resulting temperature being the permissible rise.

The question is: What room temperature should be assumed? The temperature of ordinary rooms in which electrical machinery operates varies from 20 deg. to 40 deg. cent. The paper cited recommends " that 40 deg. be recognized as the upper normal limit of the cooling medium " or room temperature, which would make a standard of 50 deg. rise.

The ultimate temperature appears preferable because it eliminates the indefinite value of room temperature, but practically, the standardization of ultimate temperature would introduce confusion almost worse than no standard, because it would mean that before a sale could properly be made, investigation of the probable temperature of the customer's plant would be necessary and if one salesman estimated it at 25 deg. and another estimated it at 35 deg., the first would offer a generator for 65 deg. rise and the second salesman one for 55 deg. rise. The tendency would surely be to estimate the room temperature low, giving the customer as small a machine as possible, and endless trouble would result.

Moreover, any machine built in accordance with maximum rating must be applied to its work much more carefully than at present. Since it cannot carry any overload, it must never be overloaded, and therefore an intimate knowledge of the conditions under which each machine will have to operate, during its whole life, must be obtained. Even the most careful investigations can only go far enough to apply the motor properly in the first place, but they do not take care of future conditions. With single rating, a machine would have to be rated at the maximum load it would ever be required to carry, and who is the prophet to predict what that might be? It could not safely be depended upon to carry unexpected or even peak overloads higher than its normal rating.

For example, suppose a manufacturer buys a motor to drive a lathe and tests are made when the lathe is performing its regular work to determine the power required, the motor being selected and applied accordingly. If the manufacturer should obtain tool steel capable of taking a larger cut from the material in process, the single-rated motor would be incapable of securing the advantage of this new kind of steel.

Consider also a motor applied to a line shaft driving a number of tools. The manufacturer finds it advisable to add one more tool. If a motor of maximum rating has been chosen on the basis that its name implies, he cannot add the tool because of lack of capacity in his motor, whereas the present type of motor would carry such added load without difficulty.

If the line shaft gets a little out of alignment, which is very likely to occur, a motor of maximum rating would be incapable of meeting this new condition.

It is argued that single rating is safe provided the temperature limit is put low enough. The machine would have a margin for overload, although none would be guaranteed. This would not be satisfactory to the manufacturer because he would not get and would not be entitled to any credit for extra capacity not guaranteed. Nor would it be satisfactory to the customer because he is not informed as to the danger point. If we have overload capacity in our machine, the customer will surely find it out and use it under unusual circumstances, hence we must inform him exactly what its limits are. If he is so informed, then we cease to have single rating and come around to practically the present basis of rating, which includes overload capacity.

Overload capacity is the margin corresponding to the *factor of safety* used in all branches of engineering. With maximum or single rating there is really no factor of safety at all. The two papers by the Sub-committees on Revision of Rules and Rating both clearly accept 90 deg. cent. as the maximum ultimate temperature, measured by thermometer. This is the actual physical limit and it is merely an arbitrary matter if we subdivide it and call it the sum of 40 deg. room temperature and 50 deg. cent. rise. The paper by the Sub-committee on Revision of Rules (on page 85 of this volume), allows for the fact that local spots or internal temperature will probably be about 100 deg. cent. at which the insulation defined above has "a comparatively long life." If, on the other hand, the maximum ultimate temperature of the *hottest* point does not exceed 90 deg. cent., then such insulation has "a very long life" (see page 81, line 1). Hence with the rating proposed, which permits 90 deg. ultimate maximum temperature measured by thermometer on the *surface*, the temperature will actually be 100 deg. in the *interior*, certainly in spots. At this temperature the life of such insulation is considerably shortened. The proposed limit is therefore 10 deg. above the true safety point, which is that point at which any considerable deterioration begins to occur. This point for electrical insulation corresponds closely to the elastic limit of structural materials. It is this limit and not the actual breaking point that is taken as the basis in allowing a factor of safety. The point at which the electrical insulation breaks down corresponds to the breaking point of structural materials.

The proposed rating not only fails to provide any margin or factor of safety whatever, but permits insulation be be used at a temperature at which it admittedly suffers permanent and considerable depreciation. Hence the margin is actually a *negative* quantity, and the factor of safety is less than one. Imagine a bridge that was " rated " in this way, its load being stated at more than its elastic limit. Of course with single rating this is the only figure mentioned, and most persons, either from ignorance, carelessness or something worse, would assume that to be what the bridge should actually carry.

An apparently good reason advanced for single rating is its simplicity. It sounds simple and therefore attractive, but it would be still simpler to have no standard at all. The purpose of a standard is to protect the public against ignorance and unscrupulousness, and the " single " standard would do neither. The purchaser would naturally assume that he could use a machine to carry the load at which it is rated. Certainly the seller would have every reason to allow him to think so. We are very far from the millennium when every customer will be so wise that he will correctly allow for a certain margin of capacity and when every salesman will conscientiously unbosom himself of all the limitations of the apparatus which he is endeavoring to sell.

As one who has always had a deep interest in the Standardization Rules of the Institute and who took part in their original formulation as well as both of their revisions, the writer firmly believes that electrical apparatus should not be single-rated. The manufacturer, the salesman, the purchaser and the user all need to be protected against their natural tendency, which is deep down in all of them, to take that rating as the proper actual load for the apparatus. Engineering is responsible for the introduction of proper factors of safety in the building of machines and structures. It is a violation of the fundamental principles of engineering and seems nothing short of foolhardy for electrical engineers to attempt to get along without factors of safety. Not only figuratively, but literally, it is Ajax defying the lightning!

James Burke: My understanding of the two papers is as follows: That the intention is to limit the ultimate temperature at which different kinds of insulation will be graded. The practise heretofore has been to have various temperature increases stated in the specifications. Some specifications are for 35 deg. increase, some for 40 deg. some for 50, some for 55, and some for higher increase. My understanding of the first paper is that independent of the increase in temperature, the paper intends to set an absolute limit so that if a temperature of the surrounding air is higher than 40 deg. the increase would be considered as a special case and set at a lower figure, and that the real intent of the paper is that it calls attention to the penalty of running at higher ultimate temperatures than specified in the paper. That determines the danger point from the user's standpoint.

Now, in order to apply that to commercial condition, it is necessary to agree upon a certain average of maximum air temperature for special purposes, and this paper suggests 40 deg. as the maximum which will permit of 50 deg. increase in temperature in machines with fibrous insulation as a standard commercial machine. As Mr. Lamme has pointed out, it is the ultimate temperature that determines the safety working point. But in the manufacture of machinery it is necessary to bring into consideration the increase in temperature, because on this condition the ratings of machines are determined.

I further understand that the intent of these papers is not to recommend that an increase of 50 deg. be standard, but that it be considered as a limit, so that if one wants to establish a lower temperature increase it is not inconsistent with this paper, it simply calls attention to the danger point, and that no temperature increases in excess of this should be considered good engineering practise, or should be recognized by the Institute.

On the suggestions of different ratings in the paper by Messrs. Merrill, Powell and Robins the intent seems to be to show six different kinds of ratings that may occur. The first is the continuous rating, which probably covers the majority of the machines today. That is classed as A 1, and then come five ratings that refer to highly specialized applications, not for motors that are usually sold under every day conditions, but in which the particular service has been carefully studied, the nature of the load, and the nature of the load variations, in order to employ entirely special motors for each instance. The A 2, A 3, B 1, B 2, B 3 have been suggested. My personal view is that they are suggested more for the purpose of bringing out discussion, although I am not posted on whether that is correct or not. The three A ratings relate to machines that reach an ultimate temperature. All the three curves show that the test is continued until a maximum temperature is reached. The A 1 is for a continuous load, constant load, the A 2 for a load that is on, and then entirely off for a period, and then on again, and then entirely off, and the A 3 for a load which varies between the maximum and minimum, and in which there is some load on all the time. The *B* ratings are in the same class as the *A* ratings, but for conditions in which the ultimate temperature is not reached, or, in other words, in which the service is such that the period of use is so short that the ultimate temperature is not reached, and that is the distinction between the two. I do not understand how it is to be determined what the equivalent test is. For continuous service, the equivalent test is a continuous test. For the A 2 class an 80 minute test is considered as an equivalent test, in getting the ultimate and final temperature for the service, which fluctuates between no-load and full load, or between out of service and in service. Now, that 80 minute test might be a proper equivalent for one condition of load, but it would certainly not be the equivalent for all A 2's. I would like to ask the authors of the paper to tell us how the temperature of equivalent tests would be determined for each kind of service.

The use of the initials of the Institute A. I. E. E. on the nameplates of machines, I think is intended to mean simply that the temperature increase is not more than 50 deg., for fibrous insulation, and a higher temperature for other insulation, and also that the basis of rating is on the single rating or rating without overload, in other words the A. I. E. E. mark on the name-plate is a provision for 50 deg. increase and no overload. I do not think it is intended to mean that the A. I. E. E. approves of the A 2 rating being of the equivalent test of 20 minutes, or that there is any approval connected with that.

One very important point brought out in these papers is the matter of temperature correction, and I will say something on that subject after the papers on Temperature Correction are read.

Henry G. Stott: I wish to say one word in regard to the work of the Standards Committee. I think if you will review the past history of the Institute you will find that the present standing of the Institute work is largely due to the magnificent work and the unselfish support of the Standards Committee. I think it would be difficult to find a contract for electrical apparatus which has been drawn in the United States or in Canada in which you will not find the A. I. E. E. rules referred to as being part of that contract. I simply wish to convey my expression of appreciation to the Chairman and through him to the other members of the Standards Committee for the large amount of unselfish work they have done in helping along the Institute in this way.

Referring to the first paper by Dr. Steinmetz and Mr. Lamme, and coming to the curve Fig. 1, which applies to insulation of class A, I would say that the results shown in this curve do not conform to my experience with this class of insulation. I have had some rather bitter experiences covering some fairly large machines, and it is my experience that four years more nearly represents the life of insulation of this kind than ten years under the conditions outlined. In one plant the machines were 3500 kw. in size, operating at 6500 volts. The temperature rise on the average was about 40 deg. cent., and the surrounding air was 30 to 35 deg. cent., making an ultimate temperature of 75 deg. cent. The life of those machines has averaged very nearly four years, and as a result we have now changed the insulation from class A to class B, in the endeavor to get a longer life from the machines.

In regard to the second paper on Methods of Rating Electrical Apparatus, I would like to suggest, referring to the proposed name-plates on the last page, that where the letters "h.p." occur there should be substituted "kw.," and then within brackets after that, in very small letters, should be stamped h.p., so as to make the important thing the kilowatts and the unimportant thing the horse power.

Leo Schuler: I have listened with great pleasure to what Dr. Steinmetz said in regard to the new Standardization Rules, especially that it is the intention to adjust them to the standardization rules of other countries. It was of special interest to learn that you intend to do away with the overload capacities, which was hitherto the main difference between your rules and the German rules.

You know that the International Electrotechnical Commission appointed a special committee for the rating or standardization of machinery, which committee had a meeting in Zurich in January, and the conclusions of the committee with regard to the admissible temperatures were about the same as are proposed in Dr. Steinmetz's and Mr. Lamme's paper. It was also decided to state, not the rise in temperature but the absolute temperature, as the life of the insulation depends primarily upon its temperature. The limit adopted in Zurich for class A (cotton, papers, etc.) was practically the same as proposed in the paper of Steinmetz and Lamme. That is, for impregnated cotton, we have stated that the temperature, if measured by the usual methods, should not be more than 90 deg. cent. For cotton, without impregnation, however, we have fixed the temperature at 80 deg. cent. only, because it seems quite certain that dry cotton without any impregnation will not stand mechanical stresses quite as well as impregnated cotton. However, for coils entirely impregnated with some compound so that all air is excluded, the heat conductivity is so much better than in an ordinary coil that the temperature gradient would be much less, and we therefore propose to adopt for such coils a temperature limit of 95 deg. cent. instead of 90 deg. This is, however, only proposed for continuous-current coils because in such coils the mechanical stresses are less than in coils for alternating current. The same limit should be adopted for coils which consist of only one layer of copper because necessarily the heat conductivity is much better than in ordinary coils, and the maximum temperature will be much nearer to the measured value.

Now, with regard to room temperatures, you propose to have a standard of 40 deg. cent. The International Commission has proposed 30 deg. cent. This is a great difference, of course. However, as is stated also in the paper by Steinmetz and Lamme, 40 deg. cent. will usually occur only in steam stations. Now, if we consider how many standard machines, especially motors, are used in ordinary rooms, and how many are used in steam stations—this will probably be one per cent or less—I do not think we should, therefore, introduce 40 deg. cent. as the normal air temperature for all standard machinery. Thirty deg. cent. will be quite sufficient, and even when in hot summer days 40 deg. occurs occasionally this would not have much influence on the life of the machine.

I should like to raise another question. If the curve Fig. 1 in Dr. Steinmetz's paper, for cotton in air, is assumed to be correct, there will be another somewhat lower curve for cotton in oil, as the deterioration of the cotton is due to the combined influence of heat and oxygen. We have in Germany estimated that the difference will be about 10 deg. cent., and, therefore, we will allow 100 deg. cent. for oil transformers. Perhaps the authors of the paper will give their opinion on that point.

I should like to ask what the authors mean by "resultant temperature" when the cooling of the machine is affected by both the air temperature and another cooling medium, either air taken from outside, or water. Does it mean the mean temperature? That would not be correct, because it depends upon the distribution of the cooling effect on the air and the other cooling medium. In the new German rules the corresponding paragraph is as follows: "If besides the water or artificial air cooling considerable cooling takes place by the surrounding air, then the 'temperature of the surrounding' will be considered the temperature the machine or transformer attains when working not excited under the influence of the cooling medium." That means an additional test is made with the water turned on, under no load, and no excitation on the transformer, and then the final temperature is measured.

In regard to the second paper, I may mention that we use only the continuous "rating or rating for a certain time," that time to be stated on the name-plate; we do not speak of intermittent rating, intermittent service, or anything of that kind. I think that it is an unnecessary complication to adopt two temperature limits for continuous and intermittent service. This is simply a question of choosing the correct size of motor for the conditions of service. It will be understood that a motor for intermittent service will occasionally attain a higher temperature than it attains when tested under its rated load and within the rated time. It comes to the same result whether we use for a given service a motor of say 2 h.p. of one hour's rating with 50 deg. rise, or the same motor rated 20 h.p. for 90 min., with 60 deg. rise. I may further mention that we have in Germany tried to standardize also the time for rating machinery; we have adopted the rating of 10, 30, 60 and 90 min., which will probably meet all practical conditions.

W. L. Waters: Probably the most important statement in Messrs. Steinmetz and Lamme's paper is that "A blind adherence to some particular rule or method of taking temperatures may lead to fallacious results—and in the end it is the manufacturer who must supply the necessary margin over the approximate measurements, in order to make the machine safe." I think the committee would be ill advised to make, or to encourage the making of fixed rules at the present time for the measurement of internal temperatures in electrical machinery, as it is difficult even for the expert experimenters in the large manufacturing companies, who are giving their whole time to the work, to form an accurate idea of the distribution of temperature in a large machine.

Temperature tests are primarily to determine whether the insulation is operating at a dangerous temperature; but before this question becomes of any importance, it should be first known that the design of insulation, method of manufacture, and workmanship, are satisfactory. It has been known for thirty years that mica was a very good insulator for high temperatures, when it was used under conditions where its lack of mechanical strength was no disadvantage. It was also known that asbestos could be operated satisfactorily at high temperatures, but its lack of mechanical strength and hygroscopic nature rendered it almost useless as an insulator. The progress that has been made is in the development of processes for utilizing these high temperature insulation materials so that their material weaknesses are overcome; and also in educating workmen to be capable of carrying out these processes. It should be definitely recognized that on these points, the purchaser is almost entirely dependent upon the manufacturer, and that rules are of no assistance in passing upon them.

I would suggest that at the present time, the Standards Committee should state that it is extremely difficult to obtain any exact idea of these internal temperatures, though the purchaser would do well to satisfy himself that the manufacturer has provided insulation suitable for operation at the temperatures which exist, and further that the method of applying the insulation and the workmanship are satisfactory. I think that some such statement as this would warn the purchaser or operating engineer of the danger which may be expected, and at the same time, will guard against the false feeling of security which frequently results after a few rough temperature tests have been made.

H. U. Hart: Those engaged in the design of generators of large capacity have known for a number of years that the temperatures in the middle of the core or coils were much higher than the temperatures indicated by thermometers placed on the outside of the iron or on the exposed portion of the coil. These interior temperatures have been measured by thermocouples or other means, and suitable insulation provided that will withstand the maximum temperatures.

The present rules of the A. I. E. E. for determining the temperatures on generators are inadequate, as they specify the temperatures to be taken by thermometers or by increase in resistance method of the windings. They make no mention of the maximum temperature allowable for the different types of insulation on the interior of the core of windings. Due to recent commercial development of a number of instruments to determine these maximum temperatures inside the slot, operating companies have been unduly alarmed by the temperatures obtained.

I have examined the armature coils on a large generator having class B insulation, which consists principally of mica insulation used in combination with other binding material. The coils had been in continuous service, operating at temperatures in the slot of approximately 110 deg. cent. for seven years, and there was no apparent deterioration.

We have recently developed an armature coil having no fibrous binding material, the coil containing only copper, mica and asbestos. This coil would probably come under class C insulation. Sample test coils were heated for several months in an oven at 250 deg. cent., and there was no apparent deterioration.

While it may not be advisable at the present time to work at such high temperatures, still it is interesting to note that suitable insulation can be provided, having such a wide margin over the temperatures recommended in the above paper. It would appear that instead of specifying very low temperature rises, necessitating a very expensive machine, there would really be more margin in a generator, having a fairly high temperature rise in the interior of the slot and fire-proof insulation.

I believe the recommendations of Messrs. Steinmetz and Lamme for the maximum temperature allowable on types A and B insulation to be conservative, and I would be in favor of having their recommendations incorporated in the Standardization Rules of the A. I. E. E.

B. A. Behrend: I want to say a few words about the history of the labors of the Standards Committee as I remember them during the last fifteen years. A report prepared by the former Standards Committee, of which I was a member for almost ten years, embodied rules with which we are all familiar, because they have been with us for half a score of years, or more. It is absolutely necessary in formulating a new code to consider the effect which the former code has had on the electrical industry, and I want to run over the past in a few minutes to present to you that aspect of standardization.

The old rules were conceived as expressing, as nearly as the art knew it at the time, viz., fifteen years ago, the facts known to the designers of electrical machinery. It represented partly the knowledge of the time, and partly the knowledge of the past. In fact, the code has always standardized the past. If there is any lesson at all in history, the new code is going to do the same again, viz., it will standardize the past. If we wish to standardize at all we have to standardize the past, for the obvious reason that we do not know the future, for which we should endeavor to cast our rules. With the constant progress and constant changing of conditions, there is nothing more dangerous than a rigid standardization, and while I want to express my own personal appreciation for the excellent work that has been done and which has been embodied in this large volume of papers, I cannot help but fear that the standardization report which is to be the outgrowth of so much valuable material will be a handicap again to the industry.

The old rules were of no assistance to the designing engineer, or the engineer in charge of manufacturing plants. They were mostly incomplete, misleading, and fallacious, and, with the authority of the A. I. E. E. impressed upon them, they have been extremely harmful. No amount of facts presented could break the baneful influence of these fallacious rules. For many years I waged a campaign before this Institute in papers in which I presented voluminous experimental data, in order to demonstrate that the old rules as to regulation were useless, in fact thoroughly wrong and misleading. The method of measuring temperature by thermometer I contended was fallacious. But nothing could be done—the rules existed, handicapping the industry, the engineer, and the designer.

In the papers before us many of the old faults are corrected as we see them today, but let me ask you, will the new methods take care of the conditions as they will arise in the future? It is likely that, two or three years hence, these rules, by that time embodied in a code, will be a detriment again; may they not hamper again the designing engineers as well as the users of electrical apparatus? I fear that, if these rules are drawn too rigidly, with too keen a desire to embrace all the facts as we see them today, you will make a great mistake which it will be very difficult for you to correct in the future. Single ratings, or maximum ratings, are all right, but it is a new departure which is bound to cause much confusion until conditions have adjusted themselves to them. Our experience with the old code has shown that ten years after a rule has been advocated it has been adopted. Legislation follows the past, like standardization. I think we should endeavor to anticipate the future rather than legislate for past conditions, and that is, I believe, the weakness of the new Standardization Rules, for whose adoption I shall plead in a very general form, but, in fact, in so general a form as adopted by the German society. Such general form has the same advantage as the wonderful description of creation in the first and second chapters of Genesis. This story conceived probably thirty-five hundred years ago, has been worded in such beautifully general terms that it has fitted any theory of creation from the date of the Chaldeans to the evolutionists of modern times. The Book of Genesis is couched in such poetic, general language, that it can be applied to all conditions at all times. The report of the Standards Committee will be most sucessful if it also is couched in similar language, so that future generations will say, " these men anticipated our conditions."

James M. Smith: It appears that there are three standards to be considered. Two of them are adopted standards, the third a proposed standard. The first standard is that adopted by practise, and based on the experience of many years. The second is that adopted by the American Institute several years ago, but which has not been adopted by practise, and the third is the standard proposed in the paper by Messrs. Merrill, Powell and Robbins.

The standard of practise has a normal full load operating temperature that is conservative and safe even under severe conditions of operation or application. It permits overload capacities which are stated, and are, therefore, a protection to both customer and manufacturer.

The American Institute standard which has not been adopted in practise requires the measurement of temperature by resistance which has not proved satisfactory, and allows for temperatures 10 deg. higher than that adopted by standard practise. The present proposed standard not only suggests temperatures 10 deg. higher than the present standard of practise but does away with all overload guarantee excepting one of maximum torque capacity which is stated to be 50 per cent above full load torque. It is apparent that the present proposed standard is for the purpose of making our guarantees similar to those of European manufacturers. This is for two reasons:

First, because it is considered desirable to have a world-wide standard, and

Second, to enable manufacturers in this country to compete successfully with foreign manufacturers in the export field.

For universal adoption, the first thing to consider is what are the correct standards. The present American standards are conservative and produce machinery ample in capacity to meet all conditions of ordinary service. European standards are not likewise conservative, and machines built in accordance with them must be carefully applied to avoid undue depreciation.

If the American standard is changed to conform with the European, then American purchasers educated in our conservative methods of rating will suffer from incorrect conceptions of the new rating until years have elapsed and they become educated to the new close margin standard. In determining standards of rating for electrical machinery the prime consideration must be the adoption of standards which will produce machines that will best serve the customer. It is my opinion that the European standards are not those which will best serve our American customers. It is also my opinion that the American standards will serve European customers better than the present European standards. Therefore, if a universal standard is to be adopted, I believe that standard should be the American and not the European, that is, the machine should have conservative temperature ratings at full load, moderate overload capacities, with safe temperature ratings and high maximum torques for the peak load conditions which almost every machine must take care of occasionally.

In the paper by Messrs. Merrill, Powell and Robbins the ultimate temperature is stated at 90 deg. In the paper by Messrs. Lamme and Steinmetz this is defined as being temperature measured either by thermometer or resistance, and it is recognized that the hot spot temperature may be 10 deg. higher, making the maximum temperature of the machine 100 to 105 deg., which by reference to the curves is above the charring point of fibrous insulation. Hence the machines are rated above the point of rapid deterioration, or, as stated by Prof. Crocker in his discussion, they are rated above what corresponds to elastic limit and the breaking down point is only 50 per cent higher. I am strongly opposed to the new standard, or reduction of standard, for the following reasons: It would result in the purchaser receiving a smaller, lighter, and less powerful machine than before, with the sanction of the Institute.

Since these papers were issued I made tests on certain standard motors, both alternating-current and direct-current, which show that they could be increased in their rating from 15 per cent to 25 per cent and still meet the proposed new standards. Electrical apparatus is none too good, with a tendency to over-work it, and for many years an active effort has been made to raise standards, to fix them so that when the layman buys a 100-h.p. machine he gets a large and heavy one. The proposed change would result in his receiving a smaller and lighter machine.

Schuyler Skaats Wheeler: My interest in keeping up, or if possible, raising the standards for apparatus in this country is so great that I am impelled to say a word or two about one of the subjects before the meeting today.

I am an enthusiast about standards, and about the work of our Standards Committee. I think it is simpler, and therefore right, to use a single rating for motors and generators, provided this is done under proper conditions. But as I see it, the particular method of applying the single rating which has been proposed, incidentally has the effect of lowering the standard; this I am very much opposed to. Electrical apparatus needs plenty of margin or the adherence to a high standard; it is delicate and liable, unlike almost every other kind of apparatus, to approach the breaking point without the fact being visible to the user. The first thing he knows it is gone. For that reason alone, if for no other, it is desirable that it should have plenty of margin.

We are in the habit of producing electrical machinery in this country which, besides the power that it is said to be able to give, continuously, can give a much greater power for a short time. Therefore, its real capacity is a little greater than is represented to the customer. I think this reserve, or, as Prof. Crocker has said, factor of safety, should be preserved. I think it is very important to keep up the margins or factors of safety. I have been writing and talking on this subject for 25 years. My strong feeling on this subject really has nothing to do with the question of single rating by itself. I think it is perhaps a good idea to have a single rating, but let us provide a *lower temperature* for that single rating, as Prof. Crocker has suggested, so that the machine under the single rating for a given power has to be just as large as the machine furnished at the present time. This will entirely meet the objection that I have arisen to speak about.

I do not think we should in America make a smaller machine for a given power than we now do because the foreigners do. I think as Mr. Smith said, it would be better to try to induce the foreigners to come up to our standard.

I feel that this single standard will probably be put through, in any event, and, I do not think it worth while to make any great effort to stop it, as I think that would be useless. But I do want to go on record as to what my feeling is, so that later on I can call the attention of my friends to my position.

Philip Torchio (by letter): The main criticism of the two Sub-committees' reports is that the classifications by class *A* and class *B* insulation with their subdivision in class A-1, A-2, A-3, B-1, B-2 and B-3 is in my opinion too indefinite for every-day interpretation by the average user of apparatus.

The second criticism is that the service ratings recommended by the Committee are impractical for a great class of users of electrical machinery, like the central stations. I think that in this respect the Committee has taken too narrow a point of view of the practical service requirements in making the service ratings. To recommend that " electrical apparatus be rated upon a basis of ultimate temperature and that no overloads be specified except momentary overloads not exceeding sixty seconds" is absolutely impractical for a class of apparatus, like substation transformers, transformers on network, synchronous, converters, motor-generator sets, etc.

The systems of generation and distribution have up to the present been generally laid out on the basis of providing apparatus sufficient to carry the normal load with some overload capacity to take care of any occasional burn-out of a machine on the same bus or in immediate proximity on the distributing network.

All of this class of apparatus must have a liberal overload capacity for more than sixty seconds and at least one or two hours.

On the other hand, the apparatus that is limited in maximum output, like turbines, can be rated for a maximum temperature without overload capacity as recommended by the Committee. I think that this rating is a logical one because the apparatus is self-protected against excessive overloads and can therefore be safely rated for the maximum output.

On the other hand, apparatus that is not so inherently protected against overloads must logically be rated on a more conservative basis. To make a constructive criticism of what this basis should be, I would recommend that the Sub-committee on ratings be asked to prepare a comparative statement giving the corresponding capacities of a specific line of apparatus, like transformers, converters, motors, etc., giving the corresponding values of apparatus rated as at present with 35 deg. rise and 50 per cent overload for two or three hours, and the corresponding values of some apparatus on the new rating. By having this tabulated comparison one can then have a clear idea of what the recommendations of the Committee mean and so arrive at definite conclusions.

I am sorry that without this comparison between the present ratings and the recommended rating I cannot make an intelligent criticism of the new basis of standardization.

I would also recommend that additional information be submitted by the sub-committee on the Revision of Rules, giving the effect of vibration, moisture, etc. upon insulation as its reliability is not only dependent upon temperature but upon these other causes which should therefore be taken into consideration in arriving at the new standards.

Philip Torchio: I sent in a written communication in which I made criticism of these reports stating that the classifications of insulation, etc., are complicated and indefinite and that the single rating without overload is impractical except for apparatus that is inherently protected against overloads. I further pointed out that to make a constructive criticism of these reports they should be supplemented with a statement of how these recommendations compare with the present rating of machines.—In taking the above position in the matter I don't want it to be understood that I am opposed to the single rating, as I do favor it where it is practical. In fact the company I am connected with has been credited as having originated the single rating for turbo-generators, and I remember that in 1905 I wrote for my employers the first specifications for a single rating on turbines. This rating has since been quite generally adopted. In this instance the single rating is a logical one because the turbine is limited in its maximum output, and also its efficiency is good up to full output. When it comes, however, to transformers, synchronous converters, motor-generator sets, motors, etc., the same limitation does not hold true. For brevity I will not reiterate what has been said by previous speakers on this point.

You might say that the user can easily adjust himself to the new conditions by ordering large machines where overloads are required. However, I think that this would lead us into considerable misunderstandings and troubles. The present machines which have been built on specifications considerably more conservative than the present ratings of the A. I. E. E. have given and are giving considerable trouble. These troubles have been caused entirely by overheating in localized spots. There might have been a band that was put on to hold the windings of the armature which interfered with the proper ventilation, or there might have been faults of insulation, or the effect of moisture, dirt or vibration, or many other causes, but the fact stands out that machines running at probably 70 per cent of the proposed rating are giving trouble now.

We must give good service to users of electrical apparatus. It is to the interest of the entire electrical industry to do so. The central station, the manufacturer, the consulting engineer and everybody concerned with electricity is interested in giving good service.

I assume, as do some of the previous speakers, that the new specifications would make the machines smaller and not as liberal in design as the present rating. One of the previous speakers stated the contrary. If such is the case my argument falls, but if machines are going to be smaller than they are now, I don't think we are justified in making such a departure. I recognize that the manufacturers have to meet competition in their export trade, but that is not for us to discuss. I would emphasize the necessity of consulting with the users and the central stations by submitting the problem to them in a more emphatic way than it has been done; perhaps by a circular letter asking their views and criticism. This matter should particularly be submitted to organizations like the National Electric Light Association, the Association of Edison Illuminating Companies and the Street Railway Association, forwarding to these bodies the Committee's reports supplemented by the additional information which I previously suggested, that is, giving a comparison in parallel columns the equivalent ratings of machines rated under the present standards and the proposed standards.

M. G. Lloyd: A fundamental question in discussing ratings is as to just what the word " rating " is taken to mean and what distinction is to be made between " rating " and " capacity." The capacity of a machine is limited by its ultimate temperature and this depends upon a number of conditions, such as room temperature, power factor (when this is determined by the load), wave form (when this is determined outside of the apparatus itself, as in the transformer), etc. It has been pointed out by the Committee that ratings should be based on the ultimate temperature as the limiting condition in the case of most electrical machinery, but just what distinction should be made between the capacity of the machine under working conditions and its rating? Capacity is a variable quantity determined by conditions. Should a rating also be variable, or should it be a fixed quantity for a given machine? This question is not clear in the Committee reports, especially in report No. 2. There seem to be several references to a determination of rating by the conditions under which the apparatus is to be used, and not by inherent factors of design and construction.

My own idea is that rating should be a fixed thing for a machine. The rating should be expressed for a definite voltage and definite frequency and should represent the load which may be carried under given limitations and certain definite conditions, and this quantity will not necessarily mean the capacity of the machine under any working conditions but only under some standard condition. For instance, a temperature rise of 50 degrees seems to be favored, based upon a room temperature of 40 degrees. Forty degrees is higher than the ordinary working temperatures. Nevertheless, in that case the Committee seems to have made the distinction that the rating shall be based upon a standard condition rather than the working condition. Elsewhere in the report, however, this distinction is not clear, as, for instance, in the case of power factor. Consider an alternating-current generator whose capacity, as expressed in volt-amperes, is not a constant, but varies slightly with the power factor, due to the fact that you cannot always run the excitation up to the necessary point to get the same amperes from the machine that can be taken at high power factor. Should a machine of that kind be rated in terms of conditions under which it is to be used, or should it be rated in terms of a standard power factor? As to most of these points, like temperature and wave form, the implied meaning in the reports is to base the rating on standard conditions, but with regard to other features, like power factor, this does not seem to be the case.

Charles P. Steinmetz: I wish to state, first, that there is still some misapprehension regarding the purpose of this convention. The sub-committees have endeavored to gather all the information and data they could get together, but these are necessarily incomplete, and the convention was called, therefore, for the purpose of eliciting such additional data and information as may be available, and more particularly to reach those classes of engineers who could not be reached, due to the nature of things, by the members of the committee, and to obtain their ideas. The class of engineers referred to are, more particularly, the operating engineers and the consulting engineers. The designing engineers of the country, are grouped together, locally, in a number of centers, and therefore can be reached and have been reached, but the operating engineers as well as the consulting engineers are scattered all over the country, and their views and their experiences are just as important as those of the designing engineers, but it is more difficult to reach them, because, as a rule, they do not volunteer information, and there is no place where you can go and round up, to use that expression, a very large number of them, and therefore we anticipated by such a convention as this, by bringing the matter up for final discussion in the Institute, we would be able, at least to get a considerable number of operating and consulting engineers to give their views.

In regard to the question of temperature as the basis for rating, there are two misapprehensions. The first is to mistake single rating for maximum output rating. The single rating proposed is not necessarily maximum output rating, and, secondly, the single rating as proposed, is not necessarily a higher rating than the rating which was previously specified with our old capacity, but may be higher or lower, depending on conditions of apparatus. Now, what is the purpose of rating? An ideal specification is to say the apparatus shall operate for a long term of years without self-destruction. That is the ideal specification. In practise, however, certain reference conditions, must be definitely stipulated. The room temperature must be limited to a certain maximum, perhaps 40 deg. and the maximum insulation temperature must be limited to a definite value, 100 deg., or 150 deg., respectively, according to the class of insulation employed. Then the specification of the apparatus is that that apparatus should run indefinitely, and should give a good life, for any room temperature up to 40 deg., under any conditions of operation where the maximum insulation temperature does not exceed 100 deg. cent. Now, you see that these two limitations immediately give you a definite power value. It is a definite value of output, which gives a maximum of 100 deg. cent rise in temperature at a maximum of 40 deg. temperature in room. Under any other condition you can take more power out of the apparatus or less power. What we propose then is, as single rating, to give the rating which the apparatus would have and the maximum output which it can carry at a room temperature not to exceed 40 deg., and with an insulation temperature not rising above 100 deg.

Now then, if you operate that apparatus at a lower room temperature than 40 deg. you can get a larger output. If you operate the apparatus for a short time only at 40 deg. room temperature, you can get a larger output, for time and temperature in insulation, as in many other things, are interchangeable.—It is not true without further qualification, to state that cellulose fibre carbonizes at 100 deg. cent., or 120 deg. cent. The carbonization temperature of self-destruction is a function of time, and the shorter time allows higher temperature. Now you will understand that what the present proposed single rating means is this; it gives a maximum of output which the apparatus can carry continuously at any room temperature up to 40 deg.

Let us compare that with the previous specification of the rating and overload margin. Under that specification you were no better off; the intended margin was not sufficiently definite. When buying a machine at the old rating, *i.e.*, at a certain rating with a certain overload guarantee, you knew much less what you could get from the machine than you will under the single rating. It has been said that the machine should have a safe margin of 25 per cent in output. That means, that if you know the machine must carry a certain load, you will buy a machine rated at that load. On the old basis you would buy a machine capable of carrying a 25 per cent greater load. If it should happen that the load is steady and you never have any overload, you merely have thrown away 25 per cent of the output of the machine, and have spent more money for the machine than was necessary. You have bought a larger machine than was necessary. If it should happen that the average load, equal to the rated load, should fluctuate up or down 10 per cent, then you still have an unnecessary margin of 15 per cent. If it should happen that your load varies 50 or 100 per cent, it means that your margin of 25 per cent is worthless and your machine will burn out. The margin means nothing, and you cannot say that you will buy a machine for that rated load and that it will be safe. What you must do when using the old rating is to say that the machine has a certain rating and has 25 per cent overload capacity. The maximum output required of it is a certain known value. The maximum output the machine can carry, is 25 per cent overload, and this is what you require the machine to carry. From this you must deduct 25 per cent in order to arrive at the normal rating of the machine which you require for your purpose. You can simplify things by leaving out that 25 per cent overload, which is not overload, and merely say the maximum output of the machine is so much, or your maximum load is so much, and therefore, you buy a machine rated at whatever you have to carry. You see that there is no difference in the one way of rating or the other one, with the exception that in the old way the Institute established 25 per cent overload as the average satisfactory margin. But wherever you do not need the margin you get too large a machine. Where you need a larger margin, as in the machines to which Mr. Torchio referred, the machines complained of, you burn out the machines, and the Institute rules are of no value. The new proposition is to throw on the customer the burden of determining the appropriate overload capacity to be provided in each case. At the present time the industry is far enough advanced for the operating engineer to know enough to select his own margin, to know that he must allow no margin in such a case as the steam turbine driven alternator, and that he must allow 50 per cent or 100 per cent margin or even more, in such cases as synchronous motors operating in substations. By the new method the purchaser can get a machine to suit the conditions of each case. This was not the case where we had a rating and allowed a standard uniform overload.

We propose to bring up here for discussion the proposition to go a step further than heretofore and merely give a single rating, and then say that from this single rating you have to subtract whatever margin your particular requirement may need, to arrive at the size of the machine adapted to the diversified conditions of the industry.

Henry G. Reist: I want to point out that probably we have become accustomed to rating machinery with large overloads, due to the long experience with the steam engine. A reciprocating steam engine always had large overload capacity. On the other hand, the gas engine, and the waterwheel, and now the steam turbine, are rated practically at the maximum load. There is a reason for this. The steam engine works with the greatest economy at about the point at which it is rated. The overload capacity is not put there for possible overloads which might come on unexpectedly, but it is rated at the point where it gives the highest efficiency. The same holds true with the waterwheel, the gas engine and the steam turbine, they rate them at the points where they give approximately the best efficiency. We should rate electric motors so that they will have the best average efficiency. At the present time many electric motors are shamefully underloaded. Running them at an underload means that the customer pays more for his motor than he should pay, he has lower average efficiency, and with an induction motor lower power factor, which means a more expensive transmission line, greater losses on the line, and bigger machines on the other end. We had a definition given us this morning of what an engineer ought to be, or ought to do, and I might point out that one of the duties of an engineer is to get the most use out of a given amount of material and expense.

The reason, probably, why motors are used underloaded is because the customer hesitates to put a greater load on them than is designated on the name-plate. If his work requires 40 h.p. he will probably say, " I will make it safe, and get a 50-h.p. motor," and he will run it all the rest of his life at 40 h.p., and probably under that, and consequently he has poor efficiency and all the things I have just pointed out.

I was very much interested in the proposed ratings for motors, that is, continuous and various intermittent ratings. It occurs to me that the schedule proposed is rather complicated, and I like much better the one that, as suggested, has been adopted by the German engineers, simply giving the rating, based on a run of a certain length of time; that is, it might be 10 min. and 30, 60, 90, as has been adopted by the German societies, or it might be some uniform increase, which I would like a little better, such as, for instance, 10, 20, 30, 40, and double that, or equal values geometrically, between the length of time under which they are operated. From each of these ratings the consulting engineer can select the one that is most suited for his work, after he has definitely determined, as nearly as possible, what the work to be done is. It also seems to me then instead of giving them arbitrary letters and numbers as A 1 and A 2, and B 1 and B 2, they might simply use the length of time that the motor runs at its rated load as the name by which to know this particular rating. That would somewhat simplify the nomenclature, I think.

I just want to say one other word about the first paper, regarding the recommendation that we consider ultimate temperatures rather than rises. I must confess I am a little stupid on that point and do not quite see the difference, because it seems to me that, if we use an ultimate temperature, we must take some room temperature as a standard, and it is suggested that this be 40 deg., with which I entirely agree, and that the rise on top of that, which is recommended, is 50 deg., I believe, which, after all is a rise of 50 deg. I agree with Dr. Wheeler and several other gentlemen who have argued for conservatism. I question the advantage of going to too high temperatures in most cases. There are exceptions, such as in the case of the railway motor, and probably in a good many other cases, but when it is not necessary, let us keep to low temperatures. The difference in cost of construction between the two machines is comparatively slight, whether we keep a little lower temperature or go to higher temperatures; and then, if you use mica or other material for external insulation, there is always the insulation between turns which becomes particularly difficult with the smaller apparatus, where it is difficult to put mica around each small wire of which the coil is composed. If the coil consists of a few conductors they may be taken care of, but when you have twenty to thirty, as we have in many cases, it becomes very difficult, and to me the only solution at the present time is to keep the temperature down. Some time we shall probably find insulations that will stand higher temperatures, but I do not feel that we have them for general use at the present time.

B. G. Lamme: I shall not undertake to discuss the arguments brought forward, but I wish to bring out more fully some points which seem to be very much misunderstood. There seems to be, in general, a wrong impression about the limiting temperatures which have been proposed, as some of the members appear to think that we are advocating raising the present temperature limits. On the contrary, we are proposing to cut them down. It is apparently believed that in adopting a temperature rise of 50 deg. cent. that we are insisting that apparatus be built for that temperature. However, what we actually said was that the temperature rise shall not exceed 50 deg. measured either by thermometer or resistance, *whichever gives the higher result.* We can keep as far below 50 as we please, depending upon what margin we wish to allow. We simply say that 50 deg. cent. rise shall not be exceeded, and that with the limiting air temperature of 40 deg. cent. the ultimate temperature of 100 deg. cent. in the hottest part shall not be exceeded. It should be understood, however, although it is not brought out sufficiently clearly in the committee papers, that the 10 deg. difference between the measureable temperature and the actual hottest part should apply only to ordinary low-voltage insulations, such as 2200 volts and less. For relatively high voltages, with correspondingly thick insulations, a greater difference than 10 deg. cent. must be allowed between these temperatures.

In the present Institute rules, 50 deg. cent. rise by resistance is allowed, and on top of this, an overload of 15 deg. higher is allowed, giving a total permissible rise by resistance of 65 deg. cent. We claim that that is unsafe, except in those cases where the cooling air temperature is not over 25 deg. cent., for 65 deg. + 25 + 10 deg. internal drop = 100 deg. ultimate, which is the limit of safety. But taking air temperatures of 40 deg. cent., then 65 deg. + 40 deg. + 10 deg = 115 deg. ultimate, which we contend is unsafe. Therefore, we propose to cut out any conditions of the load which will put the ultimate temperature above 100 deg. cent. However, it must be understood that temperatures above 100 deg. cent. simply shorten the life of the apparatus, and do not mean immediate destruction. This shortening of the life, in most cases, does not show up during the overload condition, as this usually represents but a small portion of the total operating period. The carbonization of the insulation, however, usually occurs on the peak load, and not under the normal operation. We therefore propose to cut out those conditions of temperature which are liable to have a material effect in shortening the life of the apparatus.

It has been mentioned several times that it is a maximum rating that we are proposing, and it is claimed that the proposed single rating and maximum rating are the same thing. However, it is not our intention to adopt a true maximum rating. I can possibly illustrate the difference by an example:

Assume a cooling air temperature of 40 deg. cent. and a rise on top of that of 50 deg. cent. and an internal temperature difference of 10 deg., giving 100 deg. ultimate. That fixes the limiting rating possible without exceeding the ultimate limit. This we call the single rating. But with cooling air at zero, with this single rating we still retain the 50 deg. rise, so that the ultimate temperatures becomes 60 deg., and not 100 deg. However, under this same condition, the machine could have a maximum rating corresponding to 90 deg. cent, rise instead of 50 deg., or the maximum rating, with air at zero, would be 80 per cent greater than the single rating which we are proposing. A machine could have all kinds of *maximum* ratings, depending upon the air conditions, whereas, they can have only one *single* rating, which is fixed in value by the conditions of air at 40 deg. cent. It seems to me that this confusion of the proposed single rating with maximum rating is back of much of the misunderstanding of the subject which has been expressed here this morning.

As the present Institute rules allow 50 deg. cent. rise under normal conditions, with 15 deg. higher temperature for overloads, and as the new method proposes 50 deg. rise as the highest permissible, it is obvious that, in fact, the machine, under this new method of rating could actually carry as much overload as under the old method, the only difference being that, under the new method, we recognize the danger in this overload, and call attention to it, and recommend against it, while in the old method, we went ahead and blindly guaranteed it. The new rules indicate a definite danger point, while the old rules did not. That is the principal difference between them.

Alexander Gray: In regard to the two papers presented this morning, there are several points about which I do not agree with the writers.

Much attention has been paid to the apparent fact that the limit of a machine is the temperature rise of the insulation. This may not always be the case, because the ordinary iron which we use is not non-aging iron. There is non-aging iron on the market, iron in which the losses are small and do not increase with time, but that iron at present is rarely used for revolving machinery, because it is found that the losses are not reduced as much as would be expected, and also that such iron is liable to crack when subjected to long-continued vibration; it is well known that the long teeth of induction motors, when made with this iron, sometimes break off and damage the windings.

Regarding the temperature limit of 90 deg. which is suggested for class A insulation, I consider it to be too high. Present practise calls for a temperature rise of 35 deg. cent. on full load, and 50 deg. cent. on 25 per cent overload, for two hours. Now, it is well known that 25 per cent overload at two hours on the top of a full load run is equivalent to 25 per cent overload continuously; that is to say, our present motors will carry 25 per cent overload continuously, with 50 deg. cent. rise, and will not deteriorate rapidly, but few designers would care to guarantee 50 deg. cent. rise, measured by conventional methods, when the motor is known to operate in a room with an air temperature of 40 deg. cent. I should suggest that, with class A insulation, the upper limit, measured by conventional methods, should be put at 80 deg. instead of 90 deg. cent.

In discussing the second paper, the first point I would draw attention to is that all alternators should be rated in kv-a. and not in kw., even although the power factor be 100 per cent. There is a great deal of misunderstanding on this point at present. The Institute rules which we now have, state that a machine of 100 kw. output and 80 per cent power factor is a 100-kv-a. machine, whereas in practise such a machine would have a rating of 100 kw., but would have a kv-a. rating of 125. This point should be emphasized and made very clear in the new rules.

With regard to the rating of machines, we have, at present, continuous ratings, intermittent ratings, and short time ratings. Our Sub-committee on Rating suggests that this classification has not been carried out in sufficient detail, and now proposes to divide machines into two main classes, and six subdivisions in all. The large bulk of electrical apparatus is built for continuous duty, it being remembered that most machines, and particularly the copper in the machines, reach their final temperature in about two hours. That being the case, we have a comparatively small number of machines which are operating for less than two hours continuously, and these, it is proposed, shall be subdivided into five sub-classes. I consider that this matter has been carried too far and will lead to endless difficulty. I have asked several engineers in what class they would put a railway motor. Many of them said under the heading A 2, others, again, would put them in class B 2. We can think of many other cases in which it would be exceedingly difficult to put the motor in its proper class.

In considering what I would suggest to take the place of these five classes, I looked over a paper published in the *Journal* of the Institution of Electrical Engineers, by Dr. Pohl, and he suggests that for intermittent ratings an intermittency factor should be used: an intermittency factor of $\frac{1}{6}$ means that out of every six minutes the motor would be operating for one minute, and would be stationary for five minutes; a large number of applications, such as crane service, have perfectly definite intermittency factors. Now, a 50-h.p. motor with an intermittency factor of $\frac{1}{6}$ would operate at 50-h.p. for one minute out of every six and have an overload torque corresponding to a 50-h.p. machine, but if this machine be rated at 50 h.p. for half an hour, or at 100 h.p. for ten minutes, or at 40 h.p. continuously, what connection is there between the horse power rating and the overload capacity? The new suggestions are far from clear on this point.

With regard to bearings, considering that a large number of electrical machines, motors in particular, are in the hands of inexperienced operators, I do not believe it advisable for the Institute to recommend that bearings should fall into the general class of other materials, and have any temperature which will permit of safe and successful operation. Just as we specify temperatures in insulation, because the material gives no indication as to when it is going to break down, so in high-speed bearings, where there is no indication given that the bearing will seize, it is advisable in the interests of the public, who in the case of bearing breakdowns, will be charged with the use of poor oil, to limit the temperature of the oil in the bearings to 70 deg. cent., unless otherwise specified in some binding specification.

It must be remembered, finally, that it is not always the final temperature which limits a machine. The mechanical parts are built to suit a certain temperature rise, and if machines are designed for an air temperature of 40 deg. cent., then they will have considerable overload capacity with an air temperature of 25 deg. cent., but this overload capacity is not available unless the mechanical parts are sufficiently strong.

R. F. Schuchardt: In general I believe in a single rating, provided it is the continuous rating, and represents a safe margin. The other five ratings proposed in the paper of Messrs. Merrill, Powell and Robbins are undesirable for several reasons. First, we would have a number of different ratings for the identical motor, which means in a central station for instance we would have to carry a large storeroom full of spares where otherwise a few would do, and the number of motors to be carried in stock would be multiplied greatly. The continuous ratings should also be given if it is desired to have any of these special ratings. Second, the proposed ratings would complicate rate-making, which is already a pretty complicated matter. The basis generally adopted for power rates contains a primary charge for the maximum demand. In motor installations the primary charge is usually based on the rated capacity of the motors. Now you can imagine what complications would be introduced in rate making by such a multiplicity of special motor ratings.

I do not agree with a preceding speaker regarding the use of the Institute symbol on name-plates. It would be a very grave error to put this symbol on apparatus, as it would be generally interpreted as putting the stamp of approval of the A. I. E. E. on the apparatus which bears the symbol.

There are two minor points which should be changed. In the paper by Messrs. Merrill, Powell and Robbins the statement is made: "Commutating apparatus should commutate not less than 150 per cent of the rated current." In railway machinery this should be "not less than 200 per cent."

In a succeeding paragraph the following statement is made: "All types of rotating machines should be so constructed that they will operate with safety at an overspeed of 25 per cent above the maximum rated speed." Synchronous converters, particularly, run away very fast, and the speed may increase at a tremendous rate, so that should read considerably higher than 25 per cent for the safe speed limit.

One word more, on the much discussed point of ultimate temperature. About five years ago we had an experience in a large central station in the West, during which we burned up a very large generator, and as a result of that we made some detailed investigations of the safe temperature limits for the insulation used on this particular class of apparatus. After we had carefully studied all of the test results we finally decided that 80 deg. is the maximum safe point below the knee of the heating curve which should be allowed, and that point determined by means of an exploring coil laid along side of the armature coils in the machine.

The paper of Messrs. Steinmetz and Lamme recognized the fact that there may be hot spots as much as 10 deg. higher than the measured temperature. We also assume that there might be hot spots even above this exploring coil measurement but we do not feel that it is safe to work on so close a margin as recommended in these papers.

C. E. Skinner: We have all been familiar for quite a while with the fact that the life of insulating material is a function of time, temperature and the mechanical conditions under which it is used. We have endeavored for a long time to find what is the ultimate upper temperature to which different classes of insulating material can be operated without getting into trouble. The German standardization of 80 deg. for cotton insulating material has been referred to. It is very rare, at this time, to find windings of electrical apparatus which are not treated in some way so as to bring them into the 90 deg. class as outlined by the Standards Committee.

J. M. Smith: There is one point which is not at all clear to me. It has been stated this morning that 90 deg. ultimate temperature was the hottest temperature in the machine. Mr. Lamme, both in his paper and his statement this afternoon recognizes that the hottest spot in a machine may be 10 deg. or 15 deg. higher than the ultimate temperature. This can be seen by reference to his paper. The difference between these two points of ultimate temperature is a difference of 10 deg. in the temperature rise, and it is the difference of between 15 per cent and 25 per cent in the capacity of the machine. I would very much like to have this important point cleared up.

B. G. Lamme: I wish to explain one little point wherein there seems to be some misunderstanding. It has been stated in the paper on " Temperature and Insulation " that a limit of 100 deg. to 105 deg. was allowed for fibrous insulations. I do not find any place where 105 deg. is either mentioned or indicated. deg. to 15 deg. is mentioned as the possible internal drop, bu 15 deg. is tied up to 85 deg. This higher internal drop ref simply to cases where there was heavier insulation than ir ordinary moderate voltage machines. It was not brought plainly enough in the paper that with very high voltage mac the internal drops should be considerably higher than indic possibly 20 deg. to 25 deg. Where 25 deg. internal drc liable to be found, then, with fibrous insulations, 75 deg. n urable temperature would be the limit, and not 90 deg. should always be borne in mind that where the ultimate tem ture limit is fixed, the internal drop must be subtracted fron to give the measurable temperature, and this may be 90 de some cases, while in other cases it may be 80 deg., or eve deg. depending upon the type of apparatus.

James Burke: If there is one point of more importance another that has been brought out in these papers, I think the viewpoint which has been put on record that we hav recognize hot spots rather than any other temperature co tion. I think a great deal of the discussion has been bro about due to the difference between the two papers. Accor to one paper, we can rate machines higher, according to the c we would have to rate them lower. So if we are discussin paper of Dr. Steinmetz and Mr. Lamme, we can come tc conclusion that we may rate machines higher than the pr commercial practise. If we discuss the paper by Messrs. Me Powell and Robbins, we are in the class of conservatism w has been advocated by so many speakers here and previousl

In the paper by Dr. Steinmetz and Mr. Lamme they attention to the fact that with fibrous insulation 100 deg. sh be the maximum temperature, and then they assume 10 for the difference between the maximum hot spot and conventional measured temperature, and then come to the clusion, when you measure temperatures by conventi methods, quoting their words, as follows: "The conventi methods of temperature measurement, as by resistance an thermometer, do not usually give the maximum tempera but give either the average or the outside surface values, when measuring the temperature by these methods, which the only ones generally applicable, an allowance must be ma windings for possible local higher temperatures." Now, i consider that the 100 deg. hot spot recognized in that p allows 10 deg. for some temperature gradient, leaving 90 ultimate by conventional methods, with an air temperatur 40 deg., we have 50 deg. increase by conventional meth That is the same as the existing rule of the Institute that been in force for several years. It is not, however, in line what the commercial practise has been. The commercial p tise has been 40 deg. or in some cases 45 deg., so that if we c pare the present commercial practise of 40 deg. with this posed 50 deg., we can rate our machines up, as Mr. Smith pointed out, from 15 to 25 per cent. I have made some similar tests and have found about the same conclusions. This comes from raising up the magnetic densities 12 per cent and the copper densities 12 per cent, and 12 per cent increase in current means approximately 25 per cent increase in copper loss and 25 per cent higher temperature, making a difference of from 40 to 50 deg. On that basis, if it is interpreted in that manner, we can rate machines up, and we are getting away from the direction of conservatism; but if we take the other paper, by Merrill, Powell and Robbins, we find that they advocate a maximum temperature of 90 deg. They say under the heading of " Temperature of Apparatus," as to ultimate temperature, the following, " a. Cotton, treated cloth, paper and similar substances which may fall in this general classification, a maximum ultimate temperature of 90 deg. cent." They evidently mean the hot spot temperature. The next thing is, how are we to determine that hot spot temperature. They do not say how it is to be determined, but they do say, " Temperature to be determined by approved method as may be specified in A. I. E. E. Rules." Now, then, the whole issue regarding temperature comes into that paragraph of what the A. I. E. E. Rules will be for determining the temperature. If the Institute Rules adopt a method that really gives the hot spot temperature, we will have lower conventional method temperatures than heretofore and more conservative.

We have among the papers to be presented at this convention one by Mr. R. B. Williamson, who shows one method of calculating the hot spots or the difference between the temperature of the laminations and the temperature of the surface of the coil, and he shows two tests, one his own, presumably with 19.5 deg. temperature gradient from the insulation, and quotes the test of some one else who shows 20.60 deg. In addition to that temperature gradient, there is another one from the outside of the coil to the inside of the coil, that is especially so if the coil is made up with round wires, with cotton insulation and air space between. So that with this temperature gradient assumed at 5 deg. added to Williamson's 20 deg., we would have 25 deg. of temperature to start with. That leaves us in the case of 100 deg. ultimate temperature, 75 deg. measured by the conventional methods, and deducting 40 deg. leaves 35 deg. increase.

This whole question of whether we are going to rate machinery up or rate it down depends entirely on what the Institute adopts in connection with this clause, "Temperature to be determined by approved method as may be specified in A. I. E. E. Rules." If we adopt the conventional 10 deg. temperature gradient, we are making a compromise. If we adopt a method of tabulating and calculating it for different thicknesses of insulating wall and different conditions, we are getting down to a more conservative rating. Difference of opinion seems to be on this point, and I think it will be determined one way or the other, depending on how these temperatures are to be determined. If the Institute adopts consistent rules for determining that temperature gradient to the inner part of the coil, the recommendations will be more conservative, and they will lead to the adoption of lower temperatures measured by conventional methods. I think if these few points are kept in mind we can come to a complete understanding, and find that every one is in accord on this subject of temperature. I think the sentiment of everybody is in the direction of more conservatism.

Charles P. Steinmetz: I wish to say that in both papers, in referring to the 90 deg. ultimate temperature, it is understood to refer to the temperature measured by thermometer or resistance. That is the average outside temperature, and not the temperature of hot spots which may be 10 deg. higher. Both papers recognize as the limiting permissible temperature rise, under extreme, conditions, 50 deg. cent. Therefore, both are equally conservative or equally unconservative. However, I wish to draw your attention to one misunderstanding. There seems to be some idea that the new rule is less conservative in allowing 50 deg. temperature rise than was the former rule. As a matter of fact, it is more conservative, because the former A. I. E. E. specification allowed in electrical apparatus 50 deg. rise of temperature at rated loads, and in addition allowed 15 deg. more, or 65 deg. rise, at certain overloads for limited times. Now, in the new rules we do not permit the additional rise of 15 deg., because 65 deg. rise is not safe for all insulations and under all conditions. If the room temperature is 25 deg. cent., then you can have the additional 15 deg. rise, and still not exceed safe temperature limits even for class *A* insulation. If your room temperature is 40 deg. cent., you cannot permit 65 deg. rise, as determined by conventional methods, without exceeding safety limits for class *A* insulation. The only change which has been made from the old rules is that of leaving 50 deg. rise as the maximum standard rise. We no longer allow that additional 15 deg. Thus you see, we have really cut down the permissible excess rise above 50 deg., leaving, however, the 50 deg. as the standard which was recognized before.

I want to take issue with one statement, that 50 deg. was the ***Institute*** standard, but the ***universal*** standard was 40 deg. When we consider these lower temperatures, we always think of the big, the important apparatus, the special high class machinery. That is all right, if we say that 50 deg. is the maximum permissible guaranteed rise, that does not exclude lower values but rather makes it desirable for us in this class of machines where we want to have extra-safe apparatus, and where we have big machines on which we can afford to spend some money to get good service conditions. There is no change from 40 deg. to 50 deg. Fifty deg. was the maximum permissible temperature. Thirty-five to 40 deg. may be specified and has been specified very largely, and will be specified in the future to suit special conditions, but there is a large mass of apparatus in which 50 deg. has been and is industrially used, and even higher temperatures, which we have to avoid and discriminate against. But the large mass of apparatus being built to-day, and which has been built in the past, has a 50-deg. rise, and we never think of it, because such small apparatus goes out in wholesale quantities. It is often put out by the smaller manufacturers, and is good enough at 50-deg. rise, as good as is usually required. We have to recognize that this apparatus which is being manufactured wholesale is giving satisfactory service, and we cannot prescribe the standard for the best class of apparatus only, but we must have a standard to satisfy all conditions. It is not always realized that the A. I. E. E. specification, while it gives the maximum permissible temperature, does not apply exclusively to the *best* construction, the *very best* class. You can get better than the rules provide, by specifying *lower* temperatures, but you should not go *higher* than 50 deg. rise. If you stipulate specially low temperature you must be prepared to pay a higher price, and sometimes, as in large, valuable machinery, it will be good policy to pay the higher price, but for the vast majority of small machines, the 50 deg. rise basis represents approved practise.

C. J. Fechheimer: It is always well, when considering alterations, to profit by the experience of others. The single rating, 50 deg. standard has been the practise in Europe for a number of years and European electrical machinery has been used in Mexico and South America. In Europe the single rating method has possibly worked out fairly well, because there the operators are generally men of intelligence and education. Many station operators in Europe are University graduates. In Mexico, on the other hand, where the operators are less intelligent, the number of burn-outs with European machines has been more frequent than with American machines built on the 40-deg. rise basis for normal load and with the usual 25 per cent overload. The 50 deg. single rating method puts the burden of proof for successful operation upon the operator.

The intelligence and education of the average operator in the United States is certainly less than that of the European, although of a higher grade than that of the Mexican. Furthermore, the indications are that the class of help in American power stations, mills, factories, etc.—especially the latter two, where generators and motors are used, will not improve. Therefore, if the single rating method is introduced, similar conditions will obtain in regard to burn-outs in this country to those which have existed in Mexico where European 50-deg. machines have been used. Even in the Commonwealth Edison Co., where a very intelligent staff is employed, machines have burnt out, as pointed out by Mr. Schuchardt. What then may we expect with the higher rated 50-deg. machines, especially if overloaded as the average station operator is liable to do?

I wish to call attention to a statement of Mr. Reist to the effect that with motors underloaded, as they are at present, the average efficiency is reduced. I do not agree with this; in nearly all standard induction motors and commutating-pole direct-current machines, the point of maximum efficiency occurs at a load lower than normal with our present system of rating. Furthermore, the majority of purchasers endeavor to buy machines of such ratings as will enable them to just carry the load required. Hence, were the ratings of motors increased by the adoption of the proposed single rating, the customer would secure motors of materially lower, rather than higher, efficiency at the operating load. Although Mr. Reist's statement applies to the majority of alternating-current generators, the gain in efficiency by increasing the ratings of the alternators would usually result in an increase rather than in a decrease in the steam or water, etc., needed to operate the prime movers. Hence, from this point of view, the customer would lose rather than gain, if the ratings of all machines are increased.

Philip Torchio: The Committee recommends one rating for all apparatus. I state again that I have no objection to Mr. Merrill's rating of 90 deg. at hot spots for steam turbine generators and perhaps for waterwheel generators, but I think it would be a mistake to use the same standard for the class of apparatus referred to this morning, which is subject to overloads. For this latter class of apparatus I suggest 75 deg. maximum temperature. This would leave 15 deg. for possible overloading, though the amount of overload may not be specified. *I therefore recommend* 90 *deg. at hot spots for machines which are not subject to overload, and* 75 *deg. for machines subject to overload.*

B. G. Lamme: I think some confusion has come from the fact that the larger internal temperature gradients with high-voltage machines have been overlooked. Mr. Torchio wants a temperature of 75 deg. on his high-voltage machines. What he really wants is 75 deg. measurable temperature. What he is really after, although he possibly does not look on it that way, is to keep within the 100 deg. ultimate temperature, for the machines he has in mind have probably 20 deg. to 25 deg. internal drop, so that his 75 deg. measured temperature means probably 100 deg. at some point inside the machine. The sub-committee paper did not bring this point out clearly. We referred to 10 deg. to 15 deg. for low-voltage machines, but Mr. Torchio is using, to a great extent, high-voltage machines.

That also explains Mr. Stott's point. He referred to machines which ran at about 40 deg. by thermometer measurement, with air temperature not over 35 deg. cent., which would make 75 deg. measured temperature, and he thought 100 deg. ultimate was unsafe. I know the type of machine to which he refers, and I think that there is fully 25 deg. internal gradient from the hottest spot to the point where he could make his temperature measurement, so that I believe his machines were at least 100 deg. cent., and possibly hotter at times.

Mr. Schuchardt suggests 80 deg. as the limit, but in his machines, if I am not mistaken, exploring coils outside the insula-

So you can see that the step in advance which we propose to take involves recognition of the hottest spot and limits the temperature of the hottest spot to not more than 100 deg. cent. But we cannot put that in any contract between manufacturer and customer because there are no means yet to determine the hottest spot, and so all specifications must still be based on the means now available for measuring temperature. This requires us to allow a lower conventional maximum temperature, say 90 deg. for Class A insulation. That is unavoidable. In short, we can *discuss* theoretical matters and the ideal conditions, but we *cannot meet* the ideal conditions in practise. That is the difficulty.

Now, what should be the limiting temperature for the hot spot? 100 deg. cent. has been advocated. Personally I believe that the hottest spot is safe at a higher temperature. Probably 100 to 105 deg. is quite safe, but we have no means yet of detecting the higher temperature, the hottest spot. And furthermore the whole question is complicated by the time factor. A temperature many degrees higher will occasion less deterioration *if only occurring during a few summer days*, than will a decidedly lower temperature *if continuously present*. We may say that if you take machines which are run hot, but do not burn out, which have been running for many years without trouble from heating, then the highest temperature which exists anywhere in one of those machines is the permissible hot spot temperature. But what is that highest temperature? Originally we imagined that that was what the thermometer and resistance methods of measurement showed. We have found, by getting more and more experience, that it is higher, and still climbing up, but I do not know whether we have, or have not, located the hot spots, and the fact is that the hottest spot is not a definite temperature which we can be sure of, but is the hottest spot in those machines which have been running for many years without burning out. I believe we are taking a step in advance to recognize this point, but we are not ready, and nobody is ready, to state what is the possible permissible maximum temperature for ordinary insulation which may be reached temporarily.

B. A. Behrend: If the hottest spot cannot be found, it is useless to say that its total temperature may be 100 deg. cent. If I cannot find it, I cannot guarantee its temperature.

C. P. Steinmetz: You may find it sometime—

B. A. Behrend: If I can find it sometime, then I like to know it and know its existence. I prefer to know that I am going to get a machine which will have parts hotter than those guaranteed.

C. P. Steinmetz: I mean, that you will know it some time in the future.

B. A. Behrend: It is surely better to know beforehand than to find out afterward. Since we designers of electrical machinery appreciate that there exist hot spots, unless we admit their existence, we lead the people to think that our guaranty defines the hottest part of the machine. Let us admit the existence of concealed hot spots and guarantee a total temperature of 90 deg., and say that this refers to the hottest part of the machine. It will mean that ninety per cent of all the machines turned out by the manufacturer will have a readily measurable total temperature of 80 deg. The purchaser will get the advantage of this, and it seems to me he is entitled to this advantage. Standardization must be for the benefit of both the manufacturers and the user, and therefore let the manufacturer be a little more generous in his dealings with the customer.

There is one point which I cannot pass over, viz., the adjustment of the efficiency to the power factor for a given rating. In neither steam engines, gas engines, induction motors, nor alternating-current generators can we say that the maximum efficiency, the best regulation, or the highest power factor, are at the point indicated by its rating. The maximum rating of machinery places the burden on the user and his consulting engineer, and it seems to me that twenty years of education should have given the customer, if not the consulting engineer, an opportunity to learn how to make allowance for the conditions he has to meet. If a 50-h.p. motor is a 50-h.p. motor maximum rating, and if I am a little doubtful as to whether my plant requires 50 h.p. or 55 h.p., I shall take a larger motor. If I wish to use ordinary cable, or a piece of shafting, or a bearing, and consult a manufacturer's catalogue, I must apply the same reasoning. There can be therefore no objection to a single rating on this score. Why the electrical engineer alone should be less able to use his judgment than the mechanical or the civil engineer it is hard to see. When we design a bridge we must know the elastic limit of the materials used in it. The really important issue lies in the application of the designer's judgment to the safety factor to be employed.

I wish to say a word regarding the use of the emblem of the Institute on the name-plates. It creates the impression that the stamp of approval of the Institute has been placed on the manufacturer's apparatus, which we know is not so. It is not compatible with the dignity of the Institute. The single rating is a sound movement, and the only point to be decided is how to use it, or whether to deceive ourselves into believing that we have cooler machines than we actually have. I do not approve of any species of make-believe. If I am to be robbed I want to know it; and if I am to be deceived, I want to know it also. If I have to use machines which will show a temperature rise of 115 deg. total rather than 90 deg., I want to know it. It is perfectly feasible to embody it in the same scheme now contemplated by the sub-committee.

R. F. Schuchardt: One more reference to finding the hot spots, with regard to having the customer find them, as suggested. It may be interesting to state more of the details of the experience mentioned in my previous discussion. At the time of the breakdown of this particular unit to which I referred, the temperatures were being taken according to the specifications in the contract, and the temperature limits of the contract had not been reached at the time of the breakdown. The customer then set about to find the hot spot, and with the permission of the manufacturer, put these exploring coils at the place where the designing engineers of the manufacturer said would likely be found the hottest spots. Then we made tests to find what is the safe maximum temperature at which to operate that insulation, and we found this to be 80 deg.

Charles P. Steinmetz: If the designer or the manufacturer, or anybody else, only knew where the hottest spots are, and how to find them and measure them, the specification of the maximum temperature at the hottest spots would be the most satisfactory to the manufacturer, operating engineer, consulting engineer and everybody else. Unfortunately, that is not the case. We may *believe* that a certain region will be hotter, and even that it is the hottest place, but we do not *know* that with certainty. We know that the place which Mr. Schuchardt referred to just now was the hottest place which could be reached. Quite possibly somewhere else, at a place not reached by the exploring coil, there may be hotter spots, and that is the difficulty.

I sympathize with Mr. Behrend that he does not want to have something sold to him which is not as described, but if he will kindly follow the suggestion which we put forward at the beginning of the convention, not to make destructive criticism but constructive criticism, and tell us how to go to work and locate the hot spots and how to measure them, he will be a great factor for good in the advancement of the electrical industry. But as long as we do not know how to find the hot spots with any certainty, or how to measure their temperature, we have to do the next best thing, and measure the temperature in ways practically available. I believe it is a step in advance to recognize the existence of the hot spots, to recognize that the measured temperature is not actually the maximum temperature, but that there is somewhere a higher temperature, and furthermore, to recognize that the highest temperatures exceed the measured temperature by various amounts, (depending on the condition of the machine, on the design, on the insulation), which exceed the measured temperature by 5 deg. or 10 deg. in some machines, like direct-current machines, or exceed the measured temperature by 20 deg. (or possibly even 30 deg.) in other machines, like those with high temperature, high voltage, heavily insulated, heavy armature coils. But we have reached something in recognizing that we want the assistance of all engineers to help us find where the highest temperatures are located and how to measure the highest temperatures. We will be glad to standardize the specification of the highest temperature and the methods to find it, if you can give us some feasible method of doing so.

W. L. Merrill: There is quite a lot of discussion on this ultimate and high temperature, and it must be remembered that it is only due to those cases in which the room temperatures are 40 deg., which is considered as the high limit of room temperature recognized by the Institute. Anything below that would be a factor of safety, which you would have in addition to these maximum temperatures. In taking up this paper, it was my understanding that the 90 deg. was to be the maximum ultimate temperature, as determined by methods approved by the Institute, either at present or in the future, whatever those rules should be. I think, perhaps, that would reconcile some of this discussion, and I would like to put in a hypothetical question—if there is a method determined, or if our present method, plus a correction which has been mentioned in the papers, were put into practise, what is the consensus of opinion of the various gentlemen who have just discussed these papers, or are interested in the subject, if the limit were put at 90 deg., if that would meet their approval, or rather, should it be 100 deg? In other words, is it 100 deg. that must be the maximum for fibrous insulation or 90 deg.?

There is one point I want to touch on—the question of bearings. The sub-committee thought it would be attacked a great deal more on the matter of bearings, perhaps, than some other things contained in its report. The question of limiting the temperature in the bearings of electrical machinery, to my mind, is not very good practise. Is there any gentleman here who has purchased a steam engine and questioned the veracity of the manufacturer as to what the heating was to be in the steam engine bearing? Is not the same true with waterwheels? Is not the same true with line shafting, gas engines, or any other piece of machinery that is purchased, that the question of bearing temperatures is not raised? I have a particular case in mind, which I think shows the fallacy of limiting the design of electrical machinery, we will say handicapping the design, by limiting the temperature of the bearings. In the case referred to, vertical waterwheel-driven units were used. At the time the engineering was done the waterwheel manufacturer was to supply the thrust bearings. They were put below the generators, and there was no reason to suppose the bearings were not all right to carry the load and give a good account of themselves. By reason of later development, the man who was to install these units decided to have the bearings on top of the generators, so they now became part of the electrical equipment, and they had to be designed to meet the temperature rise of the Institute. It seems to me electrical machinery should be put in the class of other machinery when it comes to the question of bearings.

One more point in connection with the bearings—we will assume that we have a machine in which the engineer has decided on a 5-in. shaft with a 3 by 1 bearing, making a 15-in. bearing housing. The rubbing surface of that bearing is long.

and it might heat beyond the temperature limit set down by the A.I.E.E. at the present time. That temperature can be materially lowered by shortening up the bearing housing. We could cut it down to a 5 by 10 bearing, and it is possible to still lower the temperature of the bearing, and the bearing is not as good for working as at that higher temperature limit. Another point was the rating of the temperature limits of the bearings at 50 deg. That is a physical impossibility, although we are supposed to do it today. Take the case of a small totally enclosed motor, a mill motor or railway motor, where the temperature is practically uniform throughout the whole machine, the bearing must necessarily be approximately the temperature of the rest of the machine. On the rest of the machine we are allowed 75 deg. or 90 deg. rise, for non-fibrous insulation, and necessarily the bearing must go up to that, there is no help for it. I suggest, Mr. Chairman, that you get an expression of opinion from Mr. Torchio, and various other gentlemen who have discussed this paper, if their understanding is tha tthe guarantee of the maximum hot spot should be 90 deg. instead of 100 deg., whether that would meet their objection.

Philip Torchio: In summing up the discussion, Mr. Chairman, I do not think you have taken cognizance of the point of view I have been trying to present. There is a serious objection to using the same limiting temperature of 85 or 90 deg. (which is required and allowable for turbo-generators) for that class of apparatus which is subject to overloads beyond the control of the operator or user. I want to emphasize the point that you cannot unify two distinct sets of conditions. You cannot have the same rating, the same maximum temperature, for the turbo-generator and for the synchronous converter or the motor.

Charles P. Steinmetz: I wish to say that when considering the question whether 90 deg. or 100 deg. should be the temperature of the hot spots, we should give consideration to the present existing apparatus, to all those many small motors which are turned out by the thousands by many manufacturers, in most of which apparatus, with a room temperature of 40 deg. cent., the hottest spot is above 90 deg. cent. All of this apparatus would have to be redesigned, which would be a very serious matter, not to the big manufacturers, who could easily afford to do it, but to the small manufacturers, who would have to redesign their motors and other machines to make them larger.

I would like to know whether the smaller manufacturers would be willing to rate down their apparatus, or to make them larger, and bring the limits of temperature to 90 deg., or less, maximum temperature, where it is now higher. It may not be higher by the conventional method of measuring, but undoubtedly most are higher, today, at 90 deg. maximum temperature, on the basis of 40 deg. room temperature.

W. H. Powell: The motor-generator set referred to by Mr. Torchio would be classified as A 3—Continued Pulsating

Service, or under certain conditions as class B 3—Short Time Pulsating Service. Other limitations prevail besides the ultimate temperature. The recommendation of the committee, viz., that apparatus be rated on the basis of ultimate temperature, and that no overloads be specified except momentary overloads, applies only to apparatus falling within class A 1—Constant Service, where load is continuously applied.

Charles F. Scott: The fluctuating viewpoints in this discussion raise the question, what do our Standardization Rules stand for? What is their purpose? What is standardization? Some things cannot be standardized in simple terms. For example, a few years ago the Standards Committee took up the rating of railway motors, which were given a " one-hour rating " in our old rules. A sub-committee undertook a revision; meetings were held at which engineers from manufacturing and operating companies and consulting engineers to the number of some fifteen or twenty were present, some of them among the most prominent men in the Institute. The matter was discussed first one way and then another, and we kept getting further and further from a conclusion. We adjourned for a week. Then Mr. Armstrong came with diagrams and curves to show that the one hour rating was inadequate and that a certain method was better. Mr. Storer came with his data to show that something else was best. Everybody seemed to be conscience-stricken because we could not come to a conclusion, and tell how to rate railway motors in a simple sentence or paragraph, and the chairman suggested that we had better give it up, as agreement seemed impossible. A member suggested—" Maybe it is impossible, maybe the fact that the performance of a railway motor, which has to do so many kinds of service, cannot be expressed in terms which are simple, is the lesson we have learned in our discussion here." Each of those meetings started at four in the afternoon and ran to seven or eight in the evening, and the outcome was not to express the rating of a railway motor in two or three lines but to set forth methods of selecting a motor for given service in a couple of pages in an appendix to our present rules.

What are we now attempting to do with respect to the rating of motors? Are we not attempting to express in a few paragraphs the characteristics of all motors? The types of motors and the conditions of service are so diverse that it is impossible to make simple classifications which will be adequate and complete. Stationary motors must meet a range of service conditions more extensive and more erratic than railway motors. The latter are fairly definite in type and in the nature of the service to be performed, and yet they do not admit of any simple method of rating; hence any elementary or simple classification of stationary motors must be rather general in its nature. In the proposed classification, there does not seem to be any discrimination between series motors and shunt motors, and the divisions of service conditions into several classes is only a first approximation, as can readily be seen if one selects several specific cases and endeavors to adapt them accurately to the proposed classification. If classifications were to include accurately all conditions, there would have to be a hundred or more divisions, instead of half a dozen.

What then do the Standardization Rules attempt to do? I have been a member of the committee for several years, and, speaking generally, our object has been to express what is good practise in definitions of terms and in methods of measurement. When, however, the Standardization Rules are used by the operating or designing or testing engineer, they are often regarded as something which should be absolute and complete, and the rules are criticised if they do not seem to meet definitely each particular case which may arise. The past policy seems to me to be indicated by this sentence in connection with transformer insulation tests: " The voltages and other conditions of test which are recommended have been determined as *reasonable and proper for the great majority of cases* and are proposed for general adoption, except when specific reasons make a modification desirable." In other words we are not making a set of specific rules, but we are giving sanction to practises which are good. It is expected that intelligent common sense will be used in applying the Standardization Rules. We see, for example, that it is good practise not to have the temperature of a motor rise above a certain limit, but Mr. Merrill has just pointed out the absurdity in carrying this rule too far and making a bearing of one temperature if it is considered to be a part of a dynamo and another temperature if it happens to be considered as a part of the waterwheel.

We are discussing rating; what is rating? It is the assigning of certain values to express the capability of a machine. These values depend upon the quality of its materials, upon its regulation, insulation, temperature, and other factors. All these are indefinite; they may be expressed in curves. There are no definite absolute limits like the length of a yard-stick or the weight of a cubic inch of copper. The selection of the limits which fix the rating is a matter of judgment. Take, for example, a completed motor which has lost its name-plate, and attempt to fix its rating and to determine the volts and amperes and speed and horse power that should be put upon the name-plate. Tests may be made, but judgment must be applied in selecting definite limits.

When a motor is sold, its name-plate joins together two things —its past history in the factory, which determines its electrical capability, and the service it can render, which is its mechanical performance. The motor drives a shaft and the shaft drives the load. Torque and speed are the two things which the motor produces, and torque and speed are the things which the load requires. The requirements of the load are subject to definite variations in the relations of torque and speed and time. Our problem is to specify the capability of the motor in torque and speed and time in such a way that its adaptation to power service requirements can be determined. If we were to make classifications which covered all service requirements in point of variable load and time conditions, we would have a hundred classes, instead of the half-dozen which have been proposed. Obviously, therefore, the motor classifications proposed are not complete and exhaustive, but they simply indicate what a motor can do under a few typical conditions. This must be supplemented by an intelligent comparison between the actual service requirements and the specified classifications.

What the motor specifications are accomplishing is to define, more definitely than has been done in the past, what a motor will do under several sets of conditions. The commercial engineer must then, with this larger knowledge of motors, make his selection of the proper motor for his specific case. We are assisting the seller and the buyer, not by covering definitely the various conditions of service, but by defining more completely the capability of the motor. We define what the motor can do under specified conditions, but we cannot define what it will be required to do in operating a lathe or a hoist or a pump.

The proposed specifications are general; they do not even distinguish between series and shunt motors. We simply lay a good general basis, and our rules cannot be extended much further, unless different types of motors are treated as we have already treated the railway motor, by giving to each type an extended dissertation as to how the selection is to be made for each type of service.

Comfort A. Adams: There are two functions of rating; first, to enable the customer to compare the prices of different manufacturers; and second, to enable the customer, or his agent, the engineer, properly to choose a machine for a given duty or service. It is obviously impossible for any set of rating rules to cover all kinds of service. Those who have had to do with the choice of motors for a special purpose realize that there are hardly two cases which would be covered exactly by any simple system of rating. The important thing, then, is that we come to some agreement. It is not so important that the chosen temperature rise be absolutely safe at all times, as it is that we understand what that temperature rise means. Just in so far as we know by experience or by computation, or both, what the difference is between the temperature of the hot spots and that measured by any particular method that we may agree upon, can we make intelligent use of the corresponding system of rating.

I do not agree that we must adapt our method of rating to the unintelligent unadvised customer who buys a machine on the basis of its h.p. rating, while guessing at the duty. That class of customer is a rapidly diminishing one, and should not be made the excuse for saying one thing and meaning another.

Charles F. Scott: What is the relation of the Institute to buyer and seller? We are a professional body and not a commercial body. It is our function to express what is good professionally. This was summed up in excellent form on the night of the original discussion which led to the formation of the Standards Committee, fifteen years and one month ago tonight, when Dr. Steinmetz said, that standardization came under two heads: first, a definition of terms, and second, a definition of methods of tests. We will do well to adhere to this scheme, doing professional work which directly concerns us and let the application to commercial work go to others.

Alexander M. Gray: In my previous remarks I attacked the clause on bearings, and I am still unconvinced on that point. Shaft bearings are in a different class from motor bearings; they squeak long before they break down, whereas a motor bearing gives no warning; and I still contend that the temperature of the oil in the bearings should be limited to 70 deg. cent. If the bearings are to be hotter, let us know about it, and put it in the specification.

I consider that Mr. Behrend is right in the attitude he takes about the use of the Institute initials on name-plates, but we should have something on the name-plate to show that the machines were given a single rating and not one with an overload capacity.

B. G. Lamme: I have been listening to many statements which are apparently in disagreement, and I will therefore try to do some averaging. It seems to me that many of the apparent discrepancies which have come up, are due to looking at the problem from a wrong basis. We all think of the temperatures of machines based on our everyday experience, but our everyday experience is really at 20 deg. or 25 deg. air temperature. On that basis we think of a machine which reaches 75 deg. cent. by measurement as a very safe machine. If, *at the same air temperature*, that machine is loaded until it reaches 90 deg. cent., we consider it is beyond the safety limits. That is correct, because, on the basis of 20 deg. or 25 deg. cooling air temperature, the machine showing 90 deg. temperature by measurement is an unsafe one in service, for such machine will probably be at least 100 deg. cent. at the hottest part, and therefore any increase in the cooling air temperature puts the machine above the danger point. However, if the 90 deg. measured temperature is always tied up with the 40 deg. cooling air temperature, then the case is quite different, for at ordinary air temperatures, the machine then has only 80 deg. to 85 deg. ultimate temperature. In other words, when we think of 100 deg. as the ultimate limit with this proposed method of rating, we must always think of the 40 deg. air temperature in connection with it.

I also wish to emphasize one point, namely, that if the ultimate temperature limit is set at 90 deg. cent., instead of 100 deg. cent., that is, if 90 deg. is to be the hottest spot inside the machine, then on the basis of cooling air at 40 deg. cent., a vast majority of the apparatus now built in this country will not come inside the new rules, and this applies in particular to many lines of apparatus which are now thoroughly successful and have given satisfactory service in every way; that is, if we adopt a 90 deg. standard as proposed, then we cannot live up to it rigidly without derating a great deal of thoroughly satisfactory apparatus. This appears to me as one of the strongest arguments against the 90 deg. ultimate limit. If a rule or limit is set so that it will condemn thoroughly satisfactory apparatus, then the limit must be wrong.

C. L. de Muralt: Prof. Scott pointed out a moment ago that we are essentially a professional body. That is true, but we are a unique professional body. Many of our members are representatives of the manufacturers. Some may consider this as a difficulty. I do not look at it that way. I think that we can be greatly benefited by the presence of the manufacturers and by the work which they are doing to help us establish these standardization rules. Imagine a strictly professional body establishing standardization rules. It would be much more difficult.

We have really had three different views presented to us at this convention. We have heard the manufacturer say along what standard lines he is prepared to build his machinery and guarantee it. We have heard the operator state what he thought the manufacturer should do to help him buy machinery for special conditions. And we have heard the consulting engineer present his particular troubles in bringing the two together.

As a matter of fact, listening in the background, it seemed to me that all were pretty thoroughly agreed and I think we pretty nearly accept what Dr. Steinmetz and Mr. Lamme suggest to us. Two things must be considered in order to satisfy ourselves on machine rating. One is the maximum overload capacity. The other is how much of a load, continuous or intermittent, will the insulation stand? The maximum overload capacity, as I understand it, has not been touched upon at all in this report, nor has it been much mentioned today. In most cases it is a well defined point. We may, therefore, as well limit ourselves to the question of protecting the insulation. That means determining the temperature beyond which the insulation will be damaged, and that is what I understand the sub-committee reported on. Most of the men who talked on this subject agreed that we want that temperature laid down definitely. Whether it be 90 deg. or 80 deg., or 100 deg., is possibly subject to further discussion, but I think most of us are satisfied that 90 deg. would be all right and we want the hottest point of any machine to be not in excess of 90 deg. if it is in touch with the insulation.

Then the only question remaining is the one brought up by the second paper, namely, how shall we make our ratings so that the above point is actually taken care of. Many of us have come to the conclusion that it is not well to have different ratings. It is safer to have the manufacturer rate his machines for running continuously, that is, for the worst possible operating condition from the point of reaching maximum temperature. Then, if a certain machine is to be run under different conditions, not continuously, then it is up to the man who buys that machine, or to his adviser, to find out how his particular run differs from the continuous run. This may reasonably be made the subject for another report by the Standards Committee, or possibly it may necessitate research investigation by an independent man presenting a paper, showing how certain specific runs or typical runs do make the temperature vary. Thus far it is not at all definitely laid down by anybody, and simply to anticipate a certain number of typical runs, that may or may not be met in actual practise, seems to me, and has apparently seemed to most of those who spoke on the matter, beside the point.

I maintain therefore that the outcome of this discussion is that the Standardization Rules should be on the basis of no point of any machines reaching a higher temperature than 90 deg. after a continuous run of sufficient duration to bring about maximum temperature.

H. M. Hobart: There have been intimations that the manufacturer had some object in this matter other than providing for the best interests of the industry. These intimations were not put forth strongly, but they have been repeated in several quarters. I have played different parts in the electrical industry, and I am now associated with a certain manufacturer, and I am satisfied there is absolutely no doubt about it, that the manufacturer has no greater concern than to get at the best results for the electrical industry. There is nothing altruistic in this standpoint; it resolves itself into a matter of enlightened concern for the interests of the shareholders. The manufacturer recognizes that it is a good investment to spend vast sums of money making investigations, and he thus secures special information which he gives freely to anybody who will take the trouble to read it—as I say he spends vast sums to get at these facts, and it has led his engineers to have certain views that such and such things are best. They have arrived at these views as the result of elaborate tests. If it can be shown that they are wrong, the manufacturer is willing to at once change his plans. What the manufacturer wants is to promote the very best interests of the electrical industry as a whole, and he particularly wants definiteness in the matter of standards, some definite set of standards. That would be arrived at by this single rating system. If anyone has a necessity for using lower or higher temperatures, it is simply a matter of slide rule transference, for him to decide which size of machine he requires. The matter is far simpler than one would gather from the long, though very interesting, discussion which we have had about it.

B. A. Behrend: I feel constrained to say a word in regard to the remarks of Mr. de Muralt and Mr. Hobart. The relation of the manufacturer to this Institute is a question I do not intend to discuss. I do, however, wish to point out one thing, viz., that the manufacturer is responsible for the old code and for the last edition of our Standardization Rules, and that the application of these rules to actual conditions favors the manufacturer. For instance, the application of the rules to the determination of the regulation at 100 per cent power factor would give 4 per cent regulation, while in reality the regulation of the generator may be 8 per cent. I shall be satisfied with this single reference—which I made eleven years ago before this Institute while advocating the same system of determining regulation which you have now come to recommend. I do not charge, as Mr. Hobart's remarks would imply, and I do not wish to be understood as saying that the manufacturer has done this with an evil intent, as in the end he is responsible for results, and if he sends out poor machinery he must, and does, make it good.

Comfort A. Adams: It is absolutely impossible to devise a system of rating which will take account of all kinds of overloads. The safe limit of measurable temperature differs for different overloads, since the difference of temperature between the hot spot and the point at which the measurement is made is greater, in a given machine and with a given hot spot temperature, during the transient period of a short heavy overload than under steady conditions, owing to the heat capacity of the insulation. This is appreciable only in machines of comparatively high voltage and thick insulation.

A. E. Kennelly: It seems to be the consensus of opinion that a single rating for electrical machines is desirable, based on the maximum measured temperature attained. Differences of opinion enter as to just what that maximum measured temperature should be. It is generally admitted, however, that the maximum internal temperature of class A insulation should be 100 deg. cent. That internal wall temperature is ordinarily inaccessible, and we must at present be content with measurements of the maximum temperature of the outside wall. The committee recommends 90 deg. cent., thus allowing 10 deg. cent. for drop of temperature in the wall. But whatever maximum measured temperature of the outside insulating wall is adopted between the limits, say of 80 deg. and 95 deg. cent., some allowance will have to be made by electrical engineers in ordering large machines, for the special conditions under which those machines are to operate. A considerable number of machines may be ordered for continuous service at their nominal continuous rating under the new rule; but many machines will call for the exercise of reasonable judgment. If, for instance, a generator is to be ordered for a mill, in a cold climate, with the expectation that it shall have to deliver 1100 kw. and no more, for 10 hours a day, then a machine of perhaps 1000 kw. continuous rating might be sufficient; whereas if the generator were to be used in the tropics, with the expectation of delivering 1100 kw. ordinarily, but with occasional demands for 1500 kw., then a 1500-kw. machine ı have to be ordered. Since, therefore, engineering judgmc the selection of a machine cannot be avoided on any ba: continuous rating, the exact value of the maximum measu temperature is of secondary importance. The matter of pri: importance is some one clearly defined maximum measured perature to suit the average requirement, and then data which the maximum internal temperature of the insulation cɛ predicted for large or special machines, from a given assi schedule of load through the 24 hours.

A. M. Rossman: May I offer a suggestion which, I bel is in conformity with the recommendations of the sub-comm on ratings, yet would meet the objections raised by severɛ the operating engineers? The suggestion is, that the re(mendations of the sub-committee be adopted but that at same time, a system of factors be established which would g the purchaser in the selection of the proper size of machine the class of service it is to perform.

For instance, in buying a new railway synchronous conve he would buy a machine 1.5 times the rating of his present chines, provided his present machines were purchased on the b of 50 per cent overload for two hours. In buying distribu transformers he would buy for the same duty transformers] times the rating of his present transformers. We are alre: used to turbo-generators rated for maximum continuous d and these would therefore have a factor of 1.

I thoroughly believe in the system of single rating because the manufacturer builds a machine to meet one definite tempe ture condition, (2) the consulting engineer bases his acceptaı of the machine on a single temperature test, and (3) the purcha has a common basis of comparison between machines of diff ent manufacture.

C. E. Allen (by letter): The paper by Dr. Steinmetz and M Lamme is of greater interest at this time than most of us reali but it is the writer's opinion that there are a number of interesti phases of the subject which they have not touched upon. Th state that the durability of insulation must be considered frc two standpoints, *i.e.*—mechanical and electrical. I belie they should have stated three—the third being the "meth of preparing and applying."

As their paper is written more with a view to guiding the futu than criticising the past, it seems just that they should ha considered the improved insulation and methods of applicatic in reaching a conclusion for the basis of a recommendatio:

In order to understand more thoroughly the present and futuı methods, it is necessary to dwell somewhat on the past. Unt very recently the methods of applying insulations in most (the classes of apparatus have necessitated the insulation bein made in more or less of a flexible form, which has resulted in larger amount of insulation being used than was actually neces sary or desirable. By necessary I mean that the uniformity of the insulation could not be depended upon and that, in order to be sure of a predetermined value, a greater amount was used, with the result that an occasional piece of apparatus out of the different classes could be selected which would show a much higher test voltage than it was actually designed for; yet as a complete line the average would probably not exceed the designed value. This increased amount of insulation also had its influence on the heating of the apparatus and resulted in a less uniform temperature and a greater maximum temperature of some one part of the apparatus. It was possible for this excess temperature to vary considerably in different pieces of apparatus of the same design and it was very possible for this temperature to be excessive and result in the short life of the insulation and resultant failure of the apparatus.

This past practise also permitted a defect to be taken advantage of, in that where occasional pieces of apparatus would stand a higher test voltage than designed for, many times the electrical fraternity were led to believe that the insulation value of a certain line of apparatus, based on a test of one individual piece, was much greater than the actual average of the line, while the question of the excessive temperature as a result of this, which would have a decided influence in determining the life of the apparatus, was not fully taken into consideration.

The more modern methods of preparing and applying insulation, particularly where mica is used, have resulted in the insulation being formed into a proper shape and assembled with the apparatus in such a way as to eliminate any distortion of the same, which would otherwise be likely to result in a marked decrease in its insulating value. With these new methods, which also involve certain new elements in holding the mica together, it will not be necessary or desirable to use the excessive amount of material that has been used in the past.

While it may not be probable that one piece of apparatus out of a line can be selected that will stand as high a voltage as in the past, yet the average of the line can be depended upon, with a corresponding reduction in the maximum temperature. This will not necessarily mean, however, a lower average temperature, but a more uniform temperature, resulting in apparatus that will have a longer life.

The new form of insulation and method of application is also more durable from a mechanical standpoint and is not as readily injured by the contraction and expansion of the copper and iron in the apparatus where it is used.

While there is no question of a doubt that the later methods are a decided improvement and an advance over the older methods, yet it is going to be possible from comparative test, as formerly pointed out, to deceive a prospective purchaser and demonstrate to him, on the basis of one piece of apparatus, that the standard is not as high as it has been in the past. It is, therefore, very desirable that that part of the electrical fra nity representing the central stations, as well as that part re senting the manufacturers, cooperate with the Institute C mittee and follow out its adopted standards.

When considering that this organization represents the f most talent in the world, its recommendations should be ta verbatim and every central station and manufacturer shc insist upon meeting only the A. I. E. E. standard, and permit a higher value in one characteristic at the sacrifice another. If this is followed out the result is going to be m satisfactory to the electrical fraternity as a whole.

E. A. Wagner (by letter): If we were able to build up e trical apparatus with homogeneous insulating material throu out, there would be no difficulty about classifying differ electrical apparatus as outlined in the paper on "Tempera and Electrical Insulation." It seems to me that it would be cidedly difficult for anyone to determine whether some clas of apparatus would be class A, B or C. This is particularly t of the proposed class A and class B apparatus. Take the c of generators in which the slot insulation is made up of m or asbestos, or equivalent refractory materials. In these sl there are windings made up of the cotton covering which co under the class A. An injury to the insulation between tu would put such a piece of apparatus out of business, yet wo not necessarily communicate a ground to the core. The sa thing holds true of certain makes of transformers. It might argued that if there is any cotton present at all, then the appa tus belongs to class A. If this is the case, then it would se that the classification should be limited to two, one class whi can burn out, another class which cannot, the latter class rep senting rheostats, heating elements, etc.

In the case of stationary transformers, the paper on rati loses sight of one important class of transformers whi I think should be considered in the Standardization Rul I refer to auto-transformers, sometimes called compensato It has been the practise in some cases to rate these devices l the amount of the work transformed, considering this as the ou put. However, in any device we must consider the output kilovolt-amperes as the product of volts and amperes d livered at the terminals, without regard to the work transformation taking place inside of the case. It will, ther fore, be seen that we have two methods of rating su a device, and the practise has recently been adopted of ratir these devices both in kilovolt-amperes transformed and kilovol amperes output. I believe this method of rating provides for th classification of the apparatus, both from the standpoint (apparent work done and actual work done. One rating withou the other can very readily be very misleading, and I think th Standards Committee should include a classification of thi kind in the revision of the rules.

G. I. Stadeker (by letter): Although it is evident that a new system of ratings is highly desirable at the present time, it is advisable to make these changes slowly instead of confusing the vast number of buyers of power apparatus, who have but a slight technical training, by making several radical changes all at the same time.

The first step would be to change the method of rating as suggested in the paper under discussion by establishing the output on an ultimate temperature, instead of on a temperature rise, basis. But the suggested change in rating of motors, from the horse power to the kilowatt basis, would create considerable confusion. For instance, assume a motor-generator set, rated at 10 kw. This would ordinarily mean that the generator is 10 kw. There could be no misunderstanding in the present method of rating. But under the proposed system, a motor-generator set could be rated at 10 kw. when the motor driving the set was rated at 10 kw., with the result that the generator itself could not have a capacity exceeding 8½ kw. or 9 kw. This is one possible source of confusion.

Another source of misunderstanding, if motors are rated in kilowatts instead of horse power, is that the general public would probably assume that a one-kw. direct-current motor could be used as a generator to develop one kw. This would not be true. For instance, the ampere capacity of a 10-kw., 230-volt generator is 43.5 amperes. If this machine operated as a motor on a standard 230-volt circuit it would have a capacity of only 43.5 amperes at 230 volts, whereas a 10-kw. 230-volt motor should have a capacity of 50 amperes (assuming an efficiency of 87 per cent). The 10-kw. generator could, therefore, be only rated at 8.7 kw. as a motor. Although the reasons for this are perfectly clear to the engineer, it would be confusing to the non-technical public, inasmuch as both generator and motor are given the same rating. This condition represents an apparent inconsistency in the new method.

Until the new system of temperature ratings has thoroughly adjusted itself, we should continue to use the horse power as the unit of power.

J. W. Welsh (communicated after adjournment): From the standpoint of the operating engineer, any method of rating apparatus in which the full load continuous output can be secured only at the expense of attaining the maximum permissible temperature rise, hardly appears to be a safe basis of operation.

In eliminating all overload ratings for continuously rated apparatus it is believed that too radical a step is being taken. There are certain usages where this is less objectionable than others. For example, in a large generating station where the load is comparatively steady it is possible to operate a machine continuously at its maximum rating. At the other end of the system and at intermediate points where the diversity factor is lower, the fluctuating nature of the load as well as the prominence of the peaks make an overload capacity in the apparatus highly desirable.

The maximum capacity required is determined by the peak. It is believed that for the same materials and ultimate temperature in a given piece of apparatus, a greater ultimate rating on the basis of a short peak can be given, than would be permitted for the maximum rating on a continuous full load basis. In other words, a maximum-rated machine, if operated at less than rating, should pull an overload above its maximum continuous rating for a short time with the same ultimate temperature. Moreover, as brought out in the paper of Dr. Steinmetz and Mr. Lamme, if the same ultimate temperature is attained both with peaks of short duration and for continuous operation, the life of the apparatus is increased in the former case.

A further objection to rating up apparatus to its maximum continuous output, is the bad effect on certain operating characteristics, such as the starting and running torque of motors, the commutating limit in d-c. apparatus, etc. The values proposed for these are considerably less than those which were guaranteed in specifications under the present rules. From this it appears that the margin of capacity has been cut down here as in the case of temperature rise. In other words, it is difficult to secure good operating characteristics at what amounts to overload on the old basis.

The recommendation is therefore made, that the full load continuous rating be fixed on such a basis as will still permit of overload ratings for a one-hour or two-hour peak in addition to the momentary overload. The ultimate permissible temperature should then be adjusted to meet these ratings.

Referring now to the report on *Method of Rating Electrical Apparatus*, in specifying the ultimate temperature as the basis of rating apparatus rather than the temperature rise, it is believed the exactness of the specified rating is sacrificed. If the ultimate temperature is fixed, it is also necessary to fix the temperature of the cooling medium. If, for example, the ultimate temperature is fixed at 90 deg. cent. and the cooling air at 25 deg. cent. the temperature rise is 65 deg. cent., which will certainly permit of a greater rating than if the cooling air is taken at 40 deg. cent., which corresponds to a 50-deg. rise.

While it is possible to measure the ultimate temperature of apparatus with a considerable exactness, it is believed that the determination of the true temperature of the cooling medium will be found more difficult.

The temperature of the cooling medium should be taken as that of the outside air when this is brought in through ducts for cooling purposes. To take the room temperature immediately surrounding the machine appears to be charging off the temperature rise twice, since the room temperature is the result of the fact that the heat given off from the apparatus has raised the temperature of the cooling medium. The room temperature in this case has no more bearing on the situation than has the hot temperature of the cooling water leaving a water-cooled transformer.

In case of apparatus operated without forced ventilation it is obviously unfair to take the air temperature in the immediate vicinity of the apparatus, since, as in the above case, this represents a rise in temperature of the cooling medium due to the heat generated in the apparatus itself. If there is no other heat-emitting object in the room in which the apparatus is located, and the natural ventilation through the doors and windows is such that with the apparatus running at rated load, a constant temperature gradient is reached within the room, then the case becomes similar to that of forced ventilation, the only difference being the rate at which cold air is supplied from the outside. The temperature within the room will of course vary at different points, being hottest near the apparatus and coolest at the doors or windows. In this case also, it is believed that the temperature of the cooling medium should be that of the incoming air as measured within the room, near the doors and windows.

Edmund C. Stone (communicated after adjournment): Operating men cannot take too much to heart the fact, so clearly brought out in the paper, that each overload producing an excessive temperature materially weakens the insulation of the machine and shortens its life by a perfectly definite amount.

While the gradient of 10 to 15 deg. of the hottest parts above the rise obtained by conventional methods is sufficient for machines of the best design, many manufacturers are offering apparatus having the same temperature guarantees but much less ventilation. A purchaser, therefore, is not always protected by a measurement of rise by the usual methods—he must either actually measure the temperature of the hottest parts of the machine or be able to judge fairly accurately the value of the ventilation actually provided.

Machines in the past have been so liberally designed that they have actually been good for a continuous load much above their rating. It is now possible to predetermine the performance of a given design far more closely than in the past. Hence machines now put out come very close to the guaranteed rise. If, under these conditions, the full-load guaranteed rise is made the maximum safe rise of the machine, it is obvious that the customer will not get as much for his money as heretofore.

Regarding the question of a single rating, it seems to me that in addition to the rating the manufacturer should furnish a "time-overload" curve, showing the length of time that a machine can carry various overloads.

This is of supreme importance because of the sharp peaks that are a characteristic of many types of commercial load. For instance, one substation has a one-hour peak 35 per cent in excess of its normal load, during three months of the year only. It would be needlessly extravagant to buy for this station apparatus having a continuous rating equal to this peak.

For such reasons as this it is impossible for any operating man to use his apparatus economically unless he has a good knowledge of its heating characteristics.

William F. Dawson (communicated after adjournment): Considerable timidity has been expressed over the recommendation of Dr. Steinmetz and Mr. Lamme to establish a maximum measurable temperature of insulation made from organic materials, as 90 deg. cent. The testimony of Messrs. Steinmetz and Lamme, frequently repeated, that they have found this a conservative limit should, in view of their great experience, satisfy most critics.

Their recommendation is to a large extent supported by most exhaustive and interesting tests made at the National Physical Laboratory, Teddington, London, for the Engineering Standards Committee (British), in a paper entitled " Report on Temperature Experiments " and read by Mr. Raynor and Dr. Glazebrook before the Institution of Electrical Engineers, March, 1905.

Attention is also directed to a paper on " Temperature Curves and Rating of Electrical Machinery " read at the same meeting by Mr. Rudolph Goldschmidt, both papers containing illuminating information on the subjects discussed.

The writer agrees with the sub-committee report in regard to heating of commutators, but would point out that the commutator connections of many machines, especially those of generators direct-connected to low-speed engines, and even on many motor-generator sets and synchronous converters, are of such length that the nature of the armature insulation can be happily ignored in placing limits on commutator heating.

When carbon brushes were first introduced on machines which had previously been supplied with copper brushes, commutator radiating surface was restricted and the temperature rise of 55 deg. cent. on the commutator was usually guaranteed and accepted, and proved satisfactory.

The recommendation of the sub-committee in regard to bearings is endorsed, and it is particularly pointed out that bearings, and bearing oil, can be operated at much higher temperatures than generally supposed. High temperatures are practically essential to the operation of high-speed turbine bearings, as the friction decreases with increase of temperature. With properly designed bearings and suitable oil and oiling system 100 deg. cent. may be considered a perfectly safe temperature limit.

The fields of maximum-rated turbo-alternators are so designed that if 50 per cent overload at standard fractional power factor is applied the voltage must fall below normal. This is desirable, as otherwise there will be a tendency to damage the machine from overloads. The writer suggests that the " reason to be " of the 50 per cent overload stipulation and its period are debatable. It would seem desirable to allow this overload to permit the starting of additional machines, in case of sudden and unexpected demand for extra current such as occurs in certain localities from a sudden thunder-storm or fog. He questions if 60 seconds is quite sufficient; 2½ or 3 minutes would seem more appropriate.

The absence of overload guarantees except as suggested above is exceedingly appropriate with modern high-speed machines, particularly of moderate voltage, as the rapid movement of air notably reduces the thermal " surface drop " and makes possible the comparatively high loading of the copper conductors. Conditions, however, are different on comparatively low-speed machines, such as for direct connection to steam engines, gas engines and low-speed waterwheels. Here the surface drop is not reduced to the same extent and consequently for continuous operation the conductors cannot be given the same loading and, therefore, have a considerable reserve of thermal capacity so that it would be appropriate to discuss short time overloads, say of half an hour, or an hour.

The writer endorses the Committee's recommendation to rate alternating-current generators in kilovolt-amperes rather than in kilowatts. He would, however, point out that many turbo-alternators have their capacity limited by the field, and that, as even at 80 per cent power factor the field current for kilovolt-ampere rating has not reached maximum value, the power factor should always be specified. The field current at power factors varying from 100 to 0, (kv-a. remaining constant) for three typical turbo-alternators, is indicated by the following table:

	Power Factor	Field Amperes
Example I:	100 per cent	103.2
	90 " "	122.9
	80 " "	129.2
	60 " "	136.8
	0 " "	143.5
Example II:	100 " "	85
	90 " "	101.3
	80 " "	106.9
	60 " "	113.0
	0 " "	118.5
Example III:	100 " "	61.3
	90 " "	73.5
	80 " "	77.5
	60 " "	82.0
	0 " "	86.5

Synchronous converters are susceptible to additional heating from wattless currents, and when intended for use in part as synchronous condensers the requirements should be carefully specified.

Philip Torchio (communicated after adjournment): In conformity with the understanding at the meeting of February 26 that certain parties should submit in writing their further comments on the proposed revision of rules and rating, I beg to state the following:

There seems not to have been any substantial difference of opinion upon the question of substituting a single rating in place of a normal load rating with overloads, provided the rating were sufficiently conservative. From the discussion at the meeting it developed that there is a substantial discrepancy in the recommendations of the two sub-committees.

Messrs. Steinmetz and Lamme's committee recommended "a maximum rise of temperature of 50 deg. cent. by ***conventional methods*** of measurement or 60 deg. cent. at the ***hot spots***," the difference of 10 deg. cent. being due to temperature grading in insulation. On the other hand, the committee on revision of rating recommended "a maximum rise of 50 deg. cent. at the ***hot spots***," which, in accordance with the previous report, would be equivalent to 40 deg. cent. rise by ***conventional methods.***

As everybody seems to agree that this difference of about 10 deg. cent. between conventional measurements and actual temperature at ***hot spots*** (temperature grading) is about representative of actual conditions, and as it appeared from the discussion that it is almost impossible, except for a very expert engineer, to locate the ***hot spots***, therefore, I do recommend that the ***conventional methods*** of measurement be retained and that 40 deg. cent. be the maximum rise of temperature allowable by ***conventional methods*** and 50 deg. cent. at the ***hot spots.***

In other words, the standard rating should be based on ***conventional methods*** of measurement of temperature, as the ordinary customer would not be in a position to check the rating of his apparatus if the ***elusive hot spots*** are to be the basis of rating.

In conclusion, I therefore recommend that, to safeguard the interests of the general consumer of electrical power, the Institute's standard rating be based on 40 deg. cent. rise above room temperature, the measurements to be made by any of the present ***conventional methods***, also that the machine is to operate at any room temperature up to 50 deg. cent. without making any temperature correction in determining the rise and that $50 + 40 = 90$ deg. cent. be the maximum total temperature at which machine be operated.

A paper presented at the Midwinter Convention of the American Institute of Electrical Engineers, New York, February 26, 1913.

NOTES ON INTERNAL HEATING OF STATOR COILS

BY R. B. WILLIAMSON

In the design of alternating-current generators, close inherent regulation was formerly considered desirable, and the output was frequently limited by regulation, rather than by heating. However, with the general introduction of automatic voltage regulators, close regulation has become less important, and for some classes of generators it is now recognized that it may even be very undesirable. This is so in high-speed machines of large output, in which low reactance is undesirable on account of the excessive current set up in case of accidental short circuit. The tendency is therefore towards machines having relatively poor inherent regulation, and the limiting output of such is fixed by the allowable temperature rise. It is also becoming common practise to rate generators, particularly those for connection to steam or water turbines, on a maximum basis; usually on the output that can be delivered continuously with a maximum temperature rise of 50 deg. cent. The tendency is to place the heating limit at the maximum point to which the machines can be operated safely for continuous service, thus getting the maximum output possible from a given investment in generating machinery.

Assuming the limit of output to be fixed solely by heating considerations, the question arises as to what maximum temperature is allowable, and to which part of the machine it should refer. Rotor coils on a-c. generators can, if necessary, be insulated safely to withstand maximum internal temperatures as high as 150 deg. cent. by using mica, asbestos or similar material. The excitation voltage is not high and the coils are usually of such shape that this kind of insulating material can be applied in such manner as to make a good mechanical job. With stator coils it is often difficult to use these materials by themselves on account of the relatively high voltage and also because of the irregular shape of the windings. Hence stator coil insulation, in most cases, contains more or less cellulose material, such as cotton tape, treated cloth, etc. Opinions differ as to the maximum temperature at which such material can be operated continuously without deterioration, but a maximum ultimate temperature of 100 deg. cent. in the hottest part may be taken as the limit. It should be noted in passing that metal parts of a generator, not in contact with the coils, as for example the back of the punchings, pole tips, etc., might attain a temperature considerably in excess of 100 deg. cent. without endangering the machine in any way.

Assuming the limit of output to be such that no part of the stator insulation shall exceed a certain temperature, it is important that the designer should be able to make a reasonably accurate estimate of the maximum temperature. Thermometer measurements on machines under test give the temperature of the outside of the coils and the surrounding parts. Measurements of temperature by resistance give the average temperature of the copper but do not give the maximum temperature unless the coil happens to be heated uniformly throughout its length. In machines having narrow cores, measured parallel to the shaft, considerable heat will flow along the copper to the projecting ends of the coils, which are usually cooler than the part in the slot. On the other hand, in machines such as turbo-generators and high-speed waterwheel units of large output, having very long cores, a much smaller proportion of this heat will pass from the part of the coil located in the middle of the machine out towards the ends. Consequently most of the heat liberated from the copper in the slot in the central part of such machines, should be considered as passing through the insulation into the immediately surrounding parts. The highest temperature will thus be on the copper inside the coil and near the central part of the generator. This temperature cannot be measured by taking the increase in resistance of the whole winding, and it is neither safe nor advisable to use temperature coils inside the insulation of the windings on account of the danger of insulation breakdowns and also risk in taking observations. These temperature coils would be directly in contact with the high-voltage winding, and therefore a source of danger under regular operating conditions. However, the temperature of the surrounding parts immediately outside the coil is easily obtained by thermometers or temperature coils, and if the temperature difference between copper and iron can be calculated, the safe operating temperature for the outside of a coil in any given case can easily be fixed.

It is therefore very desirable to have some means of predetermining this temperature difference, provided the method can be depended upon to give results close enough for the purpose. It is with the object in view of bringing out discussion and results obtained by others that the following is offered.

The temperature difference between the copper inside a coil and the medium with which the outside is in contact, depends on the rate at which heat is transmitted per unit area of insulating wall, the thickness of the wall, and the heat-conducting properties of the insulation. A knowledge of the heat conductivity of various kinds of insulation as used in generators must therefore form the basis of calculation of this temperature difference. Table I shows average values from tests made by Mr. T. S. Allen, and used by the writer, during the past two or three years. Tests on various materials have been recently published by Mr. H. D. Symons and Mr. Miles Walker[1], and some of their results are given in Table II.

It will not be necessary for the present purpose to describe in detail the methods used for measuring the thermal conductivity of the different materials, except to state that, in general, a given amount of power was passed through a known area of insulating material and the difference in temperature between the two sides of the wall observed. The specific thermal conductivity was then expressed in watts per square inch per one deg. cent. difference in temperature per one inch (2.54 cm.) thickness of wall. This conductivity coefficient is here denoted by k_t. It was found in the tests by Messrs. Symons and Walker that cellulose materials, such as cotton, paper, etc., had a considerable temperature coefficient. For example, the conductivity at 100 deg. cent. was about 12 per cent higher than at 30 deg. cent. On the other hand, the heat conductivity of mica was found not to change between 20 deg. cent. and 100 deg. cent.

Assuming that, within the range of thickness used for slot insulations, the temperature difference is directly proportional to the thickness of wall, Fig. 1 shows the relation between watts per square inch transmitted through the insulation, and tempera-

1. Harold D. Symons and Miles Walker, *Journal I. E. E.*, Vol. 48, May 1912.

ture difference for one-inch thickness. It is to be expected that the values of k_t will vary considerably for different samples of similar material, but Tables I and II show that, even though the tests were made by different observers by somewhat different methods, and on materials that doubtless varied considerably, the results agree quite well. With a sufficient number of tests

TABLE I

Description	Value of k_t Watts per sq. in. per 1 deg. cent. per 1 in. thickness
1. Horn fiber	0.00186
2. Fish paper	0.00175
3. Empire cloth (not impregnated)	0.00362
4. Empire cloth (impregnated)	0.00432
5. Flexible mica (not impregnated)	0.00207
6. Flexible mica (impregnated)	0.00255
7. 11,000-volt insulation (mica and empire cloth not impregnated)	0.00318
8. Same as 7, impregnated	0.00432

TABLE II

Description	Value of k_t Watts per sq. in. per 1 deg. cent. per 1 in. thickness
(Symons and Walker.)	
1. Varnished cloth tightly wrapped	0.00634
2. Presspahn untreated	0.0042
3. Rope paper untreated	0.00292
4. Rope paper treated with Sterling varnish	0.0042
5. Fullerboard varnished	0.0035
6. Empire cloth and mica—alternate layers tightly wound	0.0053
7. Empire cloth, mica and tape containing air spaces (see test on turbo-generator referred to)	0.0037
8. Built up micanite tube with about 19 per cent shellac	0.0026
9. Built up micanite tube with about 11 per cent shellac	0.0031
10. Solid mica plate	0.00915

on a given class of material, there should be no difficulty in obtaining the value of k_t closely enough for the purpose in view.

Fig. 1 shows very plainly the relatively poor heat conducting properties of air. For still air the value of k_t may be taken as 0.00052. Thus a layer of air one mil thick may retain the heat as much as 10 mils of insulation. The great importance of excluding air from the insulating wall and the desirability of a snug fit between the coils and iron are apparent. In estimating the temperature difference between copper and iron, any air spaces present must be allowed for in determining the heat conductivity of the wall as a whole.

The effect of minute air spaces is also shown by the tests on the various materials. The sample of solid plate mica showed a high conductivity, while various kinds of built-up mica had relatively low conductivity. Empire cloth (oiled cambric) is much better than mica as a heat conductor, principally because the small air spaces are well filled with varnish. In every case, im-

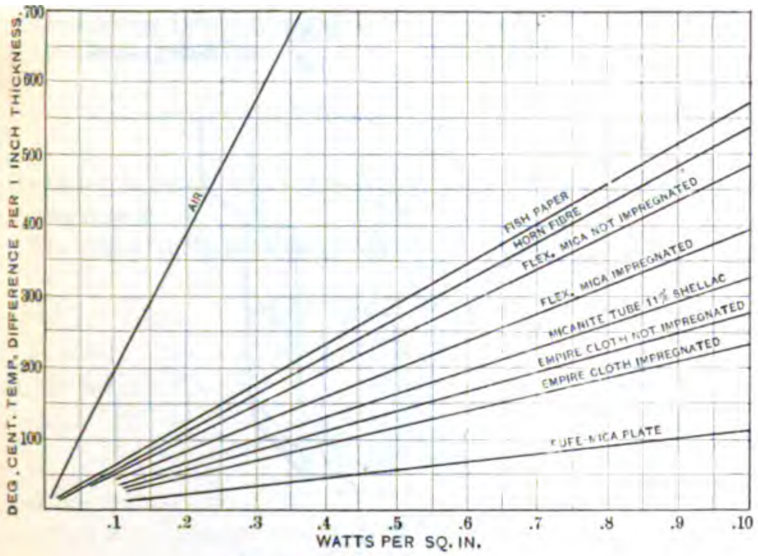


FIG. 1

pregnating a sample improved its conductivity, the increase in some cases amounting to 25 per cent, on account of elimination of air spaces.

With a knowledge of the conducting properties of the various materials making up a given insulation, together with the thickness of the layers, including such air clearance as may be present, a value of k_i for the composite wall can be determined. Considering a slot section as in Fig. 2, we will assume that this one inch running length of imbedded coil is located in the center of the machine and that all the heat liberated in the copper has to find its way out through the insulation to the immediately surrounding iron parts. That is, we will assume the worst conditions found in a long machine where little heat can pass from this part to the projecting ends of the coils. In a two-layer arrangement as shown in Fig. 2, the heat liberated in 2 may be different from that in 1 on account of the difference in eddy current loss.[2] Also the surface through which the heat passes to the iron is greater for 1 than for 2, since 1 has three faces in contact with the iron, while the other has but two. The face of 2 next to the insulating wedge will not be considered, since the wedge is usually a very poor heat conductor and has considerable thickness.

Let r = specific resistance of copper = $\frac{0.895}{10^6}$ ohm per $\frac{\text{inch}}{\text{inch}^2}$

at 100 deg. cent., or $\frac{0.83}{10^6}$ at 75 deg. cent.

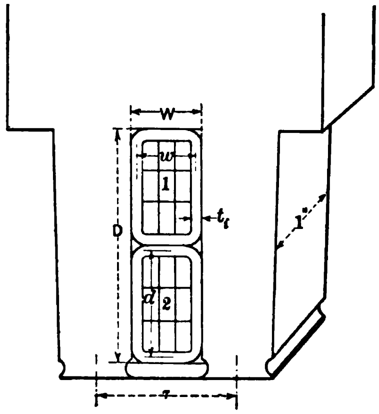


FIG. 2

A = cross-section of conductor in sq. in.
I = current in conductor—amperes.
N_c = number of conductors per half slot = number of turns per coil in Fig. 2.
N = number of conductors per slot.
k_e = factor by which eddy $I^2 R$ loss is to be multiplied to allow for eddy currents.
k_i = thermal conductivity of insulating wall including air clearance (watts per sq. in. per deg. cent. per one inch thickness.)
t_i = thickness of insulating wall in inches measured from copper to iron and including clearances.

2. A. B. Field, TRANSACTIONS A. I. E. E., 1905, Vol. XXIV, page 761.

K = ampere conductors per inch of stator circumference.
W = width of slot.
D = effective depth of slot (not including retaining wedge.)
S = current density in conductor, amperes per sq. in.
τ = slot pitch in inches.
w = mean width of coil in inches measured to center of insulation.
d = mean depth of half coil measured to center of insulation.
T_d = difference in temperature, deg. cent., between copper and outer surface of coil.

Considering one inch running length of coil, the loss including eddy currents will be

$$\text{Watts per one inch length of coil} = \frac{r\, I^2\, N_c\, k_e}{A} \tag{1}$$

the factor k_e being selected to suit the part of the winding under consideration.

The mean surface through which this heat passes is

$$\text{for } 1 = 2\,d + w \text{ sq. in.}$$

$$\text{for } 2 = 2\,d \text{ sq. in.}$$

Hence for 1

$$T_d = \frac{r\, I^2\, N_c\, k_e\, t_i}{A\;(2\,d + w)\; k_t} \tag{2}$$

and for 2

$$T_d = \frac{r\, I^2\, N_c\, k_e\, t_i}{2\, A\, d\, k_t} \tag{3}$$

$\frac{I}{A}$ = current density in stator conductor (amperes per sq.in.) = S

Hence (2) and (3) may be written

$$T_d = \frac{r\, I\, S\, N_c\, k_e\, t_i}{(2\,d + w)\; k_t} \tag{4}$$

$$T_d = \frac{r\, I\, S\, N_c\, k_e\, t_i}{2\, d\, k_t} \tag{5}$$

For most cases it will be sufficiently accurate to treat the slot and coil as a whole and take the area through which the heat passes as $2D + W$. The temperature difference then becomes

$$T_d = \frac{r\, N\, I\, S\, k_e\, l_i}{(2\, D + W)\, k_t} \tag{6}$$

In high-voltage machines using a small conductor, or in others where the design is such as to limit eddy currents to a small amount, the factor k_e can be omitted.

In (6), $N\, I =$ ampere conductors per slot = ampere conductors per inch × slot pitch, and

$$T_d = \frac{r\, K\, \tau\, S\, k_e\, l_i}{(2\, D + W)\, k_t} \tag{7}$$

It is interesting in (7) to note the various items on which the temperature difference T_d depends. For a generator of specified voltage and with given insulating materials available, the values of l_i and k_t are practically fixed within rather narrow limits. The designer must therefore proportion the slots and fix the current density S in relation to the specific loading K in such manner that T_d will be within the allowable limits. The permissible value of T_d will depend on the ultimate temperature of the surrounding parts. Thus for a maximum internal temperature of 100 deg. cent. and a maximum rise on the iron of 50 deg. cent., the outside coil temperature would be 75 deg. cent. with surrounding air at 25 deg. cent.; T_d in this case therefore should not exceed 25 deg. cent.

On the other hand, if the design is such that the temperature rise on the iron is only 40 deg. cent under these conditions, T_d could be 35 deg. cent. for the same internal temperature of 100 deg. cent.

In (7) it should be noted that the smaller the slot pitch τ the lower will be the value of T_d, other things remaining constant. That is, the more the winding is subdivided the better the conditions are as regards internal heating. However, there is a limit to which subdivision can be carried without making machines unduly large and expensive, and unfortunately this is specially the case in high-voltage generators where the thickness of insulating wall is relatively large. The internal heating of high-voltage coils thus becomes a difficult matter to handle, especially in large turbo-generators, and it is frequently desirable to wind such machines for lower voltage and use step-up transformers. In high-voltage units, the value of T_d may be surprisingly large if care is not taken to keep the current density and specific loading within such limits that the area of insulation in contact with the iron can transmit the heat without excessive temperature difference.

As regards the values of T_d obtained by the method here outlined, tests, so far as they have been made by the writer, indicate that the calculated difference can be depended on within limits close enough for the purpose. The calculations regarding this temperature difference are also very useful in comparing the merits of different designs.

In one case a 6000-kw. waterwheel generator having a core 36 in. (91.2 cm.) long was tested as follows. A temperature coil was placed in contact with the copper before the stator coil was insulated. This temperature coil was placed at the center of the machine and in the part of the stator coil lying in the top of the slot next to the inner periphery of the stator. Another temperature coil was placed outside the insulation so that the difference in the readings allowed the value of T_d to be determined. The machine was run on short circuit at about 25 per cent current overload until temperatures became constant. Under these conditions, the two temperature coils indicated a temperature difference of 19.5 deg. cent. The insulation consisted of a combination of mica and empire cloth, the value of k_t being 0.003. The various constants in this case were as follows:

$$r = (\text{for 75 deg. cent.})\ \frac{0.83}{10^6}\ \text{ohms}$$

$$I = 470 \quad S = 1675 \quad N_c = 2 \quad k_e = 1.05$$

$$t_i = 0.15 \text{ including clearance.} \quad d = 1.62 \text{ in.} \quad k_t = 0.003$$

From (5)

$$T_d = \frac{0.83 \times 470 \times 1675 \times 2 \times 1.05 \times 0.15}{2 \times 1.62 \times 0.005 \times 10^6} = 21.1 \text{ deg. cent.}$$

or 1.6 deg. cent. higher than the observed temperature difference.

In the tests by Messrs. Symons and Walker, already referred to temperature coils were placed in a 5000-kw. turbo-generator and the observed value of T_d was 20.6 deg. cent. between copper and iron in the slot. The loss per inch (2.54 cm.) length of coil was 2.26 watts, making due allowance for eddy currents. The mean area through which the heat passed was 5.3 sq. in. (34.2 sq. cm.), thus giving 0.427 watt per sq. in. (0.066 sq. cm.). The thickness of insulating wall was 0.177 in. (4.5 mm.) and k_t thus works out at 0.0037, which checks very well with thè values found for similar material when tested in the form of samples.

The writer has other tests on machines at present under way, but these will not be completed in time for the present discussion As mentioned above, the problem of internal heating is of most importance in long high-voltage machines such as turbo-generators. In case the imbedded part is insulated with mica or similar heat-resisting material, or if fabric material in combination with the mica is used simply as a binder and not depended on for insulation, a maximum internal temperature of 150 deg. cent. might be allowable. With a maximum rise of 50 deg. cent. on the iron, and with air at 25 deg. cent., this would allow a value of T_d as high as 75 deg. cent. for this class of insulation. In any event, no matter what kind of insulation is used, or what the allowable value of T_d may be, it is highly desirable that the internal temperature be predetermined as closely as possible and limited to such value that deterioration under long-continued heating will be avoided.

A paper presented at the Midwinter Convention of the American Institute of Electrical Engineers, New York, February 26, 1913.

MEASUREMENT OF TEMPERATURE IN ROTATING ELECTRIC MACHINES

BY L. W. CHUBB, E. I. CHUTE AND O. W. A. OETTING

Introduction

In some late papers* it has been pointed out that it is not the rise of temperature, but the ultimate temperature to which the insulation of electric machines may be subjected, that is the real limitation in the operation of such machines. Therefore, in the measurement of temperatures of electric machines, it is the temperature to which the insulation is subjected, and not that of the copper and iron, which is desired.

In most cases, direct measurements of the temperature of the insulation in the hottest portion of the machine are practically impossible; in order to obtain reliable results of such temperatures, it is necessary to measure some adjacent temperatures and from these derive the desired results. The accuracy of such tests will depend, of course, upon the temperature measuring device, its nearness to the point to be measured and a knowledge of the temperature gradient between the point measured and the point at which the temperature is desired. In estimating the temperature gradient, it is necessary to consider the sources of heat, the direction of flow, and the thermal constants of the conducting parts which affect the distribution of heat.

In electric machines the temperatures of the two faces of any of the insulation will be very nearly the same as that of the adjacent parts, and the temperature of this intervening insulation will be between these limits (except in rare cases of high dielectric losses). For example, the insulation on an armature

*C. E. Skinner, *Proceedings* Association of I. & S. E. E., Oct. 1912.
B. G. Lamme, Transactions A. I. E. E., page 21, this volume.

coil will have the local copper temperature on one side and the local iron temperature on the other side. One or the other of these may be the higher, depending upon the relative losses in the iron and copper, the ventilation, and the conductance of the adjacent paths of heat flow. If at all points throughout the length of the slot the iron is hotter than the copper, then the hottest point of the insulation can be measured directly by means to be described later. If the copper at any point is hotter than the iron, it is fair to assume that the hottest coil insulation is next to the copper at the center of the slot. The temperature measurement that can be taken is on the outside of the coil at the center of the slot, and to obtain the maximum temperature of the insulation it is necessary to make allowances for the temperature gradient through the insulation.

Obviously the ordinary methods of measuring temperatures give no exact indication of the distribution of the heat inside of electric machines. Heretofore special methods of measuring temperatures have been considered impractical and almost impossible. Recent developments, however, show that internal temperatures of electric machines can be readily obtained, especially those of stationary parts.

Present Standard Methods of Measuring Temperatures

1. *By Thermometers.* This is the most common method in use. Its chief recommendations are its availability, simplicity and cheapness. Its usefulness is limited to the temperature measurement of the external parts of a machine. Consistent results can be obtained by this method, provided the conditions under which it is used remain the same. On these conditions depends entirely the proper interpretation of the results, for any conclusion based on past experience is reliable only in so far as the basis of that experience remains unchanged. Wide variations in temperatures will be obtained, depending on the location of the thermometers and the method of application to the part in question. Sluggishness is an inherent characteristic and must be kept in mind in any application of thermometers. These limitations will be discussed further under methods of applying thermometers.

2. *By Rise of Resistance.* This method is applicable only to the windings of the machine and obviously represents only an average result, not distinguishing the hot portions from the cold. For windings of high resistance, such as field coils, fairly good

results may be obtained by this method. Although the resistance measurement does not represent the hottest part of the coil, since there is a temperature gradient from the center of the coil to the outside surface, yet in most field coils this gradient will not be excessive, so that the average temperature, while below the maximum, will still be a close indication of the safety of the coil.

In the case of low-resistance windings the results obtained are far from satisfactory. In the first place, laboratory methods and apparatus are required to obtain results with any degree of reliability. Variations of contact with temperature are continually entering in, offsetting the accuracy of even the most reliable measurements. The proper interpretation of correct measurements here is much more difficult than in the case of field coils of high resistance. With the winding passing through several zones of temperatures, the average value of the temperature is but a slight indication of what may be existing in the various parts. For instance, if the end windings of an armature are well ventilated so that the temperature of part of the end copper is but little higher than that of the air, there may be a small portion of the coil buried in the core which is at a considerably higher temperature, and yet have but little influence on the total resistance of the winding. Again, a portion of the end windings may be so packed together and so completely covered by bands, that the relative temperature rise in this part is high, while the armature core and the buried copper is at a relatively low temperature. This condition was brought rather emphatically to the attention of some of us in the case of a large revolving-field generator. The rises by resistance on the various tests were consistent and bore a reasonable relation to the thermometer temperatures, the maximum value being 45 deg. cent. However, on opening the windings it was found on several of the coils that the insulation surrounding the portion buried in the bottom of the slot had been heated to a much higher temperature than that indicated by the rise of resistance method.

Perhaps the most unsatisfactory results obtained by this method are those pertaining to the windings of the armatures of direct-current machines. Temperatures thus obtained are inconsistent and the least said about them the better.

Special Methods of Measuring Temperatures

1. *By Exploring Coils* (Resistance Type). This method of measuring temperatures depends on the fact that resistance of

most materials varies with the temperature of the material. There are several methods in use for measuring the resistance of such coils, any one of which gives results with a fair degree of accuracy. Most of these methods have many complications which are liable to involve inaccuracies. There have been special indicators devised, however, that enable these measurements to be made directly and with very few complications. Bulbs of special forms (suitable for various applications) are manufactured and may be inserted at any point where it is desired to measure temperature. These bulbs are made with either three or four leads which connect to suitable binding posts on the indicators. The indicators have a variable resistance which is adjusted until the galvanometer of the indicator reads zero. This variable resistance is adjusted and calibrated so that the temperature is read off directly from the scale on the indicator. The whole apparatus is convenient for reading temperatures quickly and accurately.

The chief drawback of this method is the expense of the exploring coils. For high temperatures, platinum resistance coils are used, and for lower temperatures, coils of nickel wire. These coils must all be carefully adjusted and calibrated. Likewise the same leads must always be used in conjunction with a certain exploring coil; so that if these leads are broken or if it is necessary to increase the length of the leads, the calibration of the instrument is changed and the correct temperature cannot be obtained unless a new calibration of the coil is made. To obtain a coil of small size, such as must be used within the slot of an electric generator, a considerable amount of fine wire must be used to obtain the necessary resistance. This makes the exploring coil frail and is apt to cause breakage.

2. *By Thermocouples.* Another convenient method of measuring temperatures is by means of thermocouples. This method is used largely in high temperature measurements where thermometers cannot be used. However, it has not been applied in low temperature measurements to any great extent because, until recently, there were no readily portable indicating instruments sensitive enough to give accurate results without troublesome corrections or special adjustments of the thermocouples. To obtain temperatures by this method the e.m.f. of the couple may be measured by a millivoltmeter or by the potentiometer method. This latter method is very accurate and can be used in portable form to read temperatures to a higher degree of accuracy than the ordinary thermometer. Fig. 1 shows a diagram of con-

nections of the instrument which was used in all the tests to obtain the data of internal temperatures which are given in this paper.

B is an ordinary dry battery supplying a very feeble current to the potentiometer circuit made up of the potentiometer wire *P*, a variable resistance R_v, and the resistance *R*. *S* is a special rocker switch with spring so made as to be connected between *a* and *g* on one side and *c* and *h* on the other, when neither side of the rocker switch is depressed. If the switch is depressed on side *g*, connection is made between *g* and *b*. This causes a current to flow in the circuit made up of galvanometer *G* and resistance R_s. The deflection on *G* gives a measure of the current in the potentiometer wire *P*. The resistances *R* and R_s are so adjusted and the galvanometer *G* is so calibrated that the current is at the proper value when the needle of the galvanometer reads the micro-

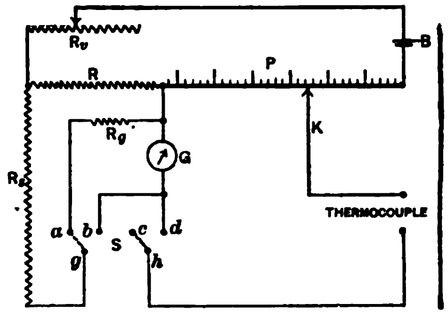


FIG. 1

volts per deg. cent. for the couple that is used in measuring the temperature. With the current set at this value the spring contact again connects *a* and *g* when the rocker switch is released. This connection substitutes R_g for the galvanometer *G*; and since R_g is made equal to the resistance of *G*, the current in the potentiometer wire *P* remains unchanged.

Now to obtain the temperature on the thermocouple, the other side of the rocker switch is depressed, which connects *d* and *h*. This puts the couple, the galvanometer *G*, and a part of *P* in series (the couple being so connected that its e.m.f. "bucks" that of *P*). Contact *K* is run along the drum holding the potentiometer wire *P* until galvanometer *G* reads zero. The result is read off on a scale along the wire *P*, which scale can be calibrated either in millivolts or some arbitrary unit. If a couple is used that has a linear calibration curve between temperature and e.m.f., the wire *P* can be calibrated directly in degrees of temperature. A couple made up of steel and advance wire gives a calibration curve that is practically a straight line up to temperatures well above those obtained in electric machines.

The accuracy of this method depends on the sensitiveness of galvanometer *G* and the length of the scale divisions on potentiometer wire *P*. The unipivot galvanometer is very satisfactory for this work, giving a meter that is portable, at the same time beng extremely sensitive. The calibration of the potentiometer wire can be made very accurately by using a large number of turns of wire wound around a drum. With the instrument used in this test, seven turns of wire give a range of 150 deg. cent. and each graduation on the scale indicates to two-tenths of a deg. cent.

The great advantage of these thermocouples, like the exploring coils, lies in the fact that they record the actual temperature of the body with which they are in contact and can be applied where it is impossible to locate thermometers. The couples can be made to respond to changes of temperature almost instantly by rolling the junction flat so as to present a large heat-absorbing area and diminish conduction along the leads. A large factor in favor of the thermocouple method with a potentiometer is the utter independence of resistances or lengths of leads; for when the temperature is read by the instrument, the galvanometer reads zero and no current flows; so the resistance in the circuit has no effect whatever.

One thing is quite needful in this method, however, and that is a sensitive galvanometer. The e.m.f. of the couple is small, especially for low temperatures; so to get accurate results, the galvanometer should be sensitive enough to respond quickly to small changes of e.m.f. Another precaution necessary is to see that the material of the couple wires is uniform. It is safe, however, to assume that all the wire on a small reel is of uniform structure, and only one calibration is necessary for each reel.

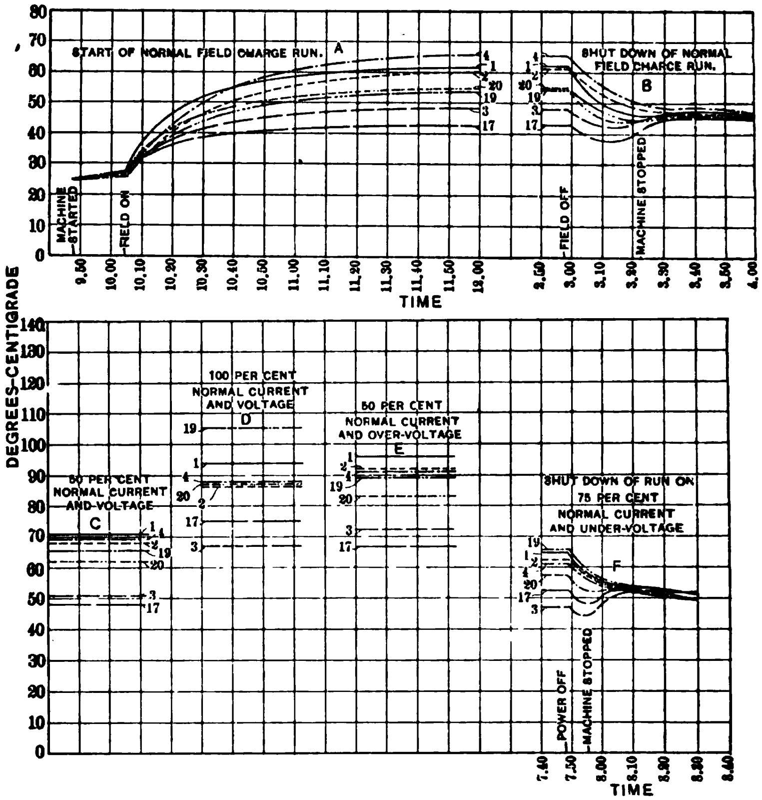
The use of thermocouples (likewise, resistance exploring coils) cannot be recommended for taking temperatures on revolving parts during the rotation of that element. To take such temperatures it is necessary to use slip rings and brushes. The number of such couples is limited by the space available for the slip rings. The mechanical equipment necessary, especially on machines of high peripheral speed, makes the test extremely expensive, and the results obtained are so questionable that this test cannot be recommended. A better method is to fasten thermocouples on the rotating element at different places where the temperatures are desired, allowing the leads to be exposed in some places where there will be no interference with the rotation of the machine. On the shut-down of the machine, leads from the measuring instrument can be clamped on these thermocouples and the temperatures read immediately. Readings of temperatures can be taken at different intervals of time; so if the time is taken between shut-down and the first reading of temperature, the temperature curve can be exterpolated to the actual running temperature of the couple before shut-down. This eliminates all the slip ring trouble and gives a very close indication of the running temperatures in the rotating element.

An interesting series of tests was made on a revolving field generator, showing the comparison of various internal temperatures of both the iron and the copper. Thirty-two thermocouples were placed in the machine; sixteen between laminations in various positions and sixteen in the slots on the coils of the machine. Leads from these different couples were brought out to a special switch and so arranged that any one of the couples could be placed on the measuring instrument and the temperature read. By this means, at least six different temperatures could be obtained in a minute's time. Thus it was possible to get results of temperature at these thirty-two points of the machine, which results, when plotted, showed the heating and cooling during the beginning and the shut-down of each run and also the constant temperatures when the machines had reached their maximum temperature during the run.

Fig. 2 gives a summary of this series of tests, showing seven of the principal temperature curves that were taken. *A* shows the heating of the machine at the beginning of a field charge run. Of course, it is reasonable to suppose that the laminations will become hotter than the coils in a run of this nature. *B* shows the shut-down of this test. When the field was removed from the machine, there was no braking effect to stop the rotating except the friction of the bearings and the windage, and the friction of the brushes on the commutators of two driving motors. It is interesting to note the various temperatures. Couple No. 17, located between a coil and a wedge, cools until the rotation almost ceases; then it becomes warmer again, because the ventilation due to the rotation ceases and the hotter parts of the machine warm the air in the gap, which causes the temperature of couple

No. 17 to rise. This effect is shown also to a lesser degree on No. 3, which is near an air duct in the iron.

C, *D* and *E* of this figure show the maximum temperatures of several other tests. *F* shows the machine shut down as in *B*, but



1— In iron 1/2 in. below top of tooth in center of package.
2—In iron 1/2 in. below bottom of slot in center of package.
3—In iron 1/2 in. below bottom of slot next to air duct.
4—In iron 4 in. below bottom of slot in center of package.
17—In slot between coil and wedge.
19—In slot between coils.
20—In slot between side of coil and iron.

FIG. 2

under somewhat different conditions. A load was on during this run and when the power was removed, the field of the machine was reduced to one-half, instead of being removed entirely as in *B*. Thus the rotation was stopped in eight minutes' time, about one-third of the time as required in test *B*. The curves show the temperature equalizing again in the same manner. These results bring up the question "What is meant by 'shut-down' temperatures?" Obviously very different results would have been obtained on these two tests *B* and *F* if thermometers had been placed on the machine after the rotation had ceased. This point will be discussed later under "Method of Stopping Rotating Element."

PRESENT STANDARDIZATION RULES INDEFINITE

1. *Room Temperature.* This term enters into practically all contracts for electric machines and its meaning should be so clear as to be beyond dispute. The temperatures of air foreign to the machine can have no effect on the machine's temperature; it is the air that immediately surrounds a machine that is effective; or in case of machines artificially cooled, it is the temperature of incoming air. Even under these limitations there is still too great a latitude for dispute. Thermometers so placed as to be affected by the outgoing air are even more undesirable than those placed at considerable distance from the machine tested. It seems that there is no absolute rule adaptable for every case, but certain general limitations may be suggested; such as the maximum distance from the machine, which may be expressed in terms of the diameter of the rotating part, or the thermometers must be so located as to indicate either the temperature of the still air surrounding the machine or else that entering it. Local conditions frequently demand special consideration, such as the effect of the temperature of poorly ventilated pits on the parts affected. In such a case the average temperature surrounding the machine is as foreign in its effect on the local parts as that of the north pole on the equator. In the case of a certain 8770-kv-a. generator in a large hydroelectric plant, this local effect was as much as 10 deg. cent. In another case, safe limits were far exceeded until the pit was properly ventilated, when the temperature of the part in question at once became normal.

2. *Method of Applying Thermometers.* As previously stated, the inferences deduced from thermometer measurements depend upon the duplication of conditions. This applies especially to the application of thermometers. The methods used depend considerably on the nature of the temperature to be measured. These may be divided into two classes: those pertaining to stationary parts, and those pertaining to rotating parts.

In the stationary parts, all thermometers may be located before the start of the test, and as the temperatures increase gradually there is no special difficulty in the thermometer following very closely. In the larger machines, the thermometers should be protected from the windage of the machine, or other drafts, by a small covering. This may be of putty, clay or felt. Some persons prefer one and some another. Putty is rather soft at working temperatures. Clay makes a very good holding medium when carefully put on, but our experience has been that more uniform temperatures, and usually higher results as well, are obtained with the felt. On windings this covering may be in the shape of a small sleeve, the bulb being exposed only on one side; the whole being tied to the coil securely with twine. On iron laminations a small felt pad about 1½ by 1½ in. (3.8 by 3.8 cm.), glued to the surface under which the thermometer is inserted, seems to give the best results. This method also has proved satisfactory when used on the larger copper conductors. The pad must not be too large, as a hot spot may be created, neither must it interefere with the radiation of the machine. On smaller machines, the liability to hot spots becomes much greater and either a very small ball of clay is to be preferred, or else the thermometers should be left bare and treated after shut-down as described below for thermometers on the rotating parts. On these machines a thermometer will not give the correct maximum values while the machine is running, but will indicate when the machine has reached a constant temperature.

The method of obtaining temperatures of rotating parts by thermometers presents quite a different problem. A very wide range of results may be obtained on account of variations due chiefly to the method of getting the heat from the body, whose temperature is to be measured, to the thermometer bulb. The thermometer bulb can be so placed, with respect to the copper in the coil, that the resistance in the heat-conducting path to the bulb is much greater than desirable. For instance, if a thermometer is simply laid on the coil whose temperature is to be measured, and covered with a little waste, and another thermometer is placed on the coil and provided with a seat made of tin-foil, two different results will be obtained. Obviously, with a metal seat, a larger amount of heat in a given time can be conducted from the coil across the insulation and thence to the bulb, than would be the case without the metal seat; for the section of the heat conduction path through the insulation is increased and the average length of the path shortened. However, if the dissipation of conduction of heat from the point to be measured to other parts of the winding is relatively slow, both thermometers should eventually indicate the same temperature. If, however, the heat dissipation is rapid, either by conduction to the air or to other parts of the winding, the thermometer which gives the quicker reading should reach a higher temperature. In a certain instance it was found that the thermometer provided with a metal seat reached this maximum six minutes before one simply laid on the winding, both being protected from the air in the same manner. The discrepancy in temperature was eight degrees, the cooling curve of the machine in question falling rapidly. Thermometers are sluggish in their action and they take a certain time before they come up to temperature. So in similar conditions as mentioned above, if thermometers are applied which have previously been heated to about the same temperature as the machine, higher results of temperature will be obtained than if the thermometers had not been heated before application to the machines.

Thus, all these methods of applying thermometers will give results that in no way will be comparable. There must be a definite ruling as to the manner of applying thermometers so as to obtain comparative results.

3. *Method of Stopping Rotating Element.* Another point which causes dispute is the method of bringing the rotating element to rest after a temperature run. Should the machine be allowed to rotate without any load until all the flywheel effect is expended, or should the magnetic field be used to retard the machines, or should the machine be braked mechanically by means of pulleys and ropes to stop it more quickly? It is quite evident that the fanning effect of the rotating element lowers the temperature of the machine very rapidly. The curves shown in Fig. 2 show temperatures in a machine which had sufficient flywheel effect to allow it to rotate freely for a period of 22 minutes after the load was removed. It is obvious that considerable variation will result in the temperature obtained if the time of bringing the rotating element to rest is varied. For instance, from the curves of the normal field charge run in Fig. 2, it is seen that there is a considerably greater drop in temperature in the 22 minutes than in five minutes. So if the machine had been stopped in five minutes by mechanical braking, higher shut-down temperatures on the rotating element would have been obtained. In another case when mechanical braking was resorted to, temperatures 10

degrees warmer were obtained when shut down in 1½ minutes than when shut down in six minutes.

Conclusion

It is evident from the foregoing discussion that the proper interpretation of temperature results depends almost entirely upon the conditions of tests and upon the temperature measuring method employed. No one method is suitable in every case. The thermocouple and exploring coil are generally applicable; but the thermometer and rise by resistance methods are preferable in many cases because of convenience, simplicity and inexpensiveness.

The experience of the authors has prompted the following recommendations in regard to taking temperatures of electric machines:

1. Thermometers should be used to measure all temperatures of exposed parts, the temperature of which is constant or is not changing too rapidly for the thermometer to follow.

2. In cases where the part to be tested is small, and the thermometer and covering may disturb the thermal conditions, the thermometer should not be used.

3. The application and covering for thermometer bulbs should be standardized.

4. The rise by resistance method should be used to measure only the temperatures of windings which are known to be at a rather constant temperature throughout, such as transformer coils, field coils, some stator windings, etc.

5. The rise by resistance method should not be used for windings or parts of windings of very low resistance.

6. Thermocouples or exploring coils may be used to measure temperatures of internal parts, especially in machines with long cores, such as high-speed turbo-generators, and also in parts where steep and doubtful temperature gradients make it impossible to draw any definite conclusions from thermometer temperatures or rises by resistance.

7. Exploring coils are either expensive, or require expensive adjustments and laborious calculations for tests. Thermocouples are preferable to exploring coils in commercial testing.

8. If internal temperatures are required in revolving parts, thermocouples may be used and readings taken after shut-down. Running temperatures with slip rings and brushes cannot be recommended.

9. Temperatures which will be affected by the permanent application of a thermometer or a pad may be taken by the momentary application of a quickly responding thermocouple.

10. The rises of temperature should be based on the temperature of the *cooling air*. The determination of the cooling air should be standardized as far as possible.

11. A convenient and probably the best method of stopping a machine after a temperature run, is by field excitation. Whenever possible, full field should be used to stop the machine quickly and thus maintain the temperatures.

It is hoped that more definite rulings can be established in regard to temperature tests. A temperature obtained within a machine by means of a thermocouple or an exploring coil may not meet guarantees as specified in the present Standardization Rules. In addition to specifying the methods to be used, the limiting temperatures must be considered in each case.

A paper presented at the Midwinter Convention of the American Institute of Electrical Engineers, New York, February 26, 1913.

METHOD OF DETERMINING TEMPERATURE OF ALTERNATING-CURRENT GENERATORS AND MOTORS AND ROOM TEMPERATURE

BY HENRY G. REIST AND T. S. EDEN

Correct determination of the temperature of apparatus is of great importance to the designing engineer. Of even greater importance, is the determination of a fair room temperature, since all temperature guarantees are made on the basis of rise above room.

The determination of the temperature of apparatus may be made by thermometer, by resistance measurement or in special cases by the use of temperature measuring coils.

Thermometer

In the use of thermometers, it is obviously necessary to select the means of applying and covering the bulbs which will give on the one hand the maximum temperature attained and on the other, will not increase temperature of part where the thermometer is applied. Several coverings are available, such as cotton or woolen waste, felt, putty, etc. To determine which of these coverings is the most suitable, the following experiment was recently made.

A copper plate was fitted over the top of a vessel containing water, so that it could be uniformly heated, and the temperature changed as desired. Bulbs covered with different materials were applied to the copper plate and temperatures read over a range from 86 deg. cent. to 32 deg. cent. The following table gives the results.

The temperatures as read across the table herewith, were taken at the same time, and after they had become constant

Covering	Putty 1 in. dia.	Fiber tube 1¼ in. dia. Top filled with waste	Felt 2 in. square	Woolen waste 2 in. dia.	Cotton waste 2 in. dia.	Uncovered between preceding	
Temp. deg. cent.	86	78	79	81	82	68	69
	71.5	66	66	68	68	59	59
	62.5	57.5	57.5	60	60	50.5	51.5
	51.5	47.5	47.5	49	49	44.5	44.5
	42	40.5	40.5	40.5	41	38.5	38
	37	35.5	35.5	36	36	34.5	34.5
	32	31.5	31.5	31.5	32	30.5	30.5

These thermometers were then carefully calibrated over a temperature range from 89 to 26 deg. cent and found to agree within one deg. at the higher temperatures and one-half deg. at the lower temperatures. The table shows that the bulb covered with putty gives the highest reading in each case.

It is therefore recommended that putty covering be used as the standard, since it not only gives the highest reading but is convenient to handle and can be applied in a compact form, not covering an unnecessarily large area, which would impede natural radiation.

Resistance Measurement

Measurements of resistance both cold and hot may be accurately taken, but in practise it is found that the temperature *cold* is uncertain.

This is due to a possible or perhaps even a probable difference of average temperature of the winding of which the resistance is to be measured, from that of the measured temperature of the air.

A resistance measurement on an armature winding taken at a temperature below 25 deg. cent. and again at a temperature above 25 deg. cent., when reduced to resistance at 25 deg. cent. will seldom check within 5 per cent. Consequently manufacturers must build with larger margins when required to guarantee temperatures by resistance.

The temperature of windings so determined, is not a gage of the highest temperature attained, but is an average temperature. The temperature of the end windings on an armature will usually be lower than that of the portion of the winding imbedded in the slot; similarly on revolving field windings, the temperature indicated by the *IR* drop, will frequently be found lower than that shown by thermometers applied to parts of the coils, for example adjacent to mechanical supports between coils.

This method of obtaining temperature rise on alternating-current generators and synchronous motors, should be limited to the revolving field windings of those machines which are so enclosed that thermometer application is impossible.

Temperature Measuring Coils

In apparatus such as steam-turbine-driven and large high-speed waterwheel-driven generators, where the stator windings are inaccessible for thermometers, it is possible to determine the temperature of these parts by using small coils of fine wire, non-inductively wound, which may be placed next to the insulated coil in the slot portion or outside, as desired. Accurate measurements of resistance of these coils by voltage drop may be taken, and the highest temperatures attained may be known.

These coils should be calibrated at a known temperature before being put into a machine.

This method may be applied to any machine and will bring to light faulty design. Insulating fabrics, such as oiled linen, varnished cambric, etc., are apt to deteriorate more rapidly than desirable between temperatures of 90 and 100 deg. cent. and consequently apparatus so built should not be permitted to run at these temperatures. Other insulating materials are in use and also in process of development, which will permit of operation at higher temperatures than these. This method is recommended for the investigation of temperatures in factory tests, but customers should not require it on more than one of several duplicate machines, unless they are willing to stand the attendant expense, since thermometer readings would be sufficient to show that two or more machines were alike.

For machines with small slots, this method of determination should not be necessary.

Its field of application should probably be in apparatus of 6600 volts and over, and on machines of outputs above 2000 kw.

A few examples are given to show how the temperature *rises* measured by thermometer, resistance, and temperature coil methods differ on one and the same heat run.

The value of this table is more as a comparison of the different methods of measuring temperature, than an indication of the maximum temperatures in a machine, since the tests were not made under full-load conditions, but under such artificial loads as could be obtained at the time the machines were in test.

Size of generator	Voltage	Thermometer	Resistance	Temperature coil
15000 kw.	9000	27	40	44
8750 kw.	6900	22	26	39
6000 kw.	6600	29	35	39
5000 kw.	4000	19	33	44
4000 kw.	4000	15	27	30
3750 kw.	6600	15	30	33
3750 kw.	6600	19	30	33

This substantiates the fact that internal temperatures are considerably higher than outside temperatures in a machine.

It is hard to form a conception of the internal temperature of a coil by external thermometer reading, since with a given loss per unit of area of surface of the coil, the external temperature is very largely dependent on the rate of flow of the cooling air over the surface, whereas the difference of temperature between the inside and outside is a factor of the rate of transmission per unit cross-section of the material used in insulating and of its thickness.

In the rotors of turbo-driven generators, on account of the construction necessary, temperatures greater than in the stators will result, requiring the use of insulations which will stand such temperatures.

On this part of a machine there appears to be no difficulty in making an insulation safe for these higher temperatures, due to the low potential employed for excitation.

The determination of these temperatures should be by resistance, calculated from the $I\ R$ drop. To apply temperature measuring coils would be extremely difficult and expensive, involving auxiliary collector rings for measuring the resistance and introducing inaccuracies of measurement as compared with coils in a stator.

Room Temperature

Commercial testing must necessarily be conducted in rooms where the temperature will vary and where apparatus and room thermometers will be subjected to air currents caused by opening of doors or windows.

Machines of small volume will respond quickly to changes in temperature of the surrounding air, whereas the temperature change in large machines will lag appreciably behind the change in room temperature.

It is therefore necessary to find some means of lagging the bulbs of thermometers used for determination of room temperature, so that the changes will follow approximately the same curve as the apparatus. For this purpose, a small steel cylinder with a hole in the center, filled with oil in which the thermometer is placed, has been used by some manufacturers.

In order to determine their heat capacity a series of these cylinders was made up, 3 in. (7.6 cm.), 2 in. (5.1 cm.), 1⅜ in. (3.5 cm.), 1 in. (2.54 cm.) and ¾ in. (1.9 cm.) in diameter, all 3 in. (7.6 cm.) high, all provided with a ⅜-in. (0.95-cm.) diameter hole, 1½ in. (3.8 cm.) deep, for the thermometer. They were heated up to approximately the maximum temperature reached by

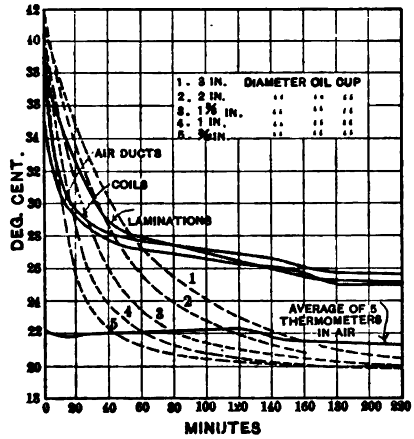


FIG. 1—COOLING CURVE OF 860-KV-A., 2300-VOLT, 720-REV. PER. MIN. 60-CYCLE ALTERNATOR, AND OIL CUPS.

a machine at the end of a heat run, and temperature recorded at frequent intervals, also of the machine running idle. These readings are shown in Fig. 1.

It will be noted that the cooling curve of the 3-in. (7.6-cm.) oil cup, as these cylinders may be called, follows the laminations and the ¾-in. (1.9-cm.) oil cup follows the coils over the higher temperatures recorded, or those temperatures which the machine parts attained under heat run.

Constant temperature of the machine parts is reached at a temperature above that of the room, due to warming up of the air passing through the machine, running at full speed.

It would therefore seem reasonable in determining the room temperature to use an oil cup 2 in. (5.1 cm.) in diameter, which will follow the average temperatures of the different parts of the machine.

Similarly on a 37 50-kv-a. unit, Fig. 2 shows that the cooling curve of the 2-in. (5.1-cm.) oil cup follows the temperature changes of the laminations very closely.

These oil cups, together with thermometers suspended in air, were together with thermometers suspended in air were then

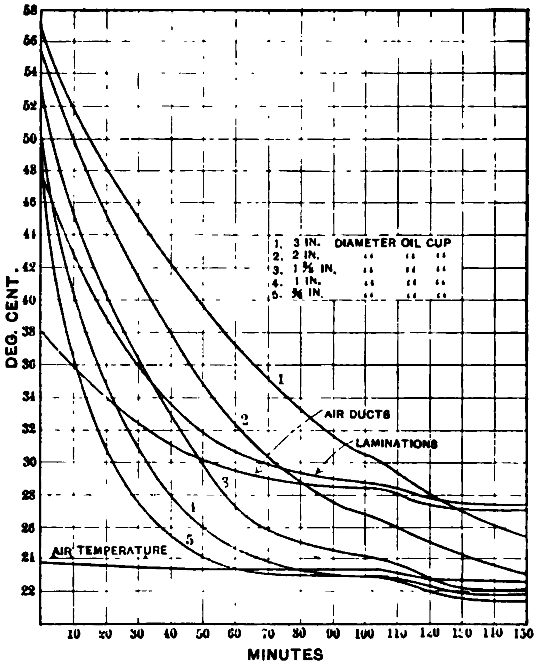


FIG. 2—COOLING CURVE OF 3750-KW., 6600-VOLT, 500-REV. PER. MIN 50-CYCLE ALTERNATOR, AND OIL CUPS.

used in connection with a machine in test, which had reached constant temperatures. Doors were opened for an hour so as to reduce room temperature, and temperatures recorded as shown in Fig. 3.

The following table shows temperature rises above the air-suspended thermometers and above the 2-in. (5.1-cm.) diameter oil cup.

This shows that the temperature rises above the oil cup are fairly uniform and those above the air thermometer vary a great deal.

	Rise over air thermometer	Rise over oil thermometer
Doors opened	28.6	30.2
10 minutes later	33.2	31
10 " "	31.7	31.1
10 " "	29.2	30
10 " "	28.4	29.7 etc.

From these and other observations of a similar nature which have been made, it is recommended that an oil cup of this sort be used for determining room temperatures, both on account of its convenience and accuracy.

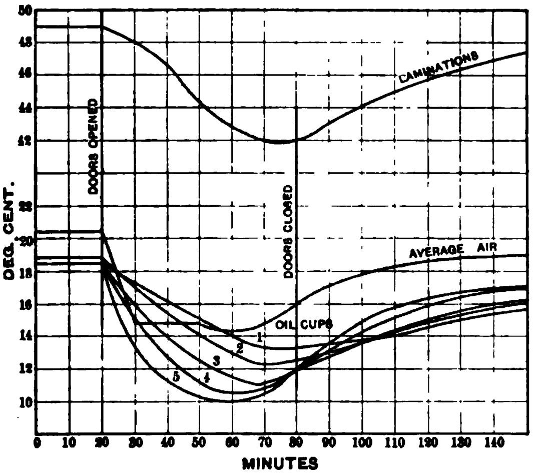


FIG. 3—COOLING CURVE OF 860-KV-A., 2300-VOLT, 720-REV. PER MIN. 60-CYCLE ALTERNATOR, AND OIL CUPS.

It is possible that for stationary apparatus, such as transformers, a satisfactory solution may be arrived at by taking the temperature of an idle machine, the cooling medium being air, water, oil, etc. However, for rotating apparatus, we believe the above method will give reasonably accurate results.

Several different methods of ventilation are used in alternating-current apparatus and the " room temperature " cannot be arrived at in the same way for all cases.

The majority of machines built, take their cooling air from the room they are in and deliver it to the same room. For these, " room temperatures " should be determined in some such manner as given above.

Frequently, with horizontal machines ventilated in this manner, a large part will be below the floor line of a station, in a pit. Unless such a pit is properly ventilated, a portion of the stator will show temperature rises which are dependent on the temperature of the air in the pit, and the rises of such a portion should be based on the pit temperature. It might be noted that the portion so affected will come below the floor line on the one side and above on the other, depending on the direction of rotation. The temperature of the rotor should be referred to an average of the pit and room temperatures, the value of each being decided on the portion of the machine in pit.

For vertical types ventilated in this manner, we would recommend placing the room temperature thermometers at the level of the center line through the magnetic material, since the temperature so obtained would be practically an average of that above and below a machine in test.

However, if a machine is totally enclosed, taking its air from and delivering it outside the room, the temperature on which the rise is based should be that of the air entering, applying a correction factor for the difference between the temperature of the ingoing air and that of the air surrounding the machine. A fair value to give to each for this type of machine might be four (4) for ingoing air, to one (1) for surrounding air: *e.g.* air entering at 15 deg. cent. room at 30 deg. cent.; basis for temperature rise, 18 deg. cent.

Where the frame of a machine is open, so that the air delivered from outside passes through into the room, the correction factor might be applied by giving a value of two (2) to the ingoing air and one (1) to the surrounding air; *e.g.*, air entering at 15 deg. cent. room at 30 deg. cent. basis for room temperature 20 deg. cent.

It would be very difficult to make an exact determination of these correction factors. The above are given as suggestions, which we believe would be fair. Doubtless, many cases will arise which can only be settled on their individual merits.

We appreciate that the tests are not sufficient to base conclusions on and that probably more extended tests will be advisable.

At best, however, the size of oil cup will have to be a compromise because it will not be practicable to change the size of cup for different machines or to use different cups for comparison with different parts of a machine.

A paper presented at the Midwinter Convention of the American Institute of Electrical Engineers, New York, February 26, 1913.

THERMOCOUPLES AND RESISTANCE COILS FOR THE DETERMINATION OF LOCAL TEMPERATURES IN ELECTRICAL MACHINES

BY J. A. CAPP AND L. T. ROBINSON

Temperatures of electrical machines may be determined in either of the two usual ways, namely, by fluid expansion thermometers, or by electrical thermometers, which may be either of the resistance or thermoelectric type.

Fluid thermometers are usually available only for the measurement of surface temperatures. In the case of rotating parts they can usually be applied only after the parts have come to rest. The thermometer, therefore, will indicate a temperature which may not be that existing on the surface during rotation, but may be higher or lower, depending upon the flow of heat during equalization. Thermometers vary greatly in the rate with which they indicate the temperature to which the bulb may be subjected. This, together with the possible error due to the equalization of temperature just mentioned, may bring about considerable errors in the results. Ordinarily the thermometer bulb is held in contact with the surface whose temperature is to be measured, by means of some plastic material, such as putty, which in itself is of relatively low heat conductivity. If the temperature changes which take place during equalization are relatively rapid, there is here a further possibility of error.

Unless some such material as putty is used, the temperature readings of the thermometer will be low, because there is not sufficiently intimate contact between the bulb and the surface whose temperature is to be measured.

As ordinarily supplied by the makers, either mercury or alcohol thermometers are calibrated with either the entire stem immersed in a liquid or atmosphere at the temperature to be measured, or at least with the stem immersed to the point of indication. In other words, the thermometers are customarily supplied with their scale adjusted for full scale immersion. In use in determining the temperature of a surface, only the bulb, or at most but a short portion of the stem above the bulb, is exposed to the temperature to be measured. There have been worked out formulas for correction for the emergent stem, as it is called, but these are cumbersome, and, indeed, such corrections are comparatively seldom made in ordinary measurements of temperature. The correction for the emergent stem may amount to as much as two to four degrees at 80 to 100 deg. cent., the reading in such case, of course, being low. It is possible to obtain thermometers specially calibrated with only the bulb and a very short portion of the stem immersed in the liquid or hot atmosphere used for calibration. Such thermometers when calibrated with, say, 2 in. (50.8 mm.) immersion, may safely be used under ordinary conditions, where the temperature of the emergent stem is approximately the ordinary room temperature at which the calibration was done. The error due to failure to correct for emergent stem is far larger than the probable error of even the ordinary commercial thermometer when used as calibrated. In fact, such commercial thermometers are readily obtainable with an accuracy of four to five tenths of one degree at 100 deg. cent. Of course the error due to the emergent stem is entirely independent of the inherent accuracy of the thermometer itself.

The second method of measuring temperatures, or the electrical resistance thermometer, is susceptible of a variety of applications. Essentially it depends upon the accuracy with which the coefficient of change of resistance with temperature is known. The simplest and most obvious application of this method is the use of windings of the motor or generator itself. In such case, the material being copper, the temperature coefficient is assumed to be 0.00428 at 0 deg. cent., 0.00386 at 25 deg. cent., and the accuracy of the results obtained is dependent upon whether the actual copper used in the winding has exactly this temperature coefficient.

Commercial copper, such as would ordinarily be used in electrical machines of the kind being considered, would very seldom vary enough from standard conductivity and hence from the temperature coefficients belonging to 100 per cent conductivity copper to influence the correctness of the temperature determination more than one deg. cent. between the limits of 25 and 100 deg. cent. This question of definitely known temperature coefficient is, therefore, not of importance in temperature determinations using the entire winding, but in resistance thermometers for accurate work, in which prepared coils are used, this small error is easily taken care of.

The error, then, in measuring temperature by resistance of windings, may be considered as governed almost entirely by the ability to determine accurately their resistance. The error is about three deg. cent. for one per cent variation from correct resistance values. When using workshop means for determining the resistance it is reasonable to expect measurements within one per cent and, therefore, the temperature should be known by this means within three or four deg. cent., if the initial temperature and resistance are known. The temperature rise of a winding of a machine determined by resistance measurements is, of course, subject to still further uncertainty due to the difficulty, especially in large machines, of knowing definitely the temperature of the winding when the cold resistance is observed.

The increase in resistance which is used as the measure of temperature corresponds to the average increase in temperature of the entire winding throughout its length, and the results obtained do not indicate the temperature at any part of the winding. There may be large differences in different parts of the same conductor.

Another application of the resistance method of measurement consists in the placing of fine wire coils at any desired location in or about the winding. Such coils may be placed with thin insulation practically in immediate contact with the conductor, or they may be embedded in the insulation at any desired depth. In such case highly accurate measurements are possible, because the actual temperature coefficient of the piece of wire used as the measuring device is easily known. Here again, the increase in resistance is the result of the average temperature rise in the immediate vicinity of the measuring coil. The extent of the coil, therefore, determines the degree to which the temperature reading obtained is local. Such a device, by bringing leads from the resistance coil to collector rings, may be used to measure temperatures while the parts in which the measuring coil is embedded are rotating.

The practical value of resistance thermometers for high temperatures is doubtful. Base metal coils are permanently changed in resistance by continued heating and if the more expensive platinum windings do not suffer from this cause, they are at least subject to mechanical damage. For the moderate temperatures being considered there is no danger from oxidation and therefore copper or some similar metal may be used, and the coil may be embedded in the machine during construction and thus well protected from mechanical damage, or it can be made up in form, to be inserted later. The temperature coefficient of these coils may be determined with any desired degree of accuracy before they are inserted and the resistance can be measured within any required limits when they are in place. For precision testing it is quite possible to obtain results without difficulty within a part of a degree, using some sort of bridge for determining the resistance; the bridge may be graduated to read directly in temperature. For workshop and central station applications of resistance thermometers, direct-reading temperature-indicating attachments, in the form of indicating switchboard instruments. may be used and satisfactory accuracy obtained. The inserted resistance coils should be reasonably non-inductive and precaution should be taken to have them so protected by insulation that they may not be the cause of damage to the windings of any machine in which they are used, or the means of transferring high tension to the switchboard instruments connected with them or to the low-tension d-c. network which supplies current to operate the device. There can be no question of the reliability of the temperature obtained by this means, representing the average of the region occupied by the coil, but no matter what system of connections is chosen and what safety devices are applied in connection with it, when the coil is embedded in or between high-tension windings it is difficult to dismiss entirely the feeling that an added element of danger to the machine and operator must be reckoned with.

The second electrical method of determining temperature is that using the thermocouple. Since the electromotive force generated by the thermocouple is a function of the difference between the temperature of the junction of the wires forming the couple and that of their free ends, it is obvious that the thermocouple may measure the temperature more nearly locally than any other device.

This same fact requires that there be accurate control of the temperature of the free ends, or the cold end temperature, as it is commonly called. With this temperature controlled by immersion of the free ends in oil or by other equally simple means it is possible to determine the electromotive force with great accuracy, and because of the ease with which a thermocouple may be calibrated for use at relatively moderate temperatures, this method of measurement is a very satisfactory one. One of its most interesting applications is in the estimation of the temperature of the rubbing surfaces of a bearing.

It is easily possible to provide a minute junction with very thin insulation and embed it in the babbitt metal in a bearing, so that it is but a fraction of an inch below the actual surface of the babbitt, and by this means the temperature of the actual rubbing surface of bearings, at predetermined points in the journal, has been measured very satisfactorily. By similar means the temperature of any part of the apparatus may be obtained if the parts are still. Measurements may also be made with the apparatus in operation. In the case of rotating parts, however, it is necessary to provide slip rings of such materials as to avoid disturbing or parasitic electromotive forces. Artificially prolonging the couple in this way permits the control of the cold end temperature and the taking of readings during operation.

For the moderate temperatures encountered in electrical apparatus it is not necessary to go to the expensive rarer metals which are required for similar thermocouples used for pyrometers at high temperatures. Copper with constantan makes a satisfactory couple, yielding a readable electromotive force even with small differences in temperature. In no case, however, with the ordinary differences in temperature to be expected, will any of the thermocouples commonly used yield sufficient electromotive force to permit the use of ordinary switchboard instruments for reading, but instead the measurements must be made by instruments equivalent to galvanometers.

Thermocouples may be calibrated to give accurate indications of temperature, and when used with directly indicating instruments, results within a degree or two may be expected. For more refined work potentiometer arrangements may be applied and by this means any desired degree of precision can be reached.

Of the methods of measurement mentioned, ordinary thermometry is the simplest, quickest, and most easily performed, but it is the least accurate. It is usually applicable only to surface conditions, and can seldom be used to determine anything concerning the internal temperatures of the apparatus. The choice between the electrical resistance method and the thermoelectric method is largely determined by conditions, and ease of application in the individual case. Both methods, compared with ordinary thermometry, are somewhat more difficult of application and require more delicate apparatus than the ordinary meters used in commercial testing. In rapidity of indication the thermocouple will rank first, if properly applied, though the indications of the electrical resistance thermometer will be nearly as rapid, provided the resistance coil is of right proportions and applied in intimate thermal contact with the parts whose temperature is to be measured.

A paper presented at the Midwinter Convention of the American Institute of Electrical Engineers, New York, February 26, 1913.

METHODS OF DETERMINING TEMPERATURE OF TRANSFORMERS AND OF COOLING MEDIUM

BY S. E. JOHANNESEN AND G. W. WADE

Of all measurements applied to transformers, none are so difficult to duplicate and to obtain accurate and consistent results from, as tests for determining temperature rise. There are a great many different conditions to be controlled or corrected for in case of variation. The difficulties are principally due to the four following causes:

1. Variations in load, including voltage, current and frequency.
2. Variations in the cooling medium with regard to its condition, its temperature, and the quantity supplied in case of artificial cooling.
3. Inaccuracies in the measurement of the effective temperature of the cooling medium.
4. Inaccuracies in the measurement of hot temperatures.

The first and second of the above causes are not within the province of this paper, but the other two will be taken up in the above order after a short discussion of the measurement of temperature.

THERMOMETERS

Temperatures are usually measured by means of mercury thermometers, although other types are sometimes used for special purposes.

Mercury thermometers are made for various ranges of temperature and for various degrees of accuracy. Other things being equal, the shorter the range the higher the degree of accuracy. It is desirable, therefore, to select a thermometer having a range just sufficient to include all the temperatures to be measured. For air temperatures a range from about 0 to 50 deg. cent.

is most suitable, while a wider range, say from 0 to 100 deg. cent., is usually necessary for determining hot temperatures. For such ranges mercury thermometers can be made for an accuracy of about 0.01 deg. plus or minus, but these are too expensive and too fragile for ordinary use. The cheapest form has an accuracy of about 1 deg. plus or minus. Mercury thermometers are best suited for the determination of temperatures of air, tanks, and other iron parts. They should not be used in strong magnetic fields on account of the heating due to eddy currents induced in the mercury. It is preferable not to use them inside the transformer case unless the bulb is very carefully protected, since short circuits may be caused by the mercury falling on the coils or leads in case the bulb is broken.

Spirit thermometers are most suitable for obtaining temperatures of coils or oil, as they are not affected by magnetic fields and as no bad results will be caused by their breakage. They are not as accurate or as reliable as mercury thermometers and it is necessary to calibrate them for the condition under which they are to be used in order to obtain correct readings. For instance, one calibrated with a 5-cm. immersion of the bulb in oil would not give accurate results when used totally immersed. With proper calibration, however, they have an accuracy of about 1 deg. plus or minus in the 0 to 100 deg. range.

Capillary tube thermometers are useful for determining temperatures at a distance below the oil surface. They have about the same degree of accuracy as spirit thermometers but are much more expensive. Care should be exercised to keep them away from the coils and leads.

Resistance thermometers are very useful for determining temperatures at points inaccessible to the other types. They can be made with a higher degree of accuracy than the capillary tube type but are somewhat more expensive.

Great care is needed in placing thermometers so as to determine the temperature of the flat surfaces, such as tanks, cores, and coils. It is best to cover the bulb entirely with some material such as putty, so as to give a good surface contact and to keep the bulb from the influence of air currents on the side away from the surface. Cotton waste may be used for this purpose, but it is not quite as desirable as putty or some substance of the same nature, because it does not entirely prevent air currents from coming in contact with the bulb. For measuring temperatures between coils of air-blast transformers, a good method is to use cork. A hole large enough to allow the thermometer bulb to be inserted should be drilled into the cork, after which it should be cut away on one side so as to leave a part of the surface of the bulb exposed, this exposed surface being set against the coil. The cork should be cut wedge-shaped with a pointed bottom, so that it will not impede the flow of the cooling medium.

Effective Temperature of Cooling Medium

As the measurement of this temperature depends to a great extent upon the method of cooling, we will take up the various methods in their order, beginning with the oil-immersed self-cooled type.

If the room temperature is kept absolutely constant throughout a heat run, its actual temperature is the effective reference base. Mercury thermometers placed in the air or in small oil cups are entirely satisfactory for the determination of this temperature. The precautions mentioned in the Standardization Rules, section 263, should be observed:

> "The thermometers indicating the room temperature should be protected from thermal radiation emitted by heated bodies, or from drafts of air, or from temporary fluctuations of temperature. Several room thermometers should be used."

As the above rule does not state that a constant room temperature is required, it may be inferred that a varying room temperature is permissible, and that the effective room temperature is the air temperature at the end of the run. Such an interpretation as this may not cause errors in the determination of rise on air-cooled apparatus but it does cause very serious errors in tests of oil-immersed transformers. The realization of this source of error has given rise to various methods of determining the effective room temperature, such as:

1. Average room temperature over several hours preceding the end of the run.
2. Final room temperature measured by thermometer in a small oil bath.
3. Average room temperature by means of oil bath for several hours preceding the end of the run.

The oil bath referred to is a small vessel filled with oil. The most commonly used is probably that standardized by the United States Navy, which consists of a steel cylinder 7.6 cm. long and 5.1 cm. in diameter, into which is drilled a hole 1.27 cm. in diameter and 3.8 cm. deep.

'. None of the above methods give the effective room temperature (except accidentally) because they do not take into account the fact that different transformers are affected to different degrees by varying air temperatures. Oil-immersed transformers are extremely slow in following changes in the temperature of the cooling medium. Their slowness varies directly with the size, which is a measure of thermal capacity or power of retaining heat, and inversely as their radiating ability, which is a measure of the power of dissipating heat. The thermal capacity of oil is very high, its specific heat being in the neighborhood of 0.40. The slowness of a transformer in following changes of air temperature is also noticed when it is heating up under load. The following table shows the approximate time required by several representative self-cooled, oil-immersed transformers to reach various percentages of their ultimate temperature rise when heated up on normal load.

Size of transformer	Per cent of final temperature rise		
	60	90	99
2 kv-a.	2 hr.	5 hr.	10 hr.
15 "	4 "	10 "	20 "
40 "	5 "	12 "	24 "
500 "	6 "	13 "	26 "

It will be easily seen from the above that the only accurate method of correcting for varying room temperatures is by means of some device which produces a lag behind the air temperatures corresponding in degree and in phase with the lag of the heated transformer. Since each design has its own rate of lag it is evident that a different device is needed in each case. The most satisfactory device which has been found is another transformer of the same design, preferably a duplicate of that under test. This auxiliary or idle unit should, of course, be without load and subjected to the same cooling medium as the heated transformer. Since its mass and working parts are the same as the one under test, it must of necessity be affected by varying room temperatures in exactly the same way as the heated transformer. The only difference between the two is that due to the load, so that the difference of temperature between them must be due entirely to the load.

In oil-immersed transformers, the effective temperature of the cooling medium is the average temperature of the idle unit.

This should be determined by taking the average of the top and bottom oil. If the bottom oil temperature cannot be measured directly, it may be approximated very closely by first measuring the temperature of the top oil, the top tank just under the oil level, and the bottom tank, and then calculating the average oil as follows:

If I_t is the top oil, O_t the top tank, and O_b the bottom tank temperature, the bottom oil is

$$\frac{I_t O_b}{O_t}$$

and the average oil temperature is

$$\frac{1}{2}\left(\frac{I_t O_b}{O_t} + I_t\right)$$

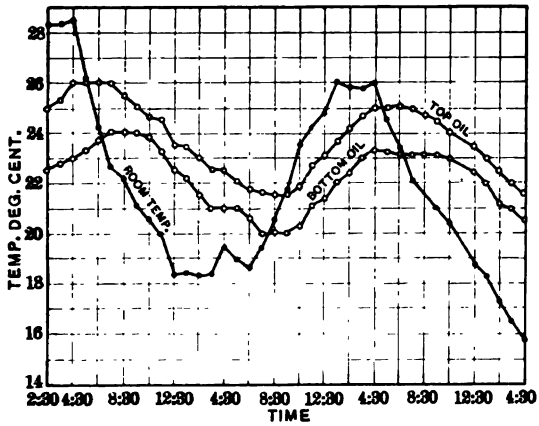


FIG. 1—IDLE TRANSFORMER RUN
185 kv-a., self-cooled, oil-immersed.

Another way in which the average temperature of the idle unit may be determined is to measure the resistance and observe the temperature before the oil is put in. Then whenever the average temperature is desired, the resistance should be measured and the temperature calculated. It is considered best, however, to use this calculated temperature merely as a check upon the temperature observed by thermometers.

Fig. 1 and Table I show the hourly readings of temperature of top and bottom oil of a 185-kv-a. idle transformer compared with room temperatures over a period of 24 hours during which the air had a total variation of approximately 10 deg. Attention is called to the corresponding cycles of the temperatures, especially to the fact that the oil cycle lags several hours behind the air in time phase and has much smaller maximum and minimum peaks than the air temperature. The bottom and top oil

TABLE I

IDLE UNIT RUN, 185-KV-A., SELF-COOLED, OIL-IMMERSED TRANSFORMER

Hour	Room temperature	Oil temperatures	
		Top	Bottom
	deg. cent.	deg. cent.	deg. cent.
2:30 p.m.	28.2	24.9	22.5
3:30	28.3	25.3	22.8
4:30	28.4	26.0	23.0
5:30	26.2	26.0	23.3
6:30	24.2	26.0	23.7
7:30	22.6	26.0	24.0
8:30	22.2	25.5	24.0
9:30	21.1	25.0	24.0
10:30	20.5	24.6	23.8
11:30	19.9	24.5	23.2
12:30 a.m.	18.3	23.5	22.5
1:30	18.4	23.4	22.2
2:30	18.2	23.0	21.5
3:30	18.3	22.5	21.0
4:30	19.4	22.5	21.0
5:30	18.9	22.0	21.0
6:30	18.6	21.8	20.6
7:30	19.4	21.6	20.0
8:30	20.5	21.5	20.0
9:30	21.6	21.5	20.0
10:30	23.5	21.8	20.3
11:30	24.3	22.7	21.1
12:30 p.m.	24.9	23.1	21.3
1:30	26.1	23.7	22.0
2:30	25.8	24.2	22.4
3:30	25.9	24.7	23.0
4:30	26.0	25.0	23.3
5:30	24.6	25.0	23.3
6:30	23.5	25.1	23.2
7:30	22.1	25.0	23.2
8:30	20.6	24.8	23.2
9:30	21.1	24.5	23.1
10:30	20.5	24.0	23.0
11:30	—	—	—
12:30	19.7	23.5	22.5

follow the same curve, the difference in temperature between them being nearly constant. The bottom oil seems to have a slight tendency to start downward later than the top oil when the room temperature is decreasing and to start upward earlier than the top oil when the room is increasing. This is probably due to the fact that the room reaches the temperature of the bottom oil later when decreasing and earlier when increasing. The following tabulation of average values for a period of 24 hours is rather interesting.

Average air temperature	22.45 deg.
" top oil "	23.65 deg.
" bottom oil temperature	22.05 deg.
" top and bottom oil temperature	22.85 deg.

The average of the top and bottom oil is 0.4 deg. higher than the average air, but since this is within the range of accuracy of the thermometers used, no attempt will be made to explain it.

The conclusion that the idle unit and the heated transformer are affected in exactly the same way by varying air temperatures is based on the supposition that the coefficients for emission and absorption of heat are exactly the same and that the heat given out is directly proportional to the temperature rise. This is so nearly true for tank surfaces within the range of transformer operation that the errors introduced due to such an assumption are negligible.

In the case of water-cooled transformers, the problem is complicated by the fact that we have to consider two cooling mediums instead of one. The water remains practically constant over any period of time usually needed for a heat run, so that no corrections for variations in the temperature of the water during the run need be considered. Some method of correcting for variations in air temperature during the run is desirable, but these variations have only a slight effect on the temperature of the hot unit because the greater part of the heat is carried away by the water. Consequently no serious error in the temperature rise will be introduced by considering the ingoing water as the true reference base. There is, however, a small error, and it is possible that the idle unit may serve to eliminate it, or at least to give more nearly correct rises than the ingoing water temperature. We have not yet been able to obtain sufficient data on this subject to prove that the idle unit gives more nearly the effective temperature of the cooling medium than the ingoing water. In the absence of such proof, it is best to continue the past practise of using the ingoing water as the base. The errors caused by this practise will depend on the relative quantities of heat dissipated by the two cooling mediums. If all of it were taken by the water, no attention need be paid to the air temperature. However, the air does take from 10 to 30 per cent ordinarily, so that the effective base is somewhere between the temperature of the water and that of the air.

Air-blast and air-cooled transformers follow changes in air temperature much more closely than do the oil-immersed types because their thermal capacities are considerably smaller. The ingoing air temperature may be taken as the effective base, precautions being taken to guard against sudden fluctuations. A small oil cup will serve to prevent errors due to small and sudden changes in the air and will also have a slight lag behind the air, corresponding approximately to that of the transformer.

Methods of Measuring Hot Temperatures

Hot temperatures should always be determined by resistance as well as by thermometers.

Thermometers should be placed so as to indicate the temperature of the hottest part. In oil-immersed transformers this is usually the top oil directly over the coils. In water-cooled units care must be taken to keep the thermometer away from the cooling coils, as the oil is much cooler in that vicinity, the coldest oil being between the cooling coils and the tank. In air-cooled transformers the hottest point is usually near the top of the coils and the thermometers should be placed at a number of different points on the coils so as to find this maximum.

Hot resistances may be measured by any one of the four following methods:

1. Voltmeter and ammeter.
2. Potentiometer.
3. Wheatstone bridge.
4. Kelvin or Thompson double bridge.

For such resistances a method giving quick readings is essential, as considerable error may be introduced by the cooling of windings after the load is taken off and before the resistances are measured. The quickest readings can be obtained by the voltmeter and ammeter method, for which reason its use is recommended, except for very high or very low resistances. For high resistances this method requires a very high voltage in order to produce an appreciable current, and unless the voltmeter has a high resistance itself, a large correction factor is required on account of the large part of the current taken by the voltmeter. The potentiometer or the Wheatstone bridge, although less rapid than the voltmeter and ammeter method, are better for the high resistances. For low resistances the voltmeter and ammeter method is not desirable, because of the very large current required to produce a voltage which can be accurately measured. The Wheatstone bridge method is not accurate for such use because its resistances include leads and contacts which are a large proportion of the total resistance in such cases. The potentiometer avoids these errors and it is therefore recommended for measurement of low resistances.

In using the voltmeter and ammeter method, care must be taken to avoid including the resistance of leads and contacts. This can be done by carrying separate leads to the voltmeter and to the ammeter and attaching them at different points on the transformer leads. If the voltmeter takes an appreciable part of the total current supplied, this part should be subtracted from the measured current before the resistance is calculated. In applying the direct current an induced counter e.m.f. lowers the volt-

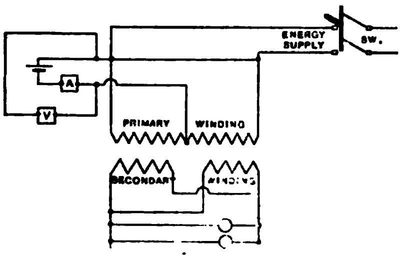


FIG. 2—APPLYING DIRECT CURRENT SO AS TO NEUTRALIZE INDUCTIVE EFFECT

meter reading for a time. This induced e.m.f. gradually disappears, but is objectionable because it delays the reading of the voltmeter. It can be caused to decrease more rapidly by raising the current at first to a value about 10 or 15 per cent in excess of that desired and then bringing it down slowly. It may be entirely eliminated in a winding provided with a middle tap or divided into two equal parts, by applying the direct current in opposite directions through the two halves as shown in Fig. 2.

In using the Wheatstone bridge the resistance of the leads from the bridge to the transformer must be subtracted from the observed value.

In determining rise by resistance by means of the voltmeter and ammeter method, the idle unit, if it is a duplicate of the heated unit, is of considerable value when used as follows: The resistance of the two units should be compared before a heat run, care being taken to see that they are at the same temperature. The corresponding windings should be connected in series and with current forced through them the drop across each should be measured. The hot resistance of the unit under test should be compared with that of the idle unit, measurement being made in the same way as before. The rise by resistance should be calculated directly from these comparative voltmeter readings. If there is a difference between the initial resistances of the two units, a correction should be made before calculating the rise by multiplying the final resistance of the idle unit by the ratio of the initial resistances of the heated unit to the idle unit, or to put in the shape of a formula, we have

$$\text{Temp. rise} = \left(\frac{V_3 \times V_2}{V_4 \times V_1} - 1\right) (233.8 + T_1)$$

where

V_1 = initial resistance voltage of loaded transformer
V_2 = " " " " idle "
V_3 = final " " " loaded "
V_4 = " " " " idle "
T_1 = " temperature " " "

and 233.8 = the inferred absolute temperature of resistance.

This method eliminates practically all errors due to any changes in the instruments between the measurement of the cold and the hot resistance. Errors in ammeter readings can have no effect, since the same current is sent through both transformers. It is not necessary to know the actual temperature corresponding to the cold resistance because all that is needed is the comparative value when the two are at the same temperature. The final temperature of the idle unit need be known only approximately, as its temperature is the effective base and the only error caused by a mistake in reading this temperature is that due to the use of a slightly incorrect temperature coefficient.

The amount of current to be used in measuring resistance should be chosen so as not to cause any appreciable heating due to I^2R when the cold resistances are measured. This means that the direct current should not ordinarily exceed 15 to 20 per cent of the rated current of the winding.

When resistances are very low, 0.001 ohms or less, it is usually difficult to obtain accurate measurements. Consequently, it is preferable to depend upon the temperature shown by thermometer or by resistance of the other winding.

Exploring Coils

The rise by resistance shows only the average temperature of the winding. It is sometimes considered desirable to ascertain temperatures at points where it is suspected that the values will be considerably above the average. This can be done by means of small exploring coils. It is also desirable in some cases to determine the temperature of the windings without cutting off the load. This may be accomplished by means of a small exploring coil which measures a local temperature, or by means of a distributed coil which extends through a large part or all of the transformer winding.

The first form, which may be called a local exploring coil, is usually wound on some insulating material, and is thoroughly insulated from the main winding. The other form, or distributed coil, may be wound turn for turn with the transformer winding. It should be grounded to the transformer winding at one point and carefully insulated elsewhere. Either form of coil should be wound non-inductively so that the alternating current will not affect the readings or cause high induced voltages.

In general, the differences in local temperatures are not large enough to warrant the use of local exploring coils except in laboratory investigations. It is desirable to know the temperature of coils when transformers are in actual operation, but the advantage gained is not considered large enough to compensate for the added element of danger, due to the presence of exploring coils in the windings.

Cooling of Windings

Mention has been made of the cooling of the windings after load is removed. As a matter of fact, the whole transformer cools, but the rate is very slow in the case of oil-immersed types. The thermal capacity of the coils, however, is much less than that of the other parts and as a result, the windings cool to the average temperature much more quickly than the entire transformer cools to the air or cooling medium. For instance, the thermal capacity of the copper of a certain 185-kv-a. transformer is about 98,000 joules, while that of the oil is about 2,200,000 joules. The rate of this cooling of windings may be calculated as follows:

The windings are at a constant temperature rise above oil before the load is taken off, so that the rate at which they are giving out heat is the same as the rate at which energy is being put into them, or in other words, it is the copper loss. This rate continues for a short period after the load is taken off. The thermal capacity of a pound of copper is about 177 joules. Then, if the coils are giving out heat at the rate of one watt per pound (0.45 kg.), one joule will be lost during the first second after shut-down. This will cool the coil 1/177th of a degree in the first second, and 60 times this amount, 0.342 deg., in the first minute. At three watts per pound (0.45 kg.) the cooling in the first minute would be approximately 1.04 deg. These values will be modified to some extent by the fact that the insulation of the coil has a certain thermal capacity, thus increasing the total value and decreasing the rate of cooling. This rate decreases as the coil comes nearer and nearer to the oil temperature, reaching the zero value when the rise above oil becomes zero. The curve of

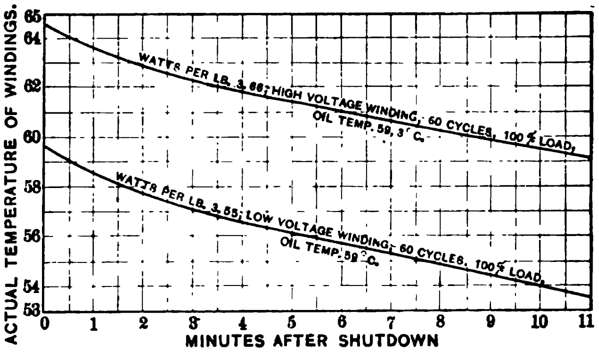


FIG. 3—COOLING CURVES OF WINDINGS
100-kv-a., self-cooled, oil-immersed transformer.

temperature against time is logarithmic in shape, the rate of cooling at any point being proportional to the rise above the base temperature. The total time of cooling varies with the design, but in ordinary cases it is between 30 and 60 minutes. We are interested mostly in the first minute or two, and during this period there is very little change in the rate. If the watts loss per pound of copper is known, an approximate correction can be made by calculating the initial rate, noting the time at which the resistance reading is obtained and multiplying this time by the rate, finally adding this to the measured temperature rise. This correction is, however, only approximate and should not be applied for periods of more than a few minutes. Another method which gives more accurate results is to take readings of resistance over a period of six or eight minutes so as to determine the shape of the cooling curve, then prolong the curve so as to show the initial resistance. This method has the disadvantage that it allows the transformer to cool considerably and thus prolongs the heat run in case any further measurements are desired, such as the rise by resistance of the other winding.

Representative cooling curves of windings of self-cooled oil-immersed transformers are shown in Figs. 3 and 4.

The rate of cooling of windings in air-blast transformers is lessened considerably if the air is shut off simultaneously with the load. In fact it is quite often found that the thermometers on the coils indicate an increase in temperature for the first few minutes. This is probably because the shutting off of the air supply leads to an equalization of temperature throughout the coils, thus reducing the higher and increasing the lower temperatures.

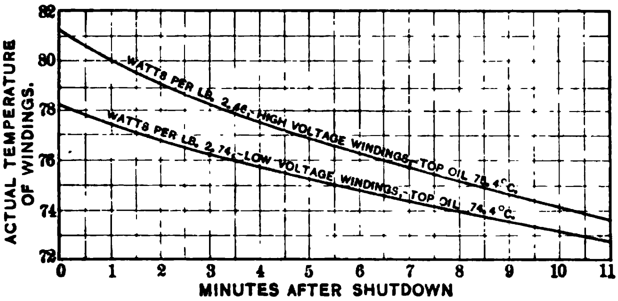


FIG. 4—COOLING CURVES OF WINDINGS
100-kv-a., self-cooled, oil-immersed transformer.

MEASURING RESISTANCE WITHOUT CUTTING OFF LOAD

Resistance of windings having a middle tap or divided in two equal parts may be measured while the load current is passing through them, by the method shown in Fig. 5. This figure shows the ordinary connections for a bucking run on two transformers, each of which is provided with a middle tap in each winding. The excitation or core loss voltage is applied to the two high-voltage windings connected in multiple, while the copper loss is supplied in series with the two low-voltage windings. When it is desired to measure the resistance of the high-voltage windings in this case the direct current is applied to the middle points of the two windings, between which there is no difference of potential due to the alternating current. The direct current flows in opposite directions in the two halves, thus neutralizing its own inductive effect. The measured resistance is that of the two windings in multiple. It will be noted that this measurement can be made without disturbing the heat run connections or losses in any way.

It is usually considered desirable, however, on account of safety, to cut off the excitation or core loss supply while the direct-current instruments are being read.

The middle points of the low-voltage windings shown in the figure are not at the same potential, because of the unbalancing due to the copper loss supply voltage which is in series with them. This makes it necessary to provide an auxiliary or equalizing coil so as to provide an artificial middle point. This equalizing coil may be one of the windings of a similar transformer. The measured resistance is that of the two halves of the equalizing coil connected in multiple with the two halves of the loaded trans-

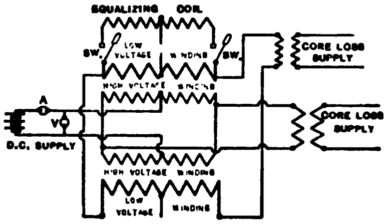


FIG. 5—METHOD OF MEASURING RESISTANCE WITHOUT CUTTING OFF LOAD

former. The actual value of the resistance of the transformer may be calculated after the resistance of the equalizing coil alone has been measured.

This method is too complicated for general use, but is desirable where it is not convenient to cut off the load for resistance measurements and where a large error would be caused by cooling of windings. It has several advantages over the old method, among which the following may be mentioned:

1. It makes it possible to measure resistance as often as desired without lengthening the heat run.

2. It eliminates drop in temperature due to cutting off load while measuring resistance.

HEAT RUN WITH IDLE UNIT

Fig. 6 and Table II give a record of a heat run on a 500-kv-a. oil-immersed self-cooled transformer; temperatures of top oil of idle unit, top oil of heated unit and of surrounding air being shown each hour during a period of about four days. The comparative rises of the heated unit above air and above the top oil of the idle unit have also been plotted. Particular attention is called to the wide variation in room temperature as compared with the top oil temperature of the two transformers. Each of the three curves shows cycles corresponding to 24-hour periods. The oil temperature cycles lag several hours behind the air, those of the idle unit being practically in phase with those of the hot unit. The cycles of rise above air are the reverse of those of air temperature and have about the same range of temperature. The rise above the top oil of idle unit also shows, faintly, cycles in phase with those of the rise above air, but the variations in the

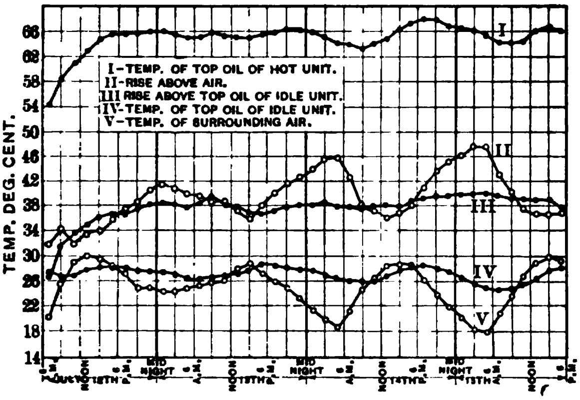


FIG. 6—HEAT RUN ON TRANSFORMERS
500-kv-a., oil-immersed, self-cooled.

value of this rise are considerably less than those of the rise above air. The fact that these cycles are in phase with the rise above room indicates that the idle unit does not entirely compensate for variations in air temperatures, or, in other words, that it is not affected to as great an extent as the heated unit by such variations. This may be true, but even if it is, the test shows that the idle unit gives far more consistent results than any other method so far suggested. The approximately correct temperature rise may be determined by calculating the rise above room each hour for a complete cycle of 24 hours after ultimate rise is reached, and averaging the result. This method, although too expensive for general use, is useful in some cases and gives some interesting data in this case.

TABLE II
NORMAL LOAD HEAT RUN ON 500-KV-A. SELF-COOLED OIL IMMERSED TRANSFORMER.

Day	Hour	Room temperature	Top oil temperatures: Loaded transformer	Top oil temperatures: Permanently screened idle transformer	Top oil temperatures: Alternately screened idle transformer
		deg. cent.	deg. cent.	deg. cent.	deg. cent.
July 12	7 a.m.	20.5	54.5	27.5	27 Screened
"	8	23.0	57.5	27.0	26.6
"	9	25.7	58.5	27.0	26.6
"	10	27.5	59.5	27.1	26.9
"	11	29.0	61.0	27.2	27
"	12	29.5	63.0	27.8	27.6
"	1 p.m.	30.0	63.2	28.1	28.0
"	2	30.0	64.0	28.2	28.0
"	3	29.5	64.8	28.6	28.5
"	4	29.0	65.0	28.6	28.5
"	5	28.5	65.7	28.7	28.6
"	6	28.0	65.6	28.5	28.7
"	7	27.5	65.7	28.3	28.8
"	8	26.7	65.7	28.1	28.5
"	9	25.3	65.7	27.9	28.4
"	10	25.0	65.7	27.7	28.2
"	11	25.0	66.0	27.6	28.1
"	12	25.0	66.3	27.5	27.9
July 13	1 a.m.	24.5	66.1	27.3	27.7
"	2	24.5	65.7	27.1	27.6
"	3	23.5	65.5	27.0	27.4
"	4	24.5	64.8	26.8	27.3
"	5	25.0	65.0	26.7	27.3
"	6	25.5	65.2	26.5	27.0
"	7	25.5	65.3	26.6	27.0 Screen removed
"	8	26.0	65.8	26.6	27.3
"	9	26.8	65.9	26.5	28.0
"	10	26.5	66.0	26.6	28.5
"	11	26.5	65.5	27.0	28.9
"	12	27.5	64.9	27.0	29.1
"	1 p.m.	28.0	65.2	27.5	29.8
"	2	28.5	65.2	28.0	29.9
"	3	29.0	65.3	28.4	30.5
"	4	28.5	65.7	28.5	30.8
"	5	27.5	65.7	28.9	31.0
"	6	27.0	66.0	28.7	30.9
"	7	26.0	66.2	28.7	30.9
"	8	25.5	66.4	28.7	30.7
"	9	25.0	66.6	28.4	30.6
"	10	24.0	66.7	29.6	30.3
"	11	23.7	66.4	28.1	30.0
"	12	23	66.3	27.8	30.0
July 14	1 a.m.	21.8	65.8	27.5	29.5
"	2	21.0	65.5	27.2	29.2
"	3	20.0	65.5	27.0	28.8
"	4	19.5	65.0	26.5	28.5
"	5	19.0	64.4	26.4	28.0
"	6	20.0	64.3	26.2	27.8
"	7	21.5	64.1	26.0	27.8
"	8	23.0	63.2	25.9	27.7 Screened

TABLE II—*Continued.*
NORMAL LOAD HEAT RUN ON 500-KV-A. SELF-COOLED OIL-IMMERSED TRANSFORMER

Day	Hour	Room temperature	Top oil temperatures: Loaded transformer	Top oil temperatures: Permanently screened idle transformer	Top oil temperatures: Alternately screened idle transformer
		deg. cent.	deg. cent.	deg. cent.	deg. cent.
July 14	9 a.m.	25.0	63.5	25.9	27.5
"	10	25.5	64.0	26.0	27.6
"	11	27.0	64.5	26.3	27.8
"	12	28.0	64.4	26.6	28.1
"	1 p.m.	29.0	65.3	27.0	28.4
"	2	29.0	65.8	27.2	28.8
"	3	29.0	66.2	27.8	29.3
"	4	29.0	66.9	28.0	29.4
"	5	29.0	67.4	28.8	30.0
"	6	28.0	67.7	28.5	29.7
"	7	26.5	67.9	28.5	29.7
"	8	26.0	68.0	28.5	29.5
"	9	24.0	67.8	28.0	29.0
"	10	22.5	67.6	27.8	28.7
"	11	22.0	67.4	27.5	28.3
"	12	21.0	67.0	27.0	27.8
July 15	1 a.m.	20.5	66.7	26.7	27.5
"	2	19.0	67.0	26.0	27.0
"	3	18.5	66.2	25.7	26.8
	4	18.5	66.0	25.5	26.2
"	5	18.0	65.5	25.0	26.0
"	6	20.0	64.8	24.7	25.5
"	7	21.4	64.5	24.8	25.4
"	8	23.2	64.5	24.8	25.8 Screen removed
"	9	24.2	64.5	24.8	26.0
"	10	25.7	64.5	25.0	26.4
"	11	27.2	64.7	25.4	27.0
"	12	27.7	65.0	26.1	27.8
"	1 p.m.	29.1	66.0	26.8	28.3
"	2	29.2	66.3	27.0	29.0
"	3	30.0	66.8	27.7	29.4
"	4	29.5	66.9	28.1	29.8
"	5	29.3	66.2	28.3	30.2

The following table shows the average value of the five curves in Fig. 6 for two periods of 24 hours each:

Average temperature for 24-hour periods:

1st day. From 13th at 2:00 P.M. to 14th at 1:00 P.M., incl.
2nd " " 14th " 2:00 " " 15th " 1:00 " "

	1st day	2nd day
Average air temperature	24.45 deg. cent.	24.10 deg. cent.
" top oil of idle unit	27.35 " "	26.60 " "
" " " hot unit	65.25 " "	66.15 " "
" rise above air	40.75 " "	42.0 " "
" " " idle unit	37.85 " "	39.4 " "

	1st day	2nd day
Max. variation of air from average.........	+4.45 to —5.45	+4.9 to —6.1
" " " idle from average........	+1.55 to —1.55	+1.9 to —1.9
" " " hot unit from average....	+1.45 to —2.05	+1.85 to —1.75
" " " rise above idle unit from average...............	+0.65 to —0.95	+1.0 to —1.0
Rise above air from average...............	+3.80 to —4.45	+5.7 to —5.2

Assuming that the average rise above air is the correct value, which is probably true, it is evident that the rise above top oil of idle unit is in this case between two and three degrees too low. This shows the importance of using the average instead of the top oil as the base temperature. Later tests, one of which is plotted in Fig. 1, show that the average of top and bottom oil for a period of 24 hours is the same as the average air. These tests also show that the top and bottom oil follow the same curve. So the rise above average oil, if it had been measured and plotted in Fig. 6, would undoubtedly follow the curve of rise above top oil, except that all values would be from two to three degrees higher.

Method of Using Idle Unit

The idle unit should be subjected to exactly the same cooling conditions as the heated unit. Care should be taken to place it far enough away so as to prevent its temperature being raised due to the heat radiated from the hot unit. This distance depends upon the size of the unit and the amount of heat radiated. In some cases it is desirable to place a heavy screen between the two units.

Fig. 7, plotted from Table II, shows the effect of screening upon an idle unit placed about 90 cm. away from the heated 500-kv-a. transformer. The hot and the idle units were duplicates, each being placed in a corrugated sheet steel tank of oval shape having floor space of 145 by 89 cm. and a height of 245 cm. They were placed with the long sides next each other. The temperature rise of the oil in the heated unit was in the neighborhood of 40 deg. above the idle unit. It will be noted that the idle unit when unscreened shows a temperature rise of top oil approximately 1.5 deg. higher than when screened. The effect of the hot unit upon the idle unit can be calculated roughly as follows:

The percentage of heat radiated by the hot unit in a corrugated tank is about 25 per cent of the total, the remainder being carried away by convection. This radiated heat is given off in straight lines in every direction. If we should surround the hot unit by means of a screen 90 cm. distant at all points, it would intercept all of the radiant heat. The idle unit does intercept a certain percentage of this heat, which percentage can be calculated by finding the ratio of the area of the idle unit to that of a complete envelope around the hot unit. In the case of the 500-kv-a. unit shown in Fig. 7, this ratio is about 0.15. This means that the idle unit receives 15 per cent of 25 per cent of the total heat dissipated by the hot unit, or 3.75 per cent. The temperature rise of the idle unit will be 3.75 per cent of that of the hot unit, which amounts to about 1.4 deg. in this case.

This test is also of interest with reference to the spacing of loaded transformers under test or in operation. If two loaded units, instead of one idle and one loaded, had been used, it is evident that since the two would be at the same temperature, neither could give heat to the other, and that the effective radiation from each would be decreased by the amount of heat given to the unscreened idle unit in the test. This would result

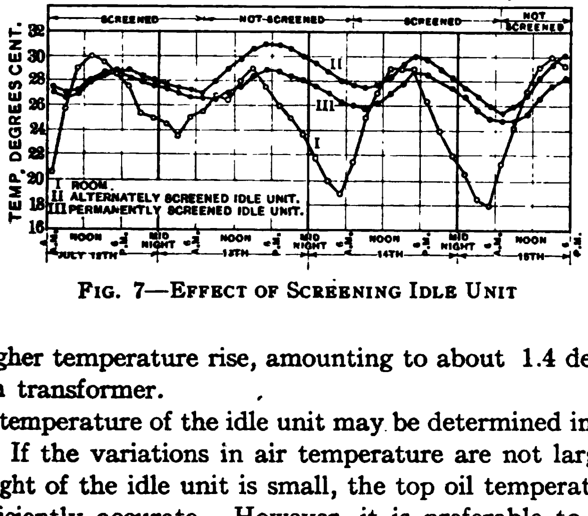


FIG. 7—EFFECT OF SCREENING IDLE UNIT

in a higher temperature rise, amounting to about 1.4 deg. cent. in each transformer.

The temperature of the idle unit may be determined in several ways. If the variations in air temperature are not large, or if the height of the idle unit is small, the top oil temperature will be sufficiently accurate. However, it is preferable to use the average of top and bottom oil temperature, the bottom oil temperature being measured directly when possible. If this is not possible the temperature may be calculated by means of the following formula:

$$\text{Average temperature of idle unit} = \frac{1}{2}\left(\frac{I_t O_b}{O_t} + I_t\right)$$

when

I_t is the top oil temperature
O_t is the top tank temperature at oil level
O_b is the bottom tank temperature.

The cold resistance of the idle unit should be carefully compared with that of the unit to be heated up, especial care being taken to have the two at the same temperature or to know the exact temperature of each. This comparison may be made before the oil is placed in the transformer, and it is best to make the comparison at this time if the oil temperature differs greatly from that of the room, because considerable time will be lost in determining the average temperature of the windings in each unit after they are filled with oil. This comparison of resistances should preferably be made by forcing current through corresponding windings connected in series and reading the drop across each. Several readings should be taken in order to make sure that the exact ratio of the cold resistances has been determined.

The heat run may be started as soon as desired after the above ratio has been measured. It is best not to take readings of resistance until the end of the run, since the load must be taken off, thus causing a drop in temperature. The heat run should be continued until the rise of the top oil of the heated unit above the average idle unit does not change more than one degree in three hours. It may then be considered that the whole transformer is operating at a constant temperature rise. The hot resistance may then be measured and the heat run discontinued.

The hot resistances are to be measured in the same way as the cold, preferably with the same instruments and the same value of current. Since the resistance of the idle unit represents the true effective temperature of the cooling medium, the rise by resistance may be calculated by means of the formula

$$\text{Temp. rise} = \left(\frac{V_3 \times V_2}{V_4 \times V_1} - 1\right) (233.8 + T_1)$$

where

V_1 = initial resistance voltage of loaded transformer
V_2 = " " " " idle "
V_3 = final " " " loaded "
V_4 = " " " " idle "
T_1 = final temperature of idle transformer
and 233.8 = the inferred absolute temperature of resistance.

Although the voltmeter and ammeter method is the only one referred to in the above discussion, it is to be noted that the other methods can be used in the same manner with good results, especially where the galvanometer is so constructed as to give quick readings. A potentiometer may be substituted for the voltmeter when a low voltage is to be measured. The bridge methods are well adapted for this use, as the two resistance ratios can be measured directly by connecting the idle unit as the known and the loaded unit as the unknown resistance in the bridge circuit.

The advantages of the idle unit for determination of rise by resistance are not restricted to the oil-immersed self-cooled type of transformer. It is equally good for other types, regardless of the method of cooling. A slight modification of the method is required in the case of water-cooled oil-immersed transformers if it is desired to use the ingoing water temperature as the base, that is, the rise should be calculated as specified above and then the difference in temperature between the average oil of the idle unit and the ingoing water should be added to the calculated rise. As stated before, we know that the ingoing water temperature is not the effective temperature of the cooling medium unless the air and water are at the same temperature. However, we have not yet obtained sufficient data to justify recommending any other base. No modifications are required for air-blast or air-cooled transformers, as the temperature of the windings is the effective reference temperature.

Recommendations

It is recommended that additions to the Standardization Rules be made as follows:

(1) The effective room or base temperature for an oil-immersed transformer is the temperature of an idle unit similar to and prepared for test the same as that run in test, and its temperature is the average of the top and bottom oil, the bottom oil temperature being measured directly by a thermometer or calculated by multiplying the bottom tank temperature by the ratio of the top oil to the top tank temperature.

(2) When a duplicate transformer is available, the rise by resistance should be determined by first comparing the resistance of the two when at the same temperature, subjecting the idle transformer to the same cooling conditions as the loaded one until hot resistances are to be measured, then comparing the resistances of the two in the same manner as before, and calculating the rise from these final readings, correcting for any difference in the two initial resistances by multiplying the final resistance of the idle unit by the ratio of initial resistance of the loaded unit to the idle unit.

A paper presented at the Midwinter Convention of the American Institute of Electrical Engineers, New York, February 26, 1913.

METHODS OF DETERMINING TEMPERATURE OF TRANSFORMERS

BY W. M. MCCONAHEY AND C. FORTESCUE

I. Introduction

Temperature, in most electrical apparatus, plays an important part in the question of satisfactory operation. It affects not only the working efficiency but also the life of the apparatus, since the fibrous materials of which the greater part of the insulation is composed soon deteriorate under high temperatures. There is perhaps no other factor of such importance to satisfactory operation which is so difficult to measure accurately, and also is influenced to such an extent by external conditions, as the temperature of electrical apparatus under load.

This paper will give a brief discussion of conditions that affect the operating temperature of transformers and the relative merits of different methods of measuring temperatures. A description will be given of the various methods of loading, and a comparison will be made between temperatures obtained by a new method of loading a single transformer with those obtained by the standard method of loading which requires two transformers.

II. Conditions Affecting the Temperature of Transformers

In air-cooled transformers such as those designed to operate by natural ventilation without oil the temperature is directly dependent upon that of the air in the room. Thermometers, to measure correctly the air temperature in a room, should be placed well in the open. As a general rule, about the height to be comfortably read will be right. Small apparatus may be affected in temperature by the cool stream of air near the floor of the room and it will therefore be advisable in such cases to have thermometers placed about half the height of the apparatus from the floor, in which position they will give about the proper temperature of the air that comes in contact with the transformers.

Variation in room temperature has but little effect on the temperature rise of transformers which depend on direct cooling by air, but where the air serves to cool some medium of high specific heat which in turn serves as the true cooling medium, fluctuation in the air temperature will produce apparent rises in temperature that are far from correct. The best of care should be taken to have the temperature of the air in the room in which such apparatus is tested, as uniform as possible. It has been suggested that a similar transformer to that on test be placed in the room and the temperature rise of the transformer on test be based on the temperature of the idle transformer. This is an admirable way of overcoming the effect of fluctuation in air temperature and will give correct results, but it is open to the objection that an additional transformer is required.

Air-cooled and oil-insulated air-cooled transformers depend for cooling chiefly on convection currents of air and it is important, therefore, that they shall have a free circulation of air from every point and that there be enough head-room to carry off the heated air.

The coils and iron in an oil-insulated transformer depend upon convection of the oil for their cooling. Ventilating ducts must therefore be well distributed among the coils in such a way that as little as possible of the winding is blanketed. The larger the transformer the more important this becomes. The rate of flow of the oil will depend upon the area of the ducts, their length, the watts per unit area of exposed surface and the viscosity of the oil. An excellent test to determine the freedom of flow of the oil is to measure the difference of temperature between the bottom and top oil, as it enters and leaves a duct. In oil-insulated air-cooled transformers, however, a large difference between top and bottom oil does not necessarily mean constricted ducts. It may be due to poor design of the cooling case, whereby the air convection currents meet with resistance, and this in turn reacts on the flow of the oil, since the source of the head of both oil and air currents is the difference in temperature between the top and bottom of the coils themselves.

The different parts of the apparatus should be ventilated according to their individual requirements. Certain limitations due to structure prevent full advantage being taken of the characteristics of different materials in a transformer. Thus, iron loss decreases as the temperature of the iron is increased, but it is impossible to take advantage of this property of iron to any great extent. Moreover, the coils are so intimately associated with the magnetic circuit that it would ordinarily be a difficult matter to maintain them at widely differing temperatures. In oil transformers the temperature of the external surface of any part of the apparatus must necessarily be limited to a temperature less than that at which oil becomes injuriously affected, since there is always a film of oil at the surface which is held there by surface tension and moves only very sluggishly.

III. Tests for Efficiency of Cooling

The places in a transformer at which the temperature is likely to be a maximum depend largely upon the type and construction. In modern, section-wound, shell and core type transformers the ventilation should be so carefully designed that there can be no hot spots, so that the difference between the maximum and average temperature of the coils will practically be the same as the difference between the average temperature of the oil and that of the top or hot oil. Thus, if the maximum temperature of the oil in the ducts is 10 deg. higher than its average temperature, the maximum temperature of the coils will also be about 10 deg. higher than their average temperature.

In order to obtain a proper conception of the efficiency of the ventilation of a transformer it is necessary to know not only the temperature of the top oil but also that of the oil as it enters the ducts, because the average temperature of the transformer winding depends upon the average temperature of the oil and the maximum temperature of the winding depends upon the maximum temperature of the oil. There may be a large difference between the maximum and average values and it is quite possible to have a transformer which seemingly runs cool but the insulation of which will in time deteriorate on account of the high maximum temperature of the winding. In wire-wound coils great care has to be exercised in the design in order to avoid large differences in temperature between the inner and outer conductors. There is no very good method of measurement which will enable one to find the temperature gradient through the coil.

The following rule may be used to estimate its approximate value when the average temperature of the coils and oil are known:

θ_1 = Estimated value of temperature gradient through external insulation of coil.

θ_2 = Average temperature of coil above average oil.

θ = Temperature gradient through coil.

$\theta = 1\frac{1}{2}\,\theta_2 - \frac{1}{2}\,\theta_1$

θ_{max} = Maximum temperature rise of oil + θ.

This formula assumes, as a matter of course, that the coils are efficiently ventilated.

Thermometers in a transformer can be used only to indicate temperatures in the coils, not to measure them. Their usefulness consists in their ability to show up anything abnormal, such as the constriction of a duct. In other words, if a thermometer indicates a winding temperature differing very much from that of the hot oil, it is an indication that there is something wrong with the ventilation at that point. It has been proposed that thermocouples be used to determine the maximum temperature of transformer coils under load. It is impossible to tell the exact location of the hottest place in a coil and it would therefore be necessary, if such a method were adopted, to place a number of couples at all the places likely to have a high temperature. It is very difficult to see how such a scheme could be used without danger both to the operator and the apparatus. The only suitable place for the application of such methods is in the laboratory where proper precautions can be taken and where the work is done by skilled operators.

The average temperature can be obtained by the resistance method. The work of measuring the resistances of the windings of an oil transformer should be done as quickly as possible because the coils, on account of their small specific heat, quickly lose their temperature above the oil. Probably the best way for measuring high resistances is by means of the Wheatstone bridge, while for low resistances the Kelvin bridge may be used. Whatever method is used, the settings of the instruments should be adjusted for the approximate values so that the time required for the final adjustment may be reduced to a minimum.

The authors are of the opinion that for commercial testing the thermometer and resistance methods of measuring temperatures should be adhered to, as by their means and with the exercise of care in making the tests the efficiency of cooling of a transformer may easily be determined.

IV. Methods of Loading Transformers for Temperature Test

Transformers may be loaded directly on resistance racks or any other means for dissipating power; or two or more of them in combination may be artificially loaded. The first method of loading, on account of the expense involved, is limited to small transformers and special transformers where there is only one on order. The second method is economical in application, flexible in operation and is by far the most satisfactory.

The methods hitherto in use for artificially loading transform-

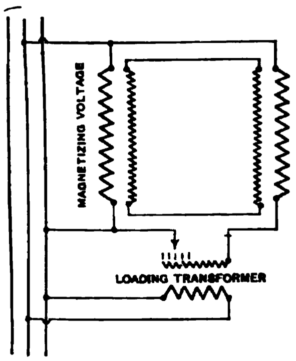


Fig. 1—Loading Scheme for Two Transformers

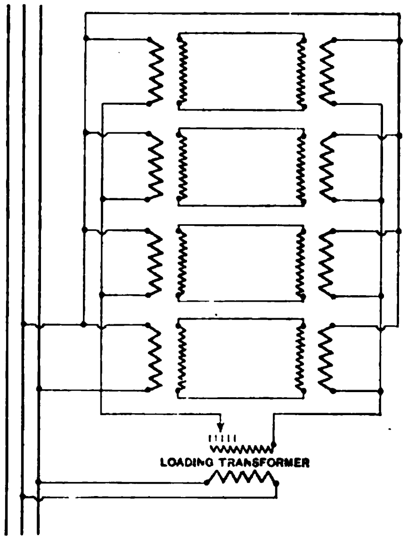


Fig. 2—Loading Scheme for More than Two Transformers

ers are applicable only when more than one transformer is available. They are all modifications of one scheme, which is that used for loading two single-phase transformers and is commonly referred to as the "opposition method," shown in Fig. 1. Excitation voltage may be supplied from the same circuit as the load current or it may be taken from an independent circuit of the proper frequency for the transformer. Where a large number of similar transformers are to be loaded at the same time their primary windings may be connected in parallel and their secondaries may also be connected in parallel, or the secondary of one transformer in a group may be paired with that of one of the opposition group independently of the secondaries of the other transformers in the same group. (See Fig. 2). When there are

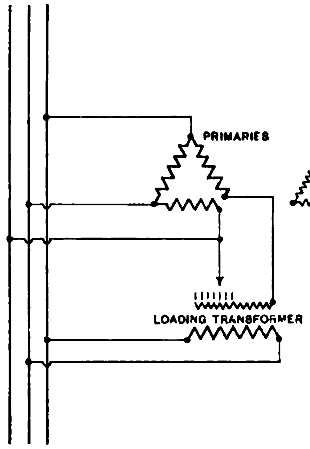


FIG. 3—SCHEME FOR THREE-PHASE LOADING

three single-phase transformers or one three-phase transformer the scheme shown in Fig. 3 may be used. The essential law to be observed in these methods when applied to polyphase loading is that the sum of the secondary induced electromotive forces

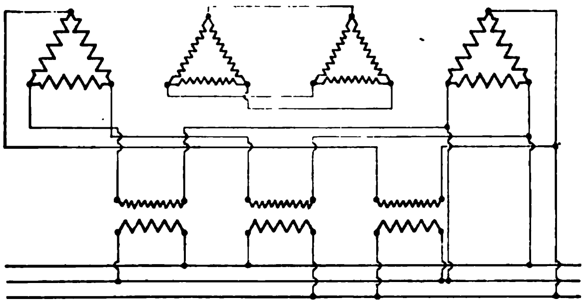


FIG. 4—SCHEME FOR THREE-PHASE BALANCED LOADING

taken around the circuit shall be zero. Six single-phase transformers or two three-phase transformers may be made to take a balanced load from a three-phase circuit by the scheme shown in Fig. 4.

V. Alternate Open–Circuit and Short–Circuit Method

The methods of loading transformers just described are applicable only when there are two or more transformers to be tested. It is sometimes necessary to test a single transformer, and it is generally impossible to obtain another suitable one to run in opposition. The following method has been devised to meet this condition.

The alternate open-circuit and short-circuit method of loading a transformer is effected by first exciting the transformer to be tested so as to obtain a core loss equal in value to the total losses of the transformer and running it on open circuit with this excitation for a fraction of a period t, previously decided upon, equal to the ratio of normal iron loss to total loss. Secondly, the transformer is run on short circuit with a current in the windings of such value as to give a short-circuit loss equal to the total loss, for the remainder of the period t which is a fraction of the total period equal to the ratio of normal copper loss to total loss. The cycle is repeated until steady conditions are reached. With length of period not too great this method will give results closely approximating those obtained by the other methods.

The theoretical proof of this method is as follows: Assume that the copper is initially at the same temperature as the oil, which is supposed to have reached its final value. The symbols used in this discussion and their definitions are as follows:

θ_c = Final average temperature rise of copper above average temperature of oil.

θ_1 = Average temperature rise of copper above average temperature of oil at end of first cycle.

θ_2 = Average temperature rise of copper above average temperature of oil at end of open-circuit run in second cycle.

θ_3 = Average temperature rise of copper above average temperature of oil at end of second cycle.

$\theta_{(2n-1)}$ = Average temperature rise of copper above average temperature of oil at end of nth cycle.

θ_{2n} = Average temperature rise of copper above oil at end of open-circuit part of $(n+1)$th cycle.

α = Emissivity of coil surface exposed to oil, in watts per sq. in. per deg.

S_c = Total surface of coil exposed to oil, in sq. in.

W = Weight of copper, in lb.

x = Time measured from start of run, in hours.
t = Period in hours.
E_c = Normal copper loss.
E_1 = Normal iron loss.
θ_a = Temperature at end of time x_1 on short-circuit.
θ_b = Temperature at end of time x_2 on open-circuit.

The value of the average rise in temperature of the coils above the average temperature of the coil is given by

$$\theta_c = \frac{E_c}{\alpha\, S_c} \qquad (1)$$

With a copper loss E and measuring the time x from the instant at which the average temperature of the oil and coils are the same, the temperature rise of the coils above the oil will be

$$\theta = \frac{E}{\alpha\, S_c}\left(1 - \frac{1}{10^{\frac{8.75\,\alpha\, S_c\, x}{W}}}\right) \qquad (2)$$

At the end of time x_1 we shall have

$$\theta_a = \frac{E\,\theta_c}{E_c}\left(1 - \frac{1}{10^{\frac{8.75\, E_c\, x_1}{W\,\theta_c}}}\right) \qquad (3)$$

If at the end of this time x_1 the load be removed and the transformer run on open circuit the copper will cool and its temperature at the end of time x_2 will be

$$\theta_b = \frac{\theta_a}{10^{\frac{8.75\, E_c\, x_2}{W\,\theta_c}}} \qquad (4)$$

Considering a cycle of period t, the length of the short-circuit portion of the period is

$$\frac{E_c}{E_c + E_f}\, t$$

The temperature rise of the coils above the average oil at the end of this time will then be

$$\theta_1 = \frac{(E_c + E_f)\,\theta_c}{E_c}\left(1 - \frac{1}{10^{\frac{8.75\,E_c^2\,t}{W\,(E_c + E_f)\,\theta_c}}}\right)$$

or, designating $\frac{(E_c + E_f)\,\theta_c}{E_c}$ by θ_T,

$$\theta_1 = \theta_T\left(1 - \frac{1}{10^{\frac{8.75\,E_c\,t}{W\,\theta_T}}}\right)$$

Similarly,

$$\theta_2 = \frac{\theta_1}{10^{\frac{8.75\,E_f\,t}{W\,\theta_T}}}$$

$$\theta_3 = \theta_T\left(1 - \frac{1}{10^{\frac{8.75\,E_c\,t}{W\,\theta_T}}}\right) + \frac{\theta_T\left(1 - \frac{1}{10^{\frac{8.75\,E_c\,t}{W\,\theta_T}}}\right)}{10^{\frac{8.75\,(E_f + E_c)\,t}{W\,\theta_T}}}$$

$$\theta_4 = \frac{\theta_3}{10^{\frac{8.75\,E_f\,t}{W\,\theta_T}}}$$

$$\theta_5 = \theta_T\left(1 - \frac{1}{10^{\frac{8.75\,E_c\,t}{W\,\theta_T}}}\right)$$

$$+ \frac{\theta_T\left(1 - \frac{1}{10^{\frac{8.75\,E_c\,t}{W\,\theta_T}}}\right)}{10^{\frac{8.75\,(E_f + E_c)\,t}{W\,\theta_T}}} + \frac{\theta_T\left(1 - \frac{1}{10^{\frac{8.75\,E_c\,t}{W\,\theta_T}}}\right)}{10^{\frac{8.75 \times 2\,(E_f + E_c)\,t}{W\,\theta_T}}}$$

$$\theta_{(2n-1)} = \theta_T\left(1 - \frac{1}{10^{\frac{8.75\,E_c\,t}{W\,\theta_T}}}\right) + \frac{\theta_T\left(1 - \frac{1}{10^{\frac{8.75\,E_c\,t}{W\,\theta_T}}}\right)}{10^{\frac{8.75\,(E_f + E_c)\,t}{W\,\theta_T}}} + \dots$$

$$+ \frac{\theta_T\left(1 - \frac{1}{10^{\frac{8.75\,E_c\,t}{W\,\theta_T}}}\right)}{10^{\frac{8.75\,(n-1)\,(E_f + E_c)\,t}{W\,\theta_T}}} \qquad (5)$$

$$\theta_{2n} = \frac{\theta_{(2n-1)}}{10^{\frac{8.75\,E_f\,t}{W\,\theta_T}}} \qquad (6)$$

The expansion for θ_{2n-1} is a geometrical progression which is convergent, hence we have

$$\theta_{(2\infty-1)} = \frac{\theta_T\left(1 - \frac{1}{10^{\frac{8.75\,E_c\,t}{W\,\theta_T}}}\right)}{1 - \frac{1}{10^{\frac{8.75(E_c+E_f)t}{W\,\theta_T}}}} \qquad (7)$$

$$\theta_{2\infty} = \frac{\theta_{(2\infty-1}}{10^{\frac{8.75\,E_f\,t}{W\,\theta_T}}} \qquad (8)$$

If the length of the period t be made exceedingly small,

$$\theta_{2\infty-1} = \theta_{2\infty} = \theta_T\,\frac{E_c}{E_c + E_f} = \theta_c \qquad (9)$$

Hence, for very small periods the final value is the same as that obtained under normal load conditions. It remains to be seen how much of an error is caused by the length of the period t being finite.

Let us apply this to an actual case of a 150-kv-a. transformer. In this transformer

$$E_f = 1390 \text{ watts}$$
$$E_c = 1136 \text{ watts}$$
$$(E_f + E_c) = 2526 \text{ watts}$$
$$\theta_c = 17 \text{ deg.}$$
$$\theta_T = 37.8 \text{ deg.}$$
$$W = 270 \text{ lb.}$$
$$t = \tfrac{1}{2} \text{ hr.}$$

$$\frac{8.75\,E_f\,t}{W\,\theta_T} = 0.596 \qquad 10^{\frac{8.75\,E_f\,t}{W\,\theta_T}} = 3.945$$

$$\frac{8.75\,E_c\,t}{W\,\theta_T} = 0.486 \qquad 10^{\frac{8.75\,E_c\,t}{W\,\theta_T}} = 3.062$$

$$\frac{8.75\,(E_c + E_f)\,t}{W\,\theta_T} = 1.082 \qquad 10^{\frac{8.75\,(E_c+E_f)\,t}{W\,\theta_T}} = 12.08$$

$$\theta_{(2\infty - 1)} = 28.5 \text{ deg.}$$
$$\theta_{2\infty} = 7.2 \text{ deg.}$$

The average of these two is 17.85 deg., which is close enough to the correct value $\theta_c = 17$ deg.

For a period $t = \tfrac{1}{4}$ hr.

$$\theta_{(2\infty-1)} = 23.3 \text{ deg.}$$
$$\theta_{2\infty} = 11.74 \text{ deg.}$$
$$\text{Average} = 17.52 \text{ deg.}$$
$$\theta_c = 17.00 \text{ deg.}$$

Evidently in this particular case there is very little to be gained in accuracy by making the period t less than one-half hour. In transformers which have high copper loss as compared with iron loss it may be found necessary to use a period of less than one-half hour.

VI—Temperature Tests on Transformers by the Alternate Open-Circuit and Short-Circuit Method

Tests were made on a 150-kv-a. 60-cycle oil-insulated air-cooled transformer to obtain a comparison between this method and the standard opposition method. The results of the various tests made indicate a good check between the two methods. The transformers chosen agreed closely in characteristics, but in order to eliminate any error due to possible differences between them, they were interchanged in the second run. The following are the principal values taken from the test results for the purpose of comparison. The thermometer readings were taken at the middle of the short-circuit part of the cycle. The temperature rises by resistances are a mean between those obtained at the beginning and end of the short-circuit part of the cycle.

TEMPERATURE RISES AT FULL LOAD AFTER STEADY CONDITIONS WERE REACHED

	Opposition method	Alternate short-circuit and open-circuit method
Temperature rise of low-tension coils by thermometer	38.5	37
Temperature rise of high-tension coils by thermometer	33.5	35
Temperature rise of top oil	31.5	32
Temperature rise of bottom oil	10.5	11

RISE OF TEMPERATURE OF WINDING BY RESISTANCE

	Opposition method	Alternate short-circut and open-circuit method
High-tension	33.9	34.6
Low-tension	40.7	41.5

These results are seen to be in very close agreement. The readings obtained on a second run with the two transformers interchanged are as follows:

TEMPERATURE RISES AT FULL LOAD AFTER STEADY CONDITIONS WERE REACHED

	Opposition method	Alternate open-circuit and short-circuit method
Temperature rise of low-tension coils by thermometer	35	36
Temperature rise of high-tension coils by thermometer	34	32
Temperature rise of top oil	29	30
Temperature rise of bottom oil	9	10

TEMPERATURE RISE MEASURED BY INCREASE OF RESISTANCE.

	Opposition method	Alternate open-circuit and short-circuit method
High-tension	33	34.4
Low-tension	39	41.0

All the results cited above were taken with a 30-minute period. The following rises by resistance were taken with a 15-minute period.

TEMPERATURE RISE MEASURED BY INCREASE OF RESISTANCE.

	Opposition method	Alternate open-circuit and short-circuit method
High-tension	33	33.3
Low-tension	39	39.6

A third test at 125 per cent load was made on the same transformers for temperature rises by the resistance method, with the following results.

TEMPERATURE RISE AT 125 PER CENT LOAD, MEASURED BY INCREASE OF RESISTANCE METHOD

	Opposition method	Alternate open-circuit and short-circuit method
High-tension	43	42.6
Low-tension	53	53.3

This method was also tried on a 2500-kv-a. water-cooled transformer, in comparison with the opposition method, the same flow of water being used in each case. The length of period was 15 minutes. The results were:

TEMPERATURE RISE ABOVE COLD WATER

	Opposition method	Alternate open-circuit and short-circuit method
High-tension	41.1	41.1
Low-tension	39.5	39.4

The tests were run a second time with the same results.

The results given indicate that the method of alternate short-circuit and open-circuit, as carried out, gives a very close approximation to the conditions of temperature obtained under load.

A paper presented at the Midwinter Convention of the American Institute of Electrical Engineers. New York, February 26, 1913.

CORRECTION OF TRANSFORMER TEMPERATURES FOR VARIATION IN ROOM TEMPERATURE, TAKING INTO ACCOUNT BOTH COPPER AND IRON LOSSES

BY C. FORTESCUE

I. INTRODUCTION

When the temperature rise of a coil of copper wire is to be measured by the increase of resistance method, it is generally required that the calculated rise above actual room temperature be corrected to give the rise corresponding to a standard room temperature, *e.g.*, the rise which would have resulted if the room temperature were 25 deg. cent. It usually happens that the temperature of the room differs from the generally accepted standard of 25 deg. For this reason the rise in temperature is found under actual conditions of the test and then a correction is made, which depends upon the difference between the actual room temperature and the standard room temperature.

It is proposed in this paper to derive first of all an expression for the corrected temperature rise, for any arbitrary room temperature, of a body consisting mainly of copper, based on the assumption that the heat loss takes place mostly by convection, and that in comparison the amount lost by radiation is negligible. Lastly, an expression will be obtained for bodies consisting of both iron and copper immersed in a fluid medium which is cooled by air. It will be shown that with both iron and copper losses taken into account, the difference between the observed temperature rise and that corrected for the standard room temperature is very small, and even when convection alone is taken into account it may be considered, as a rule, negligible.

Within the ordinary range of temperatures, there is the same increase in the resistance of a copper wire for each degree increase in temperature. The change in resistance caused by a change of one deg. cent., expressed as a fraction of the resistance at a definite standard temperature, is termed the temperature coefficient. The increase in resistance per degree is 0.00428 of the resistance at a temperature of zero deg. cent. Hence the resistance at any other temperature may be found by multiplying the number of degrees above zero by the coefficient and adding the result to the resistance at zero. The increase for each degree is 0.00386 of the resistance at 25 deg. cent. Therefore, the resistance at a higher temperature may be found by multiplying the number of degrees increase by this coefficient and adding the result to the resistance at 25 deg. These statements call attention to the difference between two coefficients which are in common use.

From the above definition of the temperature coefficient is derived the following well-known rule for calculating the temperature rise of a copper conductor above its initial temperature when the initial and final resistances are given, viz.:

$$t_2 - t_1 = \frac{R_2 - R_1}{R_1} (233.6 + t_1) \qquad (1)$$

where t_1 is the initial or room temperature, t_2 the final temperature and R_1 and R_2 the initial and final resistances.

II. Corrected Rise of Copper Conductors

The simple formula given below for determining the corrected rise of a coil carrying a current of constant value, is in its general form adapted to obtain the rise corrected for any arbitrary room temperature, when values of the initial and final resistance for any other room temperature are given. It is derived on the assumption that the rise in temperature above that of the surrounding air, of an air-cooled conductor carrying a given current, is proportional to the rate of dissipation of energy at the final temperature. The formula may be stated as follows:

$$t_b - t_a = \frac{R_2 - R_1}{R_1} (233.6 + t_a) \qquad (2)$$

where t_a is the arbitrary room temperature and t_b the temperature that would be reached by the apparatus if run in a room having this temperature. The formula applies to naturally cooled apparatus consisting of copper conductors, provided that the effect of radiation can be considered negligible, but with proper modifications may also be used for artificially cooled bodies.

A simple statement of the formula, applicable to every room temperature, may be made as follows: If a number of similar coils of copper wire in rooms, kept at different constant temperatures, carry equal and constant currents throughout a heat run, and the temperature of each body and that of the room in which the run is made is, in each case, the same at the start, the percentage increase in resistance, when steady temperatures are reached, will be the same for each of the coils.

When the observed temperature rise under any condition of room temperature is given, that for any other room temperature may be found by the formula

$$t_b - t_a = (t_2 - t_1)\frac{233.6 + t_a}{233.6 + t_1} \qquad (3)$$

Here $(t_2 - t_1)$ is the observed rise for the test room temperature t_1, and $(t_b - t_a)$ is the corrected temperature rise based on a room temperature t_a.

Substituting 25 deg. cent., the A. I. E. E. standard room temperature, in equation (2), the following simple statement of the correction formula is obtained:

If the resistances are given,

(temperature rise of body above standard room temperature)
= (observed percentage increase in resistance) × 2.586 (4)

If the observed temperatures are given,

(temperature rise of body above standard room temperature)

$$= \left(\frac{258.6}{233.6 + \text{observed room temperature}}\right) \times \text{(observed rise)} \qquad (5)$$

The derivation of formulas (2) and (3) is as follows:

From the assumed law of cooling, the following relations may be derived:

$$K\,(t_2 - t_1) = R_2\,I^2 \qquad (6)$$

$$K\,(t_b - t_a) = R_b\,I^2 \qquad (7)$$

where K is a factor depending on the surface of the body and its heat conductivity, I is the current in the conductor and R_b is the final resistance corresponding to the temperature t_b.

From the definition given for temperature coefficient, it follows that

$$\frac{R_b}{R_2} = \frac{233.6 + t_b}{233.6 + t_2}$$

and therefore, by (6) and (7),

$$\frac{t_b - t_a}{t_2 - t_1} = \frac{233.6 + t_b}{233.6 + t_2}$$

$$= \frac{233.6 + t_b - (t_b - t_a)}{233.6 + t_2 - (t_2 - t_1)}$$

$$= \frac{233.6 + t_a}{233.6 + t_1}$$

whence is derived formula (3)

$$t_b - t_a = (t_2 - t_1) \frac{233.6 + t_a}{233.6 + t_1}$$

Substituting from (1), formula (2) is obtained.

$$(t_b - t_a) = \frac{R_2 - R_1}{R_1} (233.6 + t_a)$$

To illustrate this formula by an example, take the case of a body composed of copper wires run in a room whose temperature is 35 deg. cent. throughout the run; the observed final resistance when a steady temperature is reached is 20 per cent higher than the initial resistance; to determine its temperature rise corrected for a room temperature of 25 deg. cent.

Here the temperature rise obtained by test was 53.6 deg.

Applying formula (4), the corrected rise is found to be 20 × 2.586 = 51.7 deg. cent.

The above formulas have been derived on the assumption that the rate of dissipation of heat from a body by convection is directly proportional to the difference in temperature of the body and the air. This law is commonly referred to as " Newton's law of cooling." If the effect of radiation is negligible and there is a free circulation of air, and its temperature is not raised appreciably after leaving the body, this law holds good. For bodies cooled by air flowing through constricted paths, such as air-cooled transformers, the average temperature of the coils will depend upon the average temperature of the air in the ducts, and the difference between these two values will depend upon the rate of dissipation of heat; but the average temperature of the air in the ducts, on account of the larger flow at the higher difference of temperature between coils and air obtained with higher room temperature, will not be proportional to the rate of dissipation of heat. In such cases the correction obtained by formula (**3**) will be too great.

The formulas (**2**) and (**3**) are suitable only for bodies whose external radiating surface is very small compared with the surface reached by convection currents. They will therefore apply to bodies cooled by forced convection. If the body is to be cooled by natural air circulation and has a smooth external blackened surface, so that the heat lost by radiation becomes appreciable, the formulas will give corrections very much too high, or even, in some cases, indicate a positive correction where it should properly be negative.

III. Corrected Rise Considering both Copper and Iron

It will be noted that the formulas which have been developed for obtaining corrected temperature rise by the increase of resistance method apply only to electrical apparatus composed wholly of copper conductors, or where the copper part of the apparatus may be considered separately from the rest on account of being heat-insulated from it. For air-cooled bodies consisting of both iron and copper, as, for instance, generators, it is very difficult to obtain a correction formula for room temperature; but in the case of apparatus in which both copper and iron are cooled by a common medium, such as oil, it is possible to obtain an approximate expression for corrected rise. Considering the case of oil-insulated air-cooled apparatus, such as oil transformers, let t_1 = initial room temperature, t_2 = final temperature of the copper, t_s = standard room temperature, t_c = corrected temperature. Let corresponding values of R be designated by R_1, R_2 and R_b; let R_0 = resistance of body at 0 deg. cent., and let W be the iron loss. Then, approximately,

$$K(t_2 - t_1) = I^2 R_2 + W \tag{8}$$

$$K(t_b - t_a) = I^2 R_b + W \tag{9}$$

$$\frac{t_b - t_a}{t_2 - t_1} = \frac{I^2 R_b + W}{I^2 R_2 + W}$$

$$= \frac{\frac{I^2 R_0}{233.6}(233.6 + t_b) + W}{\frac{I^2 R_0}{233.6}(233.6 + t_2) + W}$$

$$= \frac{\frac{I^2 R_0}{233.6}[(233.6 + t_b) - (t_b - t_a)] + W}{\frac{I^2 R_0}{233.6}[(233.6 + t_2) - (t_2 - t_1)] + W}$$

$$= \frac{\frac{I^2 R_0}{233.6}(233.6 + t_a) + W}{\frac{I^2 R_0}{233.6}(233.6 + t_1) + W}$$

$$t_b - t_a = (t_2 - t_1)\frac{I^2 R_a + W}{I^2 R_1 + W} \tag{10}$$

This may be stated in the following words:

In air-cooled electrical apparatus composed of copper conductors and iron, immersed in a common medium, in which the copper carries a current of given value and the iron is subjected to a periodically changing induction of given magnitude, the temperature rise, when steady conditions are reached for any arbitrary room temperature, may be obtained from that calculated for any other room temperature, by multiplying this observed rise by the ratio of the total losses at the arbitrary and the given room temperature.

The observed rise at the room temperature of the test may be either that obtained by thermometer or by the increase in resistance method, using formula (1).

When it is desirable to obtain a closer approximation than that given above, the following procedure may be followed.

First, obtain the average temperature rise of the oil or common cooling medium above the test room temperature and correct to standard room temperature by formula (**10**). Next obtain the average rise of copper above the average temperature of the oil under test conditions and correct to the corrected oil temperature by equation (**3**); the sum of these two will give the temperature rise of the copper corrected to standard room temperature.

Applying the formula to a practical example—An oil-insulated air-cooled transformer has copper loss, at 25 deg. cent., 1000 watts, iron loss 1000 watts. At a room-temperature of 35 deg. cent. the oil rose to an average of 25 deg. above the air. The average temperature of the copper above the oil was 15 deg. cent. What is the corrected average temperature rise of the copper at a room temperature of 25 deg. cent.?

Total loss at 25 deg. cent. = 2000 watts

Total loss at 35 deg. cent. = 2038 watts

$$\text{Corrected rise of oil} = \frac{I^2 R_a + W}{I^2 R_1 + W}(t_2 - t_1)$$

$$= \frac{2000}{2038} \times 25 = 24.5 \text{ deg. cent.}$$

Corrected average rise of coils above average temperature of oil

$$= \text{observed rise} \times \frac{233.6 + \text{corrected oil temp.}}{233.6 + \text{observed room temp.}}$$

$$= 15 \times \frac{233.6+49.5}{233.6+60} = 14.45 \text{ deg. cent.}$$

Corrected temp. rise of coils above air = 38.95 deg. cent.

The observed value was 40 deg. cent.

Thus, even when cooling by convection of air only is taken into account, the correction is so small as to be negligible in most cases. In fact, the correction is, as a rule, less than the probable errors in observation.

It has been assumed in the deduction of formula (**10**) that the average temperature rise of the oil is proportional to the total heat dissipated. This would be true only if the rate of flow of the convection currents of oil were constant. As a matter of fact, the oil will flow faster as the temperature rise of the coils above the oil increases. The flow will also be inversely proportional to the viscosity of the oil, which decreases as its temperature increases. The correction, therefore, given by the above rule, is always too great, and if, in addition, the heat lost by radiation on account of the higher coil temperatures reached with higher room temperature be taken into account, the correction will probably be reduced to zero, and, in some cases, instead of a positive correction as assumed in the rules, a negative correction may be necessary.

A paper presented at the Midwinter Convention of the American Institute of Electrical Engineers, New York, February 26, 1913.

THE TEMPERATURE RISE OF STATIONARY INDUCTION APPARATUS AS INFLUENCED BY THE EFFECTS OF TEMPERATURE, BAROMETRIC PRESSURE AND HUMIDITY OF THE COOLING MEDIUM

BY J. J. FRANK AND W. O. DWYER

The proper correction to be applied to the temperature rise of apparatus when tested or operating under conditions differing from those which have been accepted as a reference standard has been investigated and discussed, and various conclusions drawn.

It is the purpose of this discussion to present a short theoretical resume of the considerations that enter into the above, together with the results of an investigation that has been made on some transformers under test to verify the conclusions which have been reached.

The room temperature to be considered as a reference standard as given in the Standardization Rules of the A. I. E. E. is 25 deg. cent., as stated in section 268, reading as follows:

" Room Temperature. The rise of temperature should be referred to the standard condition of a room temperature of 25 deg. cent."

The barometric pressure to be considered standard as given in the Standardization Rules of the A. I. E. E. is 760 mm. (29.92 in.) as stated in Section 270 and 271, reading as follows:

" Barometric Pressure. Ventilation. A barometric pressure of 760 mm. and normal conditions of ventilation should be considered as standard, and the apparatus under test should neither be exposed to draft nor enclosed, except where expressly specified. The barometric pressure needs to be considered only when differing greatly from 760 mm.

"Barometric Pressure Correction. When the barometric pressure differs greatly from the standard pressure of 760 mm. of mercury, as at high altitudes, a correction should be applied. In the absence of more nearly accurate data, a correction of one per cent of the observed rise in temperature for each 10 mm. deviation from the 760 mm. standard is recommended. For example, at a barometric pressure of 680 mm. the observed rise of temperature is to be reduced by $\frac{760-680}{10} = 8$ per cent."

In artificially cooled apparatus no mention is made of the proper correction to be applied when the temperature of the cooling medium differs from the temperature of the enveloping air. In water-cooled apparatus no instructions are given for a standard reference temperature of the water. In apparatus cooled by air blast, no instructions are given in case the supply of air is taken from a source other than the room in which the apparatus is being tested.

No correction for changes in humidity is given.

General Theory of Dissipation of Heat from a Body

The rate of dissipation of heat from a body is dependent upon

1. Radiation
2. Convection
3. Conduction.

The transfer and final dissipation of heat is accomplished in several stages, each of which involves some or all of the above factors, *i.e.*, radiation, convection and conduction. We may roughly group the steps, in stationary induction apparatus, as follows:

a. From the core or from the conductor, through the insulation, to the cooling medium in contact with it.

b. Through the cooling medium to the case, tank or cooling coils.

c. Through the casing or cooling coils to the medium which finally carries away the heat.

Classification based on the several methods of cooling divides stationary induction apparatus into five groups as follows:

(i) *Self-Cooled Natural Draft.* In this class of apparatus the cooling is dependent upon (a) only. The heat is carried along the conductor or through the conductor to its exposed portion by conduction, through the insulation by the same process, and dissipated by radiation and convection.

(ii) *Self-Cooled Oil-Insulated.* In this class of apparatus steps (a), (b) and (c) are involved. The transfer of heat is accomplished in this case by conduction through the insulation, convection and some radiation through the oil and convection and radiation from the tank.

(iii) *Artificially Cooled by Circulation of Oil.* In this class of apparatus steps (a), (b) and (c) are involved. The transfer of heat is accomplished by conduction through the insulation, convection and some radiation through the oil, and convection and radiation from (a) external radiators (b) water-cooled coils.

(iv) *Artificially Cooled Water-Cooled Apparatus.* In this class of apparatus the transfer of heat is similar to that in (ii) with the modification that most of the heat is carried away by convection by water circulated in cooling coils.

(v) *Artificially Cooled Air-Blast Apparatus.* In this class of apparatus the cooling may be dependent upon (a) only, where the air blast is in direct contact with the windings and core, or the cooling may be dependent on (a), (b) and (c), in such apparatus in which the blast of air comes in contact with the outside of the case or tank.

Radiation

The quantity of heat dissipated by radiation may be expressed by Stefan-Boltzman's law by

$$W_R = K\,(T_1^4 - T_2^4) \qquad (1)$$

where K = constant depending upon the diathermancy or transmissive power of the medium, upon the nature and color of the surface and upon the size of the enclosure in which body is dissipating heat.

T_1 = absolute temperature of the apparatus.

T_2 = absolute temperature of surrounding objects.

Convection

The calculation by mathematical considerations of the amount of energy dissipated by convection requires the use of one or more empirical constants. Much work along this line has been done by Prof. Kennelly and Dr. Langmuir, the results of the latter's investigations being published in the *Physical Review* and the A. I. E. E. TRANSACTIONS.*

These investigations have been made for the most part on the dissipation of energy from fine wires at high temperatures where the amount dissipated by radiation is very small. Dr. Langmuir has shown, however, that in the dissipation from plane surfaces where the radiation factor is considerable the formulas developed may be used with excellent results.

Dr. Langmuir's theory is that the dissipation of heat by convection occurs by conduction through a film of adhering gas, the energy being conducted through this film by molecular diffusion to the moving convection currents of the surrounding gas and so carried away. Further, the results of his experiments show that the thickness of this film is constant (0.43 cm.) (for constant room temperature and pressure) and that the dissipation of energy by convection is almost entirely dependent upon the thickness of this conducting film.

While no data are given on the relative values of dissipation by convection from horizontal and vertical plane surfaces, the experimental values as obtained from wires show that the convection is somewhat less for vertical wires than for horizontal.

This difference will be more pronounced in case of plane surfaces, but the decreased efficiency will be more than compensated for by the flue or chimney action where the convection currents are constrained to move in restricted spaces such as ducts, corrugations, flues, etc. For this type of surface the effectiveness of dissipation by convection will probably be greater than for horizontal plane surface, provided that the surface does not become so complicated that friction will seriously affect the convection currents.

Assuming that the dissipation of heat by convection takes place by conduction through a film of adhering gas, Dr. Langmuir has developed a formula for this, of the form

$$W_c = \frac{\phi_1 - \phi_2}{B} = \text{watts dissipated per sq. cm.}$$

where B = thickness of adhering film and ϕ is a function of T of the form

$$\phi = 1.93 \times 10^{-6} (1 + 0.00012\, T) \left[2/3\, T^{3/2} - 248\, T^{1/2} + 124^{3/2} \tan^{-1} \sqrt{\frac{T}{124}} \right] \qquad (2)$$

for air.

*TRANSACTIONS, A. I. E. E., 1912, XXXI, Part I, page 1229.

Conduction

Conduction consists of the transfer of heat from one part of one body to another part of same body or from one body to another without any bodily transfer.

The amount of energy transferred by conduction is proportional to the specific heat resistance, to the density of the heat flux and to the length of the path, *i.e.*, the rate of transmission of heat by conduction is proportional to the gradient or fall of temperature per unit length of path along the lines of flux flow.

$$W_d = \frac{K\,(T_1 - T_2)\,A\,t}{l} \text{ (for uniform gradient)} \qquad (3)$$

where t = Time
A = Area
l = Length
K = Conductivity

Effects of Temperature of Cooling Medium on Heat Dissipation

Section 269 of the A. I. E. E. Standardization Rules covers the correction to be applied to the temperature rise of apparatus when the reference temperature varies from 25 deg. cent. This rule reads:

"If the room temperature during test differs from 25 deg. cent., correction on account of difference in resistance should be made by changing the observed rise of temperature by one-half per cent for each degree centigrade."

The correction as applied means an increase of one-half of one per cent per degree in the observed rise when room is below 25 deg. cent. and a decrease of one-half of one per cent if the room temperature is greater than 25 deg. cent. The reference of 25 deg. cent. refers to a constant room temperature, a condition which is hard to maintain in practise. No provision is made for applying correction when room temperature is varying, or for correction for artificially cooled apparatus.

a. *Influence of Room Temperature on Radiation*. From equation (1) the quantity of heat dissipated by radiation from a hot body at temperature T_1 to surrounding objects at temperature T_2 is given by

$$W_R = K\,(T_1^4 - T_2^4)$$

It is apparent from the nature of the equation that W_R will increase with T_2 for a constant difference between T_1 and T_2 or in other words, for a given rise above room temperature, more watts will be dissipated the higher the room temperature, or the dissipation of a given quantity of heat by radiation will be accompanied by a less rise above room temperature the higher the room temperature.

Fig. 1 gives values of $T_1^4 - T_2^4$ for constant rises above different temperatures of surrounding objects. An inspection of these curves will show that for a 40-deg. rise at 0 deg. cent., $T_1^4 - T_2^4$ is 400×10^8, while for the same rise at 60 deg. cent. the value of $T_1^4 - T_2^4$ is 720×10^8, an increase of 80 per cent in the effective-

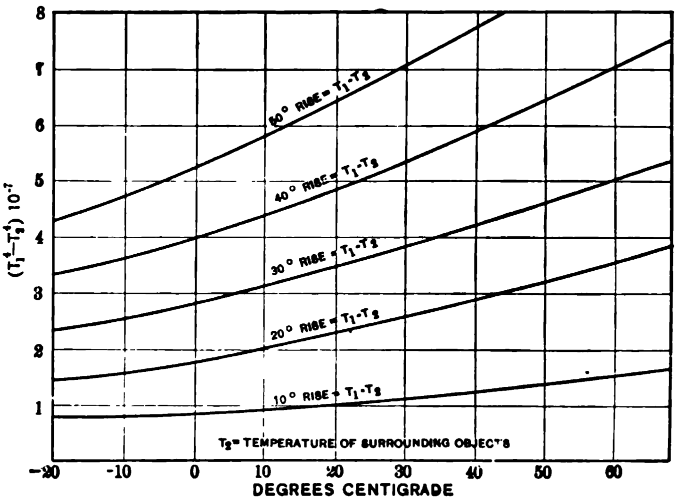


FIG. 1

ness of radiation. Consequently the same watts dissipated by radiation for a 40 deg. rise above 0 deg. cent. gives only a 24 deg. cent. rise at 60 deg. cent.

Fig. 2 gives the watts loss per sq. cm. dissipated by radiation for different rises above various room temperatures, assuming $K = 4.78 \times 10^{-12}$, *i.e.*, 90 per cent value of a perfect black body.

b. *Influence of Room Temperature on Convection.* The amount of energy dissipated by convection at constant room temperature (30 deg. cent.) is given in equation (2)

$$W_c = \frac{\phi_1 - \phi_2}{B} \quad \text{where } B = 0.43 \text{ cm.}$$

and

$$\phi = 1.93 \times 1.0^{-6} (1 + 0.00012\, T) \left[2/3\, T^{3/2} - 248\, T^{1/2} + 2 \times 124^{3/2} \tan^{-1} \sqrt{\frac{T}{124}} \right]$$

In Fig. 3, ϕ has been plotted against temperature. Now it has been found that while B is independent of temperature of body T_1 it varies with T_2, and Dr. Langmuir has found for free convection it is proportional to it, *i.e.*,

$$B_1 = B \times \frac{T_2}{T} \text{ where } B = 0.43 \text{ at 30 deg. cent.}$$

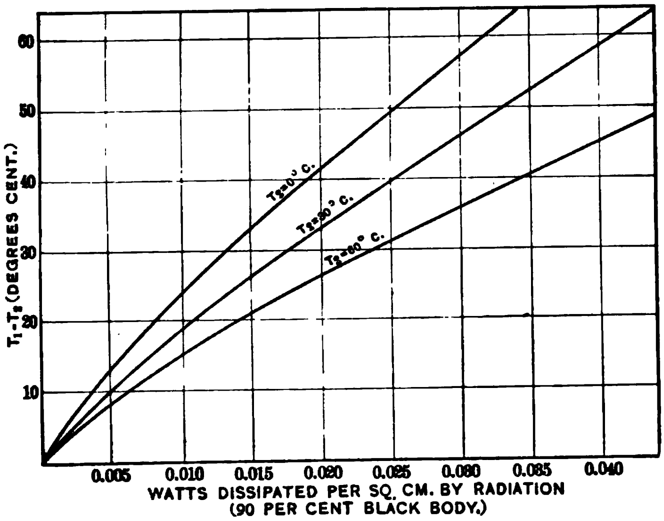


FIG. 2

Fig. 4 gives watts dissipated by convection for rises above various room temperatures. Unlike the dissipation by radiation, the energy dissipated by convection for a constant rise decreases with increasing room temperature, but to a much less extent, being almost independent of temperature.

In case of oil-insulated apparatus, convection also comes into play in the transfer of heat from the coils or core to the surface of the enclosing case or cooling coils. The process is no doubt similar to that for air, consisting in a transfer of heat through the adhering film of oil by conduction, and then by convection currents to the case, where a similar transfer takes place. The film of oil is probably subject to same changes due to velocity and temperature as is the air film. Further, with increasing temperature the viscosity of the oil will change considerably, with a corresponding increase in velocity of convection currents. The net result of a higher room temperature on this step will be a more uniform distribution of temperature, so that while the top oil will be cooler and the bottom oil hotter, the average mean temperature will be lowered. This is shown in Fig. 6, while in Fig. 5 are given curves showing changes in viscosity with temperature of some transformer oils.

In connection with the effect of oil on temperature rise of ap-

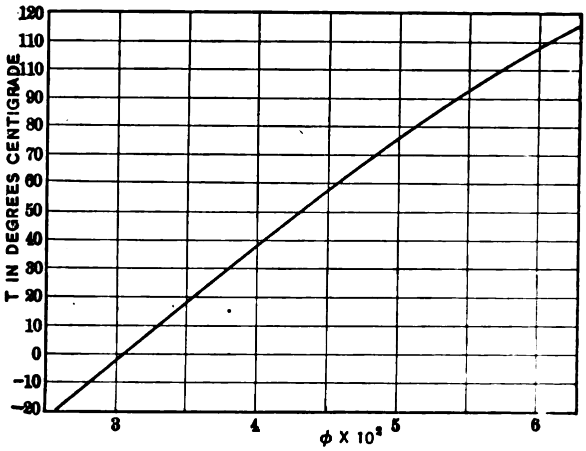


FIG. 3

$$\phi \times 100 = 4.19 \times 4.6 \times 10^{-4} (1 + 0.00012T) \times \left(\frac{2}{3} T^{3/2} - 248 T^{1/2} + 2760 \tan^{-1} \sqrt{\frac{T}{C}}\right)$$

paratus it should also be noted that with increasing temperature the volume of oil and the wetted surface is increased. The linear expansion of transil oil may be expressed by

$V_T = V_0 (1 + 0.000895\, T)$
When V_T = Volume at temperature T
V_0 = Volume at deg. cent.
T = Temperature in deg. cent.

c. *On Conduction.* From equation (**3**) we had

$$W d = \frac{K (T_1 - T_2)}{l} A\, t$$

The transfer is inversely proportional to the resistance to the passage of heat.

Copper has practically no temperature coefficient for conduction, while the coefficient for iron is 0.00075.

Of the insulations, the conductivity of mica is unaffected by changes in temperature, while fibrous and cellulous materials such as are frequently used for turn and coil insulation, have a very considerable temperature coefficient, the thermal resistance being about 25 per cent less at 100 deg. cent. than at 0 deg. cent.

The effect of room temperature on conduction will in any case be small, but will always tend to decrease the resistance of this step with increasing temperature of room.

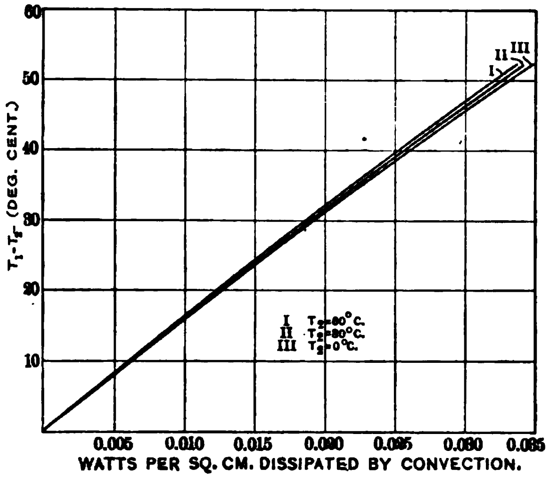


FIG. 4

The net effects of changes of room temperature on the dissipation of heat energy from stationary induction apparatus are

a. Effective radiation is greater the higher the room temperature.

b. The dissipation by convection in air is practically independent of room temperature. The transfer of heat by the convection currents in oil, immersed transformers will, however, be somewhat increased with higher room temperature.

c. Conduction will be better at higher room temperature.

CONSTANT LOSSES

For a constant loss we would expect, therefore, that apparatus would operate at a lower temperature rise the higher the temperature of the surrounding air. The actual change in rise will of course depend upon the proportion of losses dissipated by radiation and convection. In the case of dissipation from tank surfaces this proportion varies greatly with different styles of

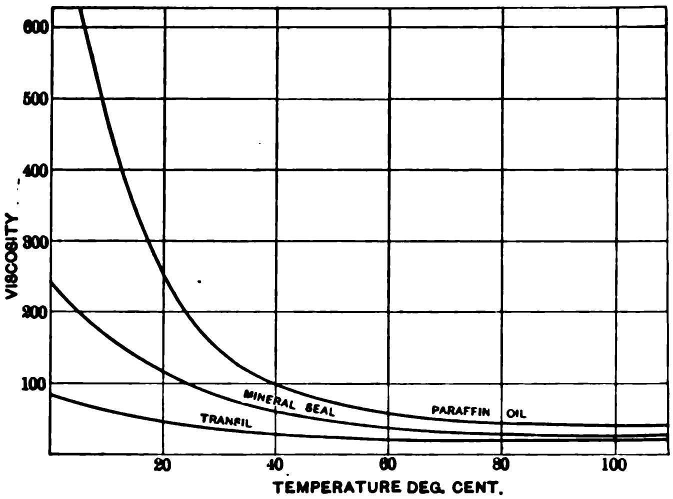


FIG. 5

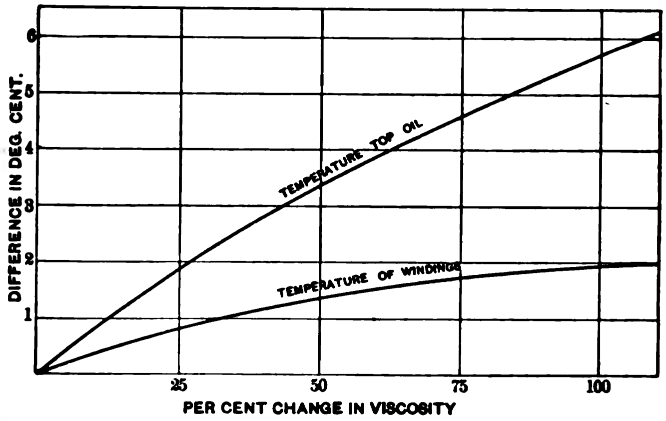


FIG. 6

tanks. The radiation value will be greatest on plain tanks, decreasing as the surface becomes more complicated by ribs, corrugations, flues, pipes, radiators, etc., the addition of which tends only to increase the convection component if the external dimensions remain the same.

In order to apply any correction to the emissive constants it is necessary to segregate the values for radiation and convection and apply to each separately.

Fig. 7 gives watts dissipated by various styles of tanks per square inch surface at 0 deg. cent. and 60 deg. cent., the percent-

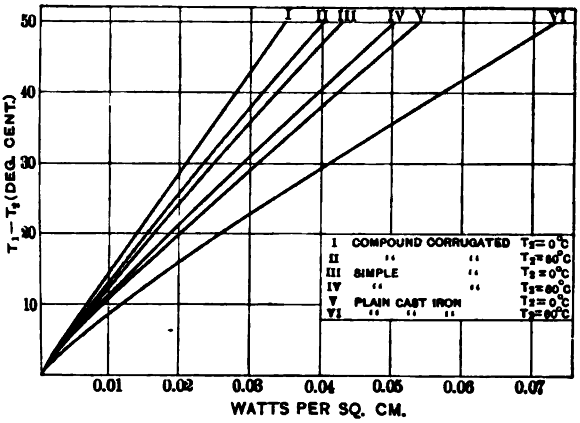


FIG. 7—TOTAL WATTS DISSIPATED PER SQ. CM. (90 PER CENT RADIATION + 100 PER CENT CONVECTION.)

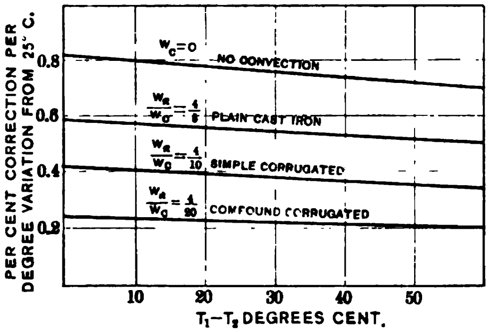


FIG. 8—TANK DISSIPATION

Corrections to be applied in reducing observed rises to room temperature of 25 deg. cent.—Constant losses.
When $T_2 > 25$ deg. cent. subtract correction.
When $T_2 < 25$ deg. cent. add correction.

age dissipation by radiation varying from 40 per cent to 16.6 per cent at 25 deg. cent.

Fig. 8 gives correction for temperature for constant losses for same tanks, to reduce to a standard reference of 25 deg. cent. room.

Increasing Losses

While the core loss is practically independent of the temperature, the load losses (I^2R) will increase with the temperature, due to the temperature coefficient of copper. This difference in $I^2 R$ amounts to 0.0042 per cent for each degree variation and allowance must be made for this increase in losses in determining temperature rise at high room temperatures, together with the increased effectiveness of the emissive constants.

Fig. 9 gives the correction for room temperature for various proportions of I^2R, also for different proportions of radiation and convection.

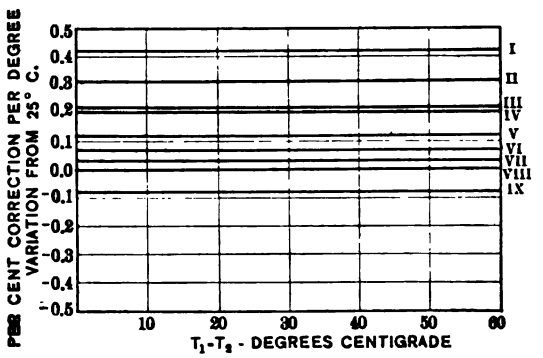


Fig. 9—Tank Dissipation.

Corrections to be applied in reducing observed rises to room temperature of 25 deg. cent.—Increasing losses with temperature.

When $T_2 > 25$ deg. cent. subtract correction.
When $T_2 < 25$ deg. cent. add correction.

I—Plain cast iron. I^2R: core loss	= 1 :2
II— " " " " " "	= 1 :1
III— " " " " " "	= 2 :1
IV—Simple corrugation. " "	= 1 :2
V— " " " " "	= 1 :2
VI— " " " " "	= 2 :1
VII—Compound " " " "	= 1 :2
VIII— " " " " "	= 1 :1
IX " " " " "	= 2 :1

Water-Cooled Apparatus

This type of apparatus depends on the dissipation of heat by two factors, either of which may vary greatly in temperature. While the greater part of the heat is carried away by convection by cooling water, the dissipation from the tank may have a considerable influence on the temperature rise of such apparatus, as the table on the following page shows.

It is possible in the case of this class of apparatus, under certain conditions of temperature of room and ingoing water, to have the tank absorbing heat from the air while under test and contributing to a higher rise of the apparatus.

The surface of the cooling coils will usually approximate to that of the tank in area, but effectiveness of the cooling water is much superior to the air. Following is a table of rises with different temperatures of ingoing water and air temperatures:

Loss watts	Surface		Temperatures deg. cent.			Watts dissipated		Oil rises above		
	Tank	Cooling coil	In water	Air	Idler	Tank	Cooling coil	In water	Air	Idler
5000	10000	10000	25	25	25	900	4100	16°	16°	16°
"	"	"	25	0	8	2700	2300	13°	13°	30°
"	"	"	0	25	17	—400*	5400	18°	—7°	1°
"	"	"	0	50	33	—1800*	6800	20°	—30°	—13°
"	"	"	50	0	18°	4600	400	10°	60°	42°

*Heat absorbed by tank.

The radiating surface of the tank is greater than that of the cooling coils on the smaller units (up to say 1000 kw.), while on the larger sizes it is much less, so that the influence of room temperature will not be felt so much.

Changing from one temperature to another of ingoing water will introduce very little difference in rise (provided the air temperature is same as ingoing water) since the specific heat of water increases only slightly with temperature and the increased effectiveness of radiation of tank with temperature may be neglected.

Effect of Barometric Changes on Dissipation of Heat

a. *Radiation*. Since changes in pressure are unaccompanied by any change in the diathermancy of the surrounding medium the heat dissipated by radiation will be unaffected by changes in pressure.

b. *Convection*. It has been found that for constant room temperature, the energy dissipated by convection is proportional to B (thickness of film).

For free convection Dr. Langmuir's experiments show that the thickness of the adhering film varies with the 0.75th power of the pressure, while for forced convection this value was somewhat less. Using 90 per cent radiation value, the writers found the value for free convection to be somewhat less than this, being about the 0.64th power (free convection).

Fig. 10 gives watts dissipated by convection plotted against pressure for a 40 deg. rise at 25 deg. cent. using constant 0.75.

c. *Conduction.* This will be unaffected by changes in pressure.

The net result of a change in pressure of surrounding air on dissipation of energy from a heated body will be that with increased pressure a less rise will be experienced with constant losses.

The difference in rise with different pressures will of course depend, as it did in case of room temperature, on the proportion

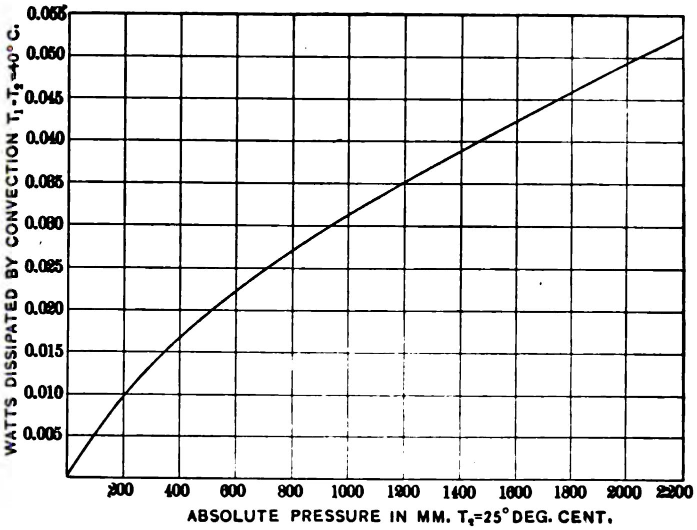


FIG. 10

of the emissive constants. Thus, apparatus having smooth contours, where the proportion of heat dissipated by radiation is a maximum, will be least affected by changes in pressure, while those with complicated contours will have a large correction for pressure.

The correction for room temperature will also vary with pressure, increasing with decreasing pressure. It is not possible to apply the same correction factor for room temperature at different atmospheric pressures.

AIR-BLAST APPARATUS

In this type of apparatus, the cooling of both core and coils is accomplished almost entirely by convection, the heat being carried away by the convection currents caused by the blast. Very little heat will be dissipated by radiation from the windings, their insulation covering being at best a poor radiator, while the windings are generally so placed that what heat is radiated by one surface is absorbed by the next, and vice versa.

Air-blast apparatus is either rated for operation at a certain air pressure or with a certain volume passing through the coils per minute.

Where the rating is based on volume of air it is evident that the weight of air per cubic foot increases with the atmospheric pressure, decreases with the temperature and is practically independent of the quantity of moisture (except where fog-laden air is used). Since all the heat must be carried off by convection, it is evident that with increasing altitude or increasing temperature, the rise will increase for a constant volume of air passing through the ducts, the per cent increase being proportional to the temperature and inversely proportional to the pressure. It is probable that, as in the case for free convection, the correction for forced convection will vary more nearly as the 0.75th power which Dr. Langmuir found.

When the rating is based on air pressure, since the viscosity does not change with pressure, the same volume of air will pass through for the same difference in pressure, and correction will be the same as (1). The viscosity changes with the temperature, however, so that for the same pressure the volume will change with temperature while the specific heat will also change slightly with temperature. It should be expected, therefore, that since the viscosity varies as $T^{\frac{1}{2}}$ (approximately) and the weight as T^{-1} the weight per second would vary as $T^{-\frac{1}{2}}$ neglecting change in specific heat.

Fig. 11 gives corrections to be applied to the observed readings for variations in atmospheric pressure (constant temperature) for free and forced convection.

Fig. 12 gives correction to be applied to observed readings for variation in atmospheric temperature (constant losses) for forced convection.

Effect of Humidity Changes on Heat Dissipation

Radiation. Since water vapor is not perfectly diathermous, radiation from a heated body will be affected by its presence in the atmosphere surrounding the body.

While there is practically no absorption in dry air, the presence of water vapor does increase the absorption somewhat.

Investigators differ greatly in their estimates of the absorption of water vapor and its effect on radiation. While the heat absorbed by the vapor will increase the temperature of the air a small amount, it is evident that the effect on radiation will in any case be small, below saturation.

Convection. The presence of water vapor in the atmosphere will affect the quantity of energy dissipated by convection in so much as it affects the conductivity of the adhering film. We would expect, therefore, since the specific heat of water vapor is greater than dry air, that the conduction of the film *B* would be increased, and the effectiveness of dissipation by convection increased.

An inspection of Table III shows that for ordinary tempera-

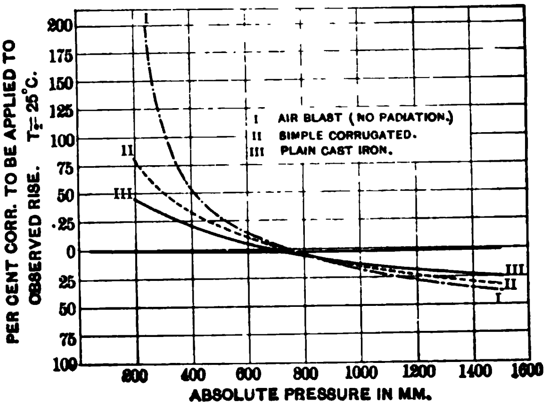


Fig. 11

tures and barometric pressures, even up to the saturation value, the percentage of moisture compared to air (by weight) is very small, amounting at most to a few per cent, so that it can have very little effect on the convection value. The values as obtained by experiments seem to verify this, the difference in rises with varying percentage of moisture being entirely negligible.

In the case of fog-laden air where particles of water are actually present, the case is very different, and a very considerable correction would have to be made.

Experimental Observations

Tests were made on two duplicate standard lighting transformers (oil-insulated, self-cooled), with losses within ½ of 1 per cent of each other. It was necessary to increase the losses above normal so as to obtain a 40-deg. rise by oil. This increase in losses was accomplished by holding over-load current, but normal excitation.

Heat tests were conducted in large pressure tanks, one transformer being placed in each tank, as shown in Fig. 16. The transformers were placed on wooden tables resting on the bottoms of the tanks, the tops of the tables being nearly same area as base of transformer cases. Tanks were supplied with radiating coils connected to a steam and water source. An insulating asbestos shield was placed over the cooling coils so as to protect the transformers from their direct radiation.

Both tanks were provided with chloride driers through which

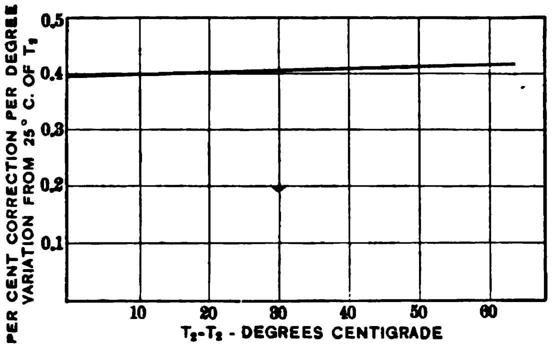


FIG. 12—CORRECTION FOR AIR-BLAST APPARATUS ONLY—ASSUMING CONSTANT LOSSES.

When $T_1 > 25$ deg. cent. add correction.
When $T_1 < 25$ deg. cent. subtract correction.

air could be introduced under a pressure. In addition, vacuum connection could also be made.

The system of switching was so arranged that when the heat run was interrupted resistance measurements could be quickly taken.

Temperatures were taken by means of generator bulb thermometers read directly from a central dial while run was on. The thermometers were placed as in Fig. 16, check thermometers being used at the more important places. In addition, wet and dry bulb check readings were taken by spirit thermometers.

Very little heat was conducted from the transformer tank to the cover, as a gasket of felt packing was placed between them. The temperature of the cover was very little above the surrounding air, so it was neglected in calculating radiating surface. Since the base rested on a wooden table, practically no hêat was conducted from it. The total wetted surface at 25 deg. cent. was 6620 sq. cm. which corresponds to a radiating surface on outside of tank of 7020 sq. cm. The actual effective surface due to conduction to unwetted portion will be greater than this, and in this case equal to 8500 sq. cm.

Further, the temperature of the case was different at different heights, being a maximum at top oil level, and decreasing toward top and bottom, the bottom temperature being less, the less the viscosity of the oil. To reduce to effective temperature for the whole tank it would be necessary to integrate from top to bottom.

TABLE I

Watts loss	Press. in mm.	P — 0.378 E in mm.	Grains H_2O per liter	Temp. deg. cent.		Rise deg. cent. top oil
				Air	Top oil	
422	236	216	0.342	27.5	78.0	50.5
437	235	208	0.703	59.0	99.2	40.2
423	475	470	0.264	30.0	76.0	46.0
436	477	470	2.11	59.5	98.0	38.5
422	740	737	0.194	24.5	71.4	41.9
435	735	733	0.362	45.0	86.3	41.3
440	734	731	0.622	61.0	96.0	35.0
420	1245	1233	0.292	26.0	66.0	40.0
435	1244	1231	0.565	61.0	93.0	32.0
413	1780	1772	0.256	26.5	63.0	36.5
435	1778	1770	1.44	59.5	91.5	32.0

In the present case, we have taken average temperatures, which will probably be sufficiently close.

Test Observations

I. *Readings taken to Determine Effect of Temperature of Surrounding Air.* Corrections made in pressure readings for elastic vapor pressure E of the water vapor present according to

$$P = P_1 - 0.378\,E$$

P = Calculated pressure in mm.
P_1 = Observed " " "
E = Elastic vapor pressure in mm.

Fig. 13 gives curves of the above with values in Table I (neglecting humidity), plotted with observed values and also corrected for constant losses. As would be expected from the previous discussion, the correction is greatest where the convection value is least, that is, at low pressures.

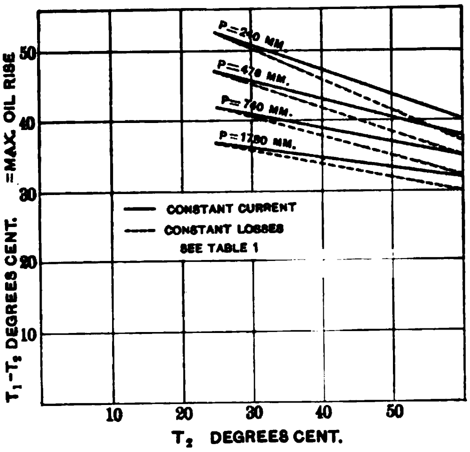


FIG. 13

II. *Effect of Pressure.*

TABLE II A

Watts loss	Press in mm.	P — 0.378 *E* in mm.	Grains H_2O per liter	Temp. deg. cent. Air	Temp. deg. cent. Oil	Rise deg. cent. top oil
422	236	224	0.077	29.0	80.0	51.0
423	358	355	0.099	29.5	78.0	48.5
423	475	471	0.117	28.2	75.5	47.3
420	734	732	0.176	30.0	71.8	41.8
415	1252	1245	0.236	29.6	69.5	40.0
410	1780	1774	0.256	26.5	63.0	36.5
410	2280	2273	0.301	25.0	59.0	34.0

TABLE II B

438	236	210	0.705	59.0	99.2	40.2
437	476	470	2.08	59.5	98.0	38.5
435	731	731	0.622	61.0	96.0	35.0
433	1248	1237	0.565	61.0	93.0	32.0
432	1780	1774	1.44	59.5	91.5	32.0

Fig. 14 gives curves plotting temperature rise against pressure for 30 and 60 deg. cent. room temperature.

Heat tests were also conducted on three 125-kw. self-cooled, oil-insulated transformers assembled in simple corrugated tanks

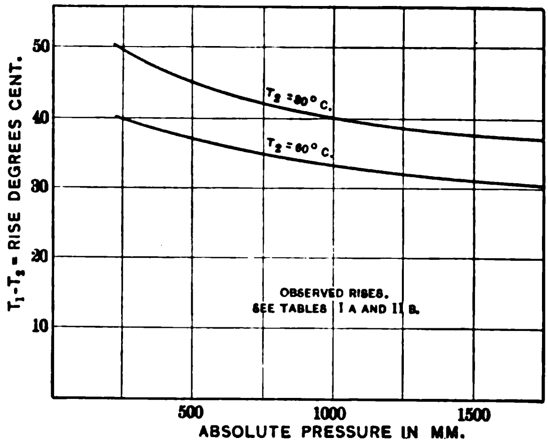


FIG. 14

at Schenectady, Pittsfield, Grand Junction, Colo., and Leadville, Colo., to determine what effect the altitude had on heating. Following is a summary of the tests made.

TABLE II C

Place	Watts loss	$P - 0.378\ E$	Grains H_2O per liter	Oil rise deg. cent.
Schenectady.....	4820	758	0.151	36.0
Pittsfield........	4820	732	0.116	37.5
Grand Junction..	3965	631	0.202	32.5
Leadville........	4570	518	0.197	39.6

Reduced to same loss the rises are given in Fig. 15 and Table II D.

TABLE II D

Place	Watt loss	P. 0.378 E	Grs. H_2O per liter	Oil rise deg. cent.
Schenectady........	4820	758	0.151	36.0
Pittsfield...........	4820	732	0.116	37.5
Grand Junction.....	4820	631	0.202	39.5
Leadville...........	4820	518	0.197	44.0

III. *Effect of Humidity.* Humidity measurements were taken by wet and dry bulb thermometers, the elastic force of the vapor being calculated according to formula.

$$E = E_1 - 0.00066\, P\,(t - t_1) \quad \{1 + 0.00115\,(t - t_1\}$$

where E = Vapor pressure in mm.

E_1 = " " of saturated aqueous vapor.

P = Pressure in mm.

t = Temperature of dry bulb (deg. cent.)

t_1 = " " wet " (deg. cent.)

Weight of one liter of dry air at 0 deg. cent. was taken as 19.52

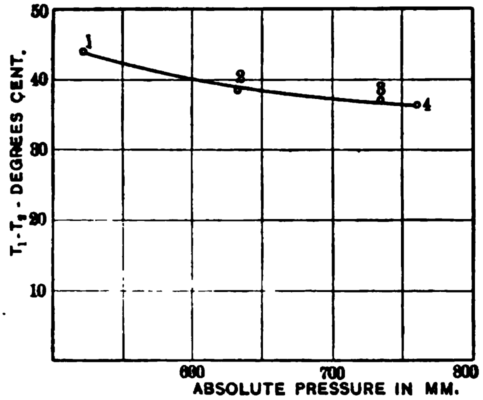


FIG. 15

Altitudes.
1. Leadville, Colo.......3190 m.
2. Grand Junction, Colo.......1525 m.
3. Pittsfield, Mass.......346 m.
4. Schenectady, N. Y.......75 m.

Observed rises on 125-kw. transformer, reduced to constant losses.

grains. Correction for vapor pressure was applied to observed pressures according to the formula

$$P = P_1 - 0.378\, E$$

where P_1 = Observed pressure.

E = Elastic vapor pressure

P = Calculated pressure.

Weight of dry air in one liter of air was calculated from

$$W = \frac{P - 0.378\, E}{29.92} \times 19.52 \text{ grains}$$

Weight in grains of water present in one liter of air was calculated from

$$W_1 = \frac{62.3 \times E}{29.92 - E} \times \frac{P - 0.378\,E}{P} \times 0.1952 \text{ grains}$$

TABLE III

Watts	Pressure	$P - 0.378\ E$	Grs. H_2O per liter	Grs. dry air per liter	Per cent moisture by wt.	Temp air.	Temp top oil	Rise top oil
422	236	234	0.078	5.65	1.3	29.0	80.0	51
"	"	232	0.156	5.62	2.6	27.5	78.5	51
"	"	216	0.342	5.50	6.2	27.5	78.0	50.5
422	358	353	0.100	9.70	0.99	29.5	78.0	48.5
"	"	352	0.123	9.70	1.31	28.0	76.5	48.5
422	475	471	0.087	12.15	0.715	27.0	74.5	47.5
"	477	473	0.117	"	0.965	28.2	75.5	47.3
"	477	473	0.202	12.10	1.67	27.0	74.0	47.0
"	476	472	0.204	"	2.19	30.0	76.5	46.5
420	735	733	0.162	19.0	0.86	30	71.3	41.3
"	736	734	0.176	"	0.93	30	71.8	41.8
418	734	733	0.187	"	0.98	28	69.3	41.3
420	734	734	0.190	"	1.01	30.5	71.8	41.3
"	737	736	0.197	"	1.04	29.5	71.4	41.9
"	735	733	0.265	"	1.40	30.0	72.0	41.5
417	1233	1231	0.194	32.1	0.55	27	66.0	39
"	1241	1236	0.236	"	0.74	26	66.0	40
418	1238	1235	0.292	"	0.91	29.5	69.5	40
414	1780	1776	0.256	44.8	0.57	26.5	63.0	36.5
413	1778	1774	0.278	"	0.62	24.0	60.0	36.0

Conclusions

Referring to Figs. 13 and 14 and Table III, it will be seen that the temperature rise of apparatus varies inversely with the temperature, inversely with the pressure and is practically independent of the water vapor over the ranges covered by test. The variations due to changes in amount of water vapor were small, and the results almost invariably showed the same rises for different quantities of water vapor present. This is to be expected, as a glance at Table III shows that even at saturation and moderate temperatures, the percentage of moisture is very small and can have little effect on the heat dissipation by convection or on the dissipation by radiation due to the change in diathermancy. With fog-laden air, *i. e.*, where particles of water are held in suspension, the case is very different, the water now being a much greater proportion by weight of the mixture, the dissipation by convection being greatly increased, while no doubt the dissipation by radiation will be somewhat diminished. The exact correction to be applied in any case will of course depend upon the proportion of heat dissipated by convection and radiation, which in turn depends upon the type of apparatus and means of cooling.

RECOMMENDATIONS

It is evident from the above discussion that the corrections to be applied for variations in atmospheric conditions are not the same for different types of stationary induction apparatus, each type requiring special consideration.

Following the line of division given in the beginning of this

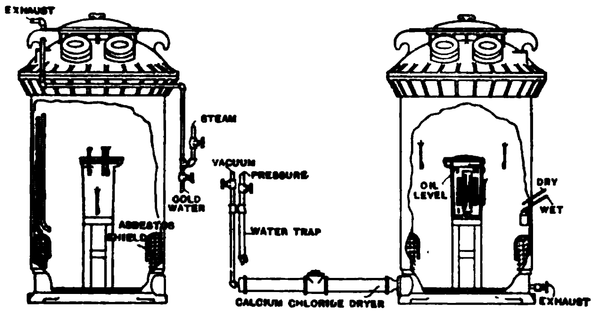


FIG. 16
(Both tanks are arranged alike.)

paper, it is recommended that the following corrections be made and precautions taken:

I—*Corrections for variation in room temperature.*

(i) Air-cooled natural draft—no correction.

(ii) Self-cooled oil-insulated—no correction.

(iii) Forced oil circulation—no correction.

(iv) Water-cooled apparatus—no correction, but precaution should be taken to have the ingoing water at as nearly as possible the same temperature as the room, which should be as near 25 deg. cent. as possible.

While in (i), (ii) and (iii) there will be a correction for variation in room temperature from standard, this correction will depend entirely upon the relative effectiveness of the emissive factors, and in order to apply any such correction accurately it would be necessary to divide such apparatus into a large number of groups. It is safe to assume that the increased losses will be about compensated for by the increased effectiveness of dissipation and make no corrections whatever.

Of course, wherever possible, the apparatus should be tested under the same conditions as it is to operate under, or under conditions as nearly as possible approaching these.

(v) Air-blast apparatus. A correction of ½ of one per cent per degree variation from 25 deg. cent., the correction to be added to the observed; use when the reference temperature is below 25 deg. cent. (This correction is based on ratio of core loss to copper loss of 1:1 and on all the heat being dissipated by convection).

The reference temperature in this class of apparatus to be temperature of ingoing air where this differs from temperature of room in which apparatus is operating.

II—*Correction for Variation in Atmospheric Pressure.*

(i) Air-cooled, one per cent correction for every 10 mm. variation from 760 mm. (This was obtained by assuming curve II, Fig. 11, as a straight line between 400 mm. and 1000 mm.).

(ii) Same as (i).

(iii) (a) External air-cooled radiators—same as (i). (b) Water-cooling coils. No correction.

(iv) Water-cooled oil-insulated. No correction.

(v) Air-blast apparatus. A correction of 2 per cent for every 10 mm. variation from 670 mm. (This was calculated from curve III, Fig. 11, assuming it to be a straight line between 400 mm. and 1000 mm.).

III—*Correction for humidity variations in atmosphere.*

(a) At and below saturation at ordinary room temperatures, no correction need be made for moisture.

(b) When fog-laden air is used the apparatus should be given a special rating, the specifications covering temperature rise when operating under such conditions.

A paper presented at the Midwinter Convention of the American Institute of Electrical Engineers, New York, February 26, 1913.

EFFECT OF ROOM TEMPERATURE ON TEMPERATURE RISE OF MOTORS AND GENERATORS

BY MAXWELL W. DAY AND R. A. BEEKMAN

Results of commercial tests on electric motors for the last few years, have given rise to a feeling of doubt concerning the correctness of the A. I. E. E. rule for correcting temperature rises for variations of room temperature from the standard of 25 deg.

In one particular case some motors were tested in the summer and easily met the specified heating limits, but when the customer installed them in the following winter, and tested them, some of the heating limits were exceeded, while on retesting them in the following summer the machines again easily met the specifications.

Arrangements were made, therefore, during the following winter, to test an enclosed motor in a room, the temperature of which could be controlled, and this motor was run for one hour, and also continuously at the following room temperatures: slightly below zero, 25 deg., and 30 deg. The results are given in Figs. 1 and 2, and show that the temperature rises in the cold room were, in most cases, more than those in the hot room. These figures show values of the rise on the different parts of the machine, with the ones obtained at the same room temperature joined by straight lines. Thus the slope of the lines is indicative only of the relative rises of the different parts under the same conditions, while the positions of the lines on the temperature scale show the relative rises under the different conditions, which is the object of our investigation.

These results seem to be reasonable, in view of the Stefan and

Boltzmann law that the radiation of heat is proportional to the fourth power of the absolute temperature. This law seems to have been ignored in many cases, but was discussed by Dr. Goldschmidt in his paper "Die Grundgesetze der Erwaermung elektrischer Maschinen", published in ***Elektrotechnische Zeitschrift***, of Sept. 10 and 17, 1908.

This same subject is treated further by Dr. Ludwig Binder in "Ueber Waermeuebergang auf ruhige oder bewegte Luft", published in 1911, in which he shows that the radiation of heat

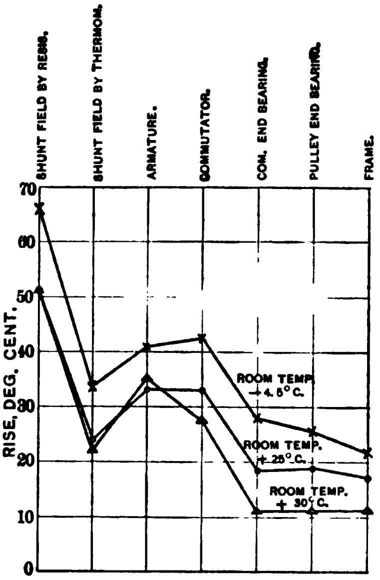


FIG. 1 — TOTALLY ENCLOSED MOTOR, 30 H.P., 550 REV. PER MIN.—52 AMPERES, 500 VOLTS—ONE-HOUR RUN.

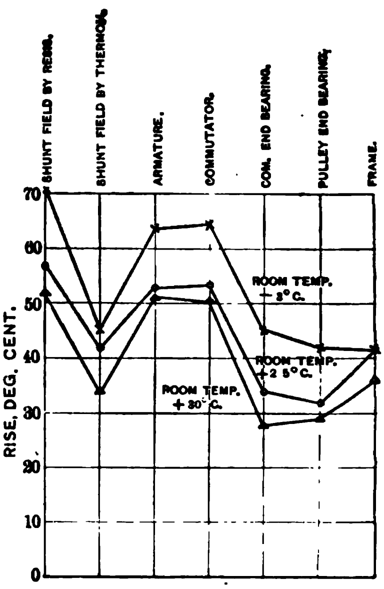


FIG. 2 — TOTALLY ENCLOSED MOTOR, 30 H.P., 550 REV. PER MIN.—30 AMPERES, 400 VOLTS—CONTINUOUS RUN.

increases 1.2 per cent for each degree of room temperature above ice zero, and 0.62 per cent for each degree of difference of temperature between motor and room. The radiation per degree difference between the motor and the room is 1.85 times as much with a room temperature of 20 deg. and a motor temperature of 100 deg., as if the room were 0 deg. and the motor one deg.

However, only a portion of the heat is carried away by radiation, as natural convection and forced ventilation must also be considered.

Considering the fact that a few tests, already made, indicated the incorrectness of the A. I. E. E. correction rule, which tests were confirmed by the papers mentioned, it was thought desirable to make some careful tests under conditions in which the room temperature could be controlled through a considerable range, and also to consider results already obtained on a large number of commercial tests, which might be comparatively inaccurate individually, but it was thought that with a sufficient number a definite tendency could be discovered.

It was not thought desirable, in the time available, to make a thoroughly scientific investigation to determine a new rule for temperature correction nor to determine the physical causes for these differences, but it was thought that some definite data could be obtained, which would be of interest in determining whether the present correction rule should be followed in the future, leaving for the future the scientific determination of a revised rule.

No attempt has been made in the following, to determine the effect of barometric pressure nor of humidity, although readings were taken during the special tests, but it is understood that moisture in the air in the form of vapor has but little effect on the temperature rises, although moisture in suspension, which is not evaporated before coming in contact with the machine, considerably reduces the temperature rise by absorbing heat from the machine through evaporation.

In the following we will first describe the special tests made in the enclosed room to determine the effect of room temperature, and later will give an analysis of a number of commercial tests made during the last few years.

Description of Special Tests

The special tests were run on two different types of direct-current motors. In each case the machine was a shunt motor with commutating field. The first set of tests was run on a motor rated at 7½ h.p., 825 rev. per min., with a normal current rating of 28.5 amperes. In the second set the motor was rated as 20 h.p., 700 rev. per min., with a normal current rating of 75 amperes.

In each case the normal voltage, 230, was held on the motor armature and commutating field in series, and also across the shunt field. The input into the motor was held constant at two values for each machine, viz.: 28.5 and 14.25 amperes for the first set of tests, while 75 and 37.5 amperes were the values for the second set. The conditions under which these two values were held will be given later.

The tests were run in a room approximately 16 by 14 ft. (4.9 by 4.3 m.). (See Fig. 3). Within this room is located an oven generally used for baking coils, the size of which is approximately 11 by 7 ft. (3.4 by 2.1 m.). In this oven are iron pipes, through which steam is ordinarily passed to gain high temperature. These coils were used in these tests for both heating and cooling purposes. In heating, steam was passed through them; in cooling, city water, at a temperature of approximately 13

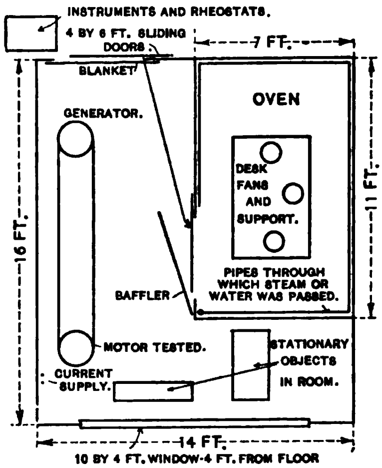


FIG. 3.—SKETCH OF TEST ROOM.
Brick walls—corrugated sheet iron ceiling—height of room 8 ft.

deg., was used to keep the room temperature as low as possible. Air was kept moving past these coils by the use of three ordinary desk fans.

This oven has a sliding door about 4 by 6 ft. (1.2 by 1.8 m.) and in the case of the hot room test the temperature of the room proper was maintained very close to 45 deg. by means of regulating the opening of this oven door. Between the motor under test and the oven door a wooden baffler was placed, in order to prevent any direct currents of air from the oven playing upon the motor.

Access to the room is obtained by means of a second sliding door of the same size as that of the oven. This door was kept closed throughout the test, except when it was necessary for the men to pass in and out. In order to prevent this passing in and out from affecting the temperature of the room a blanket was hung in front of the door, and so arranged that the openings were "staggered". The room contains one large window, approximately 10 by 4 ft. (3 by 1.2 m.), reaching from the ceiling to within 4 ft. (1.2 m.) of the floor; this window is of ground glass and was kept closed throughout the tests.

The apparatus used in testing, located in the room, consisted of the motor under test belted to a generator for load, starting box, circuit breakers, and of course, thermometers. The rheostats and instruments were placed outside of the room, so that no heat was developed in the room, except the losses of the motor and generator. There are some stationary objects located in the room, consisting chiefly of wood.

Due to the fact that no convenient place could be obtained for conducting these tests near a suitable direct-current generator, from which constant voltage could be obtained, it was necessary to run the test motor from the 250-volt shop circuit. It was found by a preliminary record of a recording voltmeter, that this voltage varied at times, by an amount as great as 20 volts. It was therefore necessary to take steps to maintain constant voltage on the motor. This was done by placing a rheostat in series with the motor and the supply. This rheostat was, as stated above, placed outside of the room and was of the type which is controlled remotely by solenoids. The control for this rheostat was placed directly on the testing table by the voltmeter indicating the motor voltage, and for part of the tests a man was kept constantly varying this in order to hold the rated (230) volts on the motor. For the remainder of the tests, however, a contact-making voltmeter was used to control the solenoids on the rheostat, and thus maintain constant voltage.

Besides the original indicating voltmeter record, a recording voltmeter record of the motor voltage was obtained during those tests in which manual control was used. This compelled constant attention on the part of the test men, and hence insured more accurate results.

As stated previously, in the case of the hot room tests the room temperature was maintained very nearly constant at 45 deg., but in the case of the cold room test our object in each case, was to have as cold a room temperature as possible after the temperatures had become constant; *i.e.*, when the heat run was ready to be taken off. Hence, while in the hot room test the room temperature was constant almost from the first, this is not true in the cold room test; but the temperature of the room rose, due to the losses of the motor and generator, until at the end, when constant conditions were obtained, it was usually considerably higher than at the beginning of the test.

It was found, by comparing the thermometers, that they differed considerably one from the other, and hence all were calibrated with two which read alike, chosen as standard. In order to eliminate the necessity of applying a correction to each thermometer reading, the same thermometers were used to read temperatures on the same parts of the machine throughout the test. In the case of the thermometers on the stationary parts, nearly all were left in the same positions from the start; the only ones removed were those on the pulley end shield and these were removed only when conditions were changed by removing the fan. In placing the thermometers on the rotating parts at the end of the run the same thermometers were also used, and the men attempted to place these exactly in the same positions.

The instruments used in the test were carefully calibrated before and after the runs, and practically no change was found, so that the relative readings were accurate.

The tests on the 7½-h.p. motor were run under six conditions:

1. Open motor with fan; that is, the motor was provided with a fan on the pulley end and the commutating end completely open to the air.

2. Open motor without fan. In this case the motor was exactly the same, except that the fan on the pulley end was removed.

3. Semi-enclosed motor with fan. Under these conditions, with a fan on the pulley end, a ventilating cover was placed over the commutating end.

4. Semi-enclosed motor without fan. The fan was removed and the same ventilating cover was used as in the preceding.

5. Totally enclosed motor with fan. In this case the ventilating cover was replaced by a totally enclosing cover and the openings in the pulley end shield were closed.

6. Totally enclosed motor without fan. The conditions were the same as in the fifth run except with the fan removed.

The tests on the 20-h.p. motor were run only under three of the above conditions, viz.: first, third, and fifth; in other words, in the case of the latter motor we did not remove the fan.

The input into the 7½-h.p. motor was held at 28.5 amperes during the tests run under the first four conditions and at 14.25 amperes during those run under the last two, as the temperatures would have been dangerously high had the normal rated current been held when running as a totally enclosed machine. The input into the 20-h.p. motor was held at 75 amperes during the tests run under the open and semi-enclosed conditions, and at 37.5 amperes during those run under the totally enclosed conditions.

It was the original intention to run four tests under each of the different conditions, two at the cold room and two at the hot room temperature. The check tests at the same conditions were really the same tests with a short interval of time between. That is, the motor was run under one fixed condition until the temperatures were constant; it was then shut down and final readings taken. Then it was immediately started up and the check run put on under the same condition.

This plan was followed, giving 24 tests on the 7½-h.p. motor, and at the end, due to the confusing results on the open motor with fan and the semi-enclosed motor without fan, it was thought necessary to repeat the tests under these conditions. In the case of the open motor with a fan all four tests were repeated, but in the case of the semi-enclosed motor without fan only three check tests were run, because the results of the third at the hot room temperature checked very closely with the two previous ones.

The list of the tests made on the 7½-h.p. motor is as follows:

	Hot room	Cold room
Open motor with fan	4 runs	4 runs
Open motor without fan	2 runs	2 runs
Semi-enclosed motor with fan	2 runs	2 runs
Semi-enclosed motor without fan	3 runs	4 runs
Totally enclosed motor with fan	2 runs	2 runs
Totally enclosed motor without fan	2 runs	2 runs

The list of the tests made on the 20-h.p. motor is as follows:

	Hot room	Cold room
Open motor with fan	2 runs	2 runs
Semi-enclosed motor with fan	2 runs	2 runs
Totally enclosed motor with fan	2 runs	2 runs

At the beginning of the tests great care was taken in measuring the cold resistance of the shunt fields. The readings were taken after the motors had been in the room a long enough time to allow the temperatures on the motor parts to become the same as that of the room. These cold resistance readings were taken on different days, at different room temperatures. The results, from which the resistance at 25 deg. was calculated, checked within 0.3 of one per cent.

In every case the heat runs were continued until the field readings showed a constant resistance for at least two hours, and the thermometers on the different parts also showed constant temperatures throughout the same period.

Precautions. Following are given precautions which were taken to secure accurate results and obtain as nearly as possible laboratory conditions. Some of these precautions have been given in the description above, but it is thought best to repeat them under this heading.

1. Recording voltmeter record of the motor voltage was kept.
2. Precautions were taken to eliminate drafts and sudden changes of room conditions.
3. Thermometers were kept in the same relative positions.
4. The data for any one run were immediately taken from the men running the test, so that their readings during the check run would not be influenced by their knowledge of values previously obtained.
5. The instruments were calibrated before and after the tests to determine if any change had taken place in their errors.
6. The same men performed the same tasks, such as reading the thermometers, and regulating the voltage, in order to eliminate, as far as possible, personal errors.
7. All final temperature readings were taken by the same men; that is, no run was taken off by the night force.
8. Four thermometers were read in different positions in the room, relative to the motor, two being directly in the open air and two being immersed in oil.

Results

Special Test. Figs. 4 to 12.

A. 7½-h.p., 825-rev. per min., 230-volt, 28.5-ampere motor.

1. Open motor without fan. Run at rated load until conditions were constant. Two runs at 23 deg. room temperature; one at 44 deg. and one at 45 deg.

Looking at Fig. 4, the results are plotted in the same way as in Figs. 2 and 3. We have averaged the results for the cold room runs and also for the hot room runs, inasmuch as the different tests under these conditions varied only a very little in the value of room temperature, and it was thought that an average of the rises would mean more than curves with check points plotted on them. The same applies to the remaining curves for the special tests.

All parts show lower rise at hot room temperature, except the commutator, which became quite rough between runs.

Correction to be applied to the rise per degree difference of room temperature from 25 deg. varies from −0.193 per cent to −0.754 per cent, for the different parts of the machine. The minus sign is used to indicate those corrections which show a

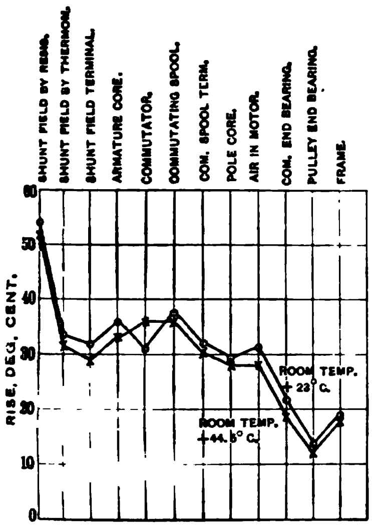


FIG. 4—OPEN MOTOR WITHOUT FAN—7½ H.P., 825 REV. PER MIN., 230 VOLTS.

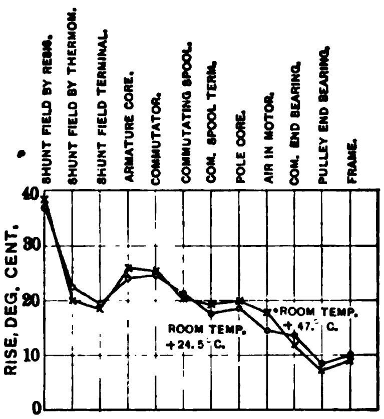


FIG. 5—OPEN MOTOR WITH FAN—7½ H.P., 825 REV. PER MIN., 230 VOLTS.

lower rise at hot room temperature, and a positive sign the opposite. These signs are assigned on the basis of the slope of the curve of rise against room temperature.

2. Open motor with fan. (See Fig. 5.) Run at rated load until conditions were constant. Three runs at practically 24 deg.; one at 26 deg.; two at 44 deg. and one at 47 deg. Results not in the same direction for the different parts.

Shunt field by resistance........	Inconsistent
Shunt field by thermometer......	Rise less at hot room temp.
Armature core and conductor....	Inconsistent.
Commutating spool.............	Inconsistent.

Commutator..................	Inconsistent.
Pole core.....................	Inconsistent.
Frame.......................	Inconsistent.
Pulley end bearing..............	Inconsistent.
Commutator end bearing........	Rise less at hot room temp.

By "inconsistent" is meant that results point in no definite direction, due, probably, to difference between actual temperature rises being less than experimental error.

Corrections not calculated.

3. Semi-enclosed motor without fan. (See Fig. 6.) Run at rated load until conditions were constant. Three runs at

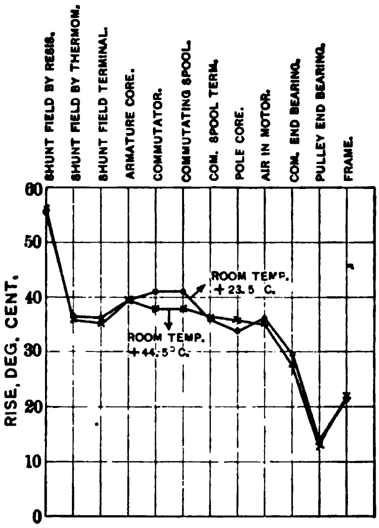


FIG. 6—SEMI-ENCLOSED MOTOR WITHOUT FAN—7½ H.P., 825 REV. PER MIN., 230 VOLTS.

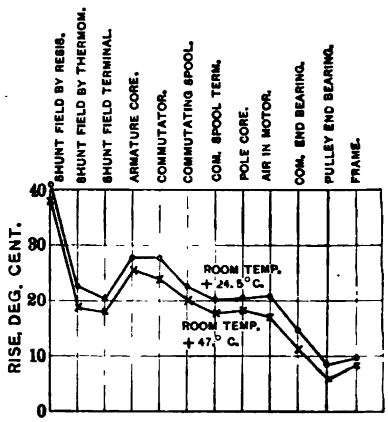


FIG. 7—SEMI-ENCLOSED MOTOR WITH FAN—7½ H.P., 825 REV. PER MIN., 230 VOLTS.

practically 24 deg.; one at 23.5 deg.; two at practically 44 and one at 45 deg. Results not in the same direction for the different parts.

Shunt field by resistance........	Inconsistent.
Shunt field by thermometer......	Inconsistent.
Armature core.................	Inconsistent.
Commutating spool.............	Rise less at hot room temp.
Commutator..................	Inconsistent.
Pole core.....................	Rise less at cold room temp.
Frame.......................	Inconsistent.
Pulley end bearing.............	Inconsistent.
Commutator end bearing........	Rise less at hot room temp.

Corrections not calculated.

4. Semi-enclosed motor with fan. (See Fig. 7.) Run at rated load until conditions were constant. Two runs at practically 25 deg.; one at 47; one at 47.5. All parts show lower rise in hot room.

Correction to be applied to rise per degree difference of room temperature from 25 deg. varies from −0.216 per cent to −1.36 per cent.

5. Totally enclosed motor without fan. (See Fig. 8.) Run at one-half rated load until conditions were constant. Two runs at practically 20.5 deg. and two at practically 45 deg. All parts show lower rise at hot room temperature.

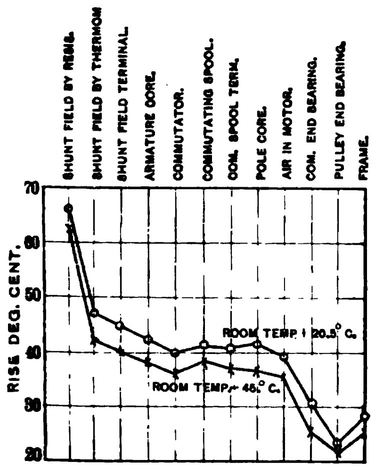


FIG. 8—MOTOR TOTALLY ENCLOSED WITHOUT FAN—7½ H.P., 825 REV. PER MIN., 230 VOLTS.

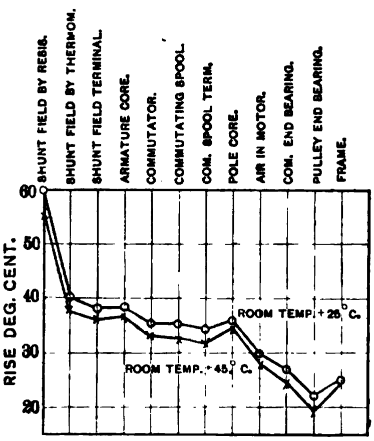


FIG. 9—MOTOR TOTALLY ENCLOSED WITH FAN—7½ H.P., 825 REV. PER MIN., 230 VOLTS.

Correction to be applied per degree difference of room temperature from 25 deg. varies from −0.233 per cent to −0.785 per cent.

6. Totally enclosed motor with fan. (See Fig. 9.) Run at one-half rated load until conditions were constant. Two runs at 25 deg.; two at 45 deg. All parts show lower rise at hot room temperature.

Correction to be applied to rise per degree difference of room temperature from 25 deg. varies from −0.164 per cent to −0.696 per cent.

It may be well to add that we also tested the open 7½-h.p.

motor with the entire frame covered with asbestos lagging, in order to eliminate radiation losses, as far as possible, and the results were not sufficiently different from the open motor without the asbestos lagging, to warrant our including them in this paper.

B. 20-h.p., 700-rev. per min., 230-volt, 75-ampere motor with fan.

1. Open motor. (See Fig. 10.) Run at rated load until conditions were constant. One run at 25.5 deg.; one at 29; and two at 41.5 deg. All parts except armature core and commutating spool show lower rise at hot room temperature.

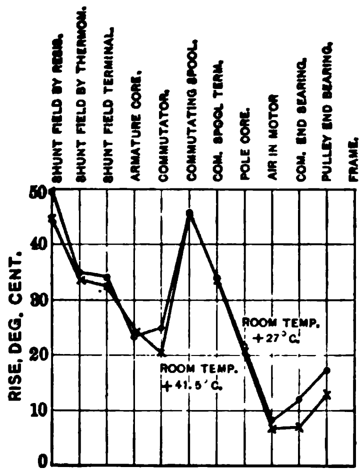


FIG. 10—OPEN MOTOR WITH FAN—20 H.P., 700 REV. PER MIN., 230 VOLTS.

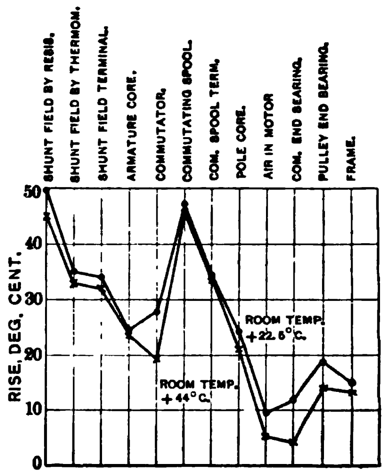


FIG. 11—SEMI-ENCLOSED MOTOR WITH FAN—20 H.P., 700 REV. PER MIN.

Correction to be applied to rise per degree difference of room temperature from 25 deg. varies from −0.152 per cent to −1.25 per cent, for all parts except those mentioned.

2. Semi-enclosed. (See Fig. 11.) Run at rated load until conditions were constant. One run at 21 deg.; one at 24 and two at 44. All parts, except armature core and commutating spool, show less rise at the hot room temperature.

Correction to be applied to rise per degree difference of room temperature from 25 deg., varies from − 0.314 per cent to −3.19 per cent.

3. Totally enclosed motor. (See Fig. 12.) Run at one-half

rated load until conditions were constant. One run at 18 deg.; one at 20 deg.; one at 43 and one at 42.

All parts show lower rise at hot room temperature.

Correction to be applied to rise per degree difference of room temperature from 25 deg. varies from − 0.108 per cent to − 0.916 per cent.

Commercial Tests. In the case of these tests, the data on a great many machines of like rating and condition of heat run, were examined, and as a first step the rises of the different parts were plotted against the room temperatures. Examples of this

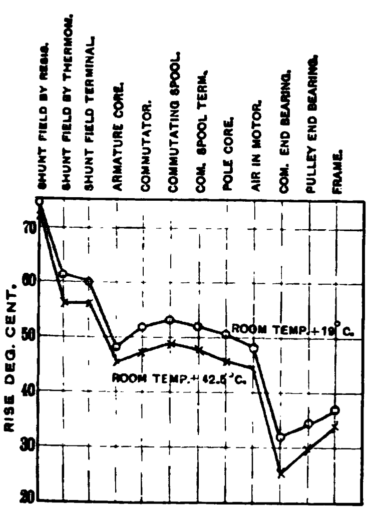


FIG. 12—TOTALLY ENCLOSED MOTOR WITH FAN—20 H.P., 700 REV. PER MIN.

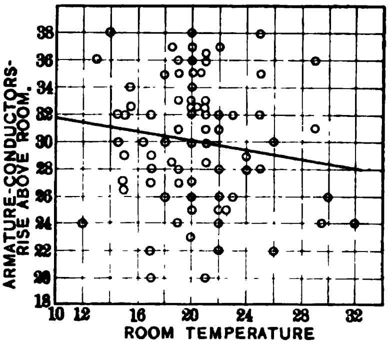


FIG. 13—ARMATURE CONDUCTORS—98 MACHINES, 3-H.P., 125 VOLTS, 22 AMPERES, 400 REV. PER MIN.

may be found in Figs. 13 and 15 if we disregard the straight line. The procedure of analyzing the results is as follows:

We have given certain observed values of temperature rise in degrees cent. on different parts of electrical machines of the same type, rating and duration of run, at different room temperatures given in degrees cent.

Considering any one part, the second step taken was to obtain an average of the observed values of rise at each of the different observed values of room temperature. This will then give us a single value of rise for each room temperature, which, when considered with the total number of observations used to get this average, will be equivalent to all the observed values of rise, at any one room temperature, considered individually. An example of this is given in Fig. 14, where any one point represents the average of the number of observations used to determine that point.

We then proceeded by making the following assumptions:

1. That there is a true value of temperature rise based on a standard room temperature of 25 deg.

2. That a change of temperature rise, due to a different room temperature, is proportional to a certain percentage of the difference between that room temperature and 25 deg.

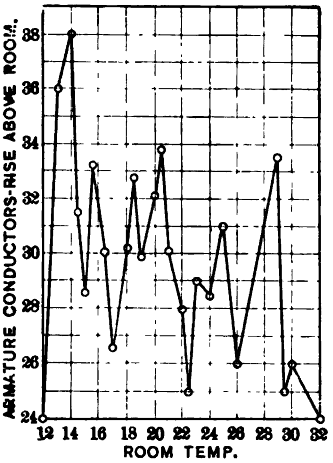


FIG. 14—ARMATURE CONDUCTORS—98 MACHINES, 3-H.P., 125 VOLTS, 22 AMPERES, 400 REV. PER MIN.

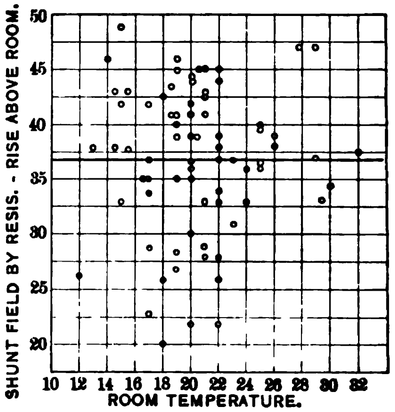


FIG. 15—SHUNT FIELD BY RESISTANCE—89 MACHINES, 3-H.P., 125 VOLTS, 22 AMPERES, 400 REV. PER MIN.

3. That the actual temperature rise at any room temperature is equal to the true rise at 25 deg. plus a certain percentage of that rise, based on the differences of actual room temperature from 25 deg. plus an error of observation, or accidental variation of test conditions.

4. That each value has a weight corresponding to the number of observations.

5. That the algebraic sum of all the errors is equal to zero.

6. That, from mechanical analogy, each average of observed values, multiplied by the number of observations giving that average, and again multiplied by its distance from the line (*i.e.*

its error) is a force pulling the line toward itself; the lever arm of said force being equal to the observed room temperature minus 25 deg. cent., and that the line which we are seeking to represent the mean of observed values of temperature rise at different room temperatures, is in equilibrium when all the moments of these forces, obtained by multiplying them into said lever arm, are jointly equal to zero.

Then, let

t_1, t_2 etc. = the observed values of room temperature.

B_1, B_2 etc. = the average of observed values of temperature rise at t_1, t_2, etc.

N_1, N_2 etc. = the number of observations for any one average of observed values.

a, b, c etc. = the error of observation of B_1, B_2, etc.

A = true value or average rise, based on 25 deg. room temperature.

x = the correction per degree difference from 25 deg. to be applied to the rise.

Then

$$B_1 = A + (t_1 - 25)\,x\,A + a$$
$$B_2 = A + (t_2 - 25)\,x\,A + b$$
$$B_3 = A + (t_3 - 25)\,x\,A + c$$

and so on. Further

$$a = B_1 - A\,\{1 + (t_1 - 25)\,x\}$$
$$b = B_2 - A\,\{1 + (t_2 - 25)\,x\}$$

Or in general, dropping the subscripts,

$$\text{an error} = B - A\,\{1 + (t - 25)\,x\}$$

Referring to assumption 5, we must take care, under our system of averages, to multiply or weight each error by the number of observations used to get the average error, and so, in accordance with the said assumption,

$$\Sigma N\,[\,B - A\,\{1 + (t - 25)\,x\}\,] = 0$$

In accordance with assumption 6, we then have

$$\Sigma N\,[\,B - A\,\{\,1 + (t - 25\text{ deg.})\,x\}]\,(t - 25) = 0$$

We then have two simultaneous equations in A and x, and hence can solve for these constants and obtain the desired line.

After using the above method we discovered an application of the method of least squares given by Weisbach in *Mechanics of Engineering*, pages 95 to 98, applicable to this problem. We have proved to our satisfaction that our method and the method of least squares, as quoted above, are identical.

In accordance with this procedure, we have then added to Figs. 13 and 15 the lines which represent the mean of observed values of temperature rise at different room temperatures.

In the following, the rating and conditions under which the heat run was made, the number of machines tested under similar conditions, and the variation of room temperature will be given. Also the variation of the correction to be applied, based on the rises for the main parts, will be recorded. The parts on the direct-current machines, whose rises were reduced to a mean slope, and corrections calculated, are:

Shunt field by resistance.
Shunt field by thermometer.
Armature core.
Commutator.

On the alternating-current machines:

Armature conductors by thermometer.
Armature conductors by resistance.
Armature core.
Pole core.

In the one case, where induction motors were investigated, only rises on the stator conductors by resistance and by thermometer were reduced to a mean slope.

A. 3-h.p., 125-volt, 400/535-rev. per min. direct-current totally enclosed motors with a two-hr. heat run at normal load.

1. Run at 535 rev. per min. All parts to which our method was applied show a negative slope, varying from − 0.959 per cent to − 2.75 per cent.

The above values are based on 79 machines, with the room temperature varying from 13 to 26 deg.

2. Run at 400 rev. per min. All slopes negative, varying from − 0.014 per cent to − 0.481 per cent.

The above values are based on 89 machines, with the room temperature varying from 12 to 32 deg.

B. 22-h.p., 650/1050-rev. per min., 120-volt, direct-current, semi-enclosed motors.

The runs were made until constant. There were 28 machines, with the room temperature varying from 23 to 34 deg. The rises of the shunt field and commutator gave a slope of from − 0.601 per cent to − 0.661 per cent, while the armature core gave a slope of + 0.094 per cent.

C. Direct-current, open motors; run under normal load until constant.

1. 20-h.p., 1100-rev. per min., 220-volt motors. Here we found 57 machines with the room temperature varying from 15 to 34 deg., which gave corrections for shunt field of − 0.066 per cent by resistance, and − 0.382 per cent by thermometer, while the armature core gave + 0.302 per cent and the commutator + 1.2 per cent.

2. 35-h.p., 600-rev. per min., 220-volt motors. Runs were obtained from 41 machines, with room temperature varying from 13 to 32 deg. Here the shunt field by resistance gave a correction of + 1.04 per cent; while the shunt field by thermometer gave − 1.84 per cent; the armature core − 2.03 per cent and the commutator + 0.118 per cent.

3. 60-h.p., 550-rev. per min., 220-volt motors. Twenty-five machines with a variation of room temperature from 12 to 31 deg., gave a correction for the shunt field and armature core of from − 0.241 to − 2.171 per cent; while the commutator gave + 0.993 per cent.

D. Single-phase, alternating-current generators. Run under normal load until constant.

1. 90-kw., 900-rev. per min., 60-cycle, 2300-volt form "B" generators. Thirty-nine machines, with room temperature varying from 18 to 29, gave all negative corrections, varying from − 0.055 per cent to − 1.46 per cent.

2. 90-kw., 900-rev. per min., 60-cycle, 2300-volt form "A" generators. Thirty-nine machines, with room temperature varying from 18 to 29 deg., gave corrections varying from − 0.032 per cent to − 2.178 per cent.

3. 120-kw., 900-rev. per min., 60-cycle, 2300-volt generators. Under this rating we found 41 machines with room temperature varying from 16 to 35 deg. All parts, except armature conductor by resistance, gave a negative correction varying from − 0.87 per cent to − 1.224 per cent; while the armature conductors by resistance gave + 0.607 per cent.

4. 120-kw., 1070-rev. per min., 125-cycle, 2300-volt generators. Thirty machines, with room temperature varying from 15 to 33 deg., gave corrections varying from − 0.7 per cent to − 2.47 per cent.

E. 1-h.p., 1800-rev. per min., 60-cycle, 110-220-volt, induction motors. Run under normal load until constant.

In the case of induction motors we have but two values of slope; one for the stator conductors, by thermometer, of − 1.21 per cent, and the other for stator conductors, by resistance, of

– 0.56 per cent. This is based on 33 machines, with a room temperature varying from 13 to 30 deg.

F. 150-h.p., 400/600-rev. per min., 220-volt, direct-current motors.

Tests on these machines are very recent ones; in fact, they have not as yet been completed, and we include them because of the marked difference in the runs at different room temperatures. There are only eight runs on four different machines, run at normal load (400 rev. per min.) for eight hours, and we do not consider these tests in the class of accurate ones. The room temperature varied from 21 to 39 deg. The corrections varied from – 0.278 per cent to – 2.78 per cent.

TABLE I

VALUES OF CORRECTION IN PER CENT

SPECIAL TESTS

Part	7½-h.p. motor				20-h.p. motor—W. F.		
	O. No F.	S. E. W. F.	T. E. No F.	T. E. W. F.	T. E.	S. E.	O.
Shunt field by resistance	–0.193	–0.318	–0.233	–0.244	–0.108	–0.417	–0.64
Shunt field by thermometer	–0.333	–0.678	–0.477	–0.364	–0.375	–0.314	–0.244
Shunt field terminal	–0.412	–0.495	–0.443	–0.164	–0.328		–0.291
Armature core	–0.272	–0.310	–0.405	–0.226	–0.248		
Commutating spool	–0.215	–0.392	–0.305	–0.416	–0.345		
Commutating spool terminal	–0.249	–0.502	–0.350	–0.366	–0.332		
Commutator	*	–0.570	–0.384	–0.366	–0.238	–1.35	–1.25
Pole core	–0.103	–0.216	–0.495	–0.209	–0.427	–0.577	–0.341
Commutator end bearing	–0.754	–1.36	–0.785	–0.334	–0.916	–3.19	–2.75
Pulley end bearing	–0.716	–1.32	–0.282	–0.696	–0.536	–1.23	–0.152
Frame	–0.410	–0.72	–0.514	–0.239	–0.300	–0.761	–0.283
Number of tests	4	4	4	4	4	4	4
Range of room temperature in deg. cent	23 to 45	25 to 47	21 to 45	25 to 45	18 to 43	21 to 44	25 to 42

* Corrections not calculated because data were considered incorrect due to rough commutator. Other blank spaces indicate that on these parts the data pointed in no definite direction and it was not considered advisable to apply the least square method to so small a number of tests.

A general summary of the corrections found, is given in Tables I and II. An explanation of the abbreviations used will probably be necessary.

0 is used to represent open motor.
S.E. " " " " semi-enclosed motor.
T.E. " " " " totally enclosed motor,
No F. " " " " motor without fan.
W.F. " " " " motor with fan.

TABLE II
VALUES OF CORRECTION IN PER CENT
COMMERCIAL TESTS

Part	3 h.p. T. E.		22 h.p. S. E.	20 h.p. O.	35 h.p. O.	60 h.p. O.	150 h.p. O.
	400 r.p.m.	535 r.p.m.	650 r.p.m.	1100 r.p.m.	600 r.p.m.	550 r.p.m.	400 r.p.m.
Shunt field by resistance	−0.014	−2.75	*	−0.066	+1.04	−2.17	−2.78
Shunt field by thermometer	−0.264	−1.03	−0.661	−0.382	−1.84	−1.24	−0.278
Armature core	−0.187	−0.959	+0.094	+0.302	−2.03	−0.241	−0.212
Commutator	−0.481	−1.09	−0.601	+1.20	+0.118	+0.993	
Number of tests	89	79	28	57	41	25	8
Range of room temperature	12 to 32	13 to 26	23 to 34	15 to 34	13 to 32	12 to 31	21 to 39

Part	Alternators				Ind. Motor 1 h.p. O
	90 kw. O-B	90 kw. O-A	120 kw. O-A	120 kw. O-A	
	900 r.p.m.	900 r.p.m.	900 r.p.m.	1070 r.p.m.	1800 r.p.m.
Armature conductors (by res.)	−0.055	−2.178	+0.607	−0.700	
Armature conductors (by ther.)	−1.46	−0.032	−1.224	−2.44	
Armature core	−1.35	−0.991	−1.09	−2.46	
Pole core	−1.00	−0.407	−0.870	−2.47	
Stator conductors (by res.)					−0.557
Stator conductors (by ther.)					−1.21
Number of tests	39	39	41	30	33
Range of room temperature	18 to 29	18 to 29	16 to 35	15 to 33	13 to 30

* Data not reliable. Other blank spaces indicate that such parts are not found on the particular machines or that such parts were not investigated.

In these tables we have given the different parts with the value of correction found for each part for each type of motor, the number of tests, and the range of room temperature.

We have not included in this table the results on the 7½-h.p.

open motor with fan and semi-enclosed motor without fan, results of which, we have said before, showed great inconsistency, and since there were only a few tests, we did not consider it advisable to apply the method used to determine the line of mean slope, that was used in the case of the large number of commercial tests.

Tables I and II represent a total of 2211 values of observed temperature rise on the various types of machines. Of this number 1921 show a negative correction, while 290 show a positive. In other words, about 86.9 per cent indicate that the temperature rise on motors and generators is less in a hot room than in a cold one, while 13.1 per cent indicate the opposite.

The average of all these values of correction is – 0.7 per cent.

In these tests no account has been taken of the temperature of the walls of the room as compared with the temperature of the air. In hot weather the temperature of the walls might be about the same as that of the surrounding air, but in cold weather the walls of a heated room would be of lower temperature than the surrounding air and might affect the direct radiation of the motor to some extent. This matter will be investigated later.

It should be noted that the field rise as determined by resistance falls less with increased room temperature than the reduction of heat dissipated; *i.e.*, if the energy dissipated by the field coil was maintained constant, the field coils would probably show a higher rise of temperature in a hot room. This condition is expected to exist in case of generators.

Conclusions

We believe that the information submitted warrants us in drawing the following conclusions:

1. The present correction rule is wrong and should be abrogated.

2. The considerable variations obtained show the difficulty of making a rule that will include all types of machines or even all parts of the same machine.

3. A consideration of the total temperature obtained when working under normal conditions in the maximum room temperature, is of more importance than the rise of temperature.

4. Further tests leading to a determination of a correct rule, if possible, are desirable.

A paper presented at the Midwinter Convention of the American Institute of Electrical Engineers, New York, February 26, 1913.

EFFECT OF AIR TEMPERATURE, BAROMETRIC PRESSURE AND HUMIDITY ON THE TEMPERATURE RISE OF ELECTRIC APPARATUS

BY C. E. SKINNER, L. W. CHUBB AND PHILLIPS THOMAS

Heat is dissipated from any given piece of electrical apparatus through conduction, convection and radiation. The proportion dissipated through conduction and radiation is usually quite small, although heat is readily conducted from one part of a machine or apparatus to other parts and to supports in contact. The heat dissipated by convection, as a rule, takes care of the major part of the loss.

The present Standardization Rules of the Institute, under the heading of "Temperature Correction" (Section 269), read as follows: "If the room temperature during the test differs from 25 deg. cent., correction on account of difference in resistance should be made by changing the observed rise of temperature by one-half per cent for each degree centigrade." This correction is required, apparently, on the assumption that the increase in resistance of the copper windings is the controlling feature causing such variation.

The factors which affect the variation in temperature rise in a piece of electrical apparatus include the following:

First: Variation in resistance with temperature. The resistance of the copper increases with temperature and the correction for copper loss should be plus or minus, depending on whether the current in a given winding is constant or whether the voltage across the winding is constant.

Second: Variation in iron loss with temperature. Other conditions being constant, iron loss decreases as temperature increases, due

a. To increased resistance reducing eddy-current losses.

b. Decreasing hysteresis losses.

Third: Variation in amount of heat radiated at different temperatures. Under ordinary conditions the amount of heat radiated will vary with the increase in difference between the temperature of the body from which heat is radiated and the temperature of the surrounding objects. The effect of radiation on temperature rise will depend on surrounding conditions, but as a rule there will be more heat radiated from higher temperatures than from lower temperatures.

Fourth: Variation in convection due to variation of viscosity of the cooling medium. There is probably not sufficient variation in the viscosity of air to warrant any correction whatever for viscosity in air-cooled apparatus. This feature becomes quite a factor in oil-cooled apparatus, where the fluidity of the cooling medium increases with temperature, and consequently, its ability to carry away heat by increased rapidity of circulation.

Fifth: Variation due to change in thermal conductivity of the air at different temperatures.

Sixth: Variation due to barometric pressure. It is to be expected that with decreased barometric pressure there will be increased temperature rise, and the results of a given set of tests under specific conditions, recorded later, indicate an increasing rise with decreased pressure.

Seventh: Variation due to humidity. It has been assumed that on account of the heat-carrying power of water vapor there should be a decreased rise due to increased humidity. This corrective factor is, however, probably very small.

Eighth: Variation in bearing friction, brush friction and windage.

Ninth: Variation depending on whether the apparatus is acting as a motor or generator at constant voltage, or whether it is operating at constant load, constant current or constant loss. Whether the total losses on a given piece of apparatus increase or decrease with temperature, depends on how the apparatus is operated and on the relation of the various segregated losses to each other. If operating as a generator with constant output the copper losses will increase with rise in air temperature and the iron losses may increase or decrease. If the iron loss predominates, the result may be a decrease in actual losses with increase in air temperature, giving a lower rise. If operating as a motor or transformer and connected to a constant voltage, increased air temperature may again either increase or decrease the total losses, and the rise, as in the case of a generator.

A number of tests indicating that the correction for temperature rise when the air temperature is higher than 25 deg. cent. should be negative instead of positive, as required by the Standardization Rules, has led to a set of tests being made to determine some of the fundamentals which govern temperature rise from different temperatures. It will readily be seen from the foregoing that the problem is an extremely complicated one and that it is necessary, therefore, to fix certain conditions and provide for the observing of one variable at a time, keeping the others constant if possible. The factors which were controlled in these tests were:

a. Radiation
b. Air temperature.
c. Barometric pressure.
d. Humidity.
e. Wind velocity.
f. Input.

All tests were made with a constant input to the test coil.

The apparatus selected for test was a small motor field coil, of about 9 ohms resistance at 20 deg. cent. The coil was about 6.35 cm. (2.5 in.) by 1.9 cm. (0.75 in.) in section, and was wound on a center block about 9 cm. (3.5 in.) by 5 cm. (2 in.). The block was removed before setting up for the tests. This coil was hung in the center of a closed cubical box, about 61 cm. (2 ft.) on a side. At two opposite ends of the box, connection was made with an air circulating pipe 30.5 cm. (1 ft.) in diameter, which formed a closed system with the box and a blower. The whole system was made as nearly air-tight as possible, and variation in internal pressure was secured by means of a motor-driven air pump used either to exhaust or compress the air in the system. A cubical sheet metal baffle was mounted inside the coil box, equipped with a fine mesh screen at the ends through which the air entered and left the coil space. About two inches (51 mm.) of free air space was left on all sides between the baffle and the coil box, in order to be sure that the temperature of the baffle wall and the air surrounding the coil should be the same. The screen across the ends of the baffle also extended across the clearance to the inside of the coil box. The pressure within the system was read on a differential mercury manometer communicating with the atmosphere outside; the external pressure was given by a carefully adjusted aneroid barometer. The velocity of the circulating air was measured by the cooling effect upon a small copper wire suspended directly beneath the coil, the air speed being calculated from the current necessary to maintain the resistance of this wire, between definite potential points, at a predetermined constant value.* The temperature of the coil, and of the air directly above and below the coil, was given by iron-advance thermocouples. The coil couple was mounted in contact with the covering of the wire on the extreme upper surface of the coil, which was subsequently given a serving of cotton tape and two coats of black armature varnish and

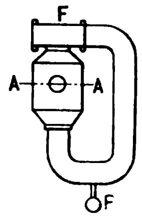


FIG. 1A—TOP VIEW OF APPARATUS.
F — Blower.
P — Air pump.

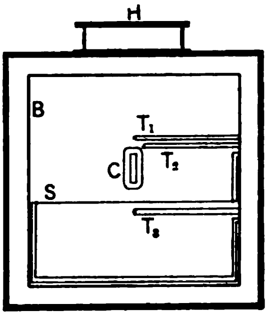
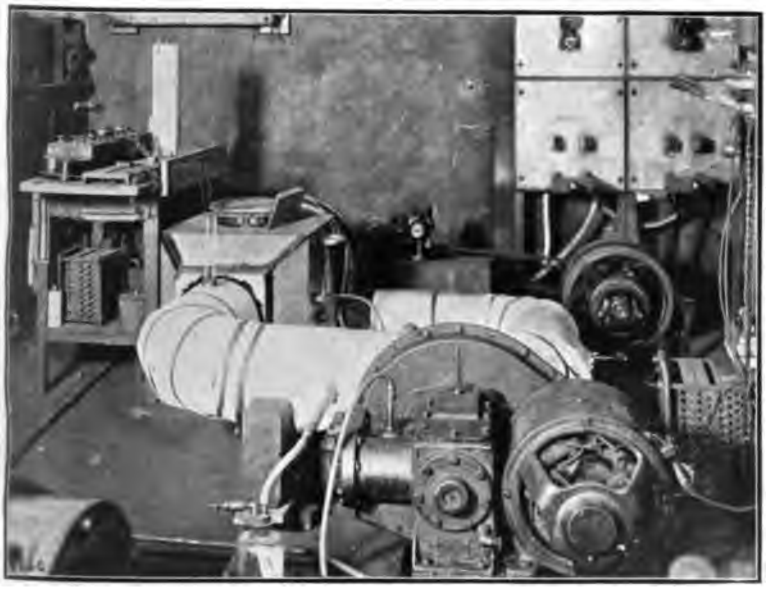


FIG. 1B—SECTION A-A, SCHEMATIC
C — Coil tested.
S — Speed wire.
T_1, T_2, T_3 — Thermocouples.
B — Air baffle.
H — Handhole for making adjustments.
Direction of flow of air is at right angles to plane of section.

baked dry. The relative humidity of the circulating air was determined from the readings of a wet and dry bulb thermometer, with the bulbs in the path of the air just before entering the coil enclosure. The temperature of the circulating air could be varied within wide limits by electric heater elements inserted in the air-pipe just after leaving the coil box, so that the heated and cooler air would be thoroughly mixed before again entering the enclosure. Fig. 1, schematic, shows the location of the coil, velocity wire and thermocouples, the leads from all of which were brought out through rubber corks in the sheet-iron covering of the coil box.

*Kennelly, Wright and Van Bylevelt, TRANSACTIONS A. I. E. E., 1909, XXVIII, I, p. 363.



[SKINNER, CHUBB AND THOMAS]

SHOWING APPARATUS USED IN MAKING TESTS.

The temperature elevation of an air-cooled coil is apparently influenced by so large a number of factors, that the futility of an attempt to determine the effect of each of them, in the time available, was apparent at the outset. The most important of the controlling factors are the temperature and pressure of the surrounding air. The major part of the work was then concentrated upon determinations of the effects of these two factors. Two sets of runs were made : First, with constant watts input in the tested coil, and constant pressure and speed of the circulating air; the temperature in the enclosure was varied by small steps from 30 to 64 deg. cent., and the temperature of the coil was taken at each point. Second, with constant watts input in the tested coil, constant temperature in the enclosure, and constant speed of air circulation, the air pressure in the enclosure was varied from 82.8 cm. (32.6 in.) to 53.4 cm. (21.0 in.) of mercury, in small steps, and the temperature rise of the coil was taken at each point. No readings were finally recorded at any point, until the instrument readings had all been constant for at least half an hour.

The current passed through the test coil was measured by a precision ammeter, and the difference of potential between the terminals of the coil was measured by a precision voltmeter connected to potential leads. Rough tests indicated that the temperature elevation of the coil was about 28 deg. cent. when carrying 32 watts, and the coil current and potential difference were kept such as to maintain this input as nearly constant as possible during the tests. The current through the speed wire was adjusted to give the same temperature elevation as in some previous work which has been presented before the Institute; this current was measured by a low-scale ammeter, and the potential across a length of the wire equal to that used in the same tests, was measured by a potentiometer.

Readings of the wet and dry bulb thermometers were made at each point in every test, with the intention of correcting for the slight variations in humidity unavoidably occurring; but a subsequent test, made with the air nearly saturated by blowing steam into the system, showed no significant change in temperature rise, and it was thought inadvisable to attempt any such corrections.

RESULTS

A. Tests at Constant Air Pressure and Speed. Fig. 2 shows in graphic form the results of these tests. Each point required several hours for its determination, so that the test as here recorded extended over several days. The curve between box temperature and coil temperature, plotted to the same scale, is plainly a straight line, within errors of observation and of adjustment of the controlling factors: the points as obtained do not indicate any regular deviation from a straight line. The intercept value of temperature rise as given by this line agrees closely with the average of the point values, which means that the temperature elevation is constant. It may be mentioned here that the readings on the speed wire, worked out by the

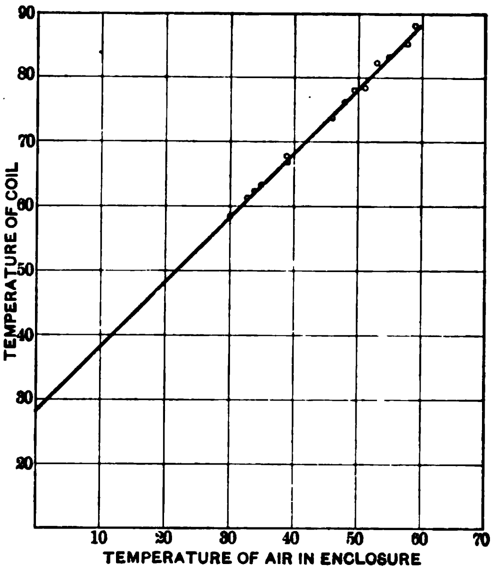


FIG. 2—VARIATION OF TEMPERATURE RISE OF COIL WITH CHANGING TEMPERATURE OF SURROUNDING AIR

equation given for the same size, length and temperature of wire, in the paper already mentioned, gave an air velocity of 146 cm. per sec. (3.27 miles per hour) for a blower speed of 789 rev. per min. This speed was read on a tachometer and was kept very closely constant during the entire test. The temperatures above and below the test coil proved to be identical, when the air was circulated at this speed. Table I gives the observational results from which the curve of Fig. 2 was plotted.

B. Tests at Constant Air Temperature and Speed. The results of these tests are shown in Table II, and graphically in Fig. 3. The column headed "coil input" shows the variations that occurred in this quantity during the test. In order to correct the temperature rise for this error, a test was subsequently run between watts input and temperature rise, at the same fan speed and box temperature as were employed in the present test, and at constant air pressure. The results showed the relation between coil temperature and coil input to be very nearly linear; the exact equation found was

$$W = K\theta^{1.015}$$

Accordingly a correction for variation in input was made at each point taken; the curve plotted in Fig. 3 is taken from

TABLE I

EFFECT OF VARIATION OF TEMPERATURE OF ENCLOSURE UPON TEMPERATURE RISE OF COIL

Couple temperatures, deg. cent			Coil input, watts	Blower speed rev. per min.
Air in box	Coil	Coil rise		
64	92	28	31.6	787
62.5	90.2	27.7	31.7	789
59.6	88	28.4	31.9	787
58	85.2	27.2	31.7	760
55	83	28	31.7	790
53.2	82.2	29	31.6	789
51	78.5	27.5	32.6	785
49.5	78	28.5	31.2	790
48.5	76.2	27.7	31.9	790
46.1	73.5	27.4	31.9	782
39.2	67	27.8	31.5	776
39.2	66.8	27.6	31.6	781
39	67.5	28.5	31.9	780
35	63.1	28.1	31.8	777
34	62.2	28.2	31.7	783
33	61	28	32	787
30	58.2	28.2	31.7	779

these corrected values. As this curve shows about a 13 per cent variation in temperature rise for a decrease of 15 cm. (5.9 in.) in atmospheric pressure, which corresponds to a change in altitude from sea level to 1.9 km. (6240 ft.) above sea level, the importance of determinations of the magnitude of this effect on finished electrical apparatus is at once apparent. It is possible that some of this increase in temperature rise, as the air pressure is decreased, may be due to changes in the air velocity at different pressures with the same speed; but operating conditions are much more nearly those of constant fan speed than of neoessarily constant air velocity.

Summary

The tests which have been completed at the time of writing this paper are:

First: The determination of the rise in temperature, all features being kept constant, except the temperature of the air and the surrounding walls.

Second: The determination of the variation in rise of temperature, all features being kept constant, except the barometric pressure.

Third: Some additional data were obtained showing variation of rise in temperature, all features being kept constant, except humidity of the surrounding air, but no difference in temperature rise due to variation in humidity was found.

Tests are under way to determine the rise in temperature,

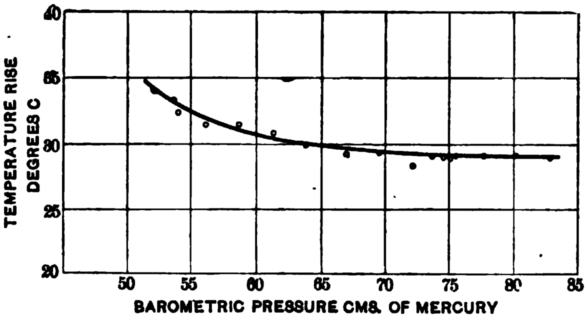


FIG. 3—VARIATION OF TEMPERATURE RISE OF COIL WITH BAROMETRIC PRESSURE

keeping all features constant, except the temperature of the surrounding air, with a view to determining the effect of varying radiation.

Results of the first set of tests are shown in Fig. 2. It will be seen that within the limits of error of observation and under the conditions of the test as made, a variation in the temperature of the air surrounding the coil does not affect the temperature rise. The slight difference in the amount of heat radiated from the coil at different temperatures, even though the surrounding walls were kept at air temperature, does not appear in the recorded results and must, therefore, be very small.

Results of the second series of tests are shown in Fig. 3 and indicate that there is a sufficient amount of variation in the temperature rise of electrical apparatus between sea level and an altitude of five thousand feet, for example, to require a relatively small corrective factor. The data obtained are, however, probably not sufficiently conclusive to warrant making provision at this time for such correction.

The tests so far recorded with variation in humidity have shown no appreciable variation in temperature rise and these results agree with other work which has been done along this line.

A theoretical discussion of the variation in temperature rise due to variation in radiation, would indicate that under certain conditions the temperature rise from higher air temperatures

TABLE II

EFFECT OF PRESSURE VARIATION UPON TEMPERATURE RISE OF COIL

Air pressure		Couple temperatures deg. cent.			Coil input, watts	Blower speed rev. per min.	Corrected coil rise
Cm.	In.	Air in box	Coil	Coil rise			
82.7	32.6	41	69.5	28.5	31.7	785	28.7
80.2	31.6	40.8	69.6	28.8	31.7	786	29
77.6	30.5	40.9	69.7	28.8	31.8	784	29
75.5	29.7	41.8	70.5	28.7	31.8	789	28.9
75.3	29.6	40.9	69.5	28.6	31.8	785	28.8
74.6	29.2	40.2	68.9	28.7	31.8	784	28.9
73.7	29	40	67.8	27.8	31.6	782	29.1
72.3	28.5	40.2	69.0	28.8	31.6	787	28.2
69.6	27.4	40	68.8	28.8	31.7	790	29.1
66.9	26.4	40.7	69.5	28.8	31.7	788	29.1
63.8	25.1	40.7	70.3	29.6	31.7	784	29.9
61.4	24.2	40.5	71.2	30.7	32	786	30.7
58.7	23.1	40.5	73.5	33	33.7	785	31.4
56.2	22.1	40.2	73.1	32.9	33.6	787	31.4
54.1	21.3	40.2	72.5	32.3	31.6	787	32.4
53.8	21.2	40.4	73.5	33.1	31.8	788	33.4

might be less than from lower air temperatures. As the general tendency of the other factors which affect temperature rise from different air temperatures is to oppose the effect of radiation, these corrections will in general tend to cancel each other.

Conclusion

It will be seen from the foregoing discussion that the problem of the variation in temperature rise from different air temperatures is quite complex and that in some cases the same item of loss may tend to increase or decrease the temperature rise, depending on the conditions under which the apparatus is operated. The conclusion which can be drawn from the tests made, is that while there are a number of things which would make the temperature rise from one air temperature differ from that of another, these tend to cancel each other and, therefore, the omission of any corrective factor for variation in air temperature would be more nearly in accordance with the facts than the provision made in the existing Standardization Rules.

A paper presented at the Midwinter Convention of the American Institute of Electrical Engineers, New York, February 26, 1913.

A LABORATORY INVESTIGATION OF TEMPERATURE RISE AS A FUNCTION OF ATMOSPHERIC CONDITIONS

BY C. B. BLANCHARD AND C. T. ANDERSON

The following is a report on laboratory tests made to determine the effect of pressure, temperature and humidity of the air surrounding a heat-dissipating body on its rise in temperature above that of the air.

The original purpose of this paper was to investigate the physical laws governing heat dissipation in air and to attempt to apply these laws to the general problem of temperature rise in electrical machinery. But in working up the results of the tests it was found that there was some uncertainty in the derived quantities because of uncertainties regarding the precise effective area of the heat-dissipating body, the nature of its surface, the temperature gradient from its interior to its surface, and, furthermore, the heat losses through the leads through which electric power was supplied to the body and by means of which its temperature was measured.

This uncertainty is considered such that careful physical derivations are not warranted, and yet the results are considered sufficiently definite for all practical purposes to determine the effect of the various atmospheric conditions on temperature rise of stationary self-cooled and oil-cooled apparatus. It is, therefore, the purpose to set forth herein the details of the apparatus and its manipulation and the data and curves expressing the desired relations, and to postpone the generalizations until further tests have been made. A rough outline of the proposed derivations will be given, however, as preliminary to a subsequent paper.

An outline of the general method of the tests will first be given. Then the apparatus, manipulation, results and discussion will be taken up in order.

General Method

The method of the tests was as follows:

A resistance coil was suspended in a steel tank containing air. The coil was supplied with electric power. After constant temperature conditions obtained, measurements of coil temperature, air temperature, tank temperature, air pressure and watts input were taken. The difference between the air temperature and the coil temperature was called the temperature rise. The variations of this rise with air temperature, air pressure and input were determined by holding two of these constant and varying the remaining one. These tests were made with dry air except when the effect of humidity was being investigated. The details of the apparatus used and the method of test are taken up in the following sections.

The Apparatus

The Test Coil. The heat-dissipating body was a coil of 100 ft. (30.48 m.) of insulated copper wire of 0.018 in. (0.457 mm.) diameter with a coat of black enamel of 0.001 in. (0.025 mm.) thickness and one layer of cotton of 0.0025 in. (0.0635 mm.) thickness. Wound turn by turn with this wire was an equal length of copper wire of 0.005 in. (0.127 mm.) diameter with a coat of enamel 0.00025 in. (0.00635 mm.) in thickness. These two wires formed a coil of 5.08 cm. internal diameter, 7.02 cm. external diameter and 0.635 cm. thickness. The leads which were soldered to the ends of these windings were of copper wire of 0.041 in. (1.04 mm.) diam. with 0.0015 in. (0.038 mm.) enamel. The pyrometer leads were 12.7 and 14 cm. in length; those of the other winding were 12.7 and 16.5 cm. in length. The leads were soldered to pieces of double lamp cord of 16 strands of 0.0125 in. (0.3175 mm.) diameter each with a coating of rubber 0.025 in. (0.635 mm.) in thickness and cloth 0.01 in. (0.25 mm.) in thickness, and 120 cm. in length. The soldered joints at the coil were taped and tied to the coil. The whole coil was wrapped transversely with 140 cm. of hemp string of about one mm. thickness to insure mechanical strength. The coil was then soaked in japan and baked. The soldered joints at the lamp cord were left untaped. These details are given for the benefit of any one who cares to investigate the results of these tests.

The Air Tank. The air tank is shown diagramatically in Fig. 1. It is of cylindrical form, 68 cm. in diameter and 68 cm. in depth. It has a flange at the upper edge to which the cover can be bolted. Pipe connections are made through one side for pressure control., The cover contains two fiber plugs through which the electrical terminals pass. The whole vessel is painted black inside. This tank is supported inside a second tank of 100 cm. diameter and 100 cm. depth, containing oil. Beneath the air tank is an electrical heating element of about two kw. capacity for controlling the temperature.

The Pyrometers. The test coil pyrometer has already been described. The air pyrometer consists of three coils of 0.003-in. (0.076 mm.) cotton-covered wire wound on fiber frames so that

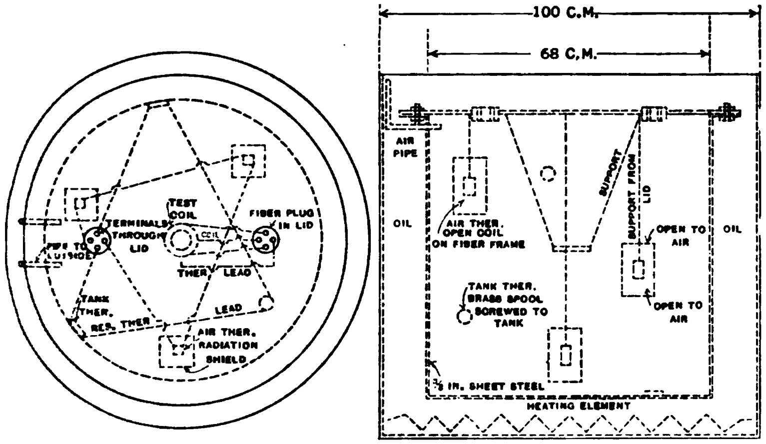


FIG. 1

practically the whole surface of the wire is exposed to the air. These coils are shielded by square tubes of tin of several times the diameter of the coil. They are open at both ends, allowing free passage of air over the coils, at the same time shielding them from any direct radiation from the test coil. These coils are connected in series. The total resistance of this pyrometer is about 110 ohms. The tank pyrometer consists of three brass spools wound with 0.003-in. (0.076 mm.) cotton-covered wire. These spools are screwed to the tank at various points. The coils are connected in series. The total resistance of this pyrometer is about 100 ohms.

Each pyrometer is connected through leads in the fiber plugs to the outside of the tank.

The Set-Up. The air tank then consisted of a test coil suspended in the center of the air tank by twin strings, and loaded by a storage battery through terminals in the cover. The power was varied by a rheostat and read by a direct potential voltmeter and ammeter.

The temperatures were read by measuring the resistances of the pyrometers with a portable bridge. The coils were very carefully calibrated and the temperatures were read from curves.

The air pressure in the tank was controlled by connections with compressed air and vacuum lines. It was measured by means of a mercury column.

Manipulation

In Tables I and II are the results of the constant input tests. Each of these tests was made at a constant air temperature in

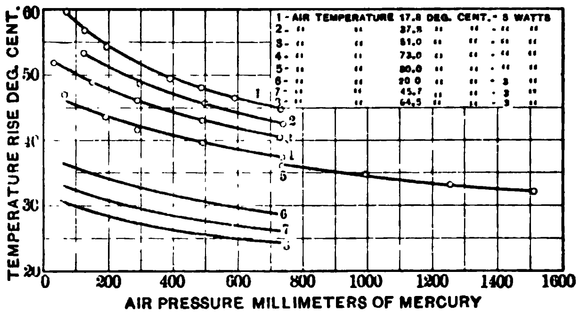


Fig. 2—Temperature Rise vs. Pressure.

order to determine the effect of pressure alone. Curves in Fig. 2 express the variation of temperature rise with pressure at various air temperatures.

In Table II are the results of the input tests. Each of these tests was made holding air pressure and air temperature constant and varying the watts input. The curves in Figs. 4, 5 and 6 express the variation of temperature rise with watts input at various air pressures and temperatures.

The tests so far outlined were made with dry air. The air before entering the tank was caused to pass through sulphuric acid, calcium chlorid and phosphorus pentoxid.

The last two items in Table I are the results of the humidity tests. In these tests the air was passed through an electric oven which contained a large wetted surface. Great care was taken to pass the air from the humidifier to the test tank without condensation.

Results

The data curves of Fig. 2 express the variation of temperature rise with pressure. The second set of curves on this figure was

TABLE I

VARIATION OF TEMPERATURE RISE WITH AIR TEMPERATURE, PRESSURE AND HUMIDITY.

Tank temp.	Air temp.	Coil temp.	Temp. rise	Pressure mm.	Humidity per cent saturation
deg. cent.	deg. cent.	deg. cent.	deg. cent.		
17.5	18	62.8	44.8	732	0
17.8	17.5	64.0	46.5	592	0
17.8	17.5	65.6	48.1	492	0
17.8	17.5	66.8	49.3	392	0
17.5	17.4	68.3	50.9	294	0
17.8	17.5	71.8	54.3	197	0
17.8	17.7	74.5	56.8	127	0
17.8	17.7	77.5	59.8	72	0
39.0	37.8	80.3	42.5	740	0
39.0	37.5	83.2	45.7	500	0
39.2	37.8	86.5	48.7	300	0
39.5	38.0	91.4	53.4	125	0
52.2	50.7	91.2	40.5	732	0
52.5	50.7	93.5	42.8	492	0
52.5	50.5	96.5	46.0	292	0
52.7	51.0	100.0	49.0	152	0
53.0	51.5	103.3	51.8	32	0
76.2	73.5	110.8	37.3	737	0
76.0	73.0	112.5	39.5	497	0
76.0	73.0	114.6	41.6	297	0
76.0	73.0	116.5	43.5	197	0
76.0	73.0	120.1	47.1	67	0
82.5	80.2	116.25	36.0	737	0
82.2	80.0	114.75	34.8	995	0
82.0	79.7	112.9	33.2	1254	0
83.0	80.2	112.3	32.1	1512	0
18.8	19.5	64.4	44.9	732	71
38.5	38.5	81.1	42.6	741	95

derived from the input curves of Figs. 4, 5 and 6 by drawing ordinates at three watts and plotting the intersections with the input curves. It will be seen from the curves on Fig. 2 that at normal air pressures there is a variation of temperature rise with pressure of about 1 deg. per 100 mm. Assuming that from

TABLE II
VARIATION OF TEMPERATURE RISE WITH WATTS INPUT, TEMPERATURE AND PRESSURE

Tank temp.	Air. temp.	Coil temp.	Temp. rise	Pressure mm.	Watts input Total
deg. cent.	deg. cent.	deg. cent.	deg. cent.		
19.3	19.6	30.6	11.0	297	0.892
19.4	19.7	41.5	21.8	297	1.911
19.4	19.9	57.3	37.4	297	3.53
19.4	20.0	73.0	53.0	297	5.27
19.6	20.0	82.5	62.5	97	5.587
19.6	20.1	68.2	48.1	97	4.107
19.6	20.0	49.0	29.0	97	2 29
19.6	20.0	37.8	17.8	97	1.318
66.5	63.8	77.1	13.3	727	1.558
66.5	63.8	78.0	12.2	487	1.560
66.5	63.8	78.8	15.0	287	1.56
66.6	64.0	79.7	15.7	147	1.563
66.5	64.4	80.9	16.5	57	1.564
66.7	64.4	81.3	16.9	17	1.564
67.0	64.5	92.2	27.7	727	3.428
66.7	64.8	116.7	51.9	14	5.121
66.5	64.4	120.9	56.5	54	5.910
66.3	63.9	117.7	53.8	144	5.915
66.6	64.2	114.8	50.6	284	5.961
66.6	64.4	112.0	47.6	484	5.941
66.6	64.4	98.4	34.0	484	4.045
46.7	44.8	59.8	15.0	744	1.626
46.7	45.1	73.8	28.7	744	3.33
47.1	45.3	99.7	54.4	744	6.837
47.3	45.5	74.4	28.8	504	3.094
47.5	45.7	76.3	30.4	304	3.078
47.5	46.6	79.5	32.4	104	3.082
47.7	46.6	108.7	61.6	104	6.205
18.7	19.1	22.8	3.9	737	0.34
18.8	19.1	32.9	14.0	737	1.34
18.8	19.3	46.1	26.9	737	2.80
19.4	19.5	69.5	50.1	737	5.73
19.2	19.4	72.7	53.4	497	5.67
19.3	19.6	58.4	39.9	497	3.95
19.3	19.6	40.3	20.8	497	1.92
19.4	19.7	29.9	10.3	497	0.895
67	64.4	83.2	18.8	724	2.287
67	64.4	84.2	19.8	484	2.272
67	64.5	85.5	21.0	284	2.262
67	64.5	86.8	22.3	144	2.252
67	64.7	88.3	23.6	54	2.246
67	64.7	89.7	25.0	14	2.237

three to five watts input for 80 sq. cm., or from 0.0375 to 0.0625 watts per sq. cm., is a practical range of input, this variation seems to be about what would be expected in stationary apparatus.

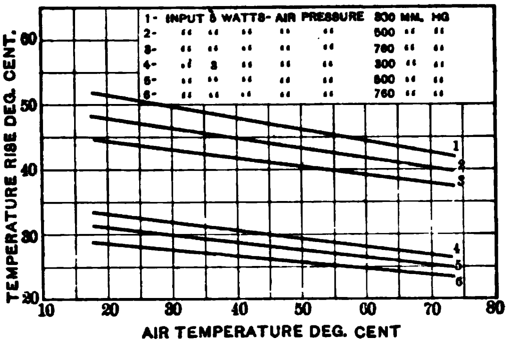


Fig. 3—Temperature Rise vs. Air Temperature.

According to the Standardization Rules, we have, for a variation of 100 mm. from 760,

$$\frac{860-760}{10} = 10 \text{ per cent correction.}$$

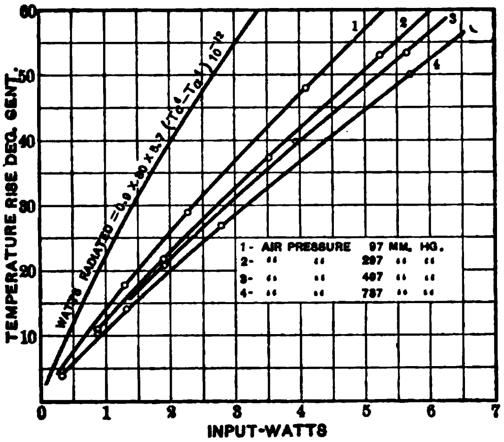


Fig 4.—Temperature Rise vs. Input.
Air temperature 20 deg. cent.

Assuming a temperature rise of 40 deg. cent., we have 40 + 4 = 44 deg. rise. This correction is four times as large as the results of this test would indicate.

The curves of Fig. 3 are derived from curves of Figs. 2, 4, 5 and 6. They express the variation of temperature rise with air temperature. These curves represent the same range of input as do the pressure curves. The variation according to these curves is about 0.15 deg. per degree centigrade variation of air temperature, in the opposite direction to that assumed in the present Standardization Rules.

The last two items of Table I are the data for the effect of humidity. These points check the dry air points, showing that the effect of humidity is negligible at normal temperatures and pressures.

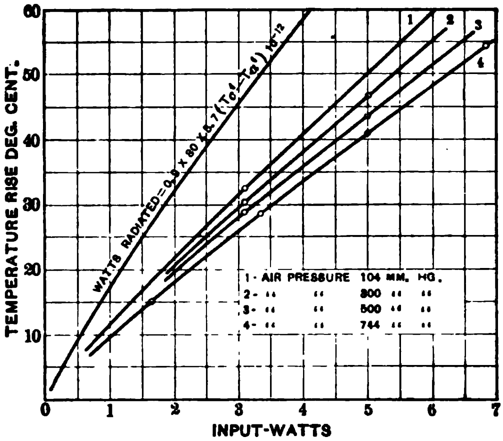


FIG 5.—TEMPERATURE RISE VS. INPUT.
Air temperature 45.7 deg. cent.

CONCLUSIONS

The temperature rise is a function of the various factors which enter into the dissipation of heat through air. These factors are (1) radiation, that is, a passage of radiant energy, (2) conduction and (3) convection, which is a flow of warm air because of decreased density.

Radiation can be figured according to the law

$$W = K\,5.7\,(T_1^4 - T_2^4)\,10^{-12}$$

where K is unity for a black body and less for any other, depending upon the nature of the heated surface. Assuming 0.9 for K for the test coil and putting

$T_1 = Tc$, the test coil temp. absolute,

$T_2 = Ta$, the air temp. absolute,

and taking the effective radiating area as 80 sq. cm., the watts radiated at various air temperatures and temperature rises * were calculated and plotted in Figs. 4, 5, and 6. These curves express temperature rise as a function of watts input at various surrounding temperatures, if radiation alone be allowed to dissipate the heat. These curves show, by comparison with the data curves, the importance of radiation in the dissipation of heat from a heated body in the open air.

As has already been stated, the results are more or less in error because of losses of heat through the leads, the temperature gradient, and the irregularity in the area.

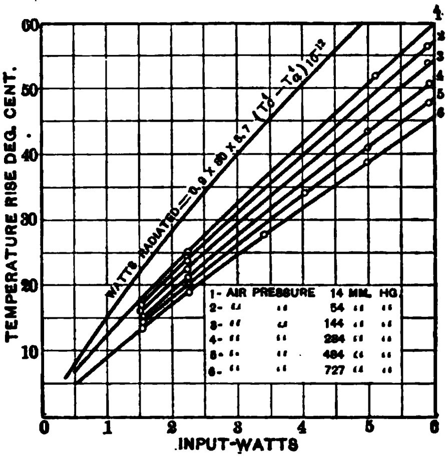


FIG. 6—TEMPERATURE RISE VS. INPUT.
Air temperature 64.5 deg. cent.

Heat is conducted through the leads a distance depending upon the rate at which it is dissipated from the surface of the leads, which, of course, depends on the temperatures and the nature and area of the surfaces. A correction for this loss, as can be seen, is no simple matter.

The temperature gradient being unknown causes an unknown difference between the coil temperature as measured, and the surface temperature upon which the heat dissipation depends.

Whereas these difficulties forbid derivation of physical laws, they are slight from the engineering standpoint. A set of results which are free from these errors is very desirable, however,

*See Table III.

TABLE III

T_a deg. cent.	T_a deg. K	$(T_a/1000)^4$	T_c deg. cent.	T_c deg. K	$(T_c/1000)^4$	$r = (T_c/1000)^4 - (T_a/1000)^4$	$w = 0.9 \times 5.7 \times r$	$W = 80\,w$	$T_c - T_a = T_r$
20	293	0.00738	40	313	0.00961	0.00223	0.01144	0.914	20
20	293	0.00738	60	333	0.01232	0.00494	0.02530	2.026	40
20	293	0.00738	80	353	0.01555	0.00817	0.04200	3.356	60
45.7	318.7	0.01035	65.7	338.7	0.01320	0.00285	0.01460	1.170	20
45.7	318.7	0.01035	85.7	358.7	0.01660	0.00625	0.0321	2.565	40
45.7	318.7	0.01035	105.7	378.7	0.02060	0.01025	0.0526	4.208	60
64.4	337.4	0.013	84.4	357.4	0.01640	0.00340	0.0174	1.40	20
64.4	337.4	0.013	104.4	377.4	0.02030	0.00730	0.0374	3.00	40
64.4	337.4	0.013	124.4	397.4	0.02490	0.01200	0.0615	4.920	60

since the laws of heat conduction and convection in air at various pressures are not as yet well established. Furthermore, the entire problem of heat dissipation in air is important, because when the laws are known electrical machinery can be so designed as to maintain the most efficient system of natural cooling.

Tests which will make possible a physical investigation of heat dissipation in air are being contemplated. A brief outline will be given of the proposed method of attack.

It is hoped to build such apparatus as to dispense with the temperature gradient, surface and heat difficulties. With this apparatus input tests can be made. A set of curves similar to those of Figs. 4, 5 and 6 will determine pressure relations at any temperature and input, temperature relations at any temperature and input, etc. Furthermore, radiation curves similar to those on Figs. 4, 5 and 6 can be plotted, and by subtracting the radiation abscissas from the total values, a second set of curves, involving conduction and convection alone, will be the result. Also, by drawing abscissas through such derived curves and noting the intersections, curves of the variation of watts conducted and convected, as a function of pressure, can be drawn. Similarly, curves can be derived expressing the variation of watts conducted and convected with air temperature. In short, from such a set of data it is possible to derive expressions of the laws of heat dissipation in air.

Such data as those just mentioned should be taken from a surface for which radiation constants are well known. A further investigation of the effect of the nature of the heat-dissipating surface is important.

A paper presented at the Midwinter Convention of the American Institute of Electrical Engineers, New York, February 26, 1913.

LAWS OF HEAT TRANSMISSION IN ELECTRICAL MACHINERY

BY IRVING LANGMUIR

Even in a simple case of heat transmission, such as a laboratory measurement of heat conductivity, the phenomena involved usually prove to be quite complicated. It is in most cases nearly impossible to separate completely the effects of conduction, radiation and convection, and therefore if quantitative results are to be obtained, it becomes necessary to make an elaborate series of corrections, to eliminate the undesired factors.

In the case of the flow of heat from the interior of a piece of electrical apparatus to the exterior, the problem is necessarily much more complicated than in the simple experiment. Since each of the three factors, conduction, radiation, and convection, are subject to totally different laws, the effect of any change in conditions can, in general, be determined only by considering what part each factor plays in the whole process and how each factor is affected by the new change in the conditions.

Failure to appreciate the necessity of separating each of these factors has rendered the majority of all investigations on heat transmission previous to 1880 almost valueless. Even today a very large number of published results on heat conductivity and surface emissivity are thoroughly unreliable from this cause.

It is surprising to what extent the old unreliable data maintain their place in present day hand-books and text-books. There is probably hardly a field in which so much care is needed in selecting proper sources for information, the literature being full of absolutely contradictory statements. For example, one investigator concludes from his experiments that the total heat loss from small wires, heated to a given temperature, varies in inverse proportion to the diameter, others state that the variation is directly proportional to the diameter, while still others find it to be independent of the diameter.

If electrical engineers are to consider seriously the question of heat transmission in electrical machinery, they must fully realize the great difference between the three modes of heat transmission and determine, first of all, what part each factor plays in each of the many types of machines.

In the present paper the writer has attempted to present in as clear a manner as possible the fundamental laws of conduction, radiation and convection of heat, and to furnish some experimental data which may seem to be of value to the electrical engineer.

Conduction of Heat

The problem of the conduction of heat through solids was the first subject of mathematical and experimental study in the development of the theory of heat. Fourier and others developed the mathematical theory long before there were any reliable experimental data by which they could test their conclusions.

Fourier considered especially the problem of the rate of heating of a body capable of conducting heat. To solve this problem, he invented special mathematical methods which are now studied under the head of Fourier's Series. Because of the extreme complexity of this subject, mathematical analysis does not promise, for the present, to be of much value to the electrical engineer. We shall therefore limit ourselves to the simpler problem of the steady flow of heat which prevails after sufficient time has elapsed for the temperature through the apparatus to become constant.

For this case of steady heat flow through solids, the general law may be stated

$$W = \frac{A}{l} \mathbf{k} (T - T_0) \quad (1)$$

where W = Watts of heat flow.

A = Area of cross-section of path of heat flow.

l = Length of heat path.

$\mathbf{k}$ = Coefficient of heat-conductivity expressed in watts per cm. per deg.

$T - T_0$ = Difference of temperature causing the heat flow.

Heat Conductivity of Solids. The heat conductivity, **k**, of solid bodies varies greatly for different substances, ranging from 4.2 watts per cm. per deg. cent. for pure silver to 0.0016 for such substances as rubber. The conductivity of finely divided solids, fibrous materials, powders, etc., is very much less than that of homogeneous solids. The best heat insulators known, such as eider-down, have a conductivity as low as 0.00020 watts per cm. per deg. cent.

Unfortunately, all the materials used for electric insulation are relatively poor heat conductors. Especially when the insulation contains air spaces, as in the case of braided coverings, the conductivity is very low.

In Table I the writer has collected together all of the *reliable* data he has, by careful search, been able to find on the conductivity of such materials as are of interest to the electrical engineer.

TABLE I

Thermal Conductivities and Resistivities of Various Materials.

Materials	Temp. deg. cent.	Conductivity watts per cm. per deg. cent.	Resistivity	References
Metals				
Pure copper	18	3.84	0.260	2
Commercial copper	18	3.50	0.285	2
Pure iron	18	0.67	1.49	2
Steel 1 per cent C	18	0.45	2.22	2
Cast iron	100	0.40		
Transformer steel 4 per cent Si	20–250°	0.32	3.12	1
Brass	"	1.30	0.77	1
German silver 30 per cent Ni	"	0.28	3.6	1
Constantan 60 Cu, 40 Ni	18	0.29	3.5	2
Calorite 65 Ni, 15 Fe 13 Cr, 7 Mn	20–250°	0.16	6.2	1
Mineral Substances				
Graphite		3.57	0.28	11
Marble		0.030	33	3
Slate		0.020	50	3
Glass		0.011	90	4
Quartz glass		0.015	67	10
Porcelain		0.010	100	5
Insulating Materials				
Shellac		0.0025	400	4
Para rubber		0.0016	620	3
Gutta percha		0.0020	500	3
Paraffin		0.0026	390	3
Ebonite		0.0018	550	4
Paper		0.0013	770	3
Asbestos paper		0.0025	400	3
Varnished cotton tape		0.0027	370	6
Varnished cambric		0.0025	400	6
Mica paper		0.0016	630	6
Bakelite and linen tape		0.0027	370	6
Rubber tape		0.0043	230	6
Varnished cloth (empire cloth)		0.0025	400	7
Presspahn (untreated)		0.0017	590	7
Rope paper (untreated)		0.0012	830	7
Rope paper and oil		0.0014	710	7
Rope paper treated with varnish		0.0017	590	7
Fullerboard varnished		0.0014	710	7
Pure mica		0.0036	280	7

TABLE I—*Continued.*

	Apparent density grams per. cu. cm.	Conductivity	Resistivity	References
Insulating Materials				
Built-up mica Micanite (19 per cent shellac)		0.0010	1000	7
Built-up mica Micanite (11 per cent shellac)		0.0012	830	7
Linen tape varnished and baked		0.0015	670	7
Miscellaneous Materials				
Cotton batting, loose	0.21	0.00046	2200	1
" " tightly packed	0.10	0.00030	3400	1
Pure woolen wadding, loose	0.015	0.00049	2040	1
" " " packed slightly	0.054	0.00036	2800	1
Pure woolen wadding, tightly packed	0.192	0.00023	4400	1
Eider down, very loose	0.0022	0.00045	2200	1
" " tightly packed	0.077	0.00025	4100	1
" " " " "	0.11	0.00019	5200	1
Soft white pine ⊥ to grain		0.0016	640	
Liquids				
Water 25 deg. cent		0.0057	176	4
Paraffin oil		0.0014	710	8
Glycerin 25 deg. cent		0.0024	410	4
Petroleum 23 deg. cent		0.0016	630	8
Gases				
Air at 20 deg. cent		0.000249	4010	9
" " 100 deg. cent		0.000300	3330	9

REFERENCES:—1. C. P. Randolph, Research Lab., General Electric Co. (1912). 2. Jäger & Diesselhorst, Wiss. *Abl: d Phys.* Techn. Reichanstalt, 3, 269 (1900). 3. Lees, *Phil. Trans* (A) 183, 481 (1892). 4. Lees, *Phil.* Trans. A 191, 399 (1898). 5. Lees and Chorlton, *Phil. Mag.* (5), 41, 495 (1896). 6. Technical Report No. 10940. General Electric Company, by C. P. Steinmetz (1910). Experiments carried out by J. L. R. Hayden. 7. Paper on Heat Paths in Electrical Machinery read before the Institution of Electrical Engineers, Nov. 1911 by H. D. Symons and M. Walker. 8. R. Weber, *Ann. Phys.*, 11. 1047 (1903). 9. Langmuir, *Phys. Rev.* 34, 406 (1912). 10. A. Eucken, *Ann. Phys.* 34, 185 (1911). 11. Koenigsberger and Weiss, *Ann. Phys.* 35, 1 (1911).

In the above table it will be noted that all the metals and alloys have a very high thermal conductivity compared with that of non-metallic substances (with the exception of graphite). Practically all of the electrical insulators have a conductivity about one-thousandth of that of copper. The conductivities of various substances of any one type do not differ very greatly from one another; for example, the range of conductivities of organic materials used for electrical insulation is from about 0.0015 to 0.0040. The presence of air spaces in a body very much decreases the conductivity, as is seen for the extremely low conductivities of such materials as wool, cotton batting, etc. That this high thermal resistance is entirely due to the presence of gas, is evident from the fact that these materials are relatively good conductors when placed in an atmosphere of hydrogen.

In the case of powdered or fibrous materials, the heat conductivity is practically independent of the true conductivity of the solid substance forming the grains or fibers. For example, finely divided metallic powders, such as zinc dust or tungsten reduced by hydrogen from finely divided oxide, are extremely good heat insulators, although the material itself is a good heat conductor.

The conductivity of liquids is quite low, and is in most cases very insignificant compared with the amount of heat carried through the liquid by convection currents. The figures given above for liquids can be used only for liquids through which heat is being transmitted downwards, or in the case where the liquid is held in the meshes of some fibrous material such as cloth. This is a common case, however, in electrical insulation.

The heat conductivity of gases is smaller than that of solids and liquids.

Temperature Coefficient of Heat Conductivity. Pure metals, although they have a large temperature coefficient of electrical conductivity, have a thermal conductivity which is practically the same at all temperatures. Alloys have a conductivity which usually increases somewhat with the temperature, the temperature coefficient being never greater than about 0.1 per cent per degree cent., and usually much less than this. In electrical machinery, therefore, with the relatively small temperature range involved, we may consider the thermal conductivity as independent of the temperature.

In the case of pure crystalline, non-metallic substances, Eucken (*Ann. d Physik*, Vol. 34, p. 185, 1911) has shown that the thermal conductivity varies approximately in inverse proportion to the absolute temperature. In other words, the temperature coefficient of such substances is usually about —0.33 per cent per deg. cent.

The same investigator has also shown that with amorphous, non-metallic substances, like glass, the conductivity increases slightly with increase of temperature, the temperature coefficient being about +0.15 per cent per deg. cent. Such substances as paraffin, ebonite, etc., are found to have practically no temperature coefficient.

Tests made by Mr. C. P. Randolph have shown that for powdered or fibrous materials the temperature coefficient of heat conductivity is very large. For example, measurements of the heat conductivity of Poplox at various temperatures up to 500 deg. cent. gave the results:

Range of temperature	Heat Conductivity Watts per cm. per deg. cent.
20 – 100	0.00030
100 – 200	0.00039
200 – 300	0.00057
300 – 400	0.00076
400 – 500	0.00133

This material (popped water glass) had an apparent density of 0.026 and contained air cells of about 0.5 mm. average diameter. The large temperature coefficient is caused by radiation across the walls of the air cells which is relatively large at high temperatures but almost negligible at room temperature. Below 100 deg. cent. the temperature coefficient for powdered and fibrous materials may be taken as + 0.3 per cent for materials with a coarse structure and proportionately less for finer structure.

In the case of gases, the mobility and the coefficient of thermal expansion are so great that convection becomes very large. It was thought for a long time that gases had no true conductivity. Maxwell, however, calculated the conductivity from the kinetic theory, and predicted that it would be independent of the pressure. His results have since been thoroughly verified. To avoid convection currents, it is only necessary to reduce the pressure of the gas to a value of several centimeters of mercury. At lower pressures the heat conductivity is found to be entirely independent of the pressure, down to a pressure of one mm. When the pressure becomes much less than this, the free path of the molecules begins to become comparable with the length of the path along which the heat is conducted. At still lower pressures, the heat conductivity becomes proportional to the pressure. It is evident that the pressure at which the change from one of these laws to the other takes place is inversely proportional to the length of the path of heat flow. In the case, therefore, where we have the space subdivided by the presence of a fibrous material, we find that the heat conductivity does not remain constant down to such low pressures as one millimeter, but may even, with very finely divided materials, decrease with decreasing pressure, from atmospheric pressure down. This subject has been very fully treated by Smoluchowski [*Ans. Akad. Wiss. Krakau* (1910) A 129-153].

In the case of the heat conduction of gases, we find there is a large temperature coefficient of heat conductivity. It has been thoroughly proven that Sutherland's formula

$$\mathbf{k} = \frac{K \mathbf{c}_v \sqrt{T}}{1 + \frac{C}{T}} \qquad (2)$$

very accurately represents the change in conductivity with the temperature. Here K is a constant, $\mathbf{c}_v$ is the specific heat of the gas at constant volume and c is a constant equal to 124 for air. In general, when $\mathbf{k}$ is a function of the temperature, we cannot use equation (1) to calculate the heat conduction through a given body. It can be shown, however, [Langmuir, *Phys. Rev.* Vol. 34, p. 406 (1912)], that if we substitute for the factor $\mathbf{k}(T - T_0)$ the integral $\int_{T_0}^{T} \mathbf{k}\, d\, T$ we are then able to calculate the heat conduction, no matter how much the heat conductivity may vary with the temperature. For such cases as these, the results can be calculated most easily by calculating or plotting the values of the integral $\int_{0}^{T} \mathbf{k}\, d\, T$. If we represent by ϕ the value of this integral, which we may call specific conduction, then the formula for heat conductivity becomes

$$W = \frac{A}{l}(\phi - \phi_0) \qquad (3)$$

Heat Conduction through Bodies of Various Shapes

Equations (1) or (3) can be used only for the heat conduction between parallel planes. In other cases, A and l are both variables, and the proper mean values must be determined, by special methods.

No matter what the shape of the body, the ratio $\frac{A}{l}$ has a definite value, if the surfaces of in-flow and out-flow of the heat are fixed. Let us call this quantity the "shape factor," and represent it by the letter s. Our equation for heat conduction thus becomes

$$W = s(\phi - \phi_0) \qquad (4)$$

The value of the shape factor for bodies of various shapes may be of interest. In a paper to be published shortly (Langmuir, Adams, and Meikle, *Trans. Am. Electrochem. Soc.*, Vol. 24, 1913), methods for calculating the shape factor of bodies of many different shapes will be given. The following cases only are worthy of considering here:

Planes

$$s = \frac{A}{l}$$

Concentric Cylinders of Diameters a and b.

$$s = \frac{2\pi}{ln\,\frac{b}{a}} \tag{5}$$

Concentric Spheres of Diameters a and b.

$$s = \frac{2\pi}{\frac{1}{a} - \frac{1}{b}} \tag{6}$$

Concentric Rectangular Prisms or Parallelopipedons. For this case, the following formula applies with great accuracy to the flow of heat where the thickness of the heat-conducting material between the two surfaces is constant and where the thickness of the layer is not more than 2½ times the smallest dimension of the prism, a condition nearly always fulfilled in practise. The formula is

$$s = \frac{A}{t} + \tfrac{1}{2}\Sigma l + 1.2\,t \tag{7}$$

Here t is the thickness of the layer of conducting material, Σl is the sum of the length of the 12 edges of the inner prism, and A is the total surface of the inner prism.

Radiation of Heat

Dulong and Petit (1817) gave an empirical formula for heat radiation which has been used very largely by engineers almost up to the present date.

The Stephan-Boltzman law that the radiation from a black body is proportional to the fourth power of the absolute temperature has been shown to be derivable from the second law of thermodynamics, and has withstood the tests of very careful experimental investigation. We may therefore look upon this law as being one of the exact laws of nature. It must be remembered, however, that it applies only to radiation from a so-called black body; that is, a body which absorbs all heat rays which fall on it. Such a body can only be approximately realized, and any actual body must radiate less heat than an ideal black body at the same temperature.

The Stephan-Boltzman law, as applied to the radiation from any given body, may be written

$$\mathbf{W} = 5.7\, e\left[\left(\frac{T}{1000}\right)^4 - \left(\frac{T_0}{1000}\right)^4\right] \qquad (8)$$

Here, **W** is the energy in watts radiated per square centimeter of surface. T is the temperature of the hot body, and T_0 the temperature of the surrounding space, e we may call the relative emissivity of the body; it is always a number less than unity and is a characteristic property of the radiating body.

The radiation constant, 5.7, is subject to some uncertainty at present. For several years, the commonly accepted value was 5.32, which was the result obtained by Kurlbaum (*Wied. Am.* 65, 746, 1898). Recently, however (1909), Féry obtained a value 6.3. Since then many investigators have redetermined this constant. Paschen and Gerlach (*Ann. d Physik*, Vol. 38, p. 30, 1912) obtained the value 5.9. Shakespeare (*Proc. of the Roy. Soc.*, Vol. 86A, p. 180, 1911) obtained 5.67. Within the next year or so the correct value of this constant will undoubtedly be determined. For the present, it would seem almost certain that the value 5.32 is too low, and that the value 5.7 must be fairly close to the true value.

The radiation of heat differs from conduction and also convection in that it is a purely surface phenomenon. The amount of heat radiated is strictly proportional to the extent of the surface, and is independent of the presence of gas adjacent to the surface. From bodies that are not black, the heat radiated from cavities in the bodies has a greater intensity than that radiated from a flat surface, for the reason that from such a cavity there is not only the heat radiated from the bottom of the cavity, but also that reflected by the bottom surface from the walls of the cavity. The above equation for radiation therefore applies only for heat that comes directly from a surface, and not for that which has been reflected from the surface. In general, however, this factor is not a difficult one to take into account in the consideration of heat radiation.

The most difficult question involved is the determination of the relative emissivity e. For clean polished surfaces of metals this quantity is small, ranging from 0.02 to 0.30. For non-metallic substances it is usually very much larger than this, ranging from about 0.3 up to about 0.9. The following table gives the values of e calculated by the writer from measurements made by C. P. Randolph. A detailed description of these measurements will be published in the *Transactions* of the Am. Electrochem. Soc., Vol. 23, 1913.

TABLE II

Copper, oxidized by heating to a red heat	0.74
Copper, calorized; that is, surface impregnated with Al	0.27
Silver, pure, polished	0.03
Cast iron, fresh machined surface	0.25
Cast iron, oxidized by heating to red heat	0.65
Aluminum paint on cast iron	0.47
Gold enamel on cast iron	0.39
Monel metal, polished	0.40
Monel metal, oxidized	0.45

The relative emissivities from highly polished metal surfaces can be calculated from the reflectivity of the surface for heat rays, the emissivity and reflectivity being complementary to each other. That is, the emissivity is equal to $1 - r$ where r is the reflectivity. There is a considerable amount of data in the literature on the reflectivities of metals for heat rays. However, it has been shown by Hagen and Rubens (*Ann. d Physik*, Vol. 8, p. 1, 1902) that the relative emissivity may be calculated by the formula

$$1 - r = e = 0.365 \sqrt{\frac{\theta}{\lambda}} \qquad (9)$$

where θ is equal to the specific electrical resistivity in ohms-centimeter units at the temperature of the metal and λ is equal to the wave length of the radiant energy in centimeters.

Recent investigations have shown that this formula is very accurate for wave lengths exceeding 0.005 mm. in length. The average wave length of the light corresponding to any given temperature is, according to Wien's radiation law, approximately $\frac{0.29}{T}$ cm. If we substitute this in the above equation, we obtain the value

$$e = 0.68 \sqrt{\theta T} \tag{10}$$

This equation enables us to calculate the relative emissivity for any highly polished metal up to about 500 or 600 deg. cent. However, because of surface oxidation, the emissivity will nearly always be much larger than this, except with such metals as silver, platinum, and gold.

Organic substances, such as oils, varnishes, resinous materials, have a very high emissivity; that is, between 0.8 and 1. Although in the visible spectrum these bodies are transparent, they are practically black bodies as regards the long heat rays that are involved in the radiation from bodies at ordinary temperature. The same is true of glass, this being practically a black body with respect to heat rays. Therefore, in calculating the amount of radiation from electrical insulating materials, we may very safely assume, whether these materials have a black color or a much lighter color, that they are nearly black bodies, as far as radiation is concerned.

The Temperature Coefficient of Radiation. For small differences of temperature, equation (8) may be written

$$\mathbf{W} = 0.0228\, e \left(\frac{T_0}{1000}\right)^3 (T - T_0) \tag{11}$$

We thus see that the amount of radiation between two bodies having a given difference of temperature, increases in proportion to the third power of absolute temperature. This means that the temperature coefficient of radiation between two bodies differing only slightly in temperature, is + 1.0 per cent per deg. cent. This is at least three times greater than the temperature coefficient of heat conduction through solids or even gases.

Convection of Heat

The transmission of heat through liquids or gases takes place principally by means of currents set up in the fluid because of differences of density produced by the unequal heating. The amount of heat carried by convection depends on the velocity of the convection currents and also on the specific heat of the fluid. The currents are produced by the differences in density between the fluid in contact with a hot body and in contact with the cold body. This difference will be proportional to the coefficient of thermal expansion and also proportional to the density of the fluid itself. The currents produced by these differences of density have to act against the force of viscosity, thus the more viscous the fluid, the lower will be the velocity of the currents. We may therefore say, in a general way, that the amount of heat carried by convection will be roughly proportional to the product of specific heat, density, and expansion coefficient, and will be aproximately inversely proportional to the viscosity of the fluid.

From the very nature of convection, we would hardly expect it to obey such simple laws as those of radiation. It will depend upon the shape of the hot body, the distance between the hot body and the cold body, and upon each of the factors mentioned in the paragraph above.

There have been a great many investigations made of the laws of convection of heat, especially in gases. Dulong and Petit, in 1817, derived several empirical laws which have been more or less verified over rather narrow ranges of temperature by Péclet, (1860). These laws are today used in many engineering handbooks as best representing the knowledge of convection of heat. Within the last thirty years, however, much more valuable work has been done, and we now know that the formulas of Dulong and Petit are only rough approximations.

Lorenz (*Ann. d Physik*, Vol. 13, p. 582, 1881) derived formulas for the convection of heat from vertical plane surfaces, making, however, certain rather arbitrary assumptions. He obtained the formula

$$\mathbf{W}_c = 0.548 \sqrt[4]{\frac{\mathbf{c}\, g\, \mathbf{k}^3}{\mathbf{h}\, H\, T}} \sqrt{\rho}\, (T_2 - T_1)^{5/4} \qquad (12)$$

where $\mathbf{W}_c$ = Heat convection per unit surface.

$\mathbf{c}$ = Specific heat of the gas at constant pressure.

$\mathbf{k}$ = Its thermal conductivity.

$\mathbf{h}$ = Its viscosity.

T = Its average temperature.

ρ = Its average density.

g = Gravitational constant.

T_2 = Temperature of plane surface.

T_1 = Temperature of the gas at a great distance from the plane.

H = Height of the plane.

Putting in the data for air, at room temperature, 27 deg. cent. and standard atmospheric pressure, this equation reduces to

$$\mathbf{W}_c = 0.000399\, H^{-\frac{1}{4}} (T_2 - T_1)^{5/4} \qquad (13)$$

where $\mathbf{W}_c$ is expressed in watts per sq. cm., and H is in cm.

This equation agrees well (within five per cent) with the results calculated by the writer from the experimental results of C. P. Randolph on the convection of heat from disks of metal 7½ in. (19 cm.) in diameter. It is very probable, from the method of derivation of this equation, that the influence of the height of the plane on convection is not as great as indicated by the term $H^{-\frac{1}{4}}$ in the equation.

Boussinesq (*Comptes Rendus*, 132, 1382, 1901) has treated the mathematical theory of free convection and obtained formulas similar in form to those of Dulong and Petit. In a later paper (*Comptes Rendus*, 133, 257, 1901) he develops the theory of forced convection from plane surfaces as well as from cylinders, spheres and ellipsoids. In all such calculations the simplifying assumptions that need to be made render it necessary to subject the formulas to very careful experimental test before much reliance can be placed upon them.

Compan [*Ann. Chim. phys.* (7) 26, 488 (1902)] has made elaborate experiments on free convection from a copper sphere two cm. in diameter placed in hollow concentric spheres of various sizes. He worked at pressures ranging from a few thousandths of a mm. up to six atmospheres. He varied the temperature of the small sphere from 300 deg. down to 50 deg. He found that over this whole range of temperature and for pressures above 20 cm., the convection varied with the 1.233 power of the temperature difference and with the 0.45 power of the pressure.

Ayrton and Kilgour [*Phil. Trans.* 1892, abstract in *Proc.* Roy Soc. 50, 166 (1891)] have made elaborate investigations of the heat losses in air from very fine platinum wires [0.0012 to 0.014 inch (0.03 to 0.35 mm.) in diameter] at temperatures from room temperature up to 300 deg. cent. Their results were expressed in tables and empirical formulas only.

They found that the heat loss from the wires was nearly independent of the diameter of the wires.

Porter (*Phil. Mag.*, Vol. 39, p. 267, 1895) pointed out that this practical independence of the convection from the diameter of the wire may be accounted for by assuming that the heat is carried from the wire principally by conduction. He derives some equations with three empirical constants, which agree excellently with Ayrton and Kilgour's results. However, he obtained values for the radiation from the wire and for the conductivity of the air which were totally different from those which we now know to be the true values. His formulas must be looked upon, there fore, practically as empirical formulas.

A. Russell [*Phil. Mag.* 20, 591, (1910)] recalculated the results of Boussinesq (*loc. cit.*) and put them in a practical form. These are purely theoretical equations which give the heat lost by convection from cylinders and planes in currents of gases or liquids.

The writer (*Phys. Rev.*, Vol. 34, p. 401, 1912) showed that the free convection of heat from small wires consisted essentially of conduction through a film of gas of definite thickness. It was shown experimentally that the thickness of the film is independent of the temperature from 100 deg. cent. up to the melting-point of platinum. The thickness of this film depends on the diameter of the wire, but in such a way that it may be calculated with no other data than the diameter of the wire and the diameter which the gas film would have in the case of convection from a plane surface. To calculate the heat lost by convection from any wire, all that is required is to know the coefficient of heat conductivity of the gas and the thickness of this gas film for a plane surface, this latter being a constant quantity characteristic for a gas at any given temperature and pressure.

It was shown [Langmuir, TRANSACTIONS, A. I. E. E., p. 1229 (1912)] that the formulas derived from this film theory agreed excellently with the experimental data of Kennelly (TRANSACTIONS A. I. E. E., Vol. 28, p. 363, 1909) on the convection of heat from small copper wires in air at various pressures and in air moving at different velocities.

In the following pages the writer has attempted to give the principal laws for the convection of heat under the different conditions which influence it.

Free Convection. The film theory of convection makes it possible to calculate the convection from plane surfaces, from cylinders or wires of any diameter, and from spheres.

In the case of plane surfaces, the heat lost by convection is calculated simply by considering that there is a layer of air 4.3 mm. thick, adhering to the surface, and through which the heat has to be carried by conduction. It must, of course, be remembered that in addition to this heat loss by convection, there is the heat lost by radiation.

This film theory fully explains the fact that for wires of very small diameter the heat lost by convection is nearly independent of the diameter. This follows from the shape factor (see equation 5) for concentric cylinders, since the logarithm of a number varies only slowly as the number increases.

The amount of convection from the surface differs only slightly, whether the surface is placed vertically or horizontally. With a surface placed in the latter position, so that it is exposed to the air above it, the heat lost by convection is about 10 per cent greater than when the surface is placed vertically. With the surface horizontal, but inverted so that it must lose its heat downward through the air, the heat lost by convection is naturally considerably less. For a 7½-in. (19-cm.) disk, placed in this position, the heat loss was actually found to be only 50 per cent of the heat lost when the surface is placed vertically.

The amount of convection from a surface probably does not depend, to any great extent, on the nature of the surface. At least, so far as the writer knows, there is no trustworthy experimental evidence that the surface has any influence.

It is a surprising fact that between the temperatures 100 and 500 deg. cent. the convection calculated from the film theory as above outlined, gives nearly identical results with those calculated by Lorenz's formula (see equation 12). At temperatures above 500 deg. cent., both for convection from plane surfaces and from wires, the convection calculated from the film theory agrees better with the experimental facts than that calculated from Lorenz's formula. At temperatures much below 50 deg. the two formulas begin to diverge quite widely. The film theory would indicate that the convection would fall off practically linearly as the temperature difference decreases; whereas, according to Lorenz's equation, the temperature would be proportional to the 5/4th power of the temperature difference. This would mean that for very slight differences of temperatures—for example, 5 or 10 deg.—that Lorenz's equation would give very much lower results than would be obtained from the calculation from the film theory.

To decide which of these two theories would give the correct result for very small temperature differences, will require careful experimental investigation. Probably the bulk of the evidence at present is in favor of Lorenz's equation.

It should be pointed out that there is no theoretical reason known why the thickness of the film should remain constant, as the temperature of the wire or plane varies. This is simply an experimentally determined fact at temperatures above 100 deg. cent. It is quite possible that for very small temperature differences, the film thickness might become greater. In any case, however, the variation in the film thickness down to temperature differences as low as 30 deg., would not be important in most calculations of heat convection.

Effect of Pressure on Free Convection. The heat conductivity of gases is independent of the pressure. According to the film theory, therefore, the effect of pressure on the amount of heat convection would depend simply upon the effect of pressure on the film thickness, which we will call B. Theoretically, it would be very difficult to determine exactly what this effect would be. But it is certain that as the density of the gas decreases by the reduction of pressure, the thickness of the film B would increase; not necessarily in inverse proportion to the pressure, however.

From Kennelly's data the writer found that B varied in inverse proportion to the 0.75 power of the pressure. From some recent experiments with small wires in air, over a wide range of pressure, the writer finds that B varies more nearly in inverse proportion to the first power of the pressure. From small wires, however, the amount of heat convection depends only slightly upon the diameter of the film. In these experiments, therefore, it is very difficult to find accurately the law according to which the film thickness varies with the pressure. As the size of the wire decreases, the convection becomes less and less sensitive to pressure changes.

For the effect of pressure on the convection from plane surfaces, there is only very meagre experimental data. Compan, in studying the convection from spheres 2 cm. in diameter, concludes that the convection varies with the 0.45 power of the pressure. According to Lorenz's equation, the amountof convection from plane surfaces would be proportional to the 0.5 power of the pressure. This would be equivalent to saying that B varies in inverse proportion to the 0.5 power of the pressure.

To decide exactly how convection varies with pressure, we require further experimental work on plane surfaces in air at various pressures. Provisionally, it is probably safe to say that for the convection from plane surfaces or wires, the value of B is inversely proportional to the 0.5 power of the pressure.

Effect of Temperature on Free Convection. From experiments by the author on convection from platinum wires in air at various temperatures from − 180 up to 700 deg. cent., it would appear that the film thickness for a plane surface is approximately proportional to the absolute temperature.

The temperature coefficient of heat convection for small wires will simply depend on the temperature coefficient of the heat conductivity of the air, since in this case the amount of heat convection depends so slightly on the film thickness. From Sutherland's equation (2) it may be shown that in the neighborhood of room temperature, the heat conductivity of air increases approximately in proportion to the 0.76 power of the absolute temperature. This would mean that the temperature coefficient of heat conductivity at room temperature is + 0.25 per cent per degree, and this would be the temperature coefficient of heat convection from the wire.

In other words, more energy would be required to maintain a wire at a given temperature elevation above its surroundings, the greater the temperature of the surroundings.

It is quite different with the convection of heat from plane surfaces. Here, the convection depends not only on the heat conductivity of the air, but also on the thickness of the film. Since the latter varies proportionally to the temperature, the amount of convection from plane surfaces will be proportional to

$$\frac{T^{0.76}}{T} = T^{-0.24}$$

In other words, the temperature coefficient of the heat convection will be − 0.08 per cent per deg. cent. From plane surfaces the convection with a given temperature difference decreases slightly with increasing air temperature.

Lorenz's equation would lead to the temperature coefficient of − 0.37 per cent per degree, for convection from plane surfaces.

Forced Convection. The effect of air currents on convection is very great, as is well known. However, the air currents that occur in an ordinary room, at some distance from the windows, have only very slight effect on the convection from wires and plane surfaces heated to several hundred degrees. By placing a few large screens around the body, very consistent results are obtained. With higher wind velocities, such as those obtained by placing an electric fan close to the body, the amount of convection, both from plane surfaces and from wires, increases about four-fold. Measurements with an anemometer showed the wind velocity in this case to be about 400 cm. per second.

Kennelly has investigated the effect of wind velocity on convection from small wires, and has found that the amount of convection varies approximately in proportion to the square root of the velocity. For velocities from 300 to 1800 cm. per second, he finds that the convection is proportional to the quantity

$$\sqrt{\frac{v + 25}{25}}$$

That is, the convection of any given wind velocity can be calculated from the velocity in quiet air by multiplying by the above factor. From some very rough experiments by the writer, on convection from plane surfaces and also from some calculations from published data (see article in *Trans.* Am. Electrochem. Soc., Vol. 23, 1913,) the amount of convection is again found to vary with the square root of the velocity, and the above factor is found to be

$$\sqrt{\frac{v + 33}{33}}$$

Compan, in his experiments on spheres, finds that the convection varies with the square root of the velocity. Boussinesq has also arrived at the same conclusion from theoretical calculations.

From Kennelly's results, the writer drew the conclusion that the film thickness varied inversely as the 0.75 power of the velocity. This gave results agreeing well with Kennelly's data, but it has been found that the film thickness, in the case of forced convection from plane surfaces, is very different from that found for small wires in wind of the same velocity. This fact renders the film theory almost useless in cases of forced convection.

The best single equation for the calculation of convection of wires is probably an equation calculated by Russell (*loc. cit.*) by the method first given by Boussinesq. This equation is

$$\mathbf{W} = 8\sqrt{\frac{\mathbf{c}\,\rho\,\mathbf{k}\,v\,a}{2\,\pi}}\,(T_2 - T_1) \qquad (14)$$

where $\mathbf{W}$ = Convection loss per unit of length from the cylinder.
$\mathbf{c}$ = Specific heat of the gas or liquid at constant pressure.
ρ = Density of the fluid.
$\mathbf{k}$ = Heat conductivity.
v = Velocity.
a = Diameter of the cylinder.
$T_2 - T_1$ = Difference of temperature between the cylinder and the fluid.

If, in this equation, instead of the coefficient 8 in front of the radical sign, we place 5 or 6, we find that the results agree well with Kennelly's experimental data. On the other hand, for large cylinders from 2 to 10 cm. in diameter, the formula gives good results with the coefficient between 7 and 8.

In the case of forced convection in air at room temperature, equation (**14**) reduces to

$$W = 0.000180\, C \sqrt{V a}\, (T_2 - T_1) \text{ watts per cm.} \qquad (\mathbf{15})$$

where c is the number that varies from 5 to 8, according to the size of the wire, as described above.

From equation (**14**) we can calculate that with a given difference of temperature and given wind velocity, the amount of convection would vary with

$$\sqrt{T^{-1+0.76}}$$

This would mean that forced convection under these conditions would have a temperature coefficient of − 0.04 per cent per degree; that is, with a given difference of temperature, the amount of convection would decrease slightly as the temperature of the air increases.

If the velocity of the air is produced by centrifugal force, as it often is in electric machinery, the velocity of the air currents will probably decrease as the air temperature increases, since the density decreases and the viscosity increases. The effect of this would be to make the temperature coefficient numerically still greater.

If the convection currents are caused by flue action (for example, because of heat given to the air in long passages), this effect will be still more marked, so that the temperature coefficient will be strongly negative.

In problems on convection of heat, the flue action is often very important. For example, in studying the convection from a horizontal, plane disk, 7½ in. (19 cm.) in diameter, we find that if we place a cylinder of asbestos paper about 10 in. (25 cm.) in diameter and 6 in. (15 cm.) high, around the disk, the convection is nearly doubled when the disk is at about 150 deg. At higher temperatures the effect of the cylinder becomes relatively much less. By observing the motion of smoke in the air above the heated disk, it is seen that the air descends along one side of the cylinder, moves across the disk, and rapidly rises along the other side of the cylinder. In general, with vertical surfaces of considerable height, any means of preventing horizontal air currents from flowing in towards the middle portion of the surface will tend to increase the convection, since it causes the air to be drawn over the lower portion of the surface with very much increased velocity. However, if such flues as are made in this way are too long, the convection again tends to decrease, since the air that comes in contact with the upper part of the surface is already heated by flowing along the lower part of the surface, and is therefore capable of taking up very little additional heat.

The whole subject of flue action in convection is as yet very little understood, and should be the subject of further careful experimental investigation.

Convection in Liquids. The convection in liquids, in principle, at least, does not differ radically from that in gases. However, the velocity of the currents set up is very much less than in the case of gases, but because of the very great heat capacity of liquids per unit volume, as compared with gases, the amount of heat carried by convection is usually much greater than with gases. The effect of the viscosity is usually of much more importance than in the convection of gases.

The viscosity of liquids, as a rule, decreases very rapidly with increase in temperature, so that the temperature coefficient of free convection in oil is always positive and has a very large value, but differs greatly for different oils.

In oil-cooled transformers, the importance of this effect was clearly pointed by S. E. Johannesen (The Effect of Different Air Temperatures on Temperature Rise of Electrical Apparatus, *The Rose Technic*, Rose Polytechnic Insitute, Terre Haute, Ind., Nov., 1904). To quote from this article:

It is well known that a transformer immersed in thin oil will run cooler than one immersed in thick oil, all other conditions being the same, and further, a transformer immersed in paraffin (poured while hot and allowed to solidify) would burn out if operated at a room temperature of 0 deg. cent., and there would even be danger if it were operated in a room having a normal temperature; but when immersed in a light oil and run in a room having a normal temperature of even say 35 deg. cent., it would operate within safe heat limits. This experiment has been tried.

Specific heat of liquids differs very little at different temperatures, their power to absorb heat being approximately the same at any temperature within the working range of electrical apparatus. Therefore, the efficiency of a liquid acting as a cooling medium (when confined) depends upon its ability to flow, assuming that the points of absorption and dissipation are equally efficient.

The table below is given as an illustration to show approximately the heat which would be dissipated at various room temperatures, and fairly represents results of a heat run of a 100-kw., 60-cycle, 2000-volt, oil-insulated, self-cooling transformer.

Room temperature	Actual temperature rise	Loss at max. temp. rise, watts
5 deg. cent.	60 deg. cent.	1154
25 "	50 "	1191
35 "	45 "	1210

He points out for the class of oil-cooling apparatus that the Institute rule does not apply, as it gives a correction which is in the wrong direction.

Combined Effects of Radiation, Convection and Conduction

The heat lost by surfaces is equal to the sum of the heat radiated and the heat carried by convection. It may be of interest to calculate the relative magnitude of these two effects under various conditions. For a surface only slightly above room temperature in air at 20 deg. cent., we may calculate the heat lost by convection by determining the amount of heat that would be conducted through a layer of air 4.3 mm. in thickness. For one degree temperature difference the heat carried through such a film would be equal to the heat conductivity of the air, *i. e.*, 0.00025, divided by the thickness of the film in centimeters. This gives 0.00059 watt per sq. cm. per deg. cent. as the convection from a body in the neighborhood of room temperature.

For a body at 100 deg. cent., taking a mean conductivity over this range as 0.000275, we find in a similar way the convection to be 0.00064 watt per sq. cm. per deg. cent.

From equation (11) the radiation from a black body only slightly above room temperature (20 deg. cent.) is 0.00057 watt per sq. cm. per deg. cent. From equation (8), considering the radiation from a black body at 100 deg. cent. in surroundings of 20 deg. cent., we find the radiation to be 0.00086 watt per sq. cm. per deg. cent.

The total loss of heat from a surface will be simply equal to the sum of the convection and radiation. For most purposes, it is more convenient to deal with surface resistivities, rather than with surface conductivities. The resistivities are obtained by taking the reciprocal of the sum of the radiation and convection. In the following table are given the surface resistivities of plane surfaces, in three different positions, vertical, horizontal with exposed surface above, and horizontal with exposed surface below. In each case the resistivity at 20 deg. and at 100 deg. is given. Also, the resistivity for the case of a body which does not radiate any heat, and the resistivity for a body radiating as much as a black body. Any actual body will, of course, have a resistivity which lies between these two extremes.

There are some cases where the radiation of heat does not play any part in the heat loss from surfaces. For example, in the interior of machines the circulation of air may carry away heat, yet no heat will be lost by radiation. In this case, the surface resistance will be independent of the nature of the surface and will be that given in the table for a case of no radiation.

The effect of air circulation will always be to cut down the surface resistance, but even with relatively high wind velocities,

TABLE III

SURFACE RESISTIVITIES OF PLANE SURFACES

Position	20 deg. cent.	100 deg. cent
Vertical		
No radiation	1700	1560
Full radiation	860	670
Horizontal, exposed above		
No radiation	1540	1430
Full radiation	820	640
Horizontal, exposed below		
No radiation	3400	3100
Full radiation	1150	850

this effect will never cut it down to less than about one-fifth of its value with no wind.

Another important case, where the effects of radiation and convection need to be separated, is in determining surface losses from a piece of apparatus in a room in which the walls are at a different temperature from the air. It is readily seen that if the average temperature of the walls of a room is 10 deg. above the temperature of the air surrounding the piece of apparatus in the room, the apparatus will receive heat by radiation which it must give up by convection. Since at room temperature radiation and convection are about equally effective in removing the heat from a body which is a good radiator, it is evident that the machine will reach a temperature which is about half way between that of the air and that of the walls; that is, the temperature of the surface of the machine will be about five deg. above that of the air surrounding it. Similarly, if the walls are colder than the air, the machine will have a temperature lower than the air. It will be readily appreciated that this effect is by no means negligible, since the walls of a room are often 10 deg. warmer or colder than the air. For example, the roof may be heated by the sun's rays and the air in the whole upper part of the room may be very warm, whereas the air in the lower part of the room may be very much colder. Placing paper or cheese-cloth screens around the machine will almost completely prevent this difference of temperature of the air and machine.

As we have seen, small bodies, such as wires, take up the temperature of the air very much more readily than large bodies, and therefore this effect of radiation from the walls will be negligible in the case of wires.

We have seen that the combined effect of radiation and convection is equivalent to that of a surface resistance which may vary from about 600 to 1700. This resistance is equivalent to that of a layer of from 23 to 65 meters of copper or to a layer of from 1 to 3 cm. of rubber.

In calculations of the flow of heat through apparatus it is usually most convenient to determine the thermal resistance of the solid parts and simply add the surface resistance. If we then multiply the result by the number of watts of heat flow we obtain the total drop in temperature from the outside of the machine to the air surrounding it. Of course in actual machines it will usually be very difficult to calculate the thermal resistance of the solid parts. In any case, however, a knowledge of the relative importance of the various factors will be a long step towards the solution ot the problem.

A paper presented at the Midwinter Convention of the American Institute of Electrical Engineers, New York, February 26, 1913.

CURRENT RATING OF ELECTRIC CABLES

BY RALPH W. ATKINSON AND H. W. FISHER

At the request of the Standards Committee this paper has been prepared, the object being to present data relative to the carrying capacity of cables, which will be of value to engineers and users of cables. Especial attention is called to Table I (to be explained later) which gives data relative to the overload capacity of cables which have been working at different percentages of the normal carrying capacity. This question comes up frequently, engineers asking what per cent of overload can be applied to different cables. The per cent of such overload must of course depend upon the per cent of normal load at which the cable has been operating for several hours previous to the application of the overload. It is hoped that the formulas presented will be of value to engineers who have to deal with the subject under consideration.

The current rating of an electric cable depends entirely upon allowable heating, and is often more dependent upon or limited by external conditions than upon conditions intrinsic in the cable itself. On account of the extreme variability of these external conditions, the current rating of any given cable may be reduced to a half or a third, or even a small fraction of its normal rating. The time during which a cable may carry any given per cent overload is shorter than the time during which most other electrical apparatus can carry the same per cent overload. The allowable overload for any given duration of time depends simply upon the rate at which the cable will be heated to its limiting temperature. This is much more dependent upon the properties of the cable itself, than is the ultimate carrying capacity.

Very many formulas have been given as representing the carrying capacity of cables, beginning with the old rules which allow a certain number of circular mils per ampere. There is only one simple formula connecting the carrying capacity of conductors of different cross-sectional area which we have found useful and accurate. For nearly all conditions, it may be stated that the carrying capacity varies as the 1.3 power of the diameter of the equivalent solid conductor.

We will first discuss the limiting temperature. When insulating material is maintained at an excessive temperature, the first noticeable permanent change is in brittleness. It is sometimes important that this is true and that the first change is not in dielectric strength or, in fact, in tensile strength. A cable will sometimes work satisfactorily long after the insulation is badly injured by overheating, but only as long as it is undisturbed. The insulation of most non-rubber-insulated cables will stand a temperature of 150 deg. cent. for a very brief time without apparent permanent injury. If this temperature is maintained for more than a very short time, a matter of minutes, permanent injury results.

The limiting temperature is very much lower than this when it is maintained for a long period. To insure that progressive deterioration does not take place, the maximum temperature has been set by some as 65 deg. cent. and by others at 75 deg. For rubber, the temperature limit is lower and has been set at 50 deg. cent. For high-voltage cables, 6000 volts working pressure and above, another factor must be considered. The dielectric loss increases rapidly at the higher working temperatures, thus causing material additional heating. By some it has been said that the dielectric strength is much less at high temperatures, but we believe that this opinion was conceived because of the greater dielectric loss at high temperatures. A temperature limit of 50 deg. cent. has been named as the limit for high-voltage fibrous insulated cables. We believe that a temperature of 65 deg. cent. will not injure paper-insulated cables, but the increasing dielectric loss with increasing temperature must be considered and no exact rule can be given, as some insulating materials have a very considerably lower dielectric loss at high temperatures than others. We most earnestly hope to hear a thorough discussion of this question.

The temperature rise of a cable may be considered to be made up of:

1. The rise of copper above lead.
2. The rise of lead above immediate surroundings.
3. The rise of temperature of the surroundings due to the presence of the cables in the neighborhood.

It is only by an analysis of these elements that a general knowledge of temperature rise under all conditions can be obtained without experiment under any specific conditions for which information is desired. We intend to devote our attention to the first two elements of temperature rise in the present paper, these being the ones which are most nearly independent of variable external conditions. (For data concerning the other conditions we would refer to a paper read before the British Institution by Melsom and Booth in 1911. This paper also contained reference to a number of others. We would refer also to pages 213, 214 and 215 of Foster's "Electrical Engineer's Handbook," these data being based upon former experiments by one of the authors). We will take up individually the two elements to be considered.

The rise of copper above lead is equal to

$$t_1 = n\, b \log D/d \;\frac{I^2 \times 1000}{C.\,M.}\ \text{deg. cent.}$$

where D/d is the ratio of the inside diameter of the lead sleeve to the outside diameter of the copper conductor.

I is the current flowing in the conductor.

$C.\,M.$ is the cross-sectional area of the cable in circular mils.

b is an empirical "constant" which depends upon the kind of insulation and somewhat upon the temperature. For practical purposes, it may be taken, for either paper-insulated or varnished-cloth-insulated cables, as 0.15. (See below for value for triplex cable.)

n is the number of conductors.

This formula may be applied directly to the case of concentric cables simply by adding the temperature rise from copper to copper and from copper to lead, remembering that the heat from two or from three conductors must pass through the outer layer of insulation.

For flat duplex cables, the temperature rise may be taken as about 15 per cent greater than that of a single-conductor cable with the same thickness of insulation. For triplex or round duplex cables we can use this formula if we use, for d, the diameter of the circle circumscribing the three conductors. We must also use a different value of "constant." By experiment, this has been found to be from 0.16 to 0.185, the latter value being for cables where the insulation is very thin, relative to the size of the conductor.

The rise of lead above air or surrounding objects is

$$t_2 = n\,a\,\frac{I^2 \times 1000}{d_1\,C.\,M.}\ \text{deg. cent.}$$

Where d_1 is the outside diameter of the lead sheath in inches a may be taken as 0.06. This value applies where a clean bright lead sheath is suspended in free air. The value of a changes considerably with the size of the sheath, being less when the diameter is small. Preventing the natural circulation of air, increases a, while even a slight forced circulation decreases it materially. A rough, black coating on the lead will also reduce the value of a. For a flat duplex cable, use the formula as for a single conductor and increase the temperature rise of cable about 20 per cent.

It is very interesting that the temperature rise of a cable is very nearly the same, regardless of the thickness of insulation. This is because the change of the rise of lead above air counterbalances the change in the rise of copper above lead when the thickness of insulation is changed. Moreover, the experiments upon the rise in temperature of cables in air, of which these formulas are an expression, show practically the same temperature rise for a cable suspended freely in air as previous experiments show for a cable of the same kind laid in a duct system, no other cables being present. The table on page 215 in the previously mentioned section of Foster's Handbook may be used directly to give the carrying capacity of a three-conductor cable in free air when the permissible temperature rise is 45 deg. cent. The values should be increased one-third for single-conductor cables. Thus we see that not always do wide variations materially affect the carrying capacity of cables.

The temperature rise of a bare conductor in free air follows a very simple and easily derived law very closely. We may neglect the variation of a number of factors, the variation in the specific heat and in the proportionality between heat radiated and temperature difference, and may even neglect, for most practical purposes, the variation in the resistance of the copper. The effect of the last-named variation is in the opposite direction from that of each of the others and accordingly is very nearly counterbalanced by them. Tests show that the following law is quite closely obeyed:

$$x = T - \frac{T}{e^{ta}} \text{ or } e^{ta} = (10)^{ta/2.3} = \frac{T}{T-x}$$

x is the rise in temperature after t minutes.

T is the temperature which would be attained by the cable after an infinite time, supposing in the calculation of this temperature that the same factors mentioned above are constant until the temperature is reached.

$$a = \frac{\text{temperature rise per minute at the beginning}}{\text{ultimate temperature rise}}$$

$$= \frac{\left(\frac{I \times 1000}{C.\ M.}\right)^2 \times 2.06}{T},$$

and is constant for a given conductor.

e is the base of the Napierian system of logarithms.

This same type formula can be applied to insulated cables if we introduce an empirically determined constant, one which can be partly verified by theory.

The formulas then become

$$e^{ta/r} = (10)^{ta/2.3r} = \frac{T}{T-x}$$

The constant r represents the ratio between the heat which is stored in the entire cable and the heat which is stored in the copper.

The constant r varies with different styles of insulation and with their thickness relative to size of conductor. It also varies in the same cable, according to the time which is given for heat to penetrate into the insulation.

The constant r varies from practically one, with a very thin insulation, to three or more in a cable having a very small conductor with thick insulation. Based upon this formula and empirically determined values for r, we have calculated Table I given herewith.

To determine the time during which a three-conductor cable may carry a given per cent overload, use the table as for a single-conductor cable having twice the cross-section area. For a two-conductor cable use the table as though for single-conductor cable of 50 per cent greater cross-section.

This table applies equally as well when the rating of a cable is changed by a change of temperature limit or by a change of initial temperature. The change in time for which an overload may be carried, caused by different thickness of insulation, will seldom be important. We have assumed in making the tables that

TABLE I

OVERLOAD CARRYING CAPACITIES OF CABLES

A Per cent	No. 6 B. & S. gage 80%	50%	0%	No. 4 B. & S. gage 80%	50%	0%	No. 2 B. & S. gage 80%	50%	0%	No. 0 B. & S. gage 80%	50%	0%
	Time in minutes			Time in minutes			Time in minutes			Time in minutes		
200	0.6	1.3	1.8	0.8	1.6	2	1	2.2	2.9	1.5	3.1	4.5
175	0.9	2	2.7	1.2	2.5	3	1.5	3.2	4.5	2.3	5	6.5
150	1.6	3.2	4.5	2	4	5.5	2.7	5.5	7	4	8.5	10
125	3.5	7	8.5	4.5	9	11	6	12	15	9	16	20
Per cent	No. 000 B.&.S. gage 80%	50%	0%	300,000 cir. mil. 80%	50%	0%	500,000 cir. mil. 80%	50%	0%	700,000 cir. mil. 80%	50%	0%
	Time in minutes			Time in minutes			Time in minutes			Time in minutes		
200	2.1	4.5	6	3.2	7.5	9.5	5	10	13	6	12	15
175	3.2	6.5	9	5	11	13	7	14	17	8.5	16	22
150	5.5	12	15	9	16	20	12	21	26	14	26	33
125	13	23	28	18	31	39	23	40	45	28	50	60
Per cent	1,000,000 cir. mil. 80%	50%	0%	1,200,000 cir. mil. 80%	50%	0%	1,500,000 cir. mil. 80%	50%	0%	2,000,000 cir. mil. 80%	50%	0%
	Time in minutes			Time in minutes			Time in minutes			Time in minutes		
200	7.5	15	19	8.5	17	22	11	21	26	12	24	30
175	11	22	27	13	25	31	16	30	36	18	34	40
150	18	33	41	20	37	45	25	46	55	28	50	65
125	35	60	73	40	70	85	50	80	95	55	95	110

5/32-in. (3.97-mm.) insulation is used. The table applies approximately for any of the styles of insulation with which lead cables are now insulated.

The data given are the lengths of time during which the loads given in the left-hand column can be carried, when the loads given in the first horizontal line of each table have been continuously applied previously to the application of the overload. Load is expressed in per cent of continuous carrying capacity, that is, normal load.

It must be noted that a considerable variation in the time frequently makes only a few degrees difference in the temperature This is most particularly true where the tables are hardest to interpolate closely.

Table II gives the time required to attain about 90 per cent of final temperature.

TABLE II

Size	Hours
No. 6	¾
1	1
No. 000	1¼
cir. mils	
300,000	1½
500,000	2
700,000	2½
1,000,000	3
1,500,000	3¾
2,000,000	4¼

It will require 50 per cent longer time to attain 97 per cent of final temperature and will require 50 per cent less time to reach about 68 per cent of final temperature.

The time given in this table may also be taken as the time interval which must elapse between overloads. If repeated oftener, the allowable duration of overload is decreased. If repeated at intervals of one-half this time, the allowable duration of overload is decreased 35 per cent.

We give an example of the use of these tables. It is required to determine the time an 800,000-cir. mil cable can carry 1200 amperes, when it carries continuously 400 amperes, its continuous capacity being given as 650 amperes. Express the loads as 185 per cent and 62 per cent of normal rating. We find from the tables that this load may be carried for about 11 minutes at intervals of 2½ hours.

For excessive overloads, we suggest that the temperature rise be calculated by the formula

$$\text{temp. rise} = \left(\frac{I \times 1000}{C.\,M}\right)^2 \times 1.15 \text{ deg. cent. per minute.}$$

The time required to reach the limiting temperature is then found directly.

A paper presented at the Midwinter Convention of the American Institute of Electrical Engineers, New York, February 26, 1913.

THE HEATING OF CABLES CARRYING CURRENT

BY SAUL DUSHMAN

The problem of determining the current-carrying capacity of a cable or wire for a given rise of temperature has been the subject of a number of investigations since the appearance of the classical paper by Professor Forbes in 1884. That, however, the last word has not been said on this subject is evidenced by the fact that during 1910 and 1911 an elaborate investigation was carried out at the National Physical Laboratory, England, at the request of the Wiring Rules Committee of the Institution of Electrical Engineers.[1]

While the present investigation was undertaken mainly with the object of obtaining data for calculating the carrying capacity of a special type of cable, it has been thought that a description of the methods used as well as the publication of some of the results obtained might be of interest in connection with the general subject of heat losses.

I. Description of Method

The carrying capacities of insulated cables may be determined either directly or indirectly. In the former case the actual rise in temperature is determined for a given current either by noting the increase in resistance of the conductor (*e.g.*, Melsom and Booth,) or by means of thermocouples placed in thermal contact with the conductor itself. By carrying out tests with a number of different sizes under varying conditions of temperature rise,

1. S. W. Melsom and H. C. Booth, The Heating of Cables with Current. *Journal* Inst. Electrical Engineers (Eng.) 47, 711-751 (1911). A fairly complete list of references to previous investigations will be found at the end of their paper.

an empirical relation is obtained, from which the carrying capacity may be calculated for any given rise in temperature.

In the indirect method the carrying capacity is calculated from the thermal resistivity of the insulation and the emissivity of the surface. The method has the obvious advantages of requiring a far smaller number of experimental data and of being much more generally applicable, especially in those cases where the design of fresh cable insulations is involved.

TABLE I

Nomenclature

d = Diameter in inches of conductor.

D_1 = Diameter over first layer of insulation.

D_n = " " nth " " "

S = Cross-section of conductor, in square inches.

$= \nu \times \delta^2 \times \pi/4$ for stranded conductors.

ν = Number of wires in stranding.

δ = Diameter of individual wire in inches.

π = 3.1416

T = Maximum rise in temperature in deg. cent.

Θ = Temperature rise (variable).

x = Distance from heated end, in inches.

R = Electrical resistance per 1 inch of conductor, at 25 deg. cent.

$$= \frac{0.691 \times 10^{-6} \times 1.02}{S} \text{ for stranded conductors.}$$

$$= \frac{0.691 \times 10^{-6}}{S} \text{ for solid conductors.}$$

R_T = Electrical resistance per 1 inch at $T + 20$.

$$= \left(\frac{258 + T}{263}\right) R$$

K_1 = Thermal resistivity (*in deg. cent. per watt per inch*) of material of first layer.

H = Surface resistivity (in deg. cent. per watt per square inch).

K_n = Thermal restivity of material of nth layer.

r_n = Thermal resistance of nth layer of insulation per 1 inch length of cable.

$$= 0.365\, K_n \log \frac{D_n}{D_{n-1}}$$

Σr = Total internal thermal resistance of insulation per 1 inch length.

r_e = external resistance per 1 inch length.

$$= \frac{H}{\pi D_n}$$

W = Permissible watts per one inch length for rise of 1 deg. cent.

$$= \frac{1}{\Sigma r + r_e}$$

i = Current in amperes.

C = Heat capacity of cable per 1 inch length, in *watts*.

b = " Constant " in equation for rate of heating.

$$= \frac{60 W}{C}$$

It is evident that after current has been passing through the conductor for a certain interval of time, a stationary state is attained at which the heat generated per unit length of cable is completely conducted through the insulation and dissipated at the surface. Under these conditions,

$$i^2 R_T = \frac{T}{\frac{2.3}{2\pi}\left[K_1 \log \frac{D_1}{d} + K_2 \log \frac{D_2}{D_1} + \ldots\right] + \frac{H}{\pi D_n}} \tag{1}$$

The nomenclature used in this and the following equations is that given in Table I.

Equation (1) may be written in the more convenient form:

$$i^2 R_T = \frac{T}{\Sigma r + r_e} \tag{2}$$

Data on the heat resistivity of electrical insulations have been published by H. D. Symons,[2] Professor Porter[3], C. P. Randolph[4] and A. Winkelmann[5].

Regarding H, it is known that for a " perfect " radiator the

2. *Journ.* Inst. Electrical Engineers (Eng.) read Nov. 29, 1911.
3. *Phil. Mag.* 20, 511 (1910).
4. *Trans.* Am. Electrochem. Soc. 21, 545 (1912).
5. Handbuch der Physik, III. 504, (1906).

value may be as low as 100 (expressed in degrees centigrade per watt per square inch), while for imperfect radiators it becomes as high as 200.

The great difficulty in applying equation (1) consists in determining what value of H should be used, since it varies so greatly with the nature of the surface. Furthermore, the data obtained for K on the insulation itself by the usual methods for determining heat conductivity may be altogether different from the value for the insulation on the cable, owing to conditions of manufacture.

In view of these considerations a method was adopted for determining the terms Σr and r_e [equation (2)] separately on the cable itself. Knowing the values of d and D for the cable it is then possible to calculate Σr and r_e for all other sizes.

This method, originally used by Despretz, is described in standard works on heat[6]. If we heat one end of a piece of cable which is so long that the other end always remains at room temperature, the relation existing at the stationary state between the rise in temperature, Θ, and the distance, x, from the heated end, will be given by the differential equation

$$\frac{d^2\theta}{dx^2} = \mu^2 \theta^p \tag{3}$$

where

$$\mu^2 = \frac{K'}{S(\Sigma r + r_e)} \tag{4}$$

K^1 = Thermal resistivity of materials of conductor.
S = Cross-section of conductor.
p = A constant.

For temperatures below 100 deg. cent., p is practically equal to 1; and the solution of (3) therefore assumes the familiar form[7]

$$\log_e \theta = -\mu x + \log_e \theta_1 \tag{5}$$

where θ_1 denotes the temperature rise at $x = 0$.

6. Preston, Theory of Heat, pp. 631-636. A. Winkelman, Handbuch der Physik, III. Wäime, p. 451 (1906)

7. For values of p only slightly greater than 1, it can readily be shown that the solution of (3) may be written in the form

$$\log_e \theta \left\{ 1 - \frac{p-1}{4} \log_e \theta \right\} = -\frac{\mu}{\sqrt{\frac{p+1}{2}}} x + \log_e \theta_1 \left\{ 1 - \frac{p-1}{4} \log_e \theta_1 \right\} \tag{5a}$$

By placing thermocouples in small holes drilled through the insulation at different points, it is therefore possible to determine μ and by noting the temperature on a couple in contact with the covering of the insulation it is further possible to determine the ratio Σr to r_e. The method thus gives, in one experiment, data from which both the thermal resistivity (K) of the particular insulation as actually put on the cable, and the surface resistivity (H) may be determined.

II. Experimental

Lengths of about five feet of each of the cables described below were used in the experiments. A few inches at one end were bared and the conductor surrounded by a heating coil. The heated end was packed in some good heat-insulating material contained in a wooden box, so that no heat would penetrate the cable insulation except through the conductor. Holes were drilled through the insulation into the copper at distances of three or five inches apart, a small mercury globule dropped into the bottom of each hole, and calibrated thermocouples (10-mil diameter calorite-advance) placed in the holes.

Description of Cables Tested. (For meaning of symbols consult Table I.)

Cable A. 500,000 cir. mils. $\nu = 61$ $\delta = 0.0906$, $d = 0.8154$. *Rubber insulation*, $D_1 = 1.003$; cotton braiding impregnated with asphaltum, $D_2 = 1.12$. $S = 0.393$ $R = 1.793 \times 10^{-6}$

Cable B. 0000 solid conductor, $d = 0.46$. *Varnished cambric insulation*, $D_1 = 0.616$; asbestos and cotton braiding, $D_2 = 1.06$. $S = 0.166$ $R = 4.16 \times 10^{-6}$

Cable B-1. Same as B; cotton and asbestos covering removed.

Cable C. 250,000 cir. mils. $\nu = 37$, $\delta = 0.0823$, $d = 0.5754$. *Varnished cambric insulation*, $D_1 = 0.952$; lead covering, $D_2 = 1.14$. $S = 0.196$ $R = 3.582 \times 10^{-6}$

Cable C-1. Same as C. Lead painted dull black.

Cable D. *Three-conductor cable*, each conductor 250,000 cir. mils. *Varnished cambric insulation* on each conductor, $D_1 = 0.70$. Jute and compound between conductors and outside layer of varnished cambric, diameter over three conductors, 1.640. *Varnished cambric insulation*, $D_1 = 1.765$; lead covering, $D_2 = 1.984$.

Cable D-1. Same as D. Lead painted dull black.

Cable E. 250,000 cir. mils, $d = 0.5754$; rubber insulation, $D_1 = 0.825$; lead casing, $D_2 = 0.935$; tape treated with rubber compound, $D_3 = 1.007$; braided with galvanized steel wire and thoroughly painted, $D_4 = 1.07$.

$S = 0.196$ $R = 3.582 \times 10^{-6}$

The thermocouples used gave an electromotive force of about 0.04 millivolts per deg. cent. Under the actual experimental conditions a rise of temperature of 1 deg. cent. produced a deflection of 6.4 millimeters on the scale, at one meter. Denoting the deflection by α, it is evident from the proportionality between Θ and α in the range of temperatures used (20-60 deg. cent.) that in equation (5) we could substitute α for Θ.

TABLE II.
EXPERIMENTS ON CABLES C AND C-1
Deflection at $x =$

No. of Expt.	2	5	8	11	14	17	20	23	μ
1	438	338	257	203	156	120	91	68	0.0860
	(438)	338	261	202	156	121	93	72	(Calculated)
2	427	320	240	180	138	103	76	55	0.0947
	(427)	320	240	180	135	101	76	57	(Calculated)
3	411	307	225	169	130	96	71	51	0.0958
	(411)	307	230	173	130	97	72	55	(Calculated)
4	420	313	231	174	132	97	71	50	0.0971
	(420)	315	236	177	133	99	74	56	(Calculated)

While theoretically it is only necessary to obtain values of Θ for two different values of x in order to calculate μ, readings were usually taken at five or more different points along the cable. The values of log α were then plotted as ordinates with corresponding values of x as abscissas. From the straight line drawn through all the points (or so as to take in as many points as possible) an average value of μ was then determined.

To check the results, the average value of μ from all the experiments on any one cable was then used to recalculate the deflections on the galvanometer scale corresponding to the different points.

The temperature at the surface was very easily determined in case of lead-covered cables by drilling holes into the lead and securing an intimate contact between the lead and the couple by means of a mercury globule. In the case of cotton-covered cables, the couple was cemented to the surface by means of Canada balsam and a narrow band of tape.

As an illustration of the results obtained, the Table II gives the observations in four experiments with cables C and C-1. In the last three of these the lead covering was painted dull black.

In experiment 1, the deflections were recalculated from $\mu = 0.086$, in the other experiments the average value from the three experiments was used to recalculate the deflections. When it is considered that a deflection of 6.4 corresponded to a difference of 1 deg. cent., the agreement must be considered quite satisfactory.

TABLE III.

Cable	μ	$\Sigma r + r_e$	W	$r_e W$	Σr	r_e	H	K	Calculated Σr	Calculated r_e
A	0.0765	47	0.0213	0.74	12.2	34.8	122	242	12.6	34.1
B	0.085	90	0.0111	0.40	54	36	120	410	——	——
B-1	0.095	72	0.0139	0.79	15.1	56.9	110	330	18	62
				0.69	22.3	49.9	97	480		
C	0.086	74	0.0135	0.69	23	51	183	290	24	53
C-1	0.096	59.5	0.0168	0.61	23.2	36.3	130	290	24	39
				0.62	22.6	36.9	132	283		
D	0.0652	43	0.0233	0.71	12.4	30.6	191	288	12.9	31
D-1	0.74	33.4	0.0300	0.69	10.3	23.1	144	240	12.9	23
				0.65	11.3	22.1	138	261	12.9	22
E	0.110	45.5	0.022	0.59	18	27.5	91	260	18	32

By means of equation (4), values of $(\Sigma r + r_e)$ were calculated from the values of μ. In accordance with the most recent data on the thermal conductivity of copper[8] I have used the value $K' = 0.108$.

In Table III are tabulated the values of μ, $\Sigma r + r_e$; W; $r_e W$; r_e and Σr. The value of

$$r_e W = \frac{r_e}{\Sigma r + r_e}$$

was obtained from the temperature readings on the couples inside and outside the layer of insulation. From r_e and D_m,

8. Jaeger and Disselhorst, Wiss. Abh. d. Phys. Techn. Reichsanstalt, 3, 269 (1900).

the value of H was determined as indicated in equations (1) and (2). From Σr the average value of K for the different materials composing the insulation was determined in the same manner.

Assuming K for treated tape to be about 270 (Symons) and that the resistivity of the cotton braiding and rubber are approximately equal, we can deduce from the above experiments the following values of K for different materials:

Rubber	250
Varnished cambric (lead-covered cables)	300
" " (cotton " "	400

In view of the influence of slight errors in the experimenta observations on the values of K, only round numbers have been given.

The difference in the thermal resistivities of the lead-covered and cotton-covered varnished cambric insulations, is no doubt due to the presence of air spaces between the insulation and covering in the latter case.

For the surface resistivity H, the numbers deduced from the above experiments are as follows:

H for different surfaces

Painted steel braided armor	100
Cotton covering	120
Lead, ordinary smooth	190
" painted dull black	140

In the last two columns of Table III are given the values of Σr and r_e calculated by means of these data.

The most striking feature about these results is the effect of painting the lead on its surface resistivity. The fact that Σr remains constant while r_e alone varies as a result of painting the surface is very strikingly brought out by the results recorded in Table III. (Compare C and C-1; D and D-1). That, consequently, the current carrying capacity of the cables is thereby increased is also seen from the carrying capacities given in Table IV as calculated by means of equation (2).

Table IV shows that by painting the single-conductor lead cable, the carrying capacity for T = 60 deg. cent, is increased about 11.4 per cent., while in the case of the three-conductor cable the increase is 13.5 per cent. While this is quite in accord with what is known about the laws of surface losses, it is interesting to note that by *painting the lead-covered cable the carrying capacity can be increased as much as* 12 *per cent.*

Note on the Calculation of K for Varnished Cambric Insulation from Σr for Three-Conductor Cable. In comparing the values obtained for Σr in the case of cables C and D and of C-1 and D-1 it was found that the results could be calculated fairly well by assuming that the only effective thermal resistance in the case of three-conductor cables is the sum of the resistance of insulation on one of the conductors together with that of the covering, to which must of course be added the surface resistance.

Thus in the case of Cable D (or D-1),

$$\Sigma r = 300\left(0.365 \log \frac{0.70}{0.5754} + 0.365 \log \frac{1.765}{1.640}\right)$$
$$= 12.9$$

instead of 10.3 to 12.4 as obtained experimentally.

TABLE IV

		i for T =				
Cable	W	60	40	30	20	10
A	0.0213			571	475	342
B	0.0111	365	307	270		
C	0.0135	433	365	321		
C-1	0.0168	482	407	359		
D	0.0233	292	247	218		
D-1	0.0300	332	280	247		
E	0.0220		460			

In other words, the thermal resistance of the jute and compound is neglected, and the effective thermal resistance is that obtained by *assuming each conductor to transmit heat through only that one-third of its surface which is in contact with the covering.*

Apparently the same idea might be extended to twin conductors. The effective resistance would in that case also be that of the insulation around one conductor and that of the covering over both conductors.

Tests on the Carrying Capacity of Cable E. In order to see how accurately the carrying capacity could be calculated from experimental determinations of μ, an actual test was carried out with cable E.

About 40 feet of the cable were stretched out along the floor, being supported about six inches above the latter by means of blocks at intervals of three feet. The temperature at the copper was determined by means of thermocouples placed in holes drilled through the insulation. The temperature at the surface was also determined by fastening a couple to the steel braided armor in the manner described above. During the heating, readings were taken on one of the couples at intervals of 5 minutes and from time to time observations were taken at other points along the cable to see that the temperature rise was uniform. The second column in Table V gives the temperature rise at different intervals of time while constant current was passing through the cable. The third column gives the excess of temperature above that of the room at different intervals of time after breaking the current.

TABLE V

Time in minutes	Temperature above that of the room	
	On heating	On cooling
0	0	37.0
5	8.3	28.6
10	13.1	22.1
15	18.1	19.0
20	21	14.3
25	25	12.6
30	26.7	10
35	29.0	7.7
40	30.7	6.1
50	32.9	4.3
55	33.3	3.6
65	34.6	2.1
75	35.2	
95	35.6	
125	37.0	

The heating and cooling curves are plotted in Fig. 1. The heating curve indicates that at the stationary state ($t=\infty$) the temperature rise would have been 38 deg. For a temperature rise of 40 deg. the carrying capacity would therefore be

$$450\sqrt{\frac{40}{38}\times\frac{1}{1.008}} = 460 \text{ amperes}$$

This agrees very well with the result calculated in Table III, and shows that the indirect *method based on the determination of μ is quite reliable.*

Temperature of the Surface. The ratio of the external resistance (r_e) to the internal thermal resistance ($\Sigma\, r$) of the insulation was determined by noting the temperature of the surface. Corresponding to a temperature rise of 38 deg. cent. at the conductor, the temperature of the surface was 23 deg. cent. above that of the room. This gives a value for $r_e\, W$ of 0.6 and agrees very well with the result, 0.59, deduced in Table III.

Rates of Heating and Cooling. The smooth curves drawn through the points on the heating and cooling curves are found to satisfy the equations

$$\Theta = 38\,(1 - \epsilon^{-0.0425\,t}) \qquad (6)$$

and

$$\Theta = 37\,\epsilon^{-0.046\,t} \qquad (7)$$

respectively, where Θ denotes the excess of temperature above that of the room at the time t (in minutes).

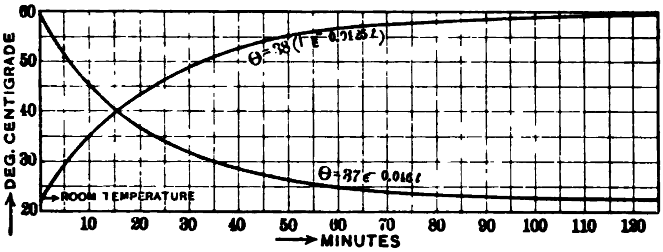


FIG. 1

III. CURRENT-CARRYING CAPACITIES OF RUBBER- AND VARNISHED-CAMBRIC-INSULATED CABLES

In the following section the data determined above have been used to calculate the current-carrying capacities of the various standard types of cables. The method of calculation is that indicated in equations (1) and (2). In Table VI are given the constructional data for rubber-insulated cables together with the calculated values of $\Sigma\, r$, r_e, W and i, based upon $K = 250$ and $H = 120$.

Table VII gives similar data for varnished-cambric-insulated cables, braided and leaded. The equations used in the calculations of r and r_e were as follows:

Braided cables $r = 400 \times 0.365 \times \log D/d$

Leaded " $r = 300 \times 0.365 \times \log D_1/d$.

Braided and leaded cables, $r_e = \dfrac{H}{\pi\, D_2}$

The values of ν, δ and d were obtained from the tables given in *G. E. Bulletin* No. 4787, pages 8 and 9. Data for D, Table VI, were taken from the tables, page 26, while values of D_1 and D_2, Table VII, were taken from the table on page 41 (thickness of insulation based on working pressure of 1000 volts).

On page 56 of the same bulletin are given the current-carrying capacities from which were taken the figures in the last two columns of Table VI and the last column of Table VII.

In all cases a room temperature of 20 deg. cent. has been assumed, and T = rise in temperature above 20 deg. cent.

Degree of Accuracy of Calculated Carrying Capacities

The most important fact brought out in these tables is the *effect of the radiating surface on the carrying capacity of the cable.* Comparing $\Sigma\, r$ and r_e for the same size of cable, it is seen that the thermal resistance of the surface varies in the different cases from 0.50 to 0.87 of the total resistance. In other words, the calculated carrying capacity depends more upon the accuracy of H than on that of K.

If the ratio $r_e/\Sigma\, r$ be denoted by n, equation (2) can be written in the form

$$i^2 R_T = \frac{T}{\Sigma\, r\,(n+1)}$$

Hence, it is readily shown that

$$\frac{\Delta\, i}{i} = -\frac{\Delta\,(\Sigma\, r)}{2\,(n+1)\,(\Sigma\, r)} \tag{8}$$

$$\frac{\Delta\, i}{i} = -\frac{-\Delta\,(r_e)}{2\left(1+\frac{1}{n}\right) r_e} \tag{9}$$

Assuming that on the average $n = 2$, it is seen that a change of 10 per cent, say, in the value of K affects the calculated value of i by no more than 1.6 per cent, whereas a similar change in the value of H, affects the value of i by 3.3 per cent.

Now with regard to the accuracy of the particular values of K and H used in the above calculations, we may assume the limit of error as 15 per cent in the case of K and 10 per cent in the case of H. Consequently the ***possible error in the calculated carrying capacities may be estimated at about* 2.5 *per cent.***

TABLE VI

Carrying capacities of rubber-insulated cables (suspended in air). Single-conductor, single braid.

Size of cable	ν	δ	S	d	D	$R \times 10^6$	Σr	r_e	W	i for T =			i according to *G. E. Bull.*	i according to *N. E. Code*
										30°C.	20°C.	10°C.		
2,000,000 cir. mils.	127	0.1255	1.57	1.632	1.99	0.449	7.9	19.2	0.0369	1501	1247	898	1400	1050
1,000,000 " "	61	0.1281	0.786	1.153	1.48	0.897	9.9	25.8	0.0280	925	769	554	900	650
500,000 " "	61	0.0906	0.393	0.8154	1.12	1.793	12.6	34.1	0.0214	572	476	342	550	390
250,000 " "	37	0.0823	0.197	0.5754	0.87	3.582	16.4	49.3	0.0166	356	296	213	320	235
0 B. & S.	19	0.0746	0.083	0.3750	0.63	8.507	20.6	60.7	0.0123	199	166	119	165	124
4	7	0.0773	0.033	0.2319	0.42	21.46	23.6	91.0	0.0087	105	88	63	75	64
6	*Solid*		0.0206	0.1620	0.35	34.21	30.6	109.1	0.0072	76	63	45	50	46
10	*Solid*		0.0082	0.1019	0.24	86.40	34.0	159.2	0.0052	41	34	24	20	24

TABLE VII

Carrying capacities of varnished-cambric-insulated cables (suspended in air). Single-conductor, leaded and braided.

Size of cable	Braided							Leaded								i according to G. E. Bull. $T = 60$
	D	Σr	r_e	W	i for T =			D_1	D_2	Σr	r_e	W	i for T =			
					60°C.	40°C.	30°C.						60°C.	40°C.	30°C.	
2,000,000 cir. mils.	2.14	17.3	17.8	0.0285	1775	1497	1318	1.89	2.14	6.9	28.3	0.0284	1773	1496	1317	1750
1,000,000 " "	1.56	19.2	24.5	0.0229	1126	949	836	1.37	1.56	9.4	38.7	0.0208	1073	904	797	1150
500,000 " "	1.19	24.0	32.1	0.0178	703	592	522	1.00	1.19	9.8	50.8	0.0165	676	570	501	660
250,000 " "	0.95	27.6	40.2	0.0139	438	370	326	0.76	0.95	13.2	63.7	0.0130	425	358	315	390
0 B. & S.	0.66	35.9	57.9	0.0107	250	210	185	0.53	0.66	15.6	91.6	0.0093	233	197	173	195
4 B. & S.	0.45	42.2	84.9	0.0079	135	114	100	0.36	0.45	21.0	134.4	0.0064	122	103	91	92
6 B. & S.	0.40	57.6	95.5	0.0065	97	82	72	0.31	0.40	25.7	151.0	0.0057	91	76	67	60

NOTE: For data on ν, δ, d, S and R consult Table VI.

Representation of Carrying Capacities by a General Formula. Melsom and Booth found that they could express the results obtained by them for the current-carrying capacities of rubber- and paper-insulated cables by an empirical equation of the type

$$\frac{i}{S} = c\left(\frac{D}{S}\right)^{N} \tag{10}$$

This may be written in the more convenient form:

$$\log\left(\frac{i}{S}\right) = N\log\left(\frac{D}{S}\right) + B^{*} \tag{11}$$

Where D = outside diameter of cable in inches,
S = cross-section of conductor in square inches,
and i = current in amperes,

they obtained the following values for N and B:

TABLE VIII

Constants for calculating carrying capacities of rubber- and paper-insulated cables according to Melsom and Booth

Type of insulation	N	B for T =		
		11.1 deg.cent.	16.7 deg.cent.	27.7 deg.cent.
Rubber-covered in air.	0.616	2.5611	2.6600	
Rubber-covered in casing.	0.66	2.4900	2.5539	
Lead-covered, paper-insulated.	0.50	2.6325	2.7292	2.8376

It is rather remarkable that the same form of equation expresses the results given in Tables VI and VII pretty closely.

In Fig. 2 the values of $\log\left(\frac{i}{S}\right)$ obtained from Table VI have been plotted as ordinates with corresponding values of $\log\left(\frac{D}{S}\right)$ as abscissas. The results given in Table VII for varnished cambric, cotton covered cables have been plotted in a similar manner in Fig. 3. The slope of the line thus obtained in each

*Logarithms to base 10 are of course meant, except where otherwise indicated.

case gives the exponent N, while B may be determined either graphically or in the usual manner.

The constants obtained for the three types of insulation tested are given in the following table:

TABLE IX

Constants for calculating carrying capacities of rubber- and varnished-cambric-insulated cables, suspended in air.

Type of insulation	N	B for T =				
		60 deg.	40 deg.	30 deg.	20 deg.	10 deg.
Rubber-covered.	0.515			2.93	2.85	2.707
Varnished cambric cotton-covered.	0.545	2.985	2.911	2.856		
Varnished cambric lead covered.	0.525	2.975	2.901	2.846		

Table X indicates the good agreement between the values of i calculated from the above interpolation formulas and the values obtained by the more elaborate calculation given in Tables VI and VII. Denoting the latter by i_1, and those calculated by means of Table IX by i_2, we find that with very few exceptions the two numbers agree to within two per cent.

TABLE X

Values of i_2/i_1

Size of cable	Rubber (T = 30 deg.)	Cambric braided (T = 60 deg.)	Cambric leaded (T = 60 deg.)
2,000,000 cir. mils.	1.007	1.012	0.985
1,000,000 cir. mils.	1.002	0.980	1.050
500,000 cir. mils.	1.003	0.989	0.982
250,000 cir. mils.	1.01	1.022	1.000
0 B. & S.	1.007	0.995	0.996
4 B. & S.	0.983	0.983	1.005
6 B. & S.	0.996	1.030	1.010
10 B. & S.	1.001		

Comparison of Results Obtained with Carrying Capacities According to G. E. Bulletin and N.E. Code. In Tables VI and VII are also given the carrying capacities recommended in the *G. E. Bulletin* and those according to the N. E. Code, 1907. From these figures the curves *AA* and *BB*, Fig. 3, and *CC*, Fig. 4, have been plotted. It must, however, be noted that the figures given in the *G. E. Bulletin* are for the cables drawn in ducts; furthermore, while the thermal resistances in Table VII have been calculated for a thickness of insulation to be used with a pressure not exceeding

1000 volts, the carrying capacities recommended in the *G. E. Bulletin* are intended to be used for pressures not exceeding 3000 volts.

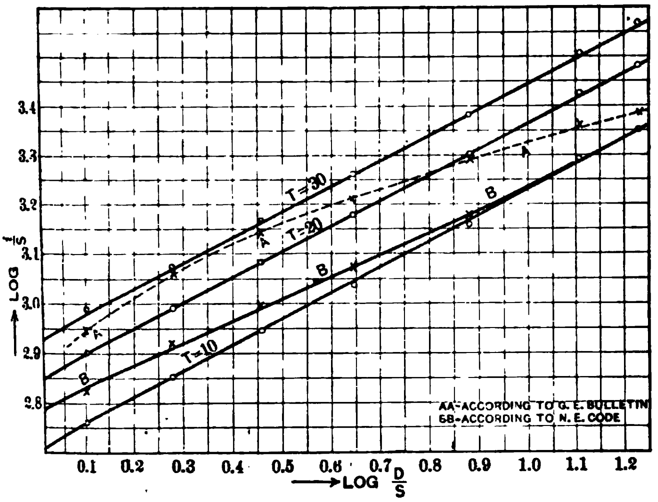


FIG 2

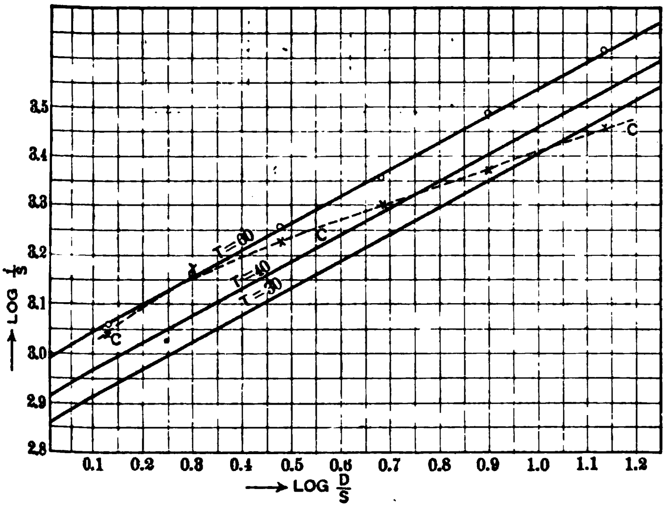


FIG. 3

The calculated carrying capacity is, however, thereby diminished only one or two per cent, because the effect of increased thickness of insulation is almost neutralized by the effect of increased radiating surface.

It will be noticed that the currents recommended by the N. E. Code are evidently based on $T =$ 15 deg. for large sizes and T = 10 deg. for smaller sizes. On the other hand, the carrying capacities recommended in the *G. E. Bulletin* follow a rather irregular law. While for the larger sizes the currents recommended are nearly equal to those calculated from thermal data, the difference between the two sets of figures becomes greater as the size is diminished. This is indicated even better by calculating the current densities according to the different methods of determining carrying capacity.

TABLE XI

Current density, amperes per sq. in.

Size of cable	Rubber insulation ($T = 30$ deg.)			Varnished cambric insulation $T = 60$ deg.		
	Thermal data	G. E. *Bulletin*	N. E. Code	Thermal data		G. E. *Bulletin*
				Braided	Leaded	
2,000,000 C.M.	956	892	669	1130	1129	1114
1,000,000	1177	1124	827	1432	1366	1463
500,000	1455	1400	992	1788	1720	1680
250,000	1807	1624	1193	2223	2158	1980
0 B. & S.	2397	1988	1494	3011	2807	2349
4 B. & S.	3182	2273	1940	4091	3698	2788
6 B. & S.	3690	2428	2233	4709	4417	2913
10 B. & S.	5000	2439	2927			

While it would thus appear as if for certain purposes the current density on the smaller sizes could be increased considerably, it must also be remembered that the temperature rise is not the only factor that has to be considered. Where the necessity arises of keeping the $i\,R$ drop within certain limits, it would be impossible to use such high current densities with smaller sizes of cables, and no doubt such considerations have been largely effective in regard to the figures given in the *G. E. Bulletin*.

Comparison of Results Obtained with those of Melsom and Booth. From the data in Table VIII it is possible to calculate the carrying capacities of rubber- and paper-insulated cables according to Melsom and Booth.

For rubber-covered cables in air, for $T =$ 10 deg.,

$$\log\left(\frac{i}{S}\right) = 0.616 \log \frac{D}{S} + 2.53 \text{ (Melsom and Booth)}$$

$$= 0.515 \log \frac{D}{S} + 2.707 \text{ (Table IX)}$$

For lead-covered cables in air (paper-insulated), for $T = 30$ deg.,

$$\log \frac{i}{S} = 0.50 \log \frac{D}{S} + 2.860 \text{ (Melsom and Booth)}$$

$$= 0.525 \log \frac{D}{S} + 2.846$$

Table XII gives a comparison of the current densities calculated for different sizes of cable according to both sets of equations.

TABLE XII

Size of cable	D/S	Rubber-covered according to		Lead-covered according to	
		Table IX	M. & B.	Table IX	M. & B.
2,000,000 cir. mils.	1.27	575	392	795	816
500,000 " "	2.85	873	646	1216	1222
0 B. & S.	7.59	1445	1180	2032	1995
6 B. & S.	17.00	2193	1941	3105	2985
10 B. & S.	29.3	2897	2716	4130	3917

On the whole, the two sets of equations give results that are within two per cent for lead-covered cables, but for rubber-covered cables the agreement becomes better only in the smaller sizes.

Calculation of Rates of Heating and Cooling and Carrying Capacities under Intermittent Use.

Let q = Rate at which energy is supplied to conductor, in watts per inch length of conductor.

C = Heat capacity of cable per 1 inch length, in watts.

W = Watts transmitted through insulation for 1 deg. difference in temperature, per 1 inch length.

$$\text{Then } q = \frac{C}{60} \frac{d\theta}{dt} + W\theta^{*} \qquad (12)$$

where θ = temperature rise at the conductor at time t, in minutes.

*This equation is only approximately true, but the value of b obtained is probably within 10 per cent of the true result.

Integrating (12),

$$\Theta = \frac{q}{W} \{1 - \epsilon^{-bt}\} = T(1 - \epsilon^{-bt}) \qquad (13)$$

where $b = 60\ W/C$
and $T = q/W$ = maximum temperature rise.

Hence, it is possible to calculate b, if C and W are known.

Referring to the record of the test on the carrying capacity of cable E, the average value of b determined from heating and cooling curves was found to be 0.044. Since $W = 0.022$, $C = 30$ *watts per* 1 *inch length.*

On the other hand, it is possible to calculate C from the heat capacities of the various materials.

Heat capacities in watts per cubic inch.

Copper	55
Rubber	22.5
Lead	23
Iron (steel)	55

From these data and the thicknesses of the various layers we obtain the value $C = 29$, which is in close agreement with the observed value.

In Table XIII, C has been calculated for several of the rubber-insulated and varnished cambric-insulated cables. From the value of C and that of W (Table VI and VII), b has been determined by means of the relation

$$b = \frac{60\ W}{C}$$

From the equation

$$\frac{\theta}{T} = 1 - \epsilon^{-bt} \qquad (13a)$$

it is seen that the heating curves of all sizes of cables can be represented by a generalized curve with the equation

$$\frac{\theta}{T} = 1 - \epsilon^{-x}$$

where the *unit of x* is the time in minutes required for the temperature rise to attain 63.21 per cent of its ultimate value; that is, the *unit of time is* $\frac{1}{b}$ minutes.

Such a generalized heating curve is represented in Fig. 4. The four values of x at which θ is 0.5, 0.75, 0.90 and 0.95, respectively, of its ultimate value have been indicated on the curve. Similarly the value of x for any other value of $\frac{\theta}{T}$ may be determined from the curve. The cooling curve $\frac{\theta}{T} = \epsilon^{-x}$ has also been plotted with the same units.

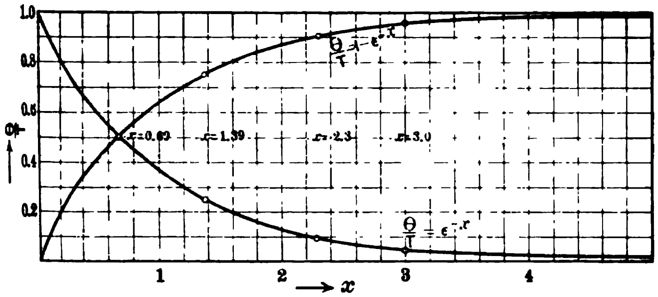


FIG. 4

The use of these curves may be illustrated thus:

If $b = 0.044$, $\frac{1}{b} = 22.73$ minutes. Consequently the intervals of time required for the temperature rise to attain 50, 75 and 90 per cent of its maximum value are 0.69×22.73 (= 16), 1.39 $\times 22.73$ (= 32) and 2.3×22.73 (= 52) minutes, respectively.

In Table XIII are given the values of t (in minutes) at which the cable would attain 50, 75, 90 and 95 per cent of its maximum temperature rise.

TABLE XIII

Size of cable	C	b	t for θ/T = 0.5	0.75	0.9	0.95	D_2	$D_2 b$
Rubber-insulated								
2,000,000 cir. mils.	110	0.022	31	63	105	136	2.0	0.044
500,000 " "	32	0.040	17	35	58	75	1.12	0.044
0 B. & S.	9	0.082	8	17	28	37	0.63	0.051
6 B. & S.	2.8	0.15	5	10	15	20	0.35	0.052
Varnished-cambric-insulated.								
2,000,000 cir. mils.	120	0.014	49	99	164	214	2.14	0.030
500,000 " "	35	0.030	23	46	77	100	1.19	0.036
0 B. & S.	10	0.060	12	23	39	50	0.66	0.039
6 B. & S.	3.3	0.11	6	13	21	27	0.40	0.044

As is seen from the last two columns, the rate of heating (or cooling) varies approximately inversely as the outside diameter for rubber-covered cables. This rule does not, however, extend to varnished-cambric-insulated cables.

A much more general problem, however, than that of calculating the rates of heating and cooling of cables is the determination of the carrying capacity under intermittent use, or the calculation of the interval of time during which a definite overload may be carried.

The exact solution of such a problem would involve the use of Fourier's series and is very difficult, but the following considerations show that a first approximation at the solution of this problem may be obtained quite readily.

Let i_1 = carrying capacity of a cable for a given rise of temperature θ_1 (as given in Tables VI and VII).

Let $i = (1 + p)\, i_1$ denote the overload current, where $100\, p$ = per cent overload.

Let T = corresponding maximum rise in temperature.

From equation (**2**) it follows that

$$i_1^2 R\,(1 + 0.004\,\theta_1) = W\,\theta_1$$
$$i^2 R\,(1 + 0.004\,T) = W\,T$$

Consequently

$$\frac{\theta_1}{T} = \frac{1 + 0.004}{\dfrac{i^2}{i_1^2}} - 0.004\,\theta_1 \qquad (\mathbf{14})$$

and the interval of time, t_1, during which the overload may be applied, is given by the relation

$$t = \frac{2.3}{b} \log \frac{T}{T - \theta_1} \qquad (\mathbf{15})$$

Thus by combining equations (**14**) and (**15**) it is possible to calculate t for given values of θ_1 and i/i_1, if b is known.

Now consider the case in which the cable has an initial load of i_0 amperes before the overload current is applied. Assuming that this initial current is over 50 per cent of the normal carrying capacity (i_1) and that it has been passing until the stationary state of temperature distribution has been attained, the relative temperature increments at the stationary state without overload (θ_0) and with overload (θ_1) are given by

$$\frac{\theta_0}{\theta_1} = \frac{i_0^2}{i_1^2} \qquad (\mathbf{16})$$

When the overload current is applied, the maximum rise of temperature above θ_0 will be $T-\theta_0$, and the interval of time taken to attain the temperature rise T will be given by the equation

$$t=\frac{2.3}{b}\log\frac{T-\theta_0}{(T-\theta_0)-(\theta_1-\theta_0)}=\frac{2.3}{b}\log\frac{T-\theta_0}{T-\theta_1}$$

$$=\frac{2.3}{b}\log\frac{\frac{T}{\theta_1}-\frac{\theta_0}{\theta_1}}{\frac{T}{\theta_1}-1} \tag{17}$$

It is evident that for $\theta_0 = O$, equation (**17**) becomes the same as (**15**).

Denoting the ratio $\frac{i_0}{i_1}$ by r, we can unite (**17**) in the form:

$$t=\frac{2\ 3}{b}\log\frac{\frac{T}{\theta_1}-r^2}{\frac{T}{\theta_1}-1} \tag{17a}$$

Now the value of the expression

$$2.3\log\frac{\frac{T}{\theta_1}-r^2}{\frac{T}{\theta_1}-1}=\phi$$

will depend only upon the three variables p, r, and θ. That is, for a given value of θ_1, the value of t is proportional to ϕ and is absolutely independent of the dimensions of the cable. The proportionality constant $\frac{1}{b}$ is the only quantity which depends upon the size of the cable. Its value may be obtained from Table XIII above.

TABLE A. $\theta_1 = 30$

t = interval time for which overload may be carried.

$= \frac{1}{b}\phi$

$100p$ = per cent overload; $r = i_0$/normal carrying capacity for $\theta = 30$ deg.

r	ϕ for p					
	0.10	0.20	0.30	0.50	0.75	1.00
0.	1.64	1.064	0.788	0.474	0.281	0.175
0.50	1.41	0.894	0.637	0.375	0.216	0.133
0.6	1.30	0.801	0.566	0.329	0.189	0.115
0.7	1.14	0.685	0.474	0.270	0.154	0.092
0.8	0.914	0.525	0.356	0.198	0.110	0.067
0.9	0.585	0.311	0.202	0.108	0.057	0.035

In Table A, the values of the expression ϕ have been tabulated for $\theta_1 = 30$ and different values of r and p.

The use of this table may be illustrated by the following example.

It is required to determine the value of t for a 2,000,000-cir. mil rubber-insulated cable carrying 60 per cent of its normal current when an overload of 30 per cent is applied.

From the table, it is found that for $r = 0.6$ and $p = 0.3$, $\phi = 0.566$. According to Table XIII, $b = 0.014$. Hence $t = \frac{0.566}{0.014} = 40.4$ minutes. If the cable were not carrying any current initially, it could stand an overload of 30 per cent for $\frac{0.788}{0.014} = 56.2$ minutes.

Tables B and C contain similar data for $\theta = 40$ and $\theta = 60$.

TABLE B. $\theta = 40$

r	ϕ for $p =$ 0.10	0.20	0.30	0.50	0.75	1.00
0.	1.61	1.03	0.746	0.440	0.246	0.138
0.5	1.38	0.865	0.605	0.348	0.191	0.092
0.6	1.27	0.775	0.536	0.302	0.163	0.078
0.7	1.108	0.660	0.449	0.246	0.131	0.060
0.8	0.891	0.505	0.336	0.182	0.097	0.039
0.9	0.564	0.30	0.191	0.099	0.051	0.012

TABLE C. $\theta = 60$.

r	0.10	0.20	0.30	0.50	0.75	1.00
0.	1.54	0.975	0.680	0.373	0.177	0.072
0.5	1.32	0.805	0.550	0.292	0.138	0.053
0.6	1.20	0.720	0.485	0.253	0.117	0.046
0.7	1.05	0.608	0.405	0.207	0.088	0.037
0.8	0.834	0.465	0.302	0.154	0.058	0.025
0.9	0.528	0.274	0.168	0.080	0.030	0.014

These data may be plotted in the form of curves giving ϕ as ordinates and p as abscissas. By the use of these curves and tables of b it ought therefore to be possible to calculate approximately the interval of time during which any definite overload may be carried.

SUMMARY

The current-carrying capacity of an insulated cable may be calculated from data on the thermal resistivity of the insulation and of the surface. It has been shown that these data may be obtained for any type of insulation from experiments on the cable itself by applying a well-known method for determing heat conductivities.

As a result of experiments on different types of cable insulation, the following values have been obtained for the thermal resistivities (deg. cent. per watt per inch):

Rubber and cotton braid cover	250
Varnished cambric and cotton braid cover	400
" " (lead-covered cables)	300

For the surface resistivities the following values were obtained (deg. cent. per watt per square inch).

Painted steel braided armor	100
Cotton covering	120
Lead, ordinary	190
" painted dull black	140

These data (which are in agreement with those obtained by other investigators) have been used to calculate the carrying capacities of standard rubber- and varnished-cambric-insulated cables suspended in air. It has been shown that over the range of sizes, No. 10 B. & S. to 2,000,000 cir. mils, the current density (i/s) and the ratio of the outside diameter (D) to the cross-section of the conductor (S) are connected by an equation of the form

$$\log\left(\frac{i}{S}\right) = N \log\left(\frac{D}{S}\right) + B$$

where N is a constant for any type of insulation and B depends upon the temperature rise.

The effect of the surface thermal resistance on the carrying capacity has been shown to be much greater, in general, than that of the internal thermal resistance. The magnitude of the surface losses is thus of great importance in determining the current-carrying capacities of insulated cables.

An approximate method has been worked out for calculating the rates of heating and cooling of insulated cables from the thermal resistances and heat capacities of the materials that enter into the construction of the cables. Furthermore, the application has been indicated to the determination of carrying capacities under intermittent use.

In conclusion the author desires to express his appreciation to Dr. I. Langmuir and Mr. W. E. Robinson for their active interest during the progress of the investigation.

GROUP I PAPERS

(Pages 153 to 357)

HEATING, HEAT MEASUREMENTS, RATING BY HEAT

(a) MOVING MACHINERY

Notes on Internal Heating of Stator Coils, by R. B. Williamson.

Measurement of Temperature in Rotating Electric Machines, by L. W. Chubb, E. I. Chute, and O. W. A. Oetting.

Method of Determining Temperature of A-C. Generators and Motors and Room Temperature, by H. G. Reist and T. S. Eden.

Thermocouples and Resistance Coils for the Determination of Local Temperatures in Electrical Machines, by J. A. Capp and L. T. Robinson.

(b) TRANSFORMERS

Methods of Determining Temperature of Transformers and of Cooling Medium, by S. E. Johannesen and G. W. Wade.

Methods of Determining Temperature of Transformers, by W. M. McConahey and C. Fortescue.

Correction of Transformer Temperatures for Variation in Room Temperature, taking into Account both Copper and Iron Losses, by C. Fortescue.

(c) TEMPERATURE CORRECTION

The Temperature Rise of Stationary Induction Apparatus, by J. J. Frank and W. O. Dwyer.

Effect of Room Temperature on Temperature Rise of Motors and Generators, by M. W. Day and R. A. Beekman.

Effect of Air Temperature, Barometric Pressure and Humidity on the Temperature Rise of Electrical Apparatus, by C. E. Skinner, L. W. Chubb and Phillips Thomas.

A Laboratory Investigation of Temperature Rise as a Function of Atmospheric Conditions, by C. B. Blanchard and C. T. Anderson.

Laws of Heat Transmission in Electrical Machinery, by Irving Langmuir.

(d) CABLE HEATING

Current Rating of Electric Cables, by R. W. Atkinson and H. W. Fisher.

The Heating of Cables Carrying Current, by S. Dushman.

DISCUSSION ON GROUP II PAPERS (HEATING, HEAT MEASUREMENTS, RATING BY HEAT), NEW YORK, FEBRUARY 26, 1913.

Comfort A. Adams: There is one question I would like to ask of Dr. Dushman, concerning Dr. Langmuir's paper. When Dr. Langmuir first published his work on convection, I happened to be interested in the subject, and put the material into such shape that I could use it easily, but as the theory was only checked by tests on very small wires, I am in doubt as to the validity of the method when applied to large wires or to plane surfaces. I should like to know how far it is safe to go in this direction.

S. Dushman: For a plane surface the thickness of the film is about 0.4 cm., and that will hold practically down to 500,000 cir. mils cable. Below that, the thickness of the film varies with the diameter of the cable, and a table is given.

Comfort A. Adams: Is that the same information as given in the paper which was published in the *Physical Review?*

S. Dushman: I do not think so.

William F. Dawson: I would ask Dr. Dushman and Dr. Langmuir if they have investigated the specific heats of the various insulating materials. We have many data in respect to the thermal resistivity, but glancing through the papers I see no mention of the specific heat of the insulating materials. The specific heat of the various metals is fairly well known, but we do not seem to have available the specific heat of the insulating materials, and that certainly is very important in determining the rapidity with which various conductors will increase in temperature.

I have made some investigations in respect to crane motor ratings, and found that, roughly, the thermal capacity could be assumed at some three to four times the thermal capacity of the copper, but, of course, that is entirely empirical and probably not very accurate. I believe it was Mr. E. H. Rayner who made the investigation at the National Physical Laboratory in 1905 for the English Standards Committee who made the broad statement that the specific heat of insulation material was six times that of copper, but I imagine that is only an approximation.

Leo Schuler: When the International Electrotechnical Commission held its meeting in Zurich in January, the experiments made in this country with regard to the influence of room temperature on the heating of electrical machinery were brought to our attention by your American representative, Mr. Mailloux, who is here present. At that time Mr. Mailloux was under the impression that the influence of air temperature would be very much higher than has been represented in the papers. As we understood from Mr. Mailloux, your experiments have shown that a machine would be heated at a lower temperature so much more than at a higher temperature that the final temperature attained by a machine would be practically the same, within certain limits, whether the air temperature is high or low.

C. O. Mailloux: That is what I said.

Leo Schuler: Between the limits of 25 deg. cent. and 35 deg. cent. air temperature, the final temperature would be practically the same. However, as far as I can see now, this is certainly not the case. If it had been the case, it would certainly be quite revolutionary compared with the former ideas we had on that subject.

The members of the Committee in Zurich were also of the opinion that it would be quite revolutionary, and that therefore we should drop all discussion on the subject of air temperature. Now, I must say that this is the most important question which I thought would be discussed here, and I should like to have some further information from the gentlemen who made the investigations in regard to the practical results they think their investigations will have on the Standardization Rules which are now to be adopted.

As far as I have seen from the papers, it is advised to leave a correction for air temperature out of the Standardization Rules, and simply stick to the present practise, at least to our present German practise, and simply add the air temperature to the rise and do nothing else.

There is another question: In the paper by Mr. Skinner allusion is made to the "fog-laden air," but there is not much said about that subject. Am I to understand that experiments have been made with fog-laden air and that the cooling of fog-laden air is much higher than ordinary air; and if this is the case, is there any prospect of a practical application of this method? One would think, of course, that the fog-laden air would very soon destroy the insulation by exposing it to the moisture, but I have seen a report in a German paper that in America experiments are being made to use fog-laden air for the more effective cooling of electrical machinery. Perhaps you will give me some information on that.

M. W. Day: With regard to the total temperature obtained under different room temperatures, the question was raised at one time whether possibly in a cold room the motor would rise to a point nearly as hot as in a hot room, but that is far from being the case. The fact that we wished to bring out was, that in a great majority of these cases the temperature rise in a hot room was less than it was in a cold room, but not enough less to make the total temperature the same. Take the case which is shown in Fig. 7, where one machine is run at 47 deg. cent. and the other at 24.5 deg. cent., the temperature at the commutating spool has 20 deg. rise in the hot room and 22 deg. rise in the cold room. That is a semi-enclosed motor. Now, with the open motor without fan shown in Fig. 4, in one or two cases the reverse of this rule holds true, but, take for instance, the shunt field by thermometer; in one test it shows 32 deg. and in other tests about 33.5 deg., a very small difference, but still enough to be considered when we come very close to meeting specifications.

Turning back to Fig. 1, where the motor is totally enclosed, there is a greater difference. Take the case of the armature where the room temperature was 4.5 deg., the rise of the armature was 41 deg., and where the room temperature was 25 deg., the armature rose about 35 deg. There is 6 deg. difference in temperature rise, with about 20 deg. difference in the room temperature. About the last of the year we sent a copy of this paper to Mr. Mailloux, so that it might be considered at the meeting of the International Electrotechnical Commission in Zurich.

H. M. Hobart: I had charge at one time of some experiments in fog-laden air. The results of the investigations were valuable. I am not prepared to disclose the results but I will state my opinion that fog-laden air will afford a commercially valuable means of cooling machinery. When we have learned to take advantage of a very great number of little points, which are necessary to success, there will be a field for it.

Leo Schuler: I ask whether it is looked upon as a practical question.

H. M. Hobart: I should be disposed to recommend going much further with it, at some time when other things do not stand in the way.

C. O. Mailloux: I would like to make clear the attitude of the delegate from America to the International Electrotechnical Commission, which I do not think was made clear by my colleague, Mr. Schuler. At the time of my departure for Zurich, there were only two of the papers presented here today which were ready, namely the one by Day and Beekman and the one by Steinmetz and Lamme. These papers revealed such radical tendencies and promised revelations of such an important character that I felt it incumbent upon me to warn the Special Committee on Rating at Zurich, when it assembled to discuss the question of rating, not to go too fast in discussing the question of ambient temperature. Feeling that the Standards Committee had a great deal more ammunition up its sleeve, to use a figurative expression, I suggested that discussion might well and profitably be postponed by the International Electrotechnical Commission until after the Midwinter Convention had been held, or until after the papers which had been promised us were thoroughly discussed; but the Committee did not coincide with me. They did not seemingly believe the Americans were quite as wise as I intimated, and they were skeptical in regard to the value of the papers and their discussion. However, by way of protest, I requested the privilege of abstaining from any vote or participating in any formal action; and there was inserted in the Proceedings a note to the effect that the American delegate abstained from voting on this question, feeling that there would soon be made public in America the results of important researches which would call for very radical modifications of the rules. That is the way the record stands today. The position I took was that any action which the Commission took at Zurich would necessarily be subject to revision and would have to undergo revision of a more or less radical character, after the results of this Convention, including the papers and the discussion, had become public.

Charles P. Steinmetz: Regarding the result, that the temperature rise at higher room temperature is less, I would say that the result is not unexpected, but is what should be expected from the radiation laws. We should expect from the radiation laws that the higher the room temperature the lower is the temperature rise at the same energy dissipation. Unexpected was the result of the single set of experiments which led to the rule previously formulated, which was introduced in the Standardization Rules away back in the last century, when the experiments were first made. The results were introduced into the rules, as they were the only evidence available at that time. Like many other things, these data were put in as the best information available, hoping that the establishment of that rule would lead investigators to study it and give us more exact information. Unfortunately, this hope was not realized until the investigations which are being published today.

W. A. Durgin: Today's discussion of permissible insulation temperatures has emphasized the importance of obtaining reliable means of getting at hot spot temperatures, and as the first group of papers is particularly concerned with such means, it seems that each should receive careful consideration. The central station company with which I am connected desires to place itself on record as being in favor of ascertaining the temperature of large unit windings by exploring coils.

These coils, according to one of the papers, are rather fragile, expensive, difficult to install and require precise measurement. Our experience is quite the reverse. First, as to fragility, we have installed a total of 120 coils in ten turbo-generators, ranging in size from 8 to 20 megawatts. Fourteen of these coils, or 12 per cent, have been damaged and the remainder have been in service from one to 4½ years, the average age of all coils being 2½ years. Some of these are installed between armature coils, some between coil and iron and some on end turns. The expense is negligible, the coils being wound of No. 30 double cotton covered wire in our own laboratory and usually consisting of a single layer some 3 feet long and 1 inch wide. The difficulty of installation will be easily met if you use the thermometer or armature resistance method of following operating temperatures as opportunity will be presented at the time of rewinding your machines. The precision of measurement required is at most merely that of a Wheatstone bridge, while there are at present on the market indicating instruments which may be permanently connected to the coils to give the operator direct temperature readings.

The value of these coils has been greater than we first expected, since they supply a ready means of following the temperature of the machine continuously, thus checking the condition of the ventilating ducts. The operating department has come to depend on our periodic heat tests for necessary information as to when to remove a field, clean the stator, and put the entire unit in first-class condition.

We appreciate, of course, that any exploring coil may not and probably will not give the hottest spot, but it does give a temperature which is strictly related to that of the conductor and this temperature is so much higher than that shown by either the mean resistance or temperature method that as yet we have not been able to get the manufacturers to admit its applicability in determining the rating of a machine.

For all these reasons, therefore, we wish to urge that in reformulating the Standardization Rules the temperature coil method be given very serious consideration, and if possible, presented in conjunction with some average temperature gradients for the various classes of insulation whereby we may determine the temperature of conductor corresponding to the temperature coil indications.

B. G. Lamme: I would like to say something in connection with what the last speaker stated. Last month I presented a paper before the Institute on the subject of turbo-generators, and in that I called attention to the fact that, in large turbo-generators, the temperature drop from the hottest part to those parts which were accessible to measuring instruments, was often excessive; that is, there might be rises of 80 deg. in the slot and 40 deg. or 50 deg. in the end windings. That is a class of machinery in which the temperature drop is liable to be excessive and no thermometer or resistance measurement is going to give indications which are worth anything unless you know approximately the internal drops. In such apparatus thermocouples or exploring coils give a much better approximation to the true temperatures. Such devices do not give the hottest temperatures, but they locate the hot parts and give an approximation of temperature at those points. In certain classes of machinery where internal temperature drops are liable to be very high, measuring devices of that sort are advisable.

M. E. Leeds: As the importance of the measurement of hot-spot temperatures is generally admitted and the two methods of making these measurements which seem to give the best results are by means of thermocouples and exploring coils or resistance thermometers, it would seem worth while to consider the relative advantages of these two methods, which may be summarized as follows:

The thermocouples have the advantage of small size and convenience for insertion, low cost, and small likelihood of deterioration. They have the disadvantage that the forces available for measurement are very small, being only thirty to fifty millivolts per degree, and require unusually sensitive and relatively expensive apparatus to measure them, and that it is necessary to make a cold-end correction which is such that the scale of the indicator does not read temperatures directly.

Resistance thermometers have the advantage that the forces available for measurement are relatively large, and robust indicating apparatus having moving parts and general construction the same as ordinary moving-coil switchboard instruments with an open easily read scale, may be used; no correction of any kind is necessary and the scale of the indicator reads temperatures directly. They have the disadvantage of greater first cost when calibrated to be direct-reading and constructed so as to insure permanent reliability.

In the case of the testing laboratory of the manufacturing plant where temperatures are to be measured at a very large number of points and where laboratory assistants may be trained to use the more delicate apparatus and make the necessary corrections, it is quite possible that thermocouples will be more economical in spite of the large expense of the reading devices.

In the case of operators of machines where the number of points to be measured is small, the situation is a different one, and the resistance thermometer seems to have decided advantages in that the temperature may be read directly from a robust instrument like other switchboard instruments without special attention, manipulation or corrections of any kind. The higher cost of the parts that go into the machine is to a very large extent offset by the lower cost of the indicating instrument. Should the service require it, the forces available from a resistance thermometer are large enough to operate a simple form of recording instrument with a commutating device which would automatically record in succession the temperature of the various parts of the machine.

I notice a slight mistake in the paper by Chubb, Chute and Oetting, apparently due to an oversight, in the statement that the exploring coils or resistance thermometers must be calibrated for the particular length of lead with which they are furnished. This is not the case for either three- or four-lead construction. The only object in having three or four leads is to compensate for the lead wires, and this compensation is rigid, and is quite independent of the length of the leads.

In the paper by Capp and Robinson is the statement: " The practical value of resistance thermometers for high temperatures is doubtful. Base metal couples are permanently changed in resistance by continued use."

The value of this statement depends on the definition of high temperatures, which is not given. There is a very considerable amount of data now available to show that properly constructed resistance thermometers of nickel wire do not permanently change their resistance when used at temperatures under 250 deg. cent.

L. W. Chubb: In answer to Dr. Leed's criticism on the relative merits of the thermocouple and the resistance coil, let me say that the greater sensibility of the bridge method in measuring with the resistance coil does not justify its use when other errors of great magnitude and of unknown quantity may creep into the results. For instance, resistance coils, consisting of very fine nickel wire, are very easily broken, when being installed in the machine. Also if you are to use such exploring coils to measure the hot spot, you must put them where there is a magnetic field, and it is well known that the resistance of nickel is not the same in a magnetic field as it is without the field.

Recent tests with such exploring coils put in the air gap of a turbo showed 2 deg. cent. jump when the turbo was excited. This was not an inductive kick, but a change in the resistance of the coil.

The thermocouple can be put right where you want it, and quick and more accurate readings of the hot spot can be taken. There are no errors of leads and it is not subject to magnetic fields in the slot of the machine. The use of the potentiometer is advocated when using a thermocouple. Portable, self-contained instruments can be had at a reasonable price and they are sufficiently sensitive to read to a small fraction of 1 deg. cent.

L. T. Robinson: I favor a resistance thermometer over the thermocouple for very obvious reasons; it is an easier thing to handle, and I think when you tack a potentiometer on to anything it is hardly a commercial device. It is a fine thing for research work, but I do not think they would want a thing like that in a central station. I do not see anything in this nickel coil. There is no necessity for it. I see no reason why copper winding is not perfectly satisfactory. Possibly it may be true that nickel has a larger temperature coefficient, it also has a larger resistivity, and that means a coarser wire, and you can in that way build up quite an argument, but, nevertheless, the fact remains that you can make perfectly satisfactory copper coils, that all the cost that goes into them is the labor of making the coil and putting it in. The real trouble is to get the thing in where you want it, without doing some damage to the machine, and to be able to find the ends after you have it there, and it has been there a while. It is quite a problem. It is introducing one class of work, that is, instrument work, into an entirely different class of apparatus, and the people who are familiar with preparing and insulating windings, etc., have not the facility and the fingers to handle the small temperature coil, and they first have to learn how to handle it, before it will be a real commercial success.

R. F. Schuchardt: It is somewhat astonishing to hear a discussion of exploring coils made of nickel or platinum, when copper coils are so simple and successful.

In the installations Mr. Durgin referred to, where we have

90 per cent of the coils installed still in service, we made them of double cotton covered No. 30 copper wire, winding the coil non-inductively and flat with a thickness of a single conductor. We placed them between the armature coil and the wooden wedge, and in a single generator we installed a dozen or more coils in various locations, so that if we lost one or two because of broken connections we still had enough to get satisfactory readings.

There is another point in connection with the use of exploring coils, in generators particularly, which is of great importance. With such coils installed, we can easily put an indicating device on the machine which will tell us the temperature at the point of measurement, which should be as near as we are able to get it to the probable hottest part.

I would like to make this suggestion: That, while, as stated, it is not the function of the Institute to tell manufacturers how to get results, we can very properly recommend to them that, as they put an oil gage on bearings to indicate when the oil level gets below the safe limit, they ought also to attach a temperature indicating device so the operator can readily know when the safe limit of load has been reached.

Elmer I. Chute: The method suggested in Messrs. Reist and Eden's paper, of artificially creating a lag in the thermometers responsible for the measurement of the room temperature, somewhat corresponding to the natural lag in the apparatus under test, has much to commend it. Constant conditions as regards surrounding air in tests on rotating apparatus are much to be desired, but are seldom attained. Under varying air conditions, especially in tests on larger machines, some such device should facilitate the obtaining of more consistent and accurate temperature rises.

The method of covering thermometers, when temperatures are to be taken while running, is without doubt the most important feature in connection with their use for this purpose. On this account, the first table given in this article should be of especial interest to all those concerned in the testing of rotating apparatus. The results here given were, in certain respects, so foreign to the experience of the present speaker, based on comparative tests previously taken on various types of machines, that additional tests were undertaken to determine if possible wherein the discrepancy lay. This discrepancy consisted chiefly in results given in this table for temperatures obtained with a covering of putty for thermometers as compared with a covering of felt in the shape of a small thin pad.

Tests as outlined by the authors of this paper were made and were thoroughly confirmed, that is, the thermometers protected by the putty gave from 5 to 8 deg. in 80 higher than those covered by the felt, and checked within ½ deg. of the true temperature. The true temperature was determined from the average of a number of readings taken around the coverings by a small quick-acting thermocouple that had been especially checked for the occasion.

It was thought that some of the discrepancy may have been due to the felt pads being affected by the excessive humidity of the surrounding air caused by the proximity of the boiling water, so the same test was repeated with electrically heated grids furnishing the required temperature. In this test at 90 deg. cent. the two methods checked within 3 deg. of each other; the one covered with the putty still giving the higher temperature.

TABLE I

Test No.	Rating of machines in kv-a.	Speed	Temperatures in deg. cent.				Part measured
			Thermometer covered with putty	Thermo-couple	Thermometer covered with felt	Thermo-couple	
1	150	1200	51	55.7	53	54.5	Core
2	1000	600	61	65.3	68	68.4	Core
3	1000	300	47.8	51.2	52.5	52.1	Core
4	1000	300	51	55.7	53	54.2	Core
5	1000	300	49	50	52	53	After shut down
6	125	300	38.5	39.9	38	38.2	Coil
7	580	750	45.3	49.3	50	49.9	Coil
8	580	750	51	52	50.5	49.9	Core
9	90	560	41	41	42	43	Coil
10	90	560	47	49	43.5	44	Core
11	400	120	28	29.5	32.5	31	Coil
12	400	120	31	32	33	32	Coil
13	400	120	46	46	47	47	Core
14	400	120	47.5	47	46.5	47	Core
15	2000	600	53	54	32	30.5	Coil
16	2000	600	33	37	37	37	Coil
17	2000	600	55	58	56	57	Core
18	75	277	33.5	34.5	33.5	32.8	Coil
19	75	277	37	37	35.5	36	Coil
20	75	277	53	54.7	54	54.2	Core

Tests were next conducted on machines themselves in actual operation. These tests were quite numerous. Table I gives a few of the results obtained.

Thermometers covered with each material were placed in corresponding conditions and the true temperature obtained in each case by exploring around the covering or pad with the thermocouple previously mentioned.

The experiments so far conducted indicate that the discrepancy is chiefly due to the amount of air passing over the coverings. The heat being readily transmitted through the covering of putty enables it to be carried off rapidly, while in the case of the felt there is considerable temperature gradient through the material.

It may be stated that after shut-down the temperature equalizes to some extent, although the thermometers covered with the putty seldom reach the maximum registered by those covered with the pads.

The absence of all mess and the facility with which the felt pad may be adapted to almost any location on either windings or laminations are strong points in its favor, but the obtaining of temperature closely approximating the true temperature is the vital point to be considered.

Charles P. Steinmetz: The exploring coil or thermocouple is very valuable in certain classes of machines, that is, high-voltage, high-power generators, while in other machines it is not applicable. The exploring coil or thermocouple has, however, two disadvantages: First, in those machines which have one coil per armature slot, it does not show the hottest spot, and if located between the coil and iron, it shows the temperature outside of the insulation, which may be much nearer to the iron temperature than to the copper temperature. If located between the coil and wedge, it shows a temperature the significance of which it is practicably impossible to interpret. Only where there are two coils per slot, and the exploring coil is located between the two coils, then it comes nearer to the copper temperature, but may be higher or lower. If, in addition to the results shown by the exploring coil, the heat resistivity of the insulation is known, you can calculate the copper temperature from the indication of the exploring coil and the flow of heat through the insulation, provided we know the flow of heat—from the copper to the rest of the machine. This, however, we do not know. We could only know this, where the entire heat energy flows transversely through the insulation. In such cases we can correctly interpret the results indicated by the exploring coil. But where a large part flows lengthwise, then the exploring coil temperature has no simple relation to the copper temperature. The exploring coil is mainly valuable, as Mr. Schuchardt pointed out, as an indicating device giving the operating engineer some notion of the temperature condition of the machine, at any moment. But it does not give us the much-desired information where the hot spot is and how hot it is.

Leo Schuler: I think it would be a great drawback if you came to the conclusion to prescribe in the Standardization Rules the adoption of a thermocouple or resistance coil, because, as Dr. Steinmetz pointed out, you will probably not be able to fix that coil at the hottest point. I might mention an experience I had some time ago on a 5000-kv-a. turbo-generator. It was prescribed by the specification that the temperature of the hottest spot should be measured. One would expect the hottest spot on such a machine to be the center of the stator coil in the middle of the machine, and we therefore put a thermocouple at that point. As a matter of fact, the temperature measured by that thermocouple was 10 deg. lower than the temperature measured by the total resistance of the coil. As a matter of fact, it was a cold spot.

This was easily understood later; the end windings of the machine were heavily wrapped with insulating tape outside and the dissipation of heat was very bad there, and when measuring by a thermometer at the end windings you got a much higher temperature than at the so-called hot spots. If you really say in your new rules that the manufacturer should place a thermocouple or something of that kind on the hottest spot of the machine, then I think the manufacturer would be very wise to find out the correct place for that coil.

A. E. Kennelly: I want to say that I noticed, in these papers that we are discussing, that there are various inferred absolute temperatures of copper, from 233.3 to 238. The most recent value given by the action of the Bureau of Standards and which, I understand, is likely to be accepted by the International Electrotechnical Commission, is 234.5. That is an easy number to remember.

Another point is that the unit of thermal resistivity is given in some of the papers as watts per deg. cent. per cu. in. That is generally admitted to be inaccurate. It should be watts-inches per degree cent.; or watts per deg. cent. in a cubic inch. That is, it should multiply by the inch and not divide by the cubic inch. This is important, when transferring to metric measure.

Another point is that one of these papers brought out to us the fact that at lower room temperatures, the temperature increase is greater than at higher room temperatures, which seems at first very surprising. We are accustomed to think of the heating effect of constant current strength in a coil of copper wire at different temperatures. Here the I^2R loss increases with the room temperature and the temperature elevation may be expected to increase also with the room temperature.

With constant power input, however, or with constant impressed difference of potential, the case would be different—here we have a higher temperature reached with respect to surrounding objects, and the radiated loss increases as the fourth power of the absolute temperature. I think that explains the fact, taken in conjunction with another important principle, that part of the core losses diminish as the temperature goes up. The eddy-current losses diminish as the temperature goes up, and therefore there is less loss at a higher temperature than at a lower temperature.

B. G. Lamme: I like the way the discussion is going this evening, because it is so nearly in line with the recommendations in the paper on temperature and electrical insulation. In that paper, we recommended certain temperature limits obtained by conventional methods of measurement, plus an internal drop which cannot be measured accurately. If, in the proposed conventional methods, thermocouples or resistance coils be included as one class of thermometer measurements, then our recommendations for determination of temperature by conventional methods of thermometer and resistance will hold for all kinds of apparatus. In those cases which were beyond the limits of the mercury or fluid thermometer the thermocouple or resistance coil could be used instead. The method proposed remains the same, whatever the method of measurement. This morning apparently there was considerable disagreement regarding our proposed method of getting at the hot spots. This evening, it is apparently the opinion that if we use a thermocouple or exploring coil in a high-voltage armature winding, and thus obtain the nearest we can to the high temperature and make a reasonable allowance for the internal drop, the result will be what is wanted;—but that is just what we proposed this morning.

I wish to take exception to one statement made by Mr. Robinson. I would limit the use of exploring coils or thermocouples to stationary apparatus, for in rotating apparatus experience shows that such devices are not satisfactory during operation, as some moving contact must be interposed to obtain readings when in operation. These are not very satisfactory, according to my experience. Moreover, rotating armatures represent a great proportion of the total apparatus on which temperature measurements are to be made, as this covers all direct-current armatures. Therefore, the thermocouple or exploring coil should be limited to stationary apparatus, or to moving apparatus only after shut-down.

L. T. Robinson: I am willing to accept Mr. Lamme's limitation for rotating members. It is just so.

James Burke: We have been considering various methods of determining hot spots. I would suggest one other method—the method of mind reading hot spots. In applying the mind reading method, if we find by thermometer we have 40 deg. temperature increase, and by resistance of the winding, 50 deg., then we can say by mind reading that the probable hot spot is 60 deg. If by thermometer we have 40, and by resistance 55, we may conclude that the hot spot is 70 deg.; if by the thermometer we have 40 and by resistance 60, we may conclude the hot spot is 80 deg. It is an approximate treatment of it, but perhaps comes near the true hot spot temperature. It does not tell us where the hot spot is and it does not tell the exact truth, but it approaches it.

C. P. Steinmetz: How about when the thermometer reading shows a higher temperature than the resistance method?

James Burke: Then we can conclude that the thermometer reading is the truth.

Robert Lundell: I wish to refer to the increase in temperature of machinery in hot and cold rooms. I cannot help thinking that the machines which were tested were rather inefficient at the low loads, that is to say, I believe the losses at no load, and particularly the excitation losses, were unduly high. Now, if the excitation losses had been very small, the main current I^2R losses would have more than offset the decrease in watts taken by the hot field in a hotter room. It is quite clear that as the temperature runs up, the shunt coils take less watts, and also that the core losses become smaller. The windage increases somewhat, consequently the radiation of the heat is better, but I believe if the machines had been designed so as to be highly efficient at low loads, this free speed current would be extremely low, and I believe the result would have been opposite to what it was. I have certain machines in which the excitation only amounts to one-third of one per cent, and I believe if I make that same test on those machines the results will be different, because then the main current I^2R losses adjust the difference.

F. D. Newbury (by letter): The paper brings out a point of importance in the rating of electrical apparatus that is often overlooked, thereby leading in many cases to positions scarcely tenable. I refer to the fact that the only important temperature is the maximum temperature of parts adjacent to the insulation. There is such a large factor of safety required—representing the difference between the measured outside temperatures and the allowable inside temperatures—with our present methods of temperature measurement, that the results from such measurements are of little real value in judging a machine, and their approximate nature certainly does not justify a rigid adherence to the limits set.

Mr. Williamson's paper points the way toward a more rational basis for judgment, but the assumption made that all of the heat from the copper passes through the slot insulation into the laminations may be far from correct. There are at least three paths in parallel for the escape of heat from the copper.

1. Through the insulation to the laminations.
2. Through the insulation and wedge to the cooling air in the air gap.
3. Through the length of the copper to the exposed ends of the coils.

The division of the flow between these paths follows laws analogous to the more familiar laws in electric circuits. The drop in temperature along a given path is proportional to the heat "resistance" of that path, and to the heat "current" flowing; and in parallel paths the heat current divides inversely with the complete resistances. Or, a part of one path only need be considered, in which case the flow of heat will be determined by the resistance of the partial length considered and the difference in temperature (potential) involved. It is evident that if the temperature of the tooth laminations is equal to the temperature of the copper, no heat can flow from one to the other and there will be no drop through the slot insulation. In that case all of the copper heat will flow through the copper to be liberated at the free ends of the coils and through the insulation adjacent to the wedge. This condition is approximated locally in many turbo-generators, it being relatively easy to secure low copper temperatures and difficult to secure uniformly low core temperatures. Or, to consider the opposite extreme, the tooth temperature may be so much lower than the copper temperature that all of the copper heat will flow through the insulation to the core. This is the assumption on which the author's formulas are based, and this condition is approximated in the short-circuit test mentioned by the author, the copper loss being increased 56 per cent above normal while the core loss is negligible. This explains why the test results and calculated results check. Similar short-circuit tests were made more than a year ago on a 14,000-kv-a. generator with the following results:

SHORT-CIRCUIT TEST, NORMAL CURRENT

Actual temperature of copper 52 deg.

Temperature outside of insulation against tooth laminations 34 deg.

Drop in temperature through insulation 18 deg.

Air temperature 26 deg.

The same generator was tested on open circuit and normal voltage with the following results:

Actual temperature of copper in different slots 42 deg. to 49.5 deg.

Temperature outside of insulation in contact with tooth laminations in one slot, 45.5 deg.

Drop through insulation, minus 3.5 deg. to plus 4 deg.

Air temperature 29 deg.

These results bring out very clearly the difference in heat flow with different distribution of losses.

In the tests referred to by Messrs. Chubb, Chute and Oetting (Fig. 2,) the effect of varying relative temperatures on temperature drop through the insulation may be seen.

In test *A* (open-circuit normal voltage), the teeth and core are of higher temperature than the copper, and the drop through the insulation between copper and tooth laminations is minus one deg., while the drop through the insulation between the copper and fiber wedge is plus 11 deg.

In test *C* (one-half normal current and full voltage), the corresponding " drops " are plus 3 deg. and plus 17 deg.

In test *E* (three-fourths normal current and nine-tenths normal voltage), in which the total temperatures are approximately the same as in *C* but in which the losses are differently distributed, the corresponding drops are plus 9 deg. and plus 14 deg. These figures show that instead of the drop between copper and iron sides of the insulation being proportional to the copper loss as in Mr. Williamson's formulas (2) and (3), the drop has increased faster than the loss (due to the increased flow of heat caused by the lower core temperature): that this drop is much less than results from these formulas (due to the other paths available for the escape of heat), and that instead of the surface of the coil adjacent to the slot wedge being ineffective in dissipating heat, as assumed by Mr. Williamson, it is more effective than the surfaces adjacent to the laminations on which Mr. Williamson places the entire burden. The reason for this, of course, is the lower temperature of the air gap side of the fiber wedge compared with the temperature of the laminations.

The general method advocated by Mr. Williamson is much nearer the facts than methods of temperature determination now in use, but to be of practical value the formulas and method of calculation must be based on assumptions nearer the truth than the assumptions made by the author.

If the method and formulas developed in this paper are used to obtain actual copper temperatures from the measurements of temperature by resistance coils or thermocouples outside of the slot insulation, the results will be entirely misleading except on short-circuit tests.

B. A. Behrend: The question whether the total temperature is affected by the outside temperature is of great importance. I do not believe in the most plausible and lucid explanation rendered by Dr. Steinmetz and Dr. Kennelly, and the reason why it is not correct, if I may quote Prof. Adams, is that the radiation plays so small a part in the cooling of the modern electrical apparatus, that the beautiful, simple and plausible explanation is altogether too beautiful, too simple, and too plausible to be true!

Comfort A. Adams: I specified that that was true of a machine with normally good ventilation. It would hardly be true, perhaps, in a machine totally enclosed.

Concerning the hot spot temperature I understood Mr. Schuchardt to say that the exploring coils were placed under the wedges, between the wedges and the outside of the insulation. The drop of temperature through the insulation in this case is certainly such as to render the results of tests wide of the mark. The discussion by Mr. Newbury touches upon this same subject. I cannot quite agree with him. In a two-layer winding, where you have two bars separately insulated in the same slot, it is quite unlikely that much of the heat flowing from the lower bar will pass out to the surface by way of the coil above it and the wedge. The heat flow paths are in that case practically restricted to two, one through the solid insulation and the iron surrounding it and the other longitudinally to the coil ends. In a long core such as is found in large turbo-alternators, say 70 in. in length, it would take, in order to carry all of the heat longitudinally along the conductors to the coil ends, a difference of temperature of from 50 to 100 deg. cent. between the center of the conductor and the outer end, according to the current density in the copper. This difference in temperature increases with the square of the length. It thus seems quite unlikely that any considerable amount of heat flows from the center to the ends. It seems still more unlikely that an appreciable amount of heat flows from the lower coil through the upper coil and the wedge. It must flow largely through the slot insulation. The results of Mr. Williamson's computations therefore seem to be quite reasonable.

R. F. Schuchardt: Just a word with reference to Dr. Adams's remarks with regard to the location of our exploring coils. It is true that these coils are laid between the wooden wedge and the outside insulation of the coil. It would be impracticable to put the coil directly adjacent to the high-tension armature copper. The problem is to get the exploring coil at the hottest point most practicably accessible and then allow for the probable higher temperature inside, and that is the reason we set 80 deg. for the measured allowable limit instead of 90 deg., even when measured so much nearer the hot spot than is possible with old-time methods.

B. F. Behrend: Suppose that the designer made a mistake and used a very heavy piece of copper; according to Mr. Burke's method of mind reading he would add 10 deg. for it, and he might equally well add 65 or 70 deg. The exploring coil may just as well be wound inside the coil. Coils should be so placed. It will help the designer and manufacturer, on the one hand, and the user, on the other hand; it would settle disputes if exploring coils could be wound inside the coil, and they should be.

Comfort A. Adams: It seems to me the explanation is the milk in the cocoanut, so far as the discussion we had this morning is concerned, and explains the recommendation of Mr. Torchio for a 75 deg. hot spot temperature, because his measurements were made apparently where the temperature was altogether likely 20 deg. or more lower than the actual hot spot temperature inside of the insulation.

Leo Schuler: I ask Mr. Schuchardt, when he made the measurements by means of that exploring coil which was outside the insulation, whether he measured the same coil by resistance, and what the relation of the two temperatures was?

R. F. Schuchardt: Our results showed about 30 deg. higher temperature with the exploring coil measured while the machine was loaded than by the resistance of the armature coil measured at the very earliest possible moment after the unit was shut down. Of course the armature temperature dropped immediately when the load was removed and while the unit was coming to rest.

Leo Schuler: Outside the insulation?

R. F. Schuchardt: Yes.

L. W. Chubb: In his discussion Mr. Durgin stated that his exploring coils were placed " some between coils, and some between coil and slot and some on the end turns." Mr. Schuchardt states that these same coils were placed " between the armature coil and the wooden wedge," and adds that the problem is to place them in the hottest point accessible.

If the coils are located between coils as Mr. Durgin states, the difference of 30 deg. cent. between exploring coil and temperature by armature coil resistance is more probable. The temperature difference under the wedge and between the two coils can readily be seen by referring to the full load test shown under *D* in Fig. 2 on page 170. Thermocouple No. 17 shows the temperature under the wedge, and thermocouple No. 19 the temperature between coils. The thermocouple under the wedge is near the cool air gap; there is a great flow of heat from the coil and a large temperature drop through the insulation. Thermocouple No. 19 is placed where there is little heat flow through the insulation and the temperature outside of the insulation more nearly approximates the copper temperature.

W. F. Dawson: I should like seriously to propose that in those machines which have two-layer windings a thermocoil placed between the upper and lower layers will probably approximate the highest temperature in the machine. The outer coil has as a rule a greater loss than the inner coil, due to Foucault currents generated by stray flux threading the outer conductor, and this excess loss probably about compensates for slightly better ventilation which the outer coil receives from proximity to the air gap. The temperatures of the upper coil and the bottom coil are approximately the same, and, therefore, there will be practically no exchange of heat between them. This middle point, therefore, should approximate the maximum temperature of the copper.

Charles P. Steinmetz: The only way of locating and measuring the hot spots would be by distributing exploring coils all along the inside winding of the high-potential coil, but today, when all station operators insist on spreading high-tension supply over a long distance and controlling it by low voltage, I do not think there would be any enthusiasm, on the part of stations which operate at 11,000 and 13,000 volts, to employ Wheatstone bridges or other such apparatus to measure the coil.

As regards the relative conductivity of the alternative paths, we are at cross purposes because we speak of different types of machinery. On the one hand, one engineer has in view high-voltage turbo-generators with 0.3 in. thickness of insulation, and a coil with several sq. in. of copper section, where the heat production is very large, while on the other hand, in the moderate or low-voltage machines, with small coils, the conduction along the copper may be negligible compared with the heat conduction across the coil insulation, so that you see there are different classes of machines, and what applies to one does not necessarily apply to the other.

Alexander Gray: It seems to me that if we put resistance coils next to the copper of the machine, they must be put near the neutral, and the neutral grounded.

There is another point of some interest. In a machine with a two-layer winding, the winding being well laminated so that there are no eddy currents in the conductors, the temperature of the two layers is the same, there is no flow of heat between the upper and the lower layer, and therefore a thermocouple placed between these layers must give very nearly the copper temperature, because there is no temperature gradient if there is no flow of heat.

I want to record the case of a large machine in Montreal which has a temperature rise of 40 deg. cent., measured by thermometer on the ends, and a temperature rise of 127 deg. cent. measured by the resistance coils placed between the upper and lower layers of the windings. It is a machine with deep conductors insulated with mica. The eddy current loss in the deep conductors is very large, and there is a flow of heat from the coil in the top of the slot to that in the bottom.

B. A. Behrend: If Dr. Steinmetz means that it is necessary to distribute 11,000-volt exploring circuits all over the power house in order to get the inside temperature of a high-tension generator, I venture to say that his statement is rather audacious, because you might easily put a small exploring coil in one of your high-tension coils, and bring the leads back to an instrument, which you might attach to the turbo-generator in a manner similar to the method of attaching steam gages to high-pressure steam turbines, without carrying high-tension current all over the power house, as he suggested.

R. B. Williamson: The temperature gradient has much to do with hot spots in a long generator. The gradient through the insulation of high-voltage machines is higher than many realize, and in machines having very long cores, we cannot count on much heat passing from the center of the machine out to the end.

As Prof. Adams has pointed out, the difference in temperature necessary to set up a flow of heat from the center to the end, increases as the square of the length. Considering the center of a long machine, nearly all of the heat liberated in the copper must pass through the insulation to the iron. On this assumption it is possible to calculate the temperature difference between copper and iron closely enough to give us some idea of the probable internal temperatures. In the case of high-voltage machines where the thickness of insulation is considerable there will be a relatively high temperature gradient, and it becomes a question to decide whether it is better to use lower voltage with thinner insulation and use step-up transformers, or use the high voltage with accompanying higher internal temperature.

B. G. Lamme: I want to refer to one point, namely, the measurement of temperature of high-voltage armature coils by means of thermocouples placed inside the insulation. I think that is all right as a research method, or one for determining temperature gradients in general. If we find by test that thermocouples placed between upper and lower coils in the same slot in a high-voltage machine, give practically the same temperature as thermocouples located inside the insulation, then we have gotten the information we need for practical purposes. I think therefore that the use of thermocouples inside the insulation would be for the purpose of calibration only, and will not become a commercial method of measurement, as there are very serious objections to such an arrangement.

Prof. Adams has spoken about the very small amount of heat conducted from the middle of a turbo-generator winding to the outer end. This is true in some cases, but in all cases there is a certain amount of heat conducted out which serves to reduce the internal drop through the insulation at the hottest point. Considering that a large turbo-generator may be 50 deg. hotter in the center of the winding than in the end winding, my calculations show that, except in very extreme cases, the longitudinal heat conduction is an item which should be considered in the calculations.

James Burke: I have prepared some notes on the papers now under discussion, in which I have, I think, shown that all the results are consistent, that the corrections for difference in temperature of the surrounding medium can be made and can be made intelligently. In general, the results shown in the tests of motors, and exploring coils, and certain experimental coils, all agree with each other within a very close margin. The negative and positive results are to be expected, and I think from the data contained in these various papers that we will be able to determine exactly what temperature correction should be introduced in the Rules. I think that a temperature correction is necessary, in view of the fact that one of these papers states specifically that certain motors fulfilled a temperature guarantee in summer, and failed to fulfill the guarantee in winter, and on retest in summer fulfilled the guarantee again. So that the importance of temperature correction seems to be supported by the facts in the papers now before us.

James Burke: (by letter) In the paper by C. P. Steinmetz and B. G. Lamme, reference is made to temperature correction as follows:

"The variation of the temperature rise has heretofore been considered as having a definite relation to the temperature of the cooling medium. However, it appears that it does not follow any definite simple law, but it is sometimes positive and sometimes negative, so that no satisfactory correction for room temperature is possible at present."

Also the following recommendation is made: "No temperature correction should be made for variation of the cooling temperatures from the reference temperature of 25 deg. cent."

From the number of very interesting papers presenting data on this subject, it would appear that a satisfactory treatment of this temperature correction can be arrived at, and it seems that the apparent confusion from the correction being sometimes positive and sometimes negative, should be fully explained.

In the existing rules, temperature correction is covered by rule No. 269, where it is stated that the correction is " On account of difference in resistance."

The adopted standard room temperature is 25 deg. cent., and the temperature coefficient for the resistance of copper at this temperature is given in appendix *E* of the present Rules, as 0.00386 for copper of 100 per cent conductivity, and this figure seems to be generally used. Taking this figure as a basis for correction, it would appear that the correction should be 0.386 per cent per deg. instead of 0.5 per cent as in the present rules.

If we consider a condition of room temperature of 40 deg. cent., which is 15 deg. above the adopted standard room temperature, all the copper resistances will be 5.8 per cent higher than at the standard room temperature of 25 deg. cent. For all copper carrying a constant current, as, for example, the shunt field of a generator in which the exciting current is kept constant for the purpose of maintaining constant voltage, the watts lost will be increased by 5.8 per cent. In the shunt field of a motor operated from a constant-voltage supply circuit, the result of the higher resistance due to higher room temperature is to reduce the current flowing in the circuit, and the watts lost in the circuit are reduced 5.8 per cent; thus we have an increase in the watts lost in a generator field and a decrease in the watts lost in a motor field, when the room temperature is higher than the adopted standard of 25 deg.; and for room temperatures of 40 deg. cent., the difference between a generator field and a motor field is 11.6 per cent. If a generator and motor were directly comparable, under this assumed room temperature of 40 deg., if the temperature increase on the motor field is 50 deg., the increase on the generator field would be approximately 6 deg. higher, or 56 deg. cent. If the value of the cooling medium is constant, we will then have a positive correction in the generator and a negative correction in the motor. If, however, the watts lost are kept constant rather than the current or the voltage, there would be no temperature correction if the value of the cooling medium remained unchanged, because in maintaining constant watts the increase in the resistance of the copper would be compensated for by a decrease in the current flowing.

Sometimes the fact is misunderstood, that the reason for temperature correction is due to change in watts lost on account of change in resistance, occasioned by the difference in temperature. For example, in the paper by Blanchard and Anderson, page 296, in referring to their curves of Fig. 3, which are based on keeping the watts constant, they say: " The variation according to these curves is about 0.15 deg. per deg. cent. variation of air temperature, in the opposite direction to that assumed in the present Standardization Rules."

As pointed out above, there would be no temperature correction for constant watts, but there would be a positive correction for constant amperes, and a negative correction for constant voltage applied to a coil.

The next important consideration, is variation in the quality of the cooling medium. In the present Rules the assumption seems to be that the cooling medium is constant, or, in other words, that with some definite watts per sq. cm. the temperature increases above the surrounding air would be the same with air at 40 deg. as with air at 25 deg. In the paper by J. J. Frank and W. O. Dwyer, entitled *Temperature Rise of Stationary Induction Apparatus*, it is shown that the radiation of heat is better with higher surrounding temperature, and taking a standard temperature of 25 deg. cent., the radiation value improves by 0.73 per cent for each degree increase in surrounding temperature. In the same paper it is shown that the convection becomes poorer with higher surrounding temperature. It will, therefore, be evident that the change in value of the cooling medium will depend upon how much of the heat is taken care of by radiation, and how much by convection. This influence is shown in Fig. 8 of their paper, in relation to tank dissipation, from which it will be seen that with a 50 deg. cent. increase in temperature, the correction for the cooling medium is as follows:

No convection..................	plus 0.73 per cent.
Combined radiation and convection from plain cast-iron surface............................	" 0.51 per cent.
Combined radiation and convection from simple corrugated surface.....................	" 0.36 per cent.
Combined radiation and convection from compound corrugated surface.....................	" 0.21 per cent.

The foregoing figures are based on stationary surfaces, without any air circulation caused by the moving part in the machine, or caused by fan for forcing air through the machine. With air circulation, the proportion of heat dissipated by convection increases rapidly, and it is not unusual to have the effect of convection ten times as great as the effect of radiation. On the compound corrugated surface, if by air circulation the convection is made four times as great as the radiation, the improvement in radiation due to higher temperature is just counterbalanced by the depreciation in convection, so that the value of the cooling medium becomes constant. In the same way it will be seen that with the simple corrugated surface, when the convection is about seven times the value of the radiation, the correction becomes zero and the value of the cooling medium constant. Similarly, with plain cast iron surface, when the air circulation is sufficient to make the convection approximately ten times the radiation, the correction becomes zero, and the value of the cooling medium constant.

This paper by Frank and Dwyer, which contains some valuable tests, recommends for air-blast transformers a correction of one-half of one per cent per deg. variation for 25 deg. cent., which incidentally is the same correction as in the present A. I. E. E. Rules. This is only recommended for air blast transformers and not for other types of transformers.

In the paper by Blanchard and Anderson, the test shown in Fig. 3, curve 3, for air pressure of 760 mm., figures out a negative correction of approximately 0.3 per cent per deg. This is for a coil suspended in still air and is probably comparable with a simple corrugated surface. The deduction which I have made from the paper of Frank and Dwyer, shows for a simple corrugated surface a negative correction of 0.36 per cent per deg., so that it is a substantial agreement.

In the paper by C. E. Skinner, L. W. Chubb and Phillips Thomas, entitled "Effect of Air Temperature, Barometric Pressure and Humidity on the Temperature Rise of Electrical Apparatus," tests are given on a coil maintained at constant watts of approximately 32, and with air circulation for the purpose of cooling the coil. These tests are tabulated in table No. 1, and show that the increase in temperature of the coil was practically constant throughout a range of temperature of the cooling air, of from 30 deg. to 64 deg. If the coil is considered as a simple corrugated surface, then their test agrees with the deductions which I have made from the paper of Frank and Dwyer, with a convection value of seven times that of radiation. From the description of the arrangement for this test and the blower for putting the air through the testing box, this amount of convection in relation to radiation would be easily expected, and therefore there is substantial agreement.

From the tests of Skinner, Chubb and Thomas, above referred to, where they show practically zero correction for a large variation in air temperature, and with constant watts, it is evident that if this coil were carrying a constant current it would have a positive correction of 0.386 per cent per deg. cent., and if it were working under a condition of constant voltage applied, it would have a negative correction of the same amount.

The paper by Maxwell W. Day and R. A. Beekman, entitled "Effect of Room Temperature on Temperature Rise of Motors and Generators," shows some interesting tests from which deductions can be made.

The results given in this paper are based on tests which appear to be at constant speed; for example, Figs. 4, 5, 6, 7, 8, and 9, are all marked "825 rev. per min." However, the description of the test shows that they were operated at constant voltage. The average difference in final temperature of fields between the low temperature test and the high temperature test in each figure is about 21 deg., which would make about 8 per cent difference in field strength, due to the higher resistance and consequently less current in the fields. This would result in a speed difference of approximately 4 per cent. As most of these tests were with fans for circulating air through the motor, and apparently the fans directly connected to the motor, this difference in speed would make a great difference in the amount of air circulated, as the amount of air circulated by a fan increases very rapidly with increasing speed of the fan. It would appear therefore, that at least part of the negative temperature correction in all these figures could be accounted for by a higher speed of motor due to the weaker field with higher temperature, and consequently increased air circulation.

Taking the figures from Figs. 4 to 12 in the said paper, and considering the bearing temperatures, the average of all the bearing temperatures is a correction of minus 0.75 per cent. If these bearings are considered as plain cast-iron surfaces, we would have a comparable figure from the deduction made from the paper of Frank and Dwyer of minus 0.51 per cent. The probable difference in speed of 4 per cent would make a difference in torque at the same load of about 4 per cent, which would mean less pressure on the bearings and consequently negative correction. If this is taken at 0.2 per cent per deg., which is figured from the average temperature, and deducted from the 0.75 per cent average from the test, the difference is 0.55 per cent, and is comparable with the 0.51 per cent for plain cast-iron surface referred to, thereby approaching substantial agreement.

Similarly, the average of all frame temperatures gives a correction of 0.46 deg. per deg. and agrees very closely with the 0.51 per cent per deg. for plain cast-iron surfaces.

Considering now the correction for shunt field by resistance, averaging all the tests shown in Table I, we get a negative correction of 0.31 per cent per deg., and similarly the average for shunt field by thermometer is 0.389 per cent per deg. These figures are directly comparable with the present A. I. E. E. rules of 0.5 per cent per deg., which, as pointed out, if based on the temperature coefficient of copper, should be 0.386 per cent per deg. The average of the two methods of tests, namely, by resistance and by thermometer, is 0.354 per cent per deg., which is very close agreement with the temperature coefficient of copper, namely, 0.386 per cent, and can be considered in substantial agreement.

In the paper by Day and Beekman, an attempt is made to draw some average from commercial tests, for example, Fig. 13, which shows the armature conductors' temperature increase on 98 machines, which are supposed to be alike. In these tests there are thirteen machines tested at 20 deg. cent. room temperature, and the tests appeared to show temperature increases varying from 23 deg. to 38 deg., or a variation of 65 per cent in the increase in temperature. Similarly, in Fig. 15, out of 11 machines tested, at a room temperature of 20 deg. cent., the shunt field temperature increase varies from 22 deg. to 45 deg., or over 100 per cent variation. It would therefore seem that any conclusion of averages drawn from these commercial tests should be avoided.

From all the foregoing and from the information contained in the various papers, it would seem that the present rules can be corrected so as to give a fairly accurate basis for temperature correction. The argument against continuing a temperature correction in the rules, seems to be that it is too close a refinement when the large variations in actual temperature measurement are taken into consideration. Nevertheless, the great importance of accurate determination of temperatures will doubtless bring about more care in this direction and better agreement between tests in the future, and probably also better methods of taking temperatures. As pointed out, the difference between a dynamo field temperature and a motor field temperature may be as much as six deg. at a surrounding room temperature of 40 deg., and yet have no difference at 25 deg. surrounding temperature, and it would seem that such a large difference should not be overlooked.

The importance of establishing proper rules for temperature correction is brought out very strongly in the paper by Day and Beekman, from which I quote:

" In one particular case some motors were tested in the summer and easily met the specified heating limits, but when the customer installed them in the following winter, and tested them, some of the heating limits were exceeded, while on retesting them again in the following summer the machines again easily met the specifications."

Now in this particular case, on account of not applying proper temperature corrections, the heating limits were exceeded. In other words, it appears that the machines did not fulfill the specifications. This might have been a cause for rejection of the machines, and yet the trouble was not with the machines, but was in not having a proper rule for correction.

If the motors in question were totally enclosed machines without forced draft ventilation and operated in a summer temperature of 35 deg., with an increase in temperature of 50 deg., then operated in a winter temperature of zero, the temperature increase might become 66 deg. for the shunt field, on account of increase in watts, due to more current flowing in the shunt fields, and also on account of decrease in quality of the cooling medium. In the assumed case, the correction for cooling medium was taken at the same as for plain cast-iron surface, namely, 0.51 per cent. which would probably be correct on account of being an enclosed motor and not having air circulation to increase the effect of convection.

In this assumed case with 35 deg. air temperature and 50 deg. increase, the ultimate temperature would be 85 deg., whereas with zero air temperature and an increase of 66 deg., the ultimate temperature would be only 66 deg. Therefore the motor would be perfectly safe, but would not be filling a temperature specification of 50 deg. increase, unless some temperature correction were applied.

If the motor in question was not a fully enclosed motor, or if it had forced circulation, the lower temperature would result in stronger fields and probably about 14 per cent difference in the amount of air circulated through the motor. So that the temperature increase would be greater, not only on account of increased watts in the field, but also on account of much less air being passed through the machine for cooling it. This condition might readily result in the shunt field having an excessive increase of temperature, compared with specifications, unless some temperature correction were applied.

Charles P. Steinmetz: I wish to say that when the Committee recommended not to make any temperature corrections, it was under consideration that the previously used temperature correction was incorrect, and that the evidence thus far available shows that there is a change of the temperature rise with the room temperature, but that no law has yet been derived from these tests. Since we have not yet been able to formulate a law, all we can do is to say that in testing we shall, as closely as possible, try to get the room temperature near the standard temperature.

A. E. Kennelly: I think it is a very interesting fact that the Standardization Rules are encountered by a question of pure science as to what is the law of the dissipation of heat energy from a heat body of a given form, and it is a remarkable fact, also, that whereas steam engineers are constantly investigating the laws of heat, for determining the input capacity of their machines, in order to utilize the heat, we are faced with the opposite difficulty of finding how far we can dissipate heat, and get rid of it. It is curious that heat should be the common science in which we both find limitations.

In regard to Dr. Langmuir's paper, he proposes a very interesting method for determining the dissipation by convection from a hot body. He proposes to consider, for example, that a round stationary wire which is heated by an electric current carries a layer or sleeve of stationary air around it, that the heat is conducted through that stationary layer, and then dissipated in any manner you please beyond that stationary sleeve. This hypothesis is likely to conflict with fact. We think there is evidence to show that the air is not stationary in the immediate neighborhood close up to the hot wire. Dr. Langmuir's formula if it will give us correct answers may be of great value to us, but it does not follow that the physical facts are in accordance with this hypothesis, even if the formula based on this hypothesis gives correct results.

In regard to dissipation of energy by free convection, in our paper presented before the Institute three years ago we first showed that when a thin wire was subjected to forced convection in air, quadrupling the speed of convection doubled the convected power, and that has been since corroborated by Prof. Norris in England. It was later discovered in the archives of a French society that Boussinesq had originated a formula leading to this result. Dr. Russell has recently developed Boussinesq's formula in practical form, and has shown that the heat should dissipate as the square root of the pressure as well as the square root of the velocity. We have lately checked this by experiments not yet published.

L. W. Chubb: In our paper on temperature rise, the curve of Fig. 2 shows a constant rise at all temperatures. This result seems to be somewhat contradictory to some of the other papers. In these tests the temperature of the walls was kept the same as that of the cooling air. and if there is any difference in convection due to the change in the viscosity of the air, this difference must be small, with forced convection, and be offset by the fourth power law of radiation through equal range of temperature at higher temperatures, within the range of temperature shown.

In the tests the ratio of dissipation by convection to radiation was very high, and if there is any great difference in convection at different temperatures, it would certainly more than offset the change in radiation.

The same apparatus illustrated was changed so that the cubical box consisted of glass plates. In this case the radiation to outside objects at ordinary temperatures caused a much lower temperature rise above the circulating air, when the air was at high temperature. This shows that corrections of temperature rise should not be based on air variations from 25 deg. cent. alone, and that the temperature of surrounding walls and objects will have a greater influence than the air variations.

If corrections are to be based on variations of air temperature alone the manufacturer can profitably entertain his customer's witness in a palm garden and do his testing in a glass conservatory where the air temperature will generally be high and most of the radiation will be to space at absolute zero.

Selby Haar: Dr. Langmuir gives a number of data on thermal conductivities and resistivities of various materials, but I believe he has overlooked a series of researches by a German physicist, Dr. Nusselt, whose method seems to be quite worthy of study. He used two concentric spheres between which he put the heat insulator which was under investigation, and he also was able to study the differences in the heat resistivities at various temperatures.

Leo Schuler: In regard to the influence of air temperature on the rise of temperature, Mr. Burke says that he has worked out a method of taking this into consideration. According to theory, and also according to the experiments shown in the paper, the influence of air temperature will be the more pronounced the more heat is taken away by radiation, though the difference will probably be greater in an enclosed motor, as is also shown in the paper, while, for instance, in the large turbo-generator where all the heat is taken away by convection, by air, there will be practically no difference. Could not Mr. Burke give us some idea how he proposes to take this into consideration for the correction to be made in the rules?

James Burke: That is covered in my written discussion.

E. W. Stevenson: Dr. Kennelly was the author of a very useful and interesting table, published twenty-five years ago, upon the carrying capacities of cables, in which he laid particular emphasis upon the fact that a dull black color on the outer surface made the carrying capacity of the conductor very much more than what it was if it were polished or bright.

I would like to ask Mr. Dushman whether, in making his experiments, he tried the value of the different colored pigments on the outside of cables carrying overload currents, and if in doing so there were any differences in the carrying capacity of these cables. Of course, it is understood that these cables are all hanging in the open air. It is possible there would be no difference with cables lying in ducts, whatever their color is.

R. W. Atkinson: I wish to confirm what has been said by Mr. Dushman about specific heats. We have looked up data for a good many materials and have tested a number of others, and find these figures are closely the same for the different materials and about of the value given.

I wish to mention that we have obtained considerable additional data since preparing this paper, which will be published in the TRANSACTIONS in a form which will make our data of much more practical use. I wish further to state what has been the basis of the formulas and equations which we have given here. These are all a result of actual tests; careful measurements have been made with thermometers and thermocouples of the temperature rise of many different sizes and types of cables. For convenience, greater accuracy, and brevity we put these in the form given in our paper.

R. W. Atkinson (by letter): In order that the constants "*a*" and "*b*" on pages 327 and 328 may be compared with other constants which are presented at this time, we may state that they correspond to a thermal restivity of approximately 1000 cent. degrees per watt per centimeter cube, and to 1200 degrees per watt per square centimeter, respectively. The neglecting of temperature coefficient in the formulas means that we assume that the negative temperature coefficient of heat resistance approximately balances the coefficient of copper ohmic resistance.

Since preparing our paper, we have made a number of further tests, and have correlated our results so that they may be conveniently applied to determining the temperature of cables installed in underground conduit systems. We are giving these data for predetermining both the ultimate temperature rise and for determining the temperature with loads of comparatively short duration. Reference will be found in what follows to some of the other papers presented at this Midwinter Convention, and also to a paper by C. T. Mosman published in the 1912 TRANSACTIONS, Vol. XXXI, Part I, page 755.

The formulas and equations which we have given, and the tables which we give herewith, with one exception which will be mentioned, are a result of direct test, careful measurements having been made with thermometers and thermocouples, also a few by rise of resistance method, of the temperature rise of many different sizes and types of cables. For convenience and brevity, and to increase the range of application, we have put these in the form given.

Table III, herewith, shows the current necessary to produce a rise in temperature above surroundings, of 25 deg. cent., for cables insulated with 1/8-in. (3.17-mm.) saturated paper and covered with a bright new lead sheath. The constants used are those just mentioned. The rise of temperature of a cable above a duct wall surrounding it is the same as the rise of the same cable in free air. This is indicated in the theory given in Langmuir's paper and is also borne out by tests made by us. As stated, the value assigned to a is correct for a new bright sheath, either in air or in conduit. This value indicates that the radiation from the sheath is about one-third of that from a perfect "black body." Painting the sheath black will reduce the temperature rise of the sheath from 20 per cent to 25 per cent below the value used in the table. It is indicated by Langmuir's theory that, depending upon the relation in size of the cable and the enclosing duct, the rise of the sheath might be increased slightly or considerably reduced below the value given, but we do not believe either condition likely to become important. The value given for b is a safe value for ordinary paper or varnished cloth cables. Our tests show, in most cases, a rise of copper above lead of 10 per cent or 15 per cent less than this, but it is believed that this value is as low as it is safe to use. The value given for varnished cloth by Dushman is 25 per cent lower than this. The same author gives a value for rubber which is 63 per cent of the value given here for paper. Table III should be used as given, for ordinary cases, and for those cases where it is not necessary to use absolutely the maximum allowable capacity. The values given will always be safe for the thickness of insulation given. Table IV gives correction factors for various thicknesses of insulation and shows how the total differences of temperature between copper and the surroundings of the cable are distributed, and thus makes it possible to make use of the correction factors given above where it is desirable and necessary. It will be noted that the total temperature rise is very nearly constant with the various thicknesses of insulation given.

Tables V and VI correspond to tables numbered III and IV respectively, tables numbered V and VI being for three-conductor cables.

Having now the temperature rise of the copper of cable above the duct walls, we will give data for determination of the rise of the duct walls above the surface of the earth. On account of the many variations which may occur with different types of duct system and laid in different kinds of earth which is itself at

TABLE III.—SINGLE-CONDUCTOR CABLE

CURRENT REQUIRED TO PRODUCE 25 DEG. CENT. RISE ABOVE SURROUNDINGS, AND WATTS LOST PER FOOT

Size B. & S. G.	Cir. mils	Current in amperes	Watts lost per foot at 66 deg. cent.
14		22	1.45
13		26	1.58
12		29	1.59
11		34	1.58
10		38	1.66
9		44	1.86
8		51	1.97
7		58	2.00
6		67	2.03
5		77	2.14
4		89	2.21
3		103	2.31
2		119	2.51
1		138	2.58
0		159	2.90
00		185	3.12
000		215	3.37
0000		250	3.62
	250,000	279	3.75
	300,000	314	3.98
	400,000	381	4.40
	500,000	442	4.74
	600,000	501	5.07
	700,000	555	5.30
	800,000	610	5.61
	900,000	661	5.90
	1,000,000	712	6.15
	1,100,000	759	6.32
	1,200,000	807	6.55
	1,300,000	851	6.73
	1,400,000	895	6 95
	1,500,000	941	7.17
	1,600,000	984	7.34
	1,700,000	1024	7.50
	1,800,000	1063	7.58
	1,900,000	1112	7.89
	2,000,000	1149	7.94
	3,000,000	1500	9.10
	5,000,000	2100	11.6

various and somewhat unknown temperatures, we believe that a direct measurement of duct temperature in combination with the data given above is the practical method of determining temperatures of existing installations. For purposes of previous calculations of new installations, we present herewith results of tests in a form for future use. Tests recently made by us show a difference of temperature between the inner and outer walls of a terra cotta duct 0.8 in. (2 cm.) in thickness to be 0.35 deg. cent. per watt of actual loss per foot length of conduit. These tests were made upon 3¼-in. (8.25-cm.) conduits, the difference in the temperature given being that through a single wall. We may thus consider that the temperature of the outside wall of the duct is lower than the temperature of the inner wall by 0.7 deg. cent. per watt per foot. This is on the assumption that the heat from the cable is radiating freely from each of the four walls. In order to measure the temperature of the duct walls surrounding any cable, it is necessary only to measure the temperature in an adjacent idle duct at the same distance from the outside of the conduit or the temperature of a duct adjacent but nearer the center of the conduit system. In the former case, the

TABLE IV. SINGLE-CONDUCTOR CABLE

A—Rise in temperature attained with current in Table III.
B—Rise of sheath in per cent of total rise.

Size B. & S. G.	Cir. mils.	4/32 Paper		8/32 Paper		16/32 Paper	
		A Deg. cent	*B* Per cent	*A* Deg. cent.	*B* Per cent	*A* Deg. cent.	*B* Per cent
14		25	52	25.6	36		
11		25	56	25.7	38	27.4	23
8		25	59	25.8	41		
2		25	66	26.4	47		
0		25	68	26.5	50	30.1	32
0000		25	70	26.9	52		
	500,000	25	73	27.5	56		
	1,000,000	25	75	28.2	58	33.6	38
	2,000,000	25	76	29	60		

temperature of the idle duct will be lower by not more than a very few degrees, which can be estimated from the data just given, and in the latter case the temperature of the idle duct will be very nearly the same as the walls of the working duct. A still more satisfactory way of measuring the temperature of the duct wall is to make the measurement actually within the working duct, but *after cutting off the current from the cable within*, for from one-half hour to two hours. This is allowable on account of the great amount of time required by the duct system to change in temperature as compared to the time required for the cable to change. If measurement of cable within a working duct is taken when there is a great difference between the temperature of the sheath and the temperature of the duct walls, it is obvious that it is not known which temperature is being measured. With any number of cables distributed throughout any duct system, the rise of the system above the surface of the earth is quite closely proportional to the total energy dissipated in all of the cables. With any given type and size of conduit construction, we may say that the temperature rise of the system is equal to a certain number of degrees per watt lost per foot of duct structure. The tests previously referred to, made by one of the authors at Niagara Falls, indicate that the rise in temperature of the outer duct of a 12-duct system is equal to 0.67 deg. cent. per watt loss per foot of structure. The rise in tem-

TABLE V.—THREE-CONDUCTOR CABLE INSULATED WITH 3/32 PLUS 3/32 PAPER

CURRENT REQUIRED TO PRODUCE 25 DEG. CENT. RISE ABOVE SURROUNDINGS

Size	Cir. mils	Current in amperes	Watts lost per foot at 66 deg. cent.
14		17	2.6
13		19.5	2 7
12		22	2.7
11		25	2.8
10		29	2.9
9		33	3.1
8		38	3.3
7		43	3.3
6		50	3.4
5		57	3.5
4		66	3.7
3		76	3.8
2		88	4.1
1		101	4.3
0		117	4.7
00		136	5.0
000		158	5.5
0000		183	5.8
	250,000	204	6.1
	300,000	231	6.5
	400,000	278	7.0
	500,000	322	7.5
	600,000	363	8.0
	700,000	402	8.3
	800,000	438	8.6

perature of a duct not adjacent to the earth may be taken as 0.8 deg. per watt loss. Mosman's tests (A. I. E. E. TRANS. Vol. XXXI, p. 771) give data for calculating the temperature rise of a conduit system containing 81 ducts. This is a fibre conduit laid in concrete. The center duct contained no cable and the rise of this and the nine surrounding it can be taken as 0.35 times the total watts lost in the system. The temperature rise of the surrounding rows taken in order may be taken respectively as 0.29, 0.21, and 0.13 deg. cent. per watt lost per foot of structure. Sufficient data are not available to make a good generalization.

Very often, a formula similar to that given by the authors has been given for the temperature rise of cables carrying currents for short periods. With the formula as a starting point and based on the assumption that the insulation takes the heat with the same rapidity as the copper conductor, theoretical curves have been deduced showing the temperature rise after various lengths of time. Probably the error due to this wrong assumption is seldom very large. The data given in the paper of the authors are based on tests of a number of different sizes, varying from No. 6 to 1,000,000 cir. mils. These experimental

TABLE VI. THREE-CONDUCTOR CABLE

A—Rise in temperature attained with current in Table V.
B—Rise of sheath in per cent of total rise.

Size B. & S. G.	Cir. mils	$\frac{3+3}{32}$ Paper		$\frac{8+8}{32}$ Paper	
		A Deg. cent.	*B* Per cent	*A* Deg. cent.	*B* Per cent
11		25	58	21.2	35
5		25	59	23	37
0		25	61	25.7	37
	500,000	25	62	29	39

results were then put in a form closely resembling the simple theoretical formula, thus largely combining the advantages of pure theory and pure experiment. We believe that our data can be expressed in a form somewhat more convenient for some uses, as given in Table VII. We have made one or two small corrections. This table shows the length of time required for different sizes of cable to reach different percentages of final temperature change, after an alteration in current strength. The data are so given that the temperature rise due to excessive overloads for short periods can be computed. If a steady current has been flowing, and an additional current is suddenly applied, the table is applicable to the temperature change due to the change in current. We do not advise application of these data to conditions which will actually cause a rise greater than that normally allowed; however, in the calculation of the final temperature rise to be used in connection with the table, one should neglect the temperature coefficient, simply assuming the final temperature rise for any current to be proportional to the square of the current.

The data so far given for short time temperature rise of cables apply to the rise of temperature above the surroundings. The heat capacity of the duct structure for underground cables, is very great as compared with the heat capacity of a cable itself, and consequently the temperature rise of the duct structure is very slow. There are not enough experimental data to make general applications. Table VIII is computed on the assumption that the only portion of the duct structure to store heat from the cables is that portion of the duct structure immediately surrounding the cable. This is far on the safe side, as shown by theory and by the results of some tests which have been compared with these tables. If it is desired to apply these tables to learn how long an experiment on an underground structure must be carried on to reach a steady temperature, it would be well to double the length of time given in the table. The columns in the center of the table (2*a* and 2*b*) may be taken as representative of most duct structures where the rise of the structure itself is important.

In order to make tables of carrying capacity of general use it is necessary to base the tables upon conditions which cause

TABLE VII

B. & S. G. or cir. mils	Time in minutes with 5/32 insulation							Bare
Per cent of final rise	10%	20%	30%	50%	70%	80%	90%	10%
6	0.52	1.2	2.4	5.3	11	16	30	0.38
4	0.66	1.6	2.8	6.6	14	21	35	0.47
2	0.90	2.1	3.8	8.5	19	30	40	0.63
0	1.2	3.0	5.5	12.5	25	37	55	0.9
000	1.8	4.5	8	18	37	50	72	1.2
300,000	2.9	7 2	12	25	48	65	92	2.0
500,000	4.0	9.8	16	31	57	78	115	2 8
1,000,000	6.8	15	24	49	85	120	165	4.7
2,000,000	9.5	21	34	67	115	155	230	7.0

Time required to reach various percentages of final temperature change. Time for bare conductor to reach 50 per cent and 90 per cent of change is 6.6 and 22 times as long, respectively, as to reach 10 per cent of final temperature change.

only the true ohmic loss in the conductors. This makes the tables true for all ordinary conditions. However, conditions do arise which greatly increase the losses, and these must sometimes be taken into account. The effect of dielectric loss has already been mentioned. Other losses which occur are analogous to certain losses which occur in various electrical machinery. Skin effect has been thoroughly treated in many places. When a single-conductor cable carries alternating current, if there be a lead sheath, there may be a very considerable loss induced in that sheath. This is treated and curves showing its amount are given in a discussion by the writer in the A. I. E. E. TRANS., Vol. XXXI, page 806. There is another loss which may sometimes occur but which, under almost all ordinary circumstances, is insignificant. Alternating current flowing in any conductor produces a field in adjacent conductors and a resultant eddy current loss. If there is already a current flowing in the other conductor, this merely constitutes a distortion of the distribution of the current, but the effect upon the loss is the same. To show the magnitude of this, we may state that if a cable of 2,000,000 cir. mil cross-section, enclosed in a sheath of lead having a diameter of 3 in. (7.6 cm.) and a thickness of 1/8 in. (3.1 mm.), is placed immediately adjacent to a cable of similar construction, a loss will be induced in the lead sheath of one by 60-cycle current in the other, of about 10 per cent of the ohmic loss in the one carrying current. It is possible for the loss in the copper conductor, induced in the same way, considerably to exceed this, unless the resistance between strands is unusually

TABLE VIII

PER CENT OF FINAL TEMPERATURE ATTAINED WITH LOADS OF DIFFERENT PERIODS

Column "a" for first application of load.
Column "b" for loads repeated daily.

Time in hours	1-*a*	1-*b*	2-*a*	2-*b*	3-*a*	3-*b*
1/2	12	12	6	6	3	3
1	22	22	12	12	6	6
2	40	40	22	23	12	12
3	55	55	31	32	17	18
4	64	64	40	41	22	25
5	72	72	47	48	27	31
6	79	79	55	56	31	36
7.5	85	85	62	64	38	46
10	92	92	72	75	47	63
15	97	97	85	87	62	75
20			92	97	72	87
24			95	100	79	100
30					85	100
48					95	100

Columns 1 are for system where one watt loss per foot in each cable causes 16 deg. cent. rise of duct structure.

Columns 2 are for system where one watt causes 8 deg. rise.

Columns 3 are for system where one watt causes 4 deg. rise. The time constant will not be shorter than this for any system.

high. This loss is proportionately less when the absolute dimensions are smaller, and also varies as the square of the distance between centers and inversely as the square of the frequency. It will be seen that this is ordinarily insignificant. It must be borne in mind that, wherever any of these losses occur, allowance must be made in computing the temperature rise and the resultant carrying capacity. This last-mentioned and usually insignificant loss must not be confused with the loss which occurs when the sheaths are connected together. This may exceed the conductor I^2R.

H. M. Hobart: Am I right in stating that Dr. Langmuir can tell fairly correctly what would be the temperature correction for any electric machine? Could he not, in a fairly reasonable space of time, figure out what the temperature correction should be?

S. Dushman: I believe he could, especially by taking into account the total losses as being due to the convection through the film and the radiation; taking that into account he can figure out the loss in each case, and I believe Dr. Langmuir will probably differ with what was said about the evidence being contrary to the existence of the film, because he has found evidence to confirm his theories.

H. M. Hobart: The impression I gained from conversation with him was that he could tell you not only whether the temperature correction was positive or negative but that he could state its value.

C. Fortescue: Referring to the use of an idle unit, recommended in the paper by Messrs. Johannesen and Wade, I think an idle unit is a very good thing, but there are some cases where one cannot always get an idle unit. I think the Standardization Rules ought to specify some method of determining the correct basis of air temperature without an idle unit. The idle unit can be used when one is available. It is a splendid method, but when one cannot be had and room temperatures are unsteady, there is always a tendency to a controversy between the representative of the purchaser, witnessing the test, and the man in charge of the test. One wants to take one plan of determining his room temperature, and the other wants to take another plan. Of course, the purchaser's representative usually wants to see as high a rise as he possibly can; he wants to be on the safe side, he wants to feel that he is showing up the apparatus, and naturally he looks at the figures that are higher as being correct. This paper by Johannesen and Wade shows distinctly that the highest figures are not the correct figures, and that some method of obtaining the correct figure must be indicated.

John J. Frank: Commenting on the last paper by Mr. Fortescue, and on the paper by Messrs. Johannesen and Wade, I would like to call your particular attention to the importance and necessity of recognizing the standard method of obtaining the room temperature or the temperature for correcting the rise under load or operating conditions. The use of an idle unit to determine this basis is clearly brought out by Messrs. Johannesen and Wade. It is recognized also by Mr. Eden in the paper presented by Messrs. Rice and Eden.

In the paper by Messrs. Fortescue and McConahey, no recommendations are offered, simply a reference to probable errors which might affect the test. Reference is made to the temperature rise of oil-insulated water-cooled transformers and the statement that the temperature of the air does not affect the cooling. This, of course, is not strictly correct, as the temperature of the air will affect the cooling of small units.

I would suggest that the over-potential test referred to should be the last or final test given the apparatus. This would insure the detection of any possible defect in the transformer created by other previous tests.

J. M. Weed: I would differ from Mr. Frank in regard to using the same method for determining equivalent room temperature in all cases, that is, with all classes of apparatus. What we really wish to find is the effect of room temperature upon the apparatus that is being tested. That is, if the room temperature is not constant, we want to know what room temperature to compare the final temperature of the apparatus with. The variations in the room temperature will affect different classes of apparatus under test quite differently. For instance, in transformers we have a large body of oil which must change its temperature, due to changes of room temperature, whereas in the case of rotating machines, etc., the active materials come in direct contact with the air and are affected more quickly than the transformer, so that, although the small oil-cup might be a satisfactory method for determining the equivalent room temperature for apparatus where the active materials come in direct contact with the air, it would not be for transformers, and in order to get an estimate of the equivalent room temperature with respect to transformers we need a large body of oil which will take time to change its temperature equivalent to the time required by the transformer itself.

W. F. Dawson: I question the theory of Mr. Weed as to the rate of cooling of the transformers after the load is taken off. I have worked with approximately the same figures expressed in "amperes per sq. in.," but it would seem that he has assumed falling temperature gradient based on thermal capacity of the copper only. My experience suggests that thermal capacity of surrounding insulation material in the core and a portion of the cooling oil has also to be considered and that the falling temperaature gradient is, therefore, much less steep. The thermal capacity of copper alone is such that at an ordinary temperature, with a density of about 1100 amperes per sq. in. there will be a temperature change of one deg. cent. per min. Generally, due to added thermal capacity of insulation and other surrounding media, there will be a divisor ranging from 2 to 4, according to circumstances. For example, a transformer may be running with 2200 amperes per sq. in., but instead of four deg. cent. change per minute, there will be two deg. change or less.

C. Fortescue: I want to remark that the difference in temperature between the top and bottom oil of the idle unit, which Mr. Weed has laid some stress upon, is only a matter of perhaps two or three deg., and the error, if we take the average of the two readings as the recommended temperature, amounts only to a fraction of a degree.

J. M. Weed: Answering the question brought up by Mr. Dawson, I did not mention the calculation by the thermal capacity of the copper as a means of determining the temperature drop, but in connection with the increase in the losses during the time that copper loss measurements were being made. The same considerations will apply, however, to the temperature drop after the load is taken off, as pointed out in one of the papers, but this calculated rate of temperature change would apply only to the first instant with accuracy. This rate will begin to change at once, so that this correction would be too large if applied to any considerable interval of time. For the first minute or two it would be approximately correct. In the heating up of the copper, the heat is all stored in the copper at the start until the copper rises in temperature, and begins to throw heat out into the surrounding insulating material and oil. Likewise, when it begins to cool, the heat is given out of the copper at a certain rate and continues to go out of the copper at that rate until the difference in the temperature between the copper and surrounding material has been reduced. Of course, that is a very short time.

In regard to Mr. Fortescue's last remarks concerning the difference in temperature between the top and the bottom of the idle transformer, I did not mean to lay stress on that consideration but was merely pointing out the possibility of not getting perfect results, but the idle transformer is so much better than any other method we have of getting an equivalent room temperature, that I have only favorable recommendations for it.

C. Fortescue: There is one point I want to bring out in connection with one of the papers in Group I, by J. J. Frank and W. O. Dwyer, entitled *The Temperature Rise of Stationary Induction Apparatus.* The paper states that if radiation is taken into account the temperature rise of the transformer instead of increasing with increased room temperature, decreases. The paper says, furthermore, that the convection introduces a factor which causes the temperature rise to tend to increase with increasing room temperature. The paper also goes on to say that the temperature gradient through the insulation introduces a factor which causes this gradient to decrease with increasing room temperature, and later on in the paper a correction is given based on the increase in viscosity of the air temperature of 0.04 of one per cent, or thereabouts, per deg. cent. to be subtracted when the room temperature is above 25 deg. cent. It appears to me that the authors of the paper have lost sight of the very points which they first bring out in their paper, namely, the fact that the cooling of such a transformer takes place in two stages; first of all, the surface of insulation is cooled by convection, so that with increase in room temperature the surface for a given pressure of air has a higher rise above the incoming air than with a lower room temperature. Secondly there is the temperature gradient through the insulation, which is a considerable part of the temperature rise in an air-blast transformer, and decreases with increasing room temperature.

In my opinion, these two factors will practically cancel each other, and instead of a correction of 0.04 of one per cent, to be subtracted for increasing room temperature, in order to bring it down to 25 deg. cent. standard room temperature, it will be zero, or thereabouts. In fact, the correction will vary with different transformers, and a good average condition would be obtained by eliminating it entirely.

Carl J. Fechheimer (communicated after adjournment): There seems to be a tendency among the engineers who have discussed the papers, as well as among the authors, to abandon the use of a temperature coefficient, since tests which have been made indicate to some extent that the air temperature has little influence upon the final temperature of the apparatus. I do not believe this stand to be correct. It is quite evident that if the loss occurs in copper only and the current in the conductors is maintained constant, the resistance and therefore the final temperature will be influenced by the air temperature. For example, such is the case with our ordinary field coils, and I believe that for apparatus of this character the proper temperature correction should be allowed: approximately 0.4 per cent per deg. cent.

It is of importance in apparatus having copper and iron loss, on which accurate tests are desired, to determine how much of the temperature rise is due to copper loss and how much to iron loss. At one time it was believed that the temperature rise was proportional to the sum of these two losses, but this has been found to be very much in error. Mr. A. M. Gray* showed conclusively that the influence of copper loss upon temperature rise in induction motors was generally considerably more than that due to iron loss, even when the two losses were equal. The reasons therefor we shall not give at the present time. Hence, there should be a temperature correction in the case of copper loss the same as with the field coil, but it is doubtful whether there should be a temperature correction in the opposite direction in the case of iron loss, due to the fact that the eddy loss decreases with increasing temperature. This would involve so much difficulty in the determination of the relative values of hysteresis and eddy current losses that it is better to omit such a correction. Even though the exponential curve of core loss is plotted, the value of the exponent would not necessarily be a criterion of the relative values of hysteresis and eddy currents, because we have found in many cases, when plotting on logarithmic paper, core losses against voltage, below the saturation point, that the exponent was greater than 2, although we know that a considerable portion of that loss was due to hysteresis.

We can easily determine the relative values of the temperatures due to iron loss and copper loss by operating the apparatus first without load at normal voltage, so that we have little or no copper loss and normal iron loss, and then operating with full load current in the windings at considerably reduced voltage (short-

***Heating of Induction Motors*, TRANS. A.I.E.E., 1909, Vol. 28, p. 527.

circuit in the case of generators, d-c. motors or transformers; and increased slip or against rotation in the case of induction motors.) It will usually be found that the sum of the temperatures obtained by these two methods is slightly greater than the full load temperature measured under normal operating conditions.*

The statement has been made that the increment in resistance due to the higher air temperature would not generally augment the final temperatures, because more heat is dissipated by radiation at the higher temperatures in accordance with the well-known radiation law. This would undoubtedly hold if a large proportion of the heat were dissipated by radiation. We do not believe, however, that much of the heat is carried away by this means. Certainly in moving machinery the greater proportion of the heat is dissipated by convection and a negligible percentage by radiation. Even in such stationary apparatus as transformers and rheostats, most of the heat is carried away by air close to the hot exposed surfaces; this air rises and is displaced in turn by cold air which takes the place of the hot air which has risen. In this way currents of air are set up, affording excellent means of dissipation by convection. Anyone who has held his hand above a rheostat carrying current has felt the currents of hot air rising from the apparatus. We could determine experimentally how much heat we could dissipate by radiation and compare with the standard methods of combined radiation and convection by enclosing the apparatus in a chamber which is ordinarily sealed and drawing a vacuum within this chamber. If this apparatus is then operated in a normal way all of the heat will be dissipated by radiation. We could then open a door at the bottom and top of the chamber and force air at various rates through the apparatus and could then quite easily determine the relative influences of radiation and convection.

Paul MacGahan (communicated after adjournment): Referring to the question of temperature indication, it should be pointed out in connection with the " exploring coil " or " resistance thermometer " method, that in cases where a continuous indication is desired for control purposes, such as where temperature indicators are to be used on switchboard panels to regulate the loading of generators or transformers, the resistance thermometer method is generally to be preferred to any thermocouple method, for the following reasons:

1. Direct readings obtained without manipulation.
2. Actual temperature indicated instead of temperature rise.
3. Standard switchboard D'Arsonval voltmeters used instead of galvometer.

Direct readings are obtained by connecting the voltmeter according to the Wheatstone-bridge method, three arms being permanent resistors of zero temperature coefficient

*This is discussed more fully under Group III papers.

and the fourth being a search coil, wound preferably with copper. Direct current of fairly constant voltage is applied and a standard switchboard type voltmeter used in place of the usual galvanometer.

With this method, a variation of the supply voltage will cause slight errors in the indications at certain points of the scale. This can be minimized by calibrating the voltmeter with its zero torque point at the critical temperature where extreme accuracy is most desired. Thus, at this point indications would be independent of the applied voltage. At other points, there would be a slight error depending upon the variation from the zero torque point on the scale and on the variation from normal voltage. The accuracy is sufficient for all practical operating purposes but probably is not of a high enough order for testing or research work.

A notable application of this form of temperature indicator is on the N. Y. N. H. and H. single-phase locomotives, the indicators being located in the cab, directly in front of the driver's seat.

C. **Fortescue** (communicated after adjournment): The paper of Messrs. Johannesen and Wade calls attention to a feature of temperature determination of transformers which has always been a source of contention between the representative of the purchaser and the manufacturer. I refer to the determination of the correct temperature rise of oil-insulated transformers with fluctuating room temperature.

According to test results obtained by the authors, there is a lag of the temperature of the oil and consequently of the temperature of the coils behind the temperature of the room. It is shown that by the use of an idle unit of the same design, a base temperature is obtained from which to measure the correct rise of temperature. The temperature of the winding of the idle transformer may be used as a basis from which to measure the true temperature rise of the coils of the working transformer.

All these results may be deduced from purely theoretical considerations; thus, assuming constant emissivity of the case with varying temperatures, let

θ_1 = room temperature.
θ_2 = average temperature of oil of hot transformer.
θ_3 = average temperature of oil of idle transformer.
W = weight of oil in absolute units.
α = emissivity of case in absolute units.
γ = specific heat of oil.
S = area of case surface.
E = rate of dissipation of energy in case in absolute units.

We have therefore

Heat dissipated from case $= \alpha S (\theta_2 - \theta_1)$

Heat retained in oil $= E - \alpha S (\theta_2 - \theta_1)$

and therefore

$$\frac{d\theta_2}{dt} = \frac{E - \alpha\, S\, (\theta_2 - \theta_1)}{\gamma\, W}$$

or

$$\gamma\, W \frac{d\,\theta_2}{dt} + \alpha\, S\, \theta_2 = E + \alpha\, S\, \theta_1 \qquad (1)$$

Consider a periodic value of θ_1 and for simplicity let us assume it to be of the form

$$\theta_1 = \theta_0 + \theta \cos c\, t \qquad (2)$$

Then the solution of (1) gives us

$$\theta_2 = \frac{E}{\alpha\, S} + \theta_0 + \frac{\alpha\, S\, \theta}{\sqrt{\alpha^2 S^2 + c^2 \gamma^2 W^2}} \cos\,(c\, t - \varphi)$$

$$- \left(\frac{E}{\alpha S} + \theta_0 + \frac{\alpha\, S\, \theta}{\sqrt{\alpha^2 S^2 + c^2 \gamma^2 W^2}} \cos \varphi\right) e^{-\frac{\alpha\, S}{W\, \gamma}\, t} \qquad (3)$$

where $\tan \varphi = \dfrac{c\, \gamma\, W}{\alpha\, S}$

The last term vanishes when t becomes infinite and at the end of a long period its effect is negligible.

The temperature condition of the idle unit is obtained by making $E = 0$ in (3). This gives

$$\theta_3 = \theta_0 + \frac{\alpha\, S\, \theta}{\sqrt{\alpha^2 S^2 + c^2 \gamma^2 W^2}} \cos\,(c\, t - \varphi)$$

$$+ \left(\theta_0 + \frac{\alpha\, S\, \theta}{\sqrt{\alpha^2 S^2 + c^2 \gamma^2 W^2}} \cos \varphi\right) e^{-\frac{\alpha\, S}{W\, \gamma}\, t} \qquad (4)$$

Subtracting this from θ_2 we have

$$(\theta_2 - \theta_3) = \frac{E}{\alpha\, S} \left(1 - e^{-\frac{\alpha\, S}{W\, \gamma}\, t}\right) \qquad (5)$$

which is also the solution for temperature rise of the transformer above a constant room temperature. It is therefore evident that the rise of the hot transformer above the idle transformer is the same as the temperature rise of the transformer under conditions of constant room temperature at every point of the temperature curve.

In addition to corroborating the results obtained by the authors, the theoretical solution clears up one point they seem to be not altogether sure of, namely the proper value of room temperature on which to base the temperature rise of the transformer when no idle transformer is available. The problem is in the case of irregular variations to a certain extent indeterminate, but the correct value may be obtained by a method of trial and error in the following manner.

Take two points on the curve of air temperature representing as nearly as possible to the eye a complete periodic variation. Obtain the average value of the room temperature between these points and draw a straight line parallel to the datum line and at a distance from it equal to the value obtained. This will intersect the curve of room temperature at three points which include between them a portion of the room temperature curve which approximates a true harmonic cycle. Proceeding as before a second average is obtained between the points on the curve indicated by the first average; repeat the process until there is no change in the position of the points obtained by the line parallel to the datum line. The last average temperature will be the true base temperature of the room between these points and the correct rise may be obtained by taking the average temperature of the oil and coils between these points and subtracting the average room temperature as obtained above.

This value of room temperature is the quantity θ_0 used in the above discussions in (**3**). The integral of the periodic portion through a complete cycle vanishes, so that if t be sufficiently large we have

$$(\theta_2 - \theta_0) = \frac{E}{\alpha S}$$

which is the temperature that would be obtained in the transformer with a constant room temperature equal to θ_0.

H. L. Wallau (communicated after adjournment): Table I, given in Messrs. Atkinson and Fisher's paper on "Rating of Cables," was most astonishing to the writer and does not agree with his experience.

Records kept during the last ten years on a 1,000,000-cir. mil cable operating at from 115 to 150 volts to ground, show that with the exception of the winter of 1906, when the load on this cable did not exceed 700 amperes, it has been subjected each year for a period of two to three months from 1902 to 1913 to an overload of 30 per cent for three hours, five days a week, and 20 per cent for six to seven hours one day a week, and has carried between 80 and 100 per cent of its normal load for four to five hours before the overload came on.

This feeder has been in continuous service all of the time and has given no trouble. Its insulation consists of 4/32 in. (3.1 mm.) oiled paper and it has a 1/8 in. (3.1 mm.) lead jacket.

The performance of this cable is not phenomenal, practically all of the low-tension cables on the system giving like results. These cables pass through subways containing twenty to forty similar cables operating at similar loads.

A 2200-volt, No. 4/0 B. & S. three-conductor cable 3/32 + 3/32 in. (2.3 + 2.3 mm.) insulation carried continuously for 10 hours a day an overload of 25 per cent and for 14 hours a day a load of 20 per cent of normal, for over six months, with but one breakdown during the period, and is still operating with a maximum load of 75 per cent of normal.

More caution should be exercised in overloading high-voltage cables, since the potential gradient through the insulation is not a straight line but very steep next to the conductor.

A. Herz (communicated after adjournment): I agree with Mr. Johannesen and Mr. Wade in their policy of using a mass of iron, copper and oil as reference standard for room temperatures in making heat runs on oil-insulated transformers. Such reference units should not have larger heat storage capacity than that which would follow changes in room temperatures in the course of about one hour. I mean that the time lag between a change of room temperature and that same temperature shown by means of the above temperature standard should be in the order of one hour. A small transformer would conform to this.

It has been the practise for some time in the past when specifications are drawn up for the purchase of transformers, to specify that the transformers are to be subjected to a double-voltage run at suitable frequency as one of the acceptance tests. I think there is no test which gives a person as much certainty of the condition of the insulation between individual turns in the windings and also between the separate parts of windings which are subjected to potential differences in normal operation as such an over-voltage run. I have elaborated somewhat on this and used a similar scheme in the purchase and testing of generators, in so far that a certain per cent over-voltage operation be part of the acceptance test, this to be obtained by excess excitation or by over speed run (not to exceed 15 per cent), or a combination of both. I have found no trouble in obtaining as high as 40 per cent excess voltage by such means and after such test in combination with the regular breakdown test I have felt reasonably safe in turning such apparatus over for regular operation. I have also on several occasions discovered defective insulation and coils by these means. The mere fact of applying the usual test voltages between the windings of apparatus and the ground does not put any potential difference between adjacent coils or between the individual turns or insulated joints made in assembling the winding. A generator for instance could not be tested for breakdown between phases or between coils unless all the individual coils were disconnected from each other at the time of such test, which is usually a practical impossibility. Even then the individual turns would not be subjected to any excess voltage. I would therefore recommend that a paragraph be incorporated in the Standardization Rules to require all apparatus to be operated at excess voltage for a period of one minute. In the case of generators which are usually not subjected to excess potential stresses, such as line surges, an over-voltage run of 40 or 50 per cent would be reasonable; in transformers, as is now the practise, an over-voltage run of one hundred per cent would be reasonable; in other apparatus such excess voltages as would be deemed best by the committee.

Core temperatures, when read by a thermometer, should have such thermometer bulbs in contact with the core in such a way that there is practically no radiation from the heat-conducting medium connecting the bulb with the core. The usual method of using putty is such that the large area of putty exposed to the air puts the thermometer bulb in the presence of a medium cooler than the core, at least on one side. I think due attention should be paid to this, and when putty is used it should be covered by heat-insulating mediums, such as cotton waste. I have obtained higher temperatures than those obtained by the usual method, when covering a thermometer bulb with metal foil, this covered with a small amount of putty, and then the whole covered with a layer of waste. This high temperature was not due to local rise, because, in the cases I have in mind, the area occupied by the thermometer, covered as above, was but a fractional part of one per cent of the total area exposed to radiation.

Whenever exploring coils or thermocouples are used in proximity to coils within the electrical apparatus in order to measure temperature existing in proximity to such devices, such means of obtaining temperature should not be used unless suitable provisions are made, that in case of a breakdown between the coil or conductors and such devices, no injury will happen to the testing apparatus or the operator. The precautions are essential, since the insulation of the apparatus under temperature test was probably never subjected to such electrical stresses as it will be subjected to while under this test, and hence due caution must be used.

D. W. Roper (communicated after adjournment): The question of the limiting temperature of lead-covered cables is one of extreme interest to all users of such cable. A careful inquiry into the records and among the men actively engaged on the work of installation and maintenance of a system whose maximum load exceeds 200,000 kw. fails to disclose a single case of failure of a cable which could be definitely traced to overload. One case was found where a low-tension cable main had carried such heavy loads that the paper insulation was charred and brittle, but it was still in service, and the condition was discovered at the time that the services were transferred to a heavier cable. In other cases low-tension feeders have carried loads far above the loads generally considered as safe for such cables and have melted the solder in the terminal lugs and in the copper sleeves connecting the sections of cable. These cables carried these excessive loads without any apparent injury, and a careful examination of sections cut from the cable after having carried these loads, failed to disclose any perceptible difference in appearance from new cable. Our experience, therefore, appears to indicate that the temperature of low-tension, paper-insulated cables is limited by other factors before we reach a temperature that will permanently injure the insulation.

With high-tension cables the behavior is somewhat different. A number of cases have occurred where cables have burned out due to local heating, as, for example, where a conduit line passed over a steam pipe, or where exhaust steam has been turned into a catch basin adjacent to a conduit line. Instances of this kind are not at all rare, and in some cases it has been necessary to re-lay a conduit line or move a steam pipe in order to avoid the frequent burn-outs of the high-tension cables at such warm locations. An inquiry among a number of the larger users of high-tension transmission cable shows that all of them have had such cables fail during, or immediately after, a particularly warm spell of weather and without any apparent cause.

A paper by Mr. Rayner in the July, 1912, number of the *Journal* of the Institution of Electrical Engineers, gives a clew to the cause of such burn-outs, and this cause is briefly touched upon by Messrs. Atkinson and Fisher in their paper, wherein they state that "the increasing dielectric loss with increasing temperature must be considered." Mr. Rayner's investigations indicate that with all types of fibrous insulation the dielectric loss increases quite rapidly with increasing temperature. It follows, therefore, that for each kind of fibrous insulation there is a temperature which may be called the "critical temperature", at which the dielectric loss is equal to the heat loss by radiation. If, therefore, the temperature of the insulation of a high-tension cable should from any cause exceed this critical temperature, then, the dielectric loss being greater than the radiation, the temperature of the insulation will continue to rise as long as the potential is applied, although the load is entirely removed. Apparently high-tension cables break down in service from this cause. The trouble does not occur on the hottest day in summer, but a day or two afterward. The cable, apparently, during the maximum load period on a hot day, reaches a temperature above the critical temperature, and then continues to increase in temperature gradually over a period of a day or two or more until it finally breaks down. While, therefore, it may not be strictly correct to state that the cables have burned out from overload, they have failed because they were allowed to exceed the critical temperature for the particular voltage at which they were operated.

So far as is known to the writer, this limiting temperature for high-tension cables has never been determined. It probably varies with the thickness of insulation, the operating voltage and with the nature of the insulation. As near as can be judged by experience, this point is of no great consequence in cables operating at voltages under 10,000, which have the usual thickness of insulation for such voltages. The experience with cables operating at higher voltages, however, indicates that in hot weather, or where the cable is subject to heat from some external source, loads which are generally considered well within the safe capacity for such cables, will suffice to heat them beyond the limiting temperature.

It is therefore suggested that in determining the safe carrying capacity of high-tension cables, due consideration be given to the dielectric loss at the higher temperatures.

Edmund C. Stone (communicated after adjournment): It is interesting to see how much more quickly the limiting temperature is reached, and how much shorter are the periods of overload, on cables than on other kinds of electrical apparatus. The tables on pages 330–1 bring out clearly the danger to cables from even short overloads and the necessity of securing immediate relief if a cable becomes unexpectedly overloaded.

It seems to me that more information than now ordinarily obtained regarding the performance of cables would be of definite value to the users.

It is evident that, other things being equal, if one make of cable would operate safely at materially higher temperatures than others, or had heat-dissipating qualities that would permit it to carry a materially heavier load at the same temperature, that cable would be distinctly advantageous to use—indeed, would be worth more in cents per foot. If one cable had a materially higher dielectric loss, it would be distinctly more subject to breakdowns under emergency conditions of overload, for the larger the dielectric loss the more rapid the internal temperature rise under abnormal conditions; hence such cable would be distinctly disadvantageous to use. Likewise the charging current would become important if excessively large, and if any of these characteristics should change materially, it would be of great importance to the operating man to know of these changes, in order that he might adjust his conditions to suit them. For example, formerly the exciting current of a transformer could not be excessive, but recently, through the use of different qualities of steel, it has become an important factor, and it is probable that a large number of transformers have been bought with entire ignorance of the excessive exciting current that is present, to cause a reduction in the power factors of the system.

C. P. Randolph (by letter): Mr. Williamson brings out clearly several important points affecting the calculations of the temperature drop in electrical machines. I wish to emphasize some of these. It is absolutely essential in order to make accurate calculations of this kind that there be no space filled with air between the different kinds of insulation. The air films play a very important part in the resistivity of electrical insulations where the heat flow is in a direction at right angles to the layers of insulation. On looking over the tables on thermal conductivity presented in Mr. Williamson's article, and also those measured by Symons and Walker, the most noticeable feature is the slight differences in the conductivity of the various materials. It is very likely that the resistivity is due chiefly to the number and thickness of the air films between the layers of mica, cloth, etc. Though the air films between layers are very thin, nevertheless they are present. The writer has found by measurement that the thermal conductivity of layers of mica 0.003 in. thick when under a pressure of five lb. per sq. in. is about 0.0024, whereas when the disks are loosely piled on each other (under a pressure of perhaps one-fourth lb. per sq. in.) the thermal conductivity is 0.00090* or about one-third of the former figure. The conductivity of a pile of mica sheets will increase with the pressure very rapidly at first, but very slowly when the pressure has passed such a value that the air films are nearly absent. This pressure is always exceeded in properly assembled coils when they are firmly imbedded in the slots, so that one should be able to calculate the temperature difference between the copper and the iron with a degree of accuracy sufficiently high for practical purposes. The temperature coefficient of the thermal conductivity of layers of mica when tightly pressed is very small, and when loosely pressed it is very high.

Problems involving the calculation of heat losses are always very complex even in carefully designed laboratory apparatus, and the calculation of the temperature at many points in a generator or motor under different operating conditions presents problems so complex that they are often practically impossible of solution. Fortunately the designer wishes to know the temperature only at the places where it is highest and often the problem can be so simplified without impairing the accuracy that the necessary calculations are comparatively simple. Mr. Williamson shows clearly how to handle one of a number of such problems that arise. There are no engineering problems that require the exercise of "good judgment" to arrive at results of value, so much as those involving the flow of heat. Practically every problem requires that some assumptions be made, as otherwise the calculation would be too complex to handle. The simplifying assumptions may allow problems that at first appear to be impossible of solution finally to be solved accurately and easily. At the same time these assumptions may lead easily to results 100 per cent in error, if "good judgment" is not exercised. Experience in dealing with problems of heat flow is necessary to obtain correct results. When a problem is so complex that

*The thermal conductivity is given in watts per degree centigrade per inch cube.

one cannot be sure of the accuracy of the calculations the result may be merely used to locate places of high temperature. Then temperature coils may be inserted at these points to determine the actual temperatures.

The designing engineer should have at his command accurate data as to the life at different temperatures of the various insulations, so that each material can be used under the temperature conditions to which it is most suited. Data on this point are needed now more than data on thermal resistivity. Such data are very difficult to determine experimentally, but without them we must use relatively high—and therefore expensive—factors of safety.

It is the actual temperature of the insulation, and not the temperature rise, which limits the output of a machine, as has been brought out in several papers presented at this meeting of the Institute. This depends not only on the temperature of the surroundings, but also on the conductivity of the insulating materials, iron, etc., and on the surface properties of the machine. For instance, to test a machine before it is painted may lead to erroneous results. Painting usually increases the emissivity, thereby making the machine run cooler than previously. Reliable data on this point are very meager, but the writer is at present carrying out some experiments to clear it up.

E. D. Edmonston (by letter): Messrs. Atkinson and Fisher have given engineers and users of cables much valuable and needed information. There has been a dearth of information on underground electric cables, and for much of the published data we have had in the past, we are indebted to Mr. Fisher and his company.

We have been given the maximum temperature limits of safe practise in working cables, but we are not as yet in possession of enough data to predetermine fairly accurately the current rating of an electric cable for service underground in various conduit formations, in various soils, under varying conditions.

The formulas given, as stated, are based upon experiments on the rise in temperature of cables in free air, whereas most of our lead-covered cables for power and lighting service are placed underground in conduit constructions where there is little appreciable circulation of air through the ducts.

From the formulas given we may compute the temperature rise of copper above lead, and the rise of lead above immediate surroundings; but in the rise of temperature of surroundings due to the presence of other cables and the nature of the surroundings, we have only meager and limited data available on which to base a guess in many of our calculations. Yet, as the paper states, heating and current rating of any given cable may be reduced to a half or a small fraction of its normal rating, due to the extreme variability of the external conditions. For underground cable work, therefore, to make the fullest use of the valuable information given in this paper, we should endeavor to obtain more information than has been published concerning the conduction of heat by the earth, when conduit lines are laid in clay, loam, sandy soil, etc., both when the soil is damp and when dry. In the usual subway construction, with manhole spacings of 200, 300 or 400 ft. I believe that there is little dissipation of the heat generated in cables through possible circulation of air in conduits, but I would like to obtain the views of others on this matter.

Turning for a few moments from the subject of cable heating from the conductors, to the heating of cables from without, you may be interested in some findings in Baltimore where we have nearly a half-million feet of several different makes of 13,200-volt transmission cables in the lighting and power company's service, installed over a period of about nine years. Some of these cables which had been in regular service for some years began to cause us trouble for the first time last summer. In a conduit line where we had twelve transmission cables, we had a cable breakdown between manholes in one section. This was followed about a week later by the breaking down of another cable at the same point in the conduit line, but of course in another tile duct. A week or so later another cable broke down at the same point in the conduit line, and so on until we had lost five cables. The cables in question were three-conductor, 4/0 B. & S. gage, 6/32 in. by 6/32 in. paper by 5/32 in. lead sheath; and the breakdowns occurred between conductors. A short time after the initial breakdown, we had a breakdown of a cable between manholes in another section of the conduit line, which was followed after short intervals of time by four additional breakdowns at about the same point in the conduit line in nearby ducts. We dug around and exposed the conduit line at the points of trouble, examined the conduit construction, examined the adjacent cables for signs of electrolysis, and found nothing unusual in the nature of the soil. All of the cables have been working well under normal rating, and the maximum temperature we were able to get between manholes in the duct line was about 52 deg. cent. by recording thermometers. We sought to find some electrical phenomena to account for the trouble, and failing in this I took our troubles to our chemists. Moisture was found to exist in quite considerable quantities in the paper insulation. Many samples of cable, and also some new cable, were carefully taken, from all of which we were able to distill (using a temperature under 120 deg. cent. with a vacuum of approximately 26 in.) from 1 to 2 grams of water per 5-in. length, which is equivalent to about 1 to 2 ounces of water per 10-ft. length of cable. We cannot account for the cause of initial breakdowns mentioned; but we feel satisfied that the succeeding breakdowns at the same points in the conduit line were caused by the localized heat from the first short-circuit vaporizing the moisture held by capillarity in the paper insulation of the adjacent cables, which vapor, driven back in the cables from the hottest point, distilled in globules of water at a nearby point where the cable was sufficiently cool to condense the vapor; and these globules of water so formed, did the rest after a period of time. All of the cables affected were ordered under the American Standard General Cable Specifications covering paper-insulated lead-covered cables prepared by the Paper Insulated Power Cable Engineers' Association, were tested at factory and after installation, and complied fully with the specifications. Replacement of the damaged sections of cable by the manufacturer, under a five year guarantee, does not compensate for the loss of prestige and service due to the interruptions caused by the cable breakdowns.

The point I wish to make, is that we have not a standard cable specification which will adequately protect the purchasers and users of cables, especially high-tension cables for underground service. I have appealed to three of the large cable manufacturers to tell me the limitations of moisture which they could assure in a specified make-up of cable; but in each case their answers have given me to believe that they had little knowledge of how complete their vacuum process was in extracting the moisture from the paper before applying the compound and sheathing; and this has led me to draw up my own specifications for the limitations of moisture.

Mr. Fisher, in a previous paper before the Institute, stated that it was to be regretted that the manufacturers and operators of electric wires and cables in this country did not draw up a reasonable set of specifications, because it would simplify matters for consulting engineers, operating companies and manufacturers. We are all striving for standardization, and I beg to suggest that the work of formulating an adequate set of cable specifications could well be taken up by the Institute and would bear good fruit for both the users of cable and the manufacturers.

In some of the big cities, millions upon millions of dollars are invested in undergound electric cables. The valuation of the underground cables in our little power and lighting system in Baltimore totals an amount in the seven figures, and approximates the total cost of all the electrical equipment in our stations; yet with some lighting companies where greater portions of their distributing systems are underground, the cost of underground cables probably exceeds the total investment in all station equipment. With such amounts involved, the operating companies are naturally most desirous of obtaining cables of the best design and construction at a fair cost, and of knowing how to work those cables to the best advantage underground in properly designed conduit construction. Reports by a number of the large lighting companies made last year to the National Electric Light Association Committee on Underground Construction, give evidence concerning high-tension cable breakdowns which indicates that there is yet much to be desired in the make-up of, at least, high-tension cables; which weaknesses in construction have not been shown by the initial tests that it has heretofore been customary to apply. Many cable users say, with much logic, " leave the make-up of cables to the manufacturer," but on high-tension paper cables the manufacturers appear not all in accord. True, many of them appear to use much the same cable compound, principally resin oil, under different trade names. The papers used appear not to be very different in character, though the quality may differ somewhat; but the thickness of the paper and the tightness of the wrapping varies considerably for high-tension cables. Some manufacturers recommend a loosely wrapped high-tension cable, others, a tightly wrapped cable. Each has certain advantages, but on account of the paper costing considerably more than the compound used, is there a temptation for the manufacturers to use a loose wrapping, though the tight wrapping may be desirable? The Institute affords a common ground for cable manufacturing engineers, and engineers of cable users, to get together in an endeavor to formulate more adequate specifications than we have yet had, and give to the profession at large recommendations which would be of tremendous value and in line with standardization. I venture to hope that an Institute Committee will take up this work.

A communication first presented at the 29th Annual Convention of the American Institute of Electrical Engineers, Boston, Mass., June 28, 1912, and discussed at the Midwinter Convention, New York, February 27, 1913.

THE MYRIAWATT

BY H. G. STOTT AND HAYLETT O'NEILL

The object of this communication is to introduce a new unit of power which, if adopted, will afford a basis of comparison of all converters of energy, thermal and mechanical; and also will be international in its character, as it is merely a new multiple of the watt.

In American and European practise, at the present time, there are in use many empirical units, the use of any one of which is restricted to a distinct territory. A few of the more important ones are, horse power, boiler horse power, kilowatt, cheval à vapeur, pferde-kraft and poncelet. Obviously an engineer, in attempting to compare data from a foreign country, is compelled to face a confusion of terms, which usually can be made intelligible only by laborious calculations.

Again, in the United States there are in vogue such units as boiler horse power and horse power, which, while similar in sound, have no logical connection; and one has yet to find where the " horse " comes in.

With the rapid development in electrical measuring instruments, and, until recently, a corresponding lack of development in steam-flow measuring instruments, the term kilowatt has become more and more used as the one unit of power output.

The term became a necessity with the growing favor of steam turbines, and all direct-connected units where it is impossible to measure accurately the mechanical and the electrical power separately.

To form a connection between the boiler or producer output, the engine and generator output, the term " myriawatt "—

derived from the Greek "myria," meaning ten thousand, and the term watt—is proposed.

For the purpose of standardization, the British thermal unit, 1/180 of the heat required to raise one pound of water from 32 deg. fahr. to 212 deg. fahr. and the equivalent evaporation from and at 212 deg. fahr. (970.4 B.t.u.) is used (Marks and Davis).

From this, the following equivalents are obtained:

1 Foot-pound	=	0.0012861	B.t.u.
1 Kilogram-meter	=	0.009302	B.t.u.
1 Gram-calorie	=	0.0039683	B.t.u.
1 Horsepower	=	2,547	B.t.u. per hr.
1 Cheval à vapeur	=	2,512	B.t.u. per hr.
1 Pferde-kraft	=	2,512	B.t.u. per hr.
1 Poncelet	=	3,349	B.t.u. per hr.
1 Kilowatt	=	3,415	B.t.u. per hr.
1 Boiler h.p.	=	33,479	B.t.u. per hr.
1 Myriawatt	=	34,150	B.t.u. per hr.

Reduction of the Myriawatt to the above units.

1 Mw.	=	34,150	B.t.u. per hr.
1 Mw.	=	8,605,000	Gram-calories per hr.
1 Mw.	=	26,552,000	Foot-pounds per hr.
1 Mw.	=	3,670,900	Kilogram-meters per hr.
1 Mw.	=	13.410	Horse power.
1 Mw.	=	13.597	Cheval à vapeur.
1 Mw.	=	13.597	Pferde-kraft
1 Mw.	=	10.197	Poncelets.
1 Mw.	=	10	Kilowatts.
1 Mw.	=	1.020	Boiler horse power

Reduction of all of the above units to Myriawatts or Myriawatt-hr.

1 B.t.u.	=	2.928×10^{-5}	Mw-hr.
1 Gram-calorie	=	1.1621×10^{-7}	Mw-hr.
1 Foot-pound	=	3.7662×10^{-8}	Mw-hr.
1 Kilogram-meter	=	2.7238×10^{-7}	Mw-hr.
1 Horse power	=	7.457×10^{-2}	Mw.
1 Cheval à Vapeur	=	7.354×10^{-2}	Mw.
1 Pferde-kraft	=	7.354×10^{-2}	Mw.
1 Poncelet	=	9.807×10^{-2}	Mw.
1 Kilowatt	=	1.00000×10^{-1}	Mw.
1 Boiler horse power	=	9.804×10^{-1}	Mw.

The last two are practically the same, differing by only two per cent. The usual practise is to rate water-tube boilers on the basis of one boiler horse power per 10 square feet of heating surface.

With modern plants, notably those in marine service, operating at from two to five times this rating, the ordinary method of determining nominal boiler capacity could be stretched 2 per cent without materially affecting the present rating: *i.e.*, the boiler might be rated at 34,150 B.t.u. per hour for each 10 square feet of heating surface, instead of at 33,479 B.t.u. per hour for each 10 square feet of heating surface.

The myriawatt as a unit of boiler or producer output, and correspondingly a unit of input to all kinds of dynamical machinery, is fixed in value by the watt, and by its very sound gives a clue to its meaning.

To compare efficiencies of direct-connected units and eliminate the various factors of quality of steam, pressure and vacuum, the term "B.t.u. per kilowatt-hour" has been used. If we use the term myriawatt,

$$\text{per cent overall efficiency} = \frac{10 \times \text{kilowatts output}}{\text{myriawatts input}}$$

Also, with the thermal efficiency of the engine known, the heating surface in the boiler room is determined (assuming the 10 square feet rule), two kilowatts per myriawatt input to an engine is equivalent to 20 per cent thermal efficiency of the engine and the heating surface in the boiler room equals kilowatts engine output $\times$ 10/2.

Obtaining an exact figure of the same with the boiler horse power unit involves a tedious operation.

The efficiency of internal combustion engine-driven units of all cycles, such as Diesel, Brayton, Otto, etc., is determined by rating the heating value of the fuel in myriawatts; thus,

$$\text{per cent efficiency} = 10 \times \frac{\text{kilowatts output}}{\text{myriawatts input}}$$

With hydraulic machinery, again rating the water power input to the wheels in myriawatts:

$$\text{per cent efficiency} = 10 \times \frac{\text{kilowatts output}}{\text{myriawatts input}}$$

Thus, in the term myriawatt lies a simple, logical and universal means of comparing outputs and inputs of all classes of energy converters, the meaning of which will be clear to all engineers wherever a piece of electrical machinery is to be found.

COMPARISONS IN EFFICIENCIES AND RATES OF OUTPUT WITH VARIOUS TYPES OF ENERGY CONVERTERS, IN TERMS OF THE MYRIAWATT

1. Boiler output:

Nominal rating = 600 boiler h.p.

Total draft head inches water gage	Boiler h.p.	Myriawatts
1.730	1375	1348

2. 5000-kw. engine:

Kw. output	Lb. steam / kw-hr.	B. t. u. / kw-hr.	Kw. output / Mw. input	Per cent thermal efficiency over-all
4977	17.2	20,160	1.70	17

3. 5500 kw. high-pressure turbine:

Kw. output	Lb. steam / Kw-hr.	B. t. u. / Kw-hr.	Kw. output / Mw. input	Per cent thermal efficiency over-all
8183	16.39	18,450	1.85	18.5

4. 15,000-kw. engine—low-pressure turbine.

Kw. output	Lb. steam / Kw-hr.	B. t. u. / Kw-hr.	Kw. output / Mw. input	Per cent thermal efficiency over-all
11,240	13.19	15,660	2.18	21.8

5. 56-in. low head water-turbine:

Brake h.p.	Brake kw.	Kw. output / Mw. input	Conversion eff. per cent over-all
283	211	8.41	84.1

6. Steam plant efficiency:

Lb. coal per kw-hr.	=	2
B.t.u. per lb. coal	=	14,250
B.t.u. per kw-hr.	=	28,500
Kilowatts per myriawatt input to boilers	=	1.2
Plant efficiency	=	12 per cent.

7. Gas power plant efficiency:

Kw. output	Cu. ft. gas / Kw-hr.	B. t. u. / Kw-hr.	Kw. output / Mw. input.	Per cent thermal efficiency overall
5200	145	14,220	2.4	24

DISCUSSION ON "THE MYRIAWATT" (STOTT AND O'NEILL), NEW YORK, FEBRUARY 27, 1913.

H. G. Stott: A great deal of criticism has been made of this paper, which was presented at the convention held in Boston last June. The greater part of this criticism, I think, is entirely due to a misconception. The misconception is that we are endeavoring to introduce a new unit. A little careful consideration of the paper would show that that conception is entirely wrong. All that was attempted to be done was to get in the thin end of the wedge, as it were, of the metric system into our measurements of mechanical power. We all know how we suffer at the present time from the numerous illogical and irrational units which are used in calculations on thermodynamics and mechanical units of power. For example, we have the boiler h.p., which was originally meant to mean that the boiler could evaporate 30 lb. of water from a temperature of 100 deg. to steam at 70 lb. Now latterly it has been modified to mean the amount of heat required to evaporate 34.5 lb. of water at a temperature of 212 deg. fahr. This is purely arbitrary, and has no scientific or rational basis.

There is still another boiler h.p., and that is one which simply means it is equal to 10 sq. ft. of heating surface in the boiler. The h.p. is sometimes used in the sense of being shaft h.p., sometimes brake h.p., and also for the indicated h.p. of an engine.

There are at least half a dozen different values attached to this term of horse power, and it seems to me that it is a happy coincidence that 10 kw. are just about equal to one boiler h.p., that is, within two per cent of it. The line of least resistance is always the best one to adopt in trying to introduce the metric system to those who have been accustomed to the abominable English system of units, and that is the basis of our attempt.

There has been a great deal of criticism directed against the unit which we propose. The first one is, why not use the kw. at once? The trouble with that is that you immediately have to introduce the factor of 10; say that 10 kw. are equal to 1 boiler h.p., which is approximately true. Then there is the difficulty in nomenclature, in referring to the fact that you require 10 kw. to deliver one kw. at the switchboard. Now, it seemed to us to be a little simpler to use the prefix "myria", meaning 10,000. The precedent for that is that in the metric tables we have the millimeter, the centimeter, the decimeter, the meter, the dekameter, the hectometer, the kilometer, and the myriameter.

Now, the same thing is true with the gram. We find there the myriagram also. So that there is absolutely nothing new about it, and we simply apply the well-known prefix of "myria" to the watt. The kilowatt is not the unit, the watt is the unit. We have not introduced a new unit, but have used a prefix well known in Continental practise, the prefix "myria" to indicate

10,000 times. Now, the whole object of this little paper, the idea of introducing the myriawatt, as I have said before, was to begin gradually, from the easiest point of attack, the introduction of the metric system into mechanical and thermodynamic calculations. The introduction of such a unit, it seems to me, will help a great deal, because naturally the next step, for example, in calculations on steam, will be the use of the centigrade thermometer scale. We are all familiar with that in electrical work. With the fahrenheit scale in calculations on thermodynamics, you are continually troubled with the plus or minus 32 deg. and it is a constant source of error.

With the centigrade scale, that is eliminated, so that the next step I hope to see introduced is the abandonment of the fahrenheit scale and the introduction of the centigrade scale for all thermodynamic work involving calculations. These things are all time-savers, just as the entire metric system is a time-saver over the old English system of units.

There is another reason for the introduction of this term. It has heretofore been the custom to specify the performance of a steam unit in pounds of steam. That was a perfectly legitimate and a perfectly safe way of expressing it, as long as there was no superheat, and as long as you worked to constant vacuum, for the vacuum varied but slightly from twenty-six inches as the standard. But nowadays, when we are going to 28½, 28¾, 29 and 29½ inches actual vacuum in guarantees, when referred to 30 in. barometer, and when the superheat may vary from 100 deg. to 200 deg., the pounds of steam per kw-hour mean nothing; you must get down to thermal units. The transformation from the thermal basis of the B.t.u., or the calories, is very simple, and is given in that little paper, so that we would naturally expect that, in expressing it in myriawatts, the conversion into the thermal units is implied, because that is a very simple matter by the use of a steam table.

The question of getting steam tables which are required in the metric system, is also now being taken up, and the authors of the tables best known today, have agreed that if the metric system of units in thermodynamics is adopted they will have their tables translated into the metric system.

Electrical engineering and mechanical engineering are now so closely allied and actually connected by the steam turbine that it is impossible to determine the efficiency of one without the other. There is no way I know of, of separating the losses in the turbine and in the generator. That comes in as an appropriate part of this matter, so that the electrical engineer is as much interested in this subject, in my opinion, as the mechanical engineer, and therefore I think if we do all we can to assist in the adoption of the metric system in all tests and guarantees, the rest of it will come very quickly. It is a very simple matter, all we have to do is to introduce the centigrade thermometer, and then the only quantity we have to translate will be pounds of water into kilograms. Then we will have our tests, reports and guarantees on precisely the same basis as they have in Europe; in other words, we will have our results in international units. At the present time, if we take up the results of tests on the steam turbine units which are practically the only sources of power in large stations now, we find that it is impossible, at first sight, to compare the European results with our own results. We have to sit down and go over a series of laborious calculations.

Now, if we become accustomed to the calorie, kilogram and the centigrade scale, which all electrical and chemical engineers are using, it would be a very simple thing to work out all our tests and all our guarantees in the metric system of units.

C. P. Steinmetz: If you want to realize the importance of getting to an intelligible system of nomenclature, you only need to consider a sentence like this: To operate ten 300-ton trains per day, at 40 miles per hour, with an acceleration of 0.5 mile per hour per second, two 2000-kw. turbo-alternators are installed. The periodicity of these generators is 25 cycles per second; they are designed for a temperature rise of 40 deg. cent. Their speed is 1500 rev. per min. and they are cooled by the circulation through each of them of 10,000 cubic feet of air per minute. This circulation is maintained by a pressure of 1.5 inches. Their consumption is 11 lb. of steam per kw-hr. at 175 lb. boiler pressure and 75 deg. fahr. superheat, and a 28-in. vacuum. Steam is supplied by six 300-h.p. boilers, consuming 2 lb. of coal per boiler h.p. The coal has a thermal value of 14,000 B.t.u. per pound. 1000 gallons of condensing water per minute are required at full load.

Now, pick out the number of heterogeneous, incompatible and erratic units in that brief statement, and then imagine how any sensible engineer can really maintain that such a system of units does not constitute a terrible and foolish handicap to progress in engineering.

Comfort A. Adams: It seems to me a crime, that men who call themselves engineers, who talk much about efficiency in the machines they develop, will continue to encourage the perfectly inhuman and wasteful system of units so aptly described by Dr. Steinmetz, when its use involves a loss in efficiency on the part of every one of us, of every man who has anything to do with engineering, which is almost incredible. I believe it is a conservative statement that the average engineer wastes a working year of his life by the use of our messy system of units. Frequently a student of limited capacity will fail to grasp the real physical significance of a problem, because of the confusion of units.

The myriawatt is only a step, but in the right direction. We are fortunate in having such a good representative as Mr. Stott in the American Society of Mechanical Engineers. He should certainly have the vigorous support of every electrical engineer.

Leo Schuler: I have learned with great pleasure that the European engineers are working at about 2.5 per cent greater efficiency than you do, and I do not think that I need say anything additional to what has been said in favor of the introduction of the metric system in engineering work in the United States. Nevertheless, I would not propose that you begin this reformation by the introduction of another new unit, the myriawatt, as proposed. The kilowatt is such a well-known unit already, even here in America, that I think confusion would be increased by another word, the myriawatt; I must also say that Mr. Stott is mistaken if he thinks that the prefix " myria " is very well known in Europe. As a matter of fact, it is not. Nobody uses it as a prefix, neither for myriameter or anything else. I think it would be better to say 10 kilowatts instead of one myriawatt.

In regard to the introduction of the kilowatt as a mechanical unit, that is making good headway and the Society which I represent here at this meeting, the Verband Deutscher Elektrotechniker, has already decided to rate electric motors in kilowatts, beginning January 1, 1914. It was arranged to give the manufacturers about two years' time for changing their name-plates and types. They could not rate a one-h.p. motor at 0.746 kilowatts but they must have it in round figures. There has been practically no difficulty in introducing the kilowatt by the Verein Deutscher Ingenieure, and the proposed new expression, the "neupferd", or in English, the " new horse," has been proposed, not by the mechanical engineers but by the electrical engineers, to facilitate its introduction by the mechanical engineers; the mechanical engineers, however, say that they do not want it, that they can understand " kilowatt " well enough.

H. M. Hobart: It has been my experience that here in America the 2000-lb. ton is the chief stumbling block to the introduction of the metric system. The other stumbling blocks to which Mr. Stott and Dr. Steinmetz have alluded are serious, but the 2000-lb. ton is the chief difficulty. There is not much good to be accomplished in getting people to change pounds into kilograms, if you are going to have the irrational relation between the kilogram and the ton, yet you will rarely find an American engineer who will countenance any notion that there can be anything better than the American ton. In this matter of the 2000-lb. ton, America stands alone. The 2000-lb. ton is never used by engineers in the British Empire. If in England it is said that the weight of anything is 100 tons it is taken as a matter of course to mean 100 tons of 2240 pounds. The English ton is 2240 pounds. The metric ton is equal to 2204 pounds, but the difference of only about one per cent is so small that it would not affect one engineering calculation in a thousand. If you can accustom people to speak in tons, you have a common bond between the metric system and the English system, and the way is smoothed for introducing the kilogram, which for all practical purposes is the one-thousandth part of the English ton. This is the connecting link which is not available in America, and in my opinion it is to a considerable extent for this reason that the metric system is used far more extensively in England than in America. America is away behind in the introduction of the metric system. Some six or eight years ago I worked out a set of steam tables, at a tremendous expenditure of labor, in which I gave the energy in kw-hr. per ton of steam for all temperatures and pressures, and with vertical columns for various degrees of superheat —no superheat, 50 deg. superheat, 100 deg. superheat, and so on, and I published these tables in a book entitled " Heavy Electrical Engineering." I do not believe these are yet used by anyone except myself. I am pleased to hear the proposal by Mr. Stott that tables of this kind should be employed. They are already available in the book to which I have alluded.

Charles P. Steinmetz: I entirely agree with Professor Adams that it is a crime for mechanical engineers to hold on to a mis-system of units, but I know of a greater crime still, and that is that men who do not have to deal with factory foremen or boiler testers or other people of limited horizons of intelligence, incapable of understanding anything new,—that men who are working, not for today, but for future generations, that is, very many educators throughout our educational institutions, our colleges and universities, in the electrical engineering departments, teach the English system with its inches, pounds, and gallons, its British thermal units and its fahrenheit degrees, first, because they desire to pose as practical men and secondly because they are too inveterately lazy to do anything but drift.

Now, there is where we can do more good than anywhere else, by *really giving the instruction* in the metric system. It would materially increase the efficiency of the work of these sound engineers to impart the metric system to them. You cannot transfer quickly from anything to anything else, say from an electrical phenomenon to a thermal phenomenon, without running the chance of being hopelessly mixed up, by the use of the English system, and you need the metric system to assist you.

I believe what ought to be done is that universities and colleges should teach, not simply and purely the metric system, but they should very thoroughly teach the method of reduction from the English to the metric, and from the metric to the English system, and educate the new generation to do all calculating work in the metric system, and to transform given data, which are in the English system, into the metric system, make calculations in the metric system, and retransform the results back from the metric system into the English system. This gives a much higher degree of reliability to the calculation, because it eliminates the enormous possibility of making a mistake in the irregular, irrational reduction factors, in using thermal units, and foot-pounds and kilowatts, and boiler horse power, and any one of the twenty-five different units for the same quantity.

I was very badly mixed up in my last attempt at using the English system, and I found it necessary to transform all the data procured from the English system, into the metric system, do all the calculating metrically, and transform the results back. It is much more efficient to do this.

After all, what we want to do in educating young engineers is to educate them to do the work efficiently. The most efficient way, naturally, is by the exclusive use of the metric system, but as long as the practical men are always lagging one or two generations behind the world, and they are still using the English system, the next efficient way would be to do all the work in the metric system and transform from the English to the metric, and from the metric back to the English, in using the terms and giving the results.

B. G. Lamme: I am fully in accord with any move to rate motors in kilowatts, instead of horse power. There is one fortunate thing, with our present mixed system, which will help us in making this change. We have been rating apparatus largely in halves, quarters and eighths, instead of in decimals of horse power, and the relation between the kilowatt and horse power is practically three-fourths, so that in a great many cases, in changing from horse power rating to the kilowatt, we do not obtain any particularly odd ratings. For instance, 50 h.p. would be changed to 37.5 kw., which is at present in common use in generating apparatus. One h.p. would mean 0.75 kw. We would therefore be able to change to the kilowatt rating with very little confusion.

James Burke: I think there is considerable advantage, sometimes, in having both systems—it sometimes gives us a chance to think what we are going to say in answer to a question, while we are apparently taking time to convert from one system to the other. There is also a certain romantic influence in maintaining some of the old units. I come from a part of the country where we still talk of " two bits " instead of twenty-five cents.

I would ask Mr. Schuler if it is not true that in Germany they still use the 60 seconds for a minute and 60 minutes for an hour. In this country the decimal hour has come largely into use in manufacturing, and time records in manufacture are kept in decimal parts of the hour. I would also like to ask Mr. Schuler if it is not true that in Germany the English system of threads on bolts, nuts and screws is still the commercial system, rather than the metric system.

Leo Schuler: Of course, Mr. Burke knows that it is so, and he need not ask.

I would say, in reply to what Mr. Lamme said, that the time lost is not only in making the transformation, but you have to transform from one kind of unit to another kind of unit in your calculations. For instance, when you wish to calculate the energy stored in a moving mass, if the mass is given in kilograms and the speed in meters per second, then the fraction $\frac{m v^2}{2}$ equals watt-seconds.

A. E. Kennelly: How far are they using the metric system in screw threads in Germany?

Leo Schuler: Metric screws are used for small apparatus only. For machinery screws, generally the Whitworth system is used.

L. W. Chubb: In expressing any physical quantity it is better to follow the most common custom. In speaking of power it is customary to speak of watts when below 1000 and kilowatts above this figure. A 20-megawatt generator was mentioned yesterday, but such an expression of rating is uncommon.

The most familiar and desirable prefixes are limited to every third digit and every third decimal place. "Kilo," "mega", "milli" and "micro" are common and desirable. Deka, hecto, myria, deci, centi, are uncommon, except in the case of centimeter, which in reality is the working unit, and one of the three fundamental metric units.

What is wanted is standardization and not deception. The myriawatt seems to have no advantage except to deceive the uninitiated regarding the efficiency of the mechanical end.

Charles P. Steinmetz: I do not agree as to the difficulty of introducing the myriawatt. I think it would be difficult to introduce the kilowatt in this connection, for two reasons; first, it means a rise of 10 points, that is, where the practical man would speak of a 100-h.p. boiler, it would be necessary to speak of a 1000-kw. boiler, and that is beyond the mental capacity of the users of the English system. We must consider that we are not all engineers, but that the majority of the people who are using the English system are working men, and factory foremen, etc., and that must be considered, and it would be a hardship to have everything increased in numerical value by ten-fold. Under the plan which we are now considering, in order to supply steam to a 100-kw. generator, a 1000-kw. boiler would be required, and that would be a difficult designation to bring about amongst the less-educated people. Moreover, the educated mechanical engineers would also rather resent the adoption of a nomenclature which would disclose the low efficiency of mechanical and thermodynamic transformations.

After all, we have to realize that the persistent adherence to the English system by mechanical engineers is not altogether conservative, but there is also a rather mixed feeling, not to say consciousness, the feeling that if they go to a system of units based upon the metric system it does not look well. It does not look bad to say you use so many pounds of coal per B.t.u., and that there are so many B.t.u's. in a certain number of kilowatt-hours, but it does look bad to say that you use 15 joules of coal to produce 10 joules of steam energy, and that 10 joules of steam energy are required to supply 1 joule of electrical energy. The metric system shows up the efficiency of transformation.

We have nothing to be ashamed of as electrical engineers, but the mechanical engineers are faced by the inherently low efficiency of mechanical and thermodynamic transformations, and they do not care to flaunt these low efficiencies too much before the public.

A paper presented at the Midwinter Convention of the American Institute of Electrical Engineers, New York, February 27, 1913.

INDUCTION MOTOR LOAD LOSSES

BY HENRY G. REIST AND A. E. AVERRETT

The question of load losses on induction motors frequently comes up when the various methods of testing are discussed.

It is usually more desirable to measure the losses and thus obtain indirectly the efficiency, than to obtain it from a direct input and output test, because a variation of two or three per cent in the losses will affect the efficiency results only slightly, while on direct measurement the error would be large.

Commercial circuits are, as a rule, more or less unsteady, both voltage and frequency changing, dependent upon the load; as both efficiency and power factor largely depend on the constancy of conditions, it is desirable to eliminate methods which require laboratory conditions to insure accuracy.

On large machines it is almost impossible to take direct measurements, on account of limited power. Therefore, some method of determining the efficiency by losses is imperative.

The losses usually measured are core losses, friction and windage, primary and secondary I^2R losses. See A. I. E. E. Standardization Rules, sections 162-167. Core loss is usually nearly constant, but under certain conditions may increase with the load and materially reduce the efficiency.

The customary method of obtaining the core losses is to run the machine without belt at gradually reduced voltage until it breaks down, reading both watts and amperes; curves of these values are plotted, and the point where the watt curve extended passes through the ordinate at zero voltage is taken as friction.

The I^2R at normal voltage is subtracted and the remainder is taken as core loss.

Primary I^2R is directly obtainable; the secondary, when of a definite phase-wound type, is also directly secured, but when a squirrel cage type, it must be obtained by indirect methods, and is approximately proportional to the slip.

Load Losses

Under certain conditions the core loss and I^2R are materially increased.

When saturation occurs in the iron, due to the load current, the core loss may be increased on account of a changed flux distribution. This is especially noticeable in motors with completely closed slots, that is, with a thin iron bridge across what would normally be the slot opening.

Such motors do not have a constant reactance or core loss, but both change with load, due to the saturation in the thin iron parts—the core loss increasing and the reactance decreasing. Under these conditions the only correct method of obtaining the efficiency is by an input-output test. The amperes at short circuit, with the rotor blocked, increase more rapidly than the voltage, whereas a motor without saturation will show amperes proportional to the voltage.

Nearly all motors, however, will show some saturation in the blocked condition when several times full load current passes, and a constant core loss should be assumed only through the range of short-circuited current where the amperes are proportional to the voltage.

Copper Losses

The copper losses are also subject to correction. Eddy currents will appear wherever heavy conductors are used. These eddy losses are shown by the wattmeter readings taken with the rotor blocked. Those chargeable to the stator are additional to the primary I^2R from measured resistance; those belonging to the rotor or secondary practically disappear under load conditions, due to the low frequency of slip.

In order to separate these, the rotor can be removed and a stator test made with amperes and watts; if there are no eddies the I^2R and measured watts will agree, but if not, the excess watts will represent additional losses which appear under load, and can be represented by an effective resistance.

Tests have been made which verify the above statements; round wires or rectangular strips of one cm. or less for a maximum dimension, apparently do not show any appreciable loss at

60 ~ or less; this maximum dimension, however, should be further studied, as sufficient tests have not been made to give the maximum limit. Certain deep bar-wound stators, however, have shown losses up to five or six times the losses due to I^2R alone.

Deep bar secondaries show a large eddy loss at standstill, which practically disappears at slip frequency, but the reduction in eddies at slip is partially made up by an increase in reactance and, therefore, a reduction in power factor; there seems no easy way at present to determine this effect, except by an input-output test.

Core Losses

Tests were made on a machine with completely closed stator slots, having a web 0.03 in. (0.76 mm.) thick, and repeated on the same stator having a 1/16-in. (1.58 mm.) and also a ⅛-in. (3.17 mm.) opening, the same rotor being used in each case, it having 1/16-in. (1.58 mm.) opening. The primary had 72 slots and the secondary 47 slots. The following are the results:

Completely closed			1/16-in. opening		1/8-in. opening	
Voltage	Watts	Amperes	Watts	Amperes	Watts	Amperes
110	52	3.62	51	3.9	54	4
220 (normal)	224	6.55	202	7.2	208	7 6
300	390	9.25	328	9.85	350	10.5

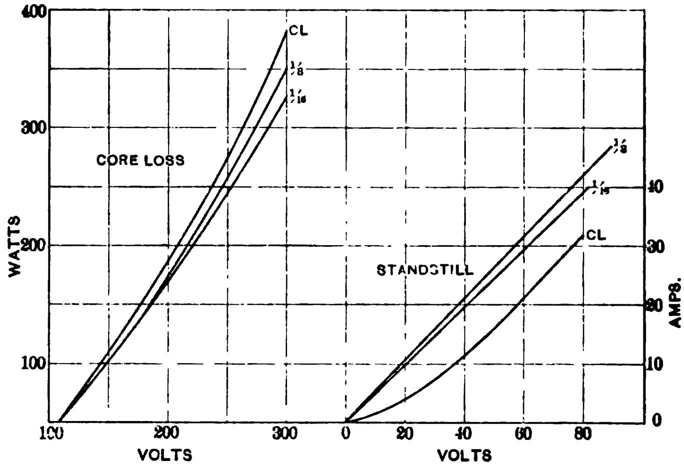
As there were eight conductors, it will be noticed that the ampere conductors per slot are low.

The full-load current is 25 amperes so that there will be considerable saturation and tufting of flux at load current, and, therefore, increased core loss. The following standstill voltages and currents show the effect of saturation.

Volts	Closed slots Amperes	1/16-in. opening Amperes	1/8-in. opening Amperes
20	4	9.5	10.5
30	6¼	14.5	15.8
40	11.6	19.5	21.0
60	22	29.5	31.2
80	32	39	41.5

The curves herewith show more clearly the saturation effect of the completely closed slots.

A method of testing induction motors largely used in Europe measures the input by wattmeters; the core loss, friction, and primary I^2R are subtracted from the input, and the result is called the primary output. This is multiplied by the per cent of synchronous speed (1 per cent slip) and called the secondary output. While not strictly correct it is as close as is commercially practicable, except in cases of bar-wound stators or non-proportional impedance curves.



The losses method does not show correct results when the standstill ampere curve is bent as shown above.

Results of Tests: Load Tests

A number of tests on wire-wound machines with partly open and straight slots have been made, checking efficiency and power factor by input-output method with the method of losses (A. I. E. E. method.) These cover machines from fractional to 150 h.p. From 29 tests ten showed lowest efficiency by loss method, and the remaining 19 showed lowest by brake test. The average is about three-fourths of one per cent lower by brake than by losses.

Two bar-wound stator machines showed 1.4 per cent and 3 per cent, respectively, lower by load test than by losses method. Unfortunately, tests were not made on the stator alone to determine eddy losses. As these were tested commercially no great accuracy should be expected, but they show a general agreement of the two methods.

The following are the results from a certain induction motor which has completely closed slots in both stator and rotor: the efficiency from method of losses was 0.881, while from an input-output test 0.774 was obtained; the power factor by losses method was 0.901, and from input-output tests 0.903.

This result is rather startling, but the tests have been made carefully, and while not conclusive, show that the question of load losses on completely closed slot motors may become serious. As both stator and rotor are wire-wound with small wire, and the I^2R total checks very closely with the watts at standstill, it would seem the extra losses are due to saturation of the

Horse power	Rev. per min.	Efficiency by losses method	Efficiency by brake method	Difference per cent
10	1800	85.85	84.2	1.65
7½	1500	81.2	79.7	1.5
5	900	85.7	85.2	0.5
3	1200	80.4	80.5	—0.1
2	1000	80.2	79.3	0.9
1	1200	80.9	82.6	—1.5
1.5	1000	75.5	74.85	0.65
½	1200	70.1	72.2	—2.1
1	800	56.2	53.5	2.7
1/5	900	36.2	34.8	1.4
½	1200	68	70.3	—2.3
1	450	51.6	50.2	1.4
¾	600	46.3	43.8	2.5
5	1800	85.9	84.6	1.3
7 ½	1800	85.5	87.4	—1.9
2	1800	82.6	84.2	—1.6
10	1800	89.8	90.3	—0.5
9	1200	84.8	86.5	—1.7
2	1800	86.6	86.3	0.3
20	1800	87.2	89.4	—2.2
1	1800	81.2	78	3.2
7 ½	1800	87	86.9	0.1
150	600	93.2	91.8	1.4 bar-wound
36	360	88.1	85.1	3 " "
50	600	89.4	89	0.4
20	800	88.2	88.6	0.4
30	800	90.5	91	—0.5
10	1200	87.5	86	1.5
10	1200	85.6	87	1.4

teeth. The core loss running light is 6 per cent of the rated output.

A further test has been made on a motor with partly closed primary slots and completely closed rotor slots, the thickness of the web being one mm.: the core loss running light was 2.3 per cent (approximately ½ of what would be obtained if one member were of the usual straight slot construction).

At one-half load the difference in efficiency between the loss method and input-output was 1.7 per cent, at three-quarter load 2.2 per cent, at full load 2.8 per cent, at one-half load 3.4 per cent. The corresponding differences in power factor were 1 per cent, ½ per cent, 0.2 per cent, and 0.2 per cent.

The load test was taken by a friction brake and is the average of a number of readings, it being necessary to take a number on account of scattering points.

The motor was designed to have a large core loss and small copper loss, therefore, the load losses, due to saturation of the thin overhang, are not as large as would be the case in a motor of more ampere-turns per slot, such as a standard type.

The results, however, show a decided load loss, increasing approximately proportionally to the load.

The results of these two load tests are in agreement, and seem to indicate that load losses of considerable magnitude may be expected when the iron paths are saturated, due to load current.

In connection with the above, to show the effect of tufted flux, certain tests were made on machines with and without magnetic wedges; the magnetic wedge approaches the condition of a nearly closed slot. Where the wedge was properly made and insulated the core loss was one-half to one-third of the corresponding straight slot; with a poorly insulated wedge, however, the losses increased from two to three times, showing that magnetic wedges require very careful design and handling. Due to the structure of the wedges more or less of an air path was included by the wedges and the short-circuited current was directly proportional to the voltage.

The effect of filing and turning on the cores is shown as an excess core loss during the excitation or running light test; it is constant for a given machine, but is largely the cause of the different core loss measurements on duplicate machines.

Designing engineers are aware of the above conditions and the general practise at present is to build motors with an air path somewhere in the magnetic circuit of each slot.

Present experience seems to indicate that where there is no saturation or bend in the standstill or impedance curve, there are no appreciable load losses; where wire winding is used no allowances are necessary for copper losses; where bar-wound machines are used it is necessary to test the stator without the rotor to determine the stator copper losses; bar-wound rotors show a greater loss and a lower inductance at standstill than when running. Half or quarter frequency tests will largely determine the eddy losses which modify secondary slip and reactance. The slip is a direct measure of the secondary loss.

A paper presented at the Midwinter Convention of the American Institute of Electrical Engineers, New York, February 27, 1913.

STRAY LOSSES IN INDUCTION MOTORS

BY A. M. DUDLEY

Section 167 of the Standardization Rules, referring to induction motors, states, "These losses (load losses) may for practical purposes be determined by measuring the total power, with the rotor short-circuited at standstill and a current in the primary circuit equal to the primary energy current at full load. The loss in the motor under these conditions may be assumed to be equal to the load losses plus I^2R losses in both primary and secondary coils."

It is the purpose of this paper to point out that the losses as measured according to this section in many cases include losses which are not present when the motor is operating at normal full-load speed and which cannot, therefore, justly be called load losses and charged against the efficiency of the motor as calculated by the " summation of losses " method.

It is the further purpose to point out a method by which the separate losses in a motor, including the load losses, may be segregated from no-load readings with reasonable assurance that the motor efficiency as figured therefrom will be as close to the efficiency measured by the input-output readings when the motor is running under load as is the limit of error necessarily encountered in taking these readings, even with laboratory methods.

The reason for presenting the problem as it is stated in the last paragraph appears from a consideration of the practical difficulties encountered in testing units of such a size as to make it difficult to secure facilities for running an actual load test, either on the premises of the manufacturer or after installation, in regular service.

The limitations of the manufacturer in this case are usually

(a) lack of mechanical apparatus for connecting the motor to a suitable load, (b) lack of a suitable load, either mechanical or in the nature of an electrical generator, and (c) insufficient power to operate the unit when developing full load.

The limitations of the ultimate user which act against a successful test after installation are (a) inability to control the load within reasonable variations while observations are being taken; (b) lack of proper facilities in the way of precision instruments and other appliances for suitably conducting such tests, and (c) lack of a sufficient number of properly trained observers for taking the readings.

Added to the foregoing disadvantages is the further one, pointed out in Mr. Olin's paper in the 1912 TRANSACTIONS*, that errors in observations on load tests, or so-called input-output tests, reduce their reliability below that obtained by making a careful determination of the separate losses from no-load readings and determining the efficiency by the "summation of losses" method.

Recognizing these facts, it has become standard practise on induction motors of small and moderate capacities to compute efficiencies in the following manner: A reading is taken of the amperes and watts input to the motor at full voltage when running idle. From this is subtracted the I^2R losses due to the no-load current and the remainder is considered to be the friction and windage plus the "rotation loss" or so-called "core loss." There is a slight error in the last item due to the fact that the "core loss" should be taken at the induced rather than at the applied voltage, but in all but very small units this error is small. To the foregoing losses are added the primary copper loss at proper full-load current and temperature, and the secondary copper loss as shown by full-load "slip" from synchronous speed, measured by brake. These total losses are added to the output at the current chosen, and the result considered the input, and the efficiency computed therefrom.

So soon, however, as the unit becomes of a size where it is difficult or impossible to take the slip under actual load, a question immediately arises as to the amount of the secondary copper loss. When recourse is had to Section 167 of the Standardization Rules quoted above, it offers no means of segregating the load losses from the secondary copper loss after the primary copper loss has been subtracted from the watts input to the motor at standstill at normal voltage.

*TRANS. A.I.E.E., 1912, XXXI, Part II, p. 1695.

It is this uncertainty that makes necessary an analysis of the nature of these so-called load losses and the outlining, if possible, in the Standardization Rules of the Institute, of a method for segregating this loss from the other losses in the machine.

A consideration of the possible causes of such losses suggests two sources:

a. A distortion of the main field form due to armature reaction caused by working load currents, or

b. Eddy currents in the copper conductors due to the main field.

On account of the symmetry of the core and distribution of the windings it may safely be assumed that there is very little, if any, distortion of the field form, due to cause "a," and any loss so set up may be neglected.

There remain the eddy currents in the copper due to the main field.

It is here to be noted that with the rotor at standstill and a current of normal frequency flowing in the primary, the frequency of the secondary current is the same as that of the primary. At normal full-load speed, however, the frequency of the rotor is very low, being only the same percentage of the primary frequency that the slip is in percentage of synchronous speed. From this it follows at once that where the cross-section of the rotor conductors is such as to permit setting up of eddy currents there may be a very appreciable proportion of the watts at standstill due to eddy currents in the rotor copper which are not present when the rotor is running up to speed.

Added to this item is one of less consequence, due to iron loss in the secondary at standstill which is not present at full speed.

It is, therefore, proper to charge against the motor the eddy current losses in the primary copper but exclude those due to eddy currents in the secondary copper. To get at a method of approximating these losses more closely, a series of readings was taken on a motor with currents of varying frequency in the primary windings. These results are shown in Fig. 1. From these readings it appears that as the frequency approaches zero the watts input becomes very nearly that due to the copper losses in primary and secondary caused by usual resistances of the windings alone. There still remains the question of segregating the increased loss at normal frequency into eddy current loss in the primary and secondary so that the machine need not be charged with a loss occurring in the secondary at standstill which is not present at full speed.

There are, no doubt, several ways in which this could be accomplished, of which two may be mentioned here.

1. The increased losses over the ordinary I^2R losses as shown in Fig. 1 may be divided into primary eddy current loss and secondary eddy current loss, in proportion to the square of the depth of the conductor in the slot in each member. This is, in a way, somewhat empirical, but it would probably give as close results as can be arrived at by other methods, and it has the

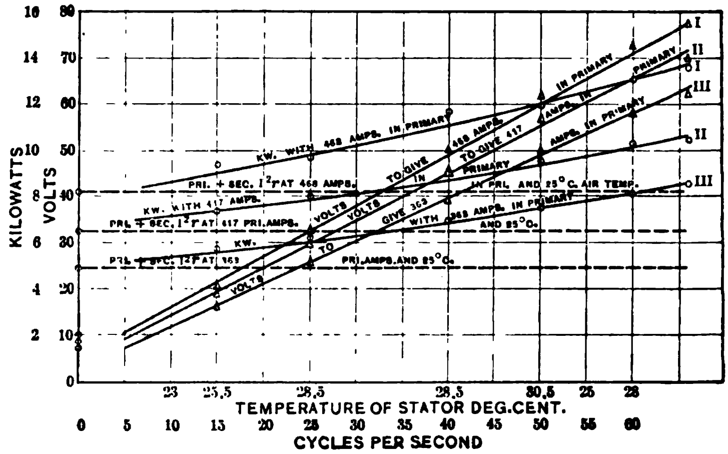


Fig. 1—Variation of Eddy Current Losses in Primary and Secondary Copper with Variation in Frequency

Induction motor, 450 h.p., three-phase, 60 cycles, 440 volts. 8 poles, 870 rev. per min. Rotor resistance at 25 deg. cent. 0.314 ω terminal to terminal.
Stator resistance at 25 deg. cent., 0.128 ω terminal to terminal
Readings taken with secondary short-circuited and locked. Amperes in primary are read per phase.

advantage of being simple, since the details of design are usually readily available when conducting tests.

2. The rotor, if of the phase-wound type, could be removed from the stator and sufficient voltage applied to its terminals to cause full-load current to flow in the windings. A measurement of the watts input under this condition as compared with the straight I^2R due to the resistance of the windings would indicate the amount of the eddy current loss in the secondary copper and this should be deducted from the total watts input to the machine at standstill. The remainder should be charged against the machine as the usual I^2R loss in the primary and secondary

plus the proper load loss due to eddy currents in the primary conductors caused by the main field.

At this point it might be brought out that the value of the secondary current at full load may be taken from the Heyland diagram or some modification thereof, or it may, for all practical purposes, be taken from the following formula: Secondary current per terminal at full load = horse power output in watts + secondary I^2R loss + bearing friction and windage $\div K \times$ volts between collector rings at standstill with normal voltage applied to the primary. $K = 1.73$ for three-phase rotor and $= 2$ for two-phase rotor.

In the case of a squirrel-cage motor the rotor could be removed from the stator and sufficient voltage applied to the terminals of the stator to cause full-load current to flow therein. A measurement of the watts input under this condition as compared with the straight I^2R loss would indicate the amount of eddy current loss in the primary copper. This amount, added to the ordinary primary and secondary copper loss determined by the method shown in Fig. 1, would indicate the proper copper loss to be charged against the machine.

The foregoing discussion has reference to units of fairly large capacity. A careful study of smaller machines, where the size of the primary conductors does not exceed a No. 10 round wire, shows that there are practically no load losses. It is not necessary to burden this paper with a long tabulated statement of the data investigated to prove this point, but a brief statement will cover the results of this investigation.

Sixty-nine motors were selected, entirely at random. They were of both the wound rotor and squirrel-cage types, open and closed slots, 60 and 25 cycles, two- and three-phase, voltages from 220 to 2200 and capacities from 3½ h.p. to 150 h.p. A comparison was made of the full-load efficiencies as determined by careful prony brake tests, with the efficiencies by summation of losses. In the efficiencies by losses, no item was entered for load losses. The only losses considered were primary and secondary I^2R losses, the rotational or core losses and the mechanical losses due to bearing friction and windage. It was assumed that if there were any load losses, the efficiency by brake test would be consistently lower than by losses. Such was not found to be the case. Out of 69 machines the efficiency by brake test was higher in 34 cases; the efficiency by losses was higher in 32 cases and the two were the same in three cases. From these data, and an experience over a period of several years covering a large number of machines, the conclusion is drawn that for machines in which the primary conductor does not exceed in size a No. 10 wire, B. & S. gage, it may be assumed that there is no load loss or that it is negligible in amount.

Based upon the foregoing argument, the recommendation is made that Section 167 be modified to cover two conditions:

1. That it be recognized that machines having primary conductors of small cross-section have no appreciable load loss, and that a summation of loss method based upon the usual I^2R losses measured by resistance and slip is sufficiently accurate for determining their efficiency.

2. That on machines having primary conductors of comparatively large cross-section and consequently some load loss in the primary copper, a method be approved for determining the efficiencies from no-load readings, substantially as described in this paper.

A paper presented at the Midwinter Convention of the American Institute of Electrical Engineers, New York, February 27, 1913.

NOTES ON INDUCTION MOTOR LOSSES

BY R. W. DAVIS

The object of this paper is not a comprehensive treatment of the subject, but a few notes on the data obtained from numerous tests on induction motors. It is hoped that this and the other papers on the same subject will lead to further discussion and be of assistance in bringing about a revision of the Standardization Rules for the determination of fixed and stray losses in induction motors.

A large number of tests on motors of various sizes for 25- and 60-cycle circuits were compared and the data tabulated. The data obtained check the losses as given by the Standardization Rules, with the exception of the fixed and stray losses.

The power input to the motor when running at rated voltage and without load is taken by the Institute rules as the fixed loss: *i.e.*, friction, windage, core and copper losses which may exist in multiple circuit windings. This gives too large a figure, since it includes the stator copper loss due to the magnetizing current. This copper loss is quite appreciable in low-speed motors. The true fixed loss should therefore be taken as the power input minus the stator copper loss produced by the magnetizing current.

The stray losses may be grouped under two general divisions: the losses in the stator, and those in the rotor. The writer is not aware of any satisfactory method of determining the stray losses in the stators of induction motors. An induction motor stator is very similar in construction to the stator of a definite pole alternator of corresponding rating, and tests on the latter apparently show that with properly built cores the stray loss in the magnetic circuits of definite pole alternators is negligible. The total measured loss checks very closely with the sum of the I^2r loss and the computed eddy current loss in the conductor.

Oscillograms showing the distribution of the flux in induction motor stators indicate that there is very little change in its distribution from no-load to full. It therefore seems to the writer that the stray losses in the magnetic materials of induction motor stators should be neglected. The data on stray loss in copper, given in the table, were computed by the same method which was found satisfactory for definite pole synchronous machines. These stray losses are expressed in per cent of the normal stator copper loss.

Horse power	Rev. per min. syn.	Volts	Cycles	Per cent stator stray loss	Full load slip		Locked losses		
					Test	Calc.	Stator	Rotor	Total stray
30	500	2200	25	*	3	2.4	41.2	23.5	35.3
40	750	440	25	5	4.2	3.5	44.3	38.2	17.5
75	720	440	60	1	3.3	3.5	48.1	33	18.9
100	500	2200	25	*	2.4	2.4	45	45	10
100	600	440	60	*	3.1	2.8	41.5	47.3	11.2
100	720	220	60	16	3.3	3.5	35.3	47	17.7
200	500	440	25	3	2.8	2.8	34.4	50	15.6
200	600	440	60	1	2.3	2.4	38	50	12
250	600	440	60	*	2.7	2.6	30.2	40.8	29
300	450	550	60	6	1.6	1.4	32.7	23.8	43.5
300	600	550	60	9	1.7	1.6	37.7	44.3	18
350	6C0	550	60	9	1.9	2	36	43.5	20.5
400	600	440	60	5	1.6	1.7	30.8	38.4	30.8
500	514	440	60	1	0.9	0.8	31	38	31
600	300	550	60	9	1.6	1.4	29	30.8	40.2
600	600	2200	60	2	1.3	1.2	29.2	35	35.8
800	750	6600	25	*	1.3	1.4	33	33	34
600	1200	2200	60	2	0.9	0.9	33.4	40.5	26.1

*Less than ½ per cent.

The stray losses in the rotor of an induction motor are a function of the rotor frequency and therefore proportional to speed. At standstill, full frequency is induced in the rotor, very greatly increasing the rotor iron loss and the eddy current loss in the conductor. The writer has taken advantage of this in designing squirrel-cage motors, obtaining good starting characteristics and low slip at full load with a resultant efficiency comparable with that of a wound-rotor motor. The present Institute rule for measuring stray losses includes these high losses at standstill as a part of the total loss in the motor at full load. The tests show that the slip at full load corresponds to the I^2r loss by resistance measurement and that the stray loss in the rotor at standstill is not present at full load in machines of small slip. The calculated slip given in the table is based upon the assumption that the only loss in the rotor is the copper I^2r loss determined from the resistance measurement. The test slip measured at full load is the average of a number of readings, and checks the calculated slip within the limit of experimental error. The copper and stray losses in the table are taken from the lock test and expressed in per cent of the total loss on short-circuit. The tests are on wound-rotor motors, with the exception of the last, which is on a squirrel-cage machine with the bars bolted and soldered to the end rings, removing all poor contacts between bars and end rings.

Many of the tests checked were incomplete in some respect. Only those tests which were complete have been included in the table, although the data of all tests, in so far as they were considered reliable, were used in arriving at conclusions. Neglecting any small stray loss which may exist in the magnetic materials of the stator, the stator stray loss is proportional to the stator copper loss, the percentage loss depending upon the degree of lamination of the conductor and the frequency. The average stray loss in both 25- and 60-cycle machines is about six per cent of the stator copper loss. In rotors of small slip there is no stray loss shown within the limits of experimental error, and in rotors of large slip the stray loss under full-load test is only a fraction of the rotor stray loss at standstill. The total rotor loss under full load is in every case less than the combined copper and stray losses obtained from the short-circuit test.

It therefore seems desirable that an average value of stator stray loss in per cent of stator copper loss be adopted as a standard. In squirrel-cage motors and small wound-rotor motors, where the slip is usually greater than two per cent, the total rotor loss should be determined from the slip reading when operating at full load. The rotor loss should be taken as the copper I^2r loss from resistance measurements, in large wound-rotor motors where the slip is less than two per cent and a full-load slip reading is not readily obtained.

A paper presented at the Midwinter Convention of the American Institute of Electrical Engineers, New York, February 27, 1913.

LOSSES IN TRANSFORMERS

BY W. W. LEWIS

The losses in transformers, as defined in the Standardization Rules of the American Institute of Electrical Engineers, Sections 157-159, are as follows:

157 MOLECULAR MAGNETIC FRICTION AND EDDY CURRENTS measured at open secondary circuit, rated frequency, and at rated voltage —$I\ r$, where I = rated current, r = resistance of primary circuit.

158 RESISTANCE LOSSES, the sum of the $I^2\ r$ losses in the primary and in the secondary windings of a transformer, or in the two sections of the coil in a compensator or auto-transformer, where I = rated current in the coil or section of coil, and r = resistance.

159 LOAD LOSSES, *i.e.*, eddy currents in the iron and especially in the copper conductors, caused by the current at rated load. For practical purposes they may be determined by short-circuiting the secondary of the transformer and impressing upon the primary a voltage sufficient to send rated load current through the transformer. The loss in the transformer under these conditions, measured by wattmeter, gives the load losses + $I^2\ r$ losses in both primary and secondary coils.

Transformer losses more naturally group themselves into two divisions: *no-load losses and load losses.*

The no-load losses are the losses that exist when potential is impressed on the primary of the transformer, with open secondary circuit, and include the loss in the iron and the loss in the insulation; or, *core and dielectric losses.*

The load losses are the losses caused by the load current, and include the loss due to the resistance of the copper and the loss due to the stray or leakage field; or, *resistance* and *stray losses.*

It is the purpose of this paper to give an idea of the magnitude of the stray and dielectric losses, and to recommend certain amendments to the above sections of the Standardization Rules. The term " load loss " will be used in the broad sense (including resistance and stray loss), and the term " stray loss " will be used when referring to the loss defined in Section 159.

TABLE I

STRAY LOSS

Frequency cycles	Rating kv-a.	Rated voltage		Impedance volts in per cent	I^2R watts	Impedance watts	Stray loss watts	Stray loss in per cent of I^2r	Stray loss in per cent of rated kv-a.
		High-tension	Low-tension						
				CORE TYPE TRANSFORMERS					
25	100	6,600	2,200	1.76	950	1041	91	9.58	0.091
"	"	11,000	460	1.81	1105	1140	35	3.16	0.035
"	"	15,000	375	1.89	1165	1190	25	2.14	0.025
* "	"	33,000	390	15.0	1565	1710	145	9.27	0.145
* "	"	22,000	740	15.7	1440	1496	56	3.89	0.056
* "	"	13,200	370	17	1475	1794	319	21.60	0.319
* 25	145	11,000	370	14.92	1828	2284	456	25	0.314
25	150	12,600	460	2.25	1865	1890	25	1.34	0.017
"	"	6,600	440	2.27	2137	2150	13	0.61	0.009
"	"	20,000	460	2.30	1490	1605	115	7.71	0.077
* 25	165	6,600	430	17	1711	2560	839	49.5	0.508
* "	"	6,600	430	17.5	1915	2445	530	27.7	0.322
* 25	185	23,500	480	15.6	2300	2910	610	26.5	0.330
25	200	22,000	440	2.13	2050	2160	110	5.37	0.055
"	"	11,000	2,200	2.25	1880	1960	80	4.25	0.040
"	"	6,600	2,300	2.44	1980	2100	120	6.06	0.060
25	250	11,000	2,300	1.46	2140	2180	40	1.87	0.016
"	"	13,200	480	2.39	2805	2870	65	2.32	0.026
25	300	11,000	2,300	2.19	2565	2663	98	3.82	0.033
25	350	12,000	2,400	7.81	2730	2950	220	8.06	0.063
"	"	11,000	430	11.7	2575	3560	985	38.2	0.282
25	400	11,000	2,200	2.63	2460	2655	195	7.94	0 049
60	100	12,480	2,300	1.92	1093	1107	14	1.28	0.014
"	"	6,600	460	2.58	1005	1060	55	5.46	0.055
"	"	10,500	2,300	2.59	797	822	25	3.14	0.025
"	"	6,600	2,400	4.23	800	1020	220	27.5	0.220
* "	"	11,000	370	15.1	832	1140	308	37.0	0.308
* "	"	13,200	370	15.3	945	1265	325	33.9	0.325
* "	"	15,000	370	16.9	893	1158	265	29.7	0.265
60	150	6,600	2,300	2.6	1100	1140	40	3.64	0.027
"	"	5,720	2,300	2.83	1214	1318	104	8.57	0.069
"	"	6,600	240	3.8	932	1400	468	50.2	0.312
* 60	165	13,200	430	16.1	1174	1880	706	60.1	0.428
60	200	6,600	2,200	1.9	1020	1160	140	13.72	0.070
"	"	6,600	2,400	1.92	1060	1118	58	5.47	0.029
"	"	9,350	2,080	2.18	1577	1705	128	8.13	0.064

TABLE I—*Continued.*

Frequency cycles	Rating kv-a.	Rated voltage High-tension	Rated voltage Low-tension	Impedance volts in per cent	I^2R watts	Impedance watts	Stray loss watts	Stray loss in per cent of I^2r	Stray loss in per cent of rated kv-a.
60	250	6,600	550	2.25	1435	1700	265	18.48	0.106
"	"	6,600	480	2.50	1338	1585	247	18.48	0.099
"	"	6,300	200	3.75	2210	2803	593	26.8	0.237
60	300	6,940	2,300	1.72	1836	1925	89	4.85	0.030
"	"	6,600	2,300	2.27	1626	1920	294	18.1	0.098
"	"	22,000	370	4.18	1790	2400	610	34.1	0.203
"	"	6,000	240	4.19	2150	2810	660	30.5	0.220
60	350	11,000	2,300	1.67	1792	1880	88	4.91	0.025
"	"	6,600	2,300	2.4	1665	2050	385	23.1	0.110
60	350	10,000	440	3.22	1865	2200	335	18.0	0.096
"	"	34,650	430	18.2	1885	3000	1115	59.2	0.319
60	400	6,800	2,200	2.19	2537	2740	203	8.02	0.051
"	"	22,000	2,500	2.89	2040	2265	225	11.02	0.056
"	"	6,300	200	3.35	2380	3040	660	27.7	0.165
60	500	10,000	3,450	2.03	1905	2330	425	22.3	0.085
"	"	6,600	2,300	2.20	2080	2700	620	29.8	0.124
"	"	10,000	2,300	3.14	3216	3471	255	7.94	0.051
SHELL TYPE TRANSFORMERS									
40	200	10,000	90	3.76	2350	2740	390	16.6	0.195
25	250	6,600	110	1.64	2310	2400	90	3.9	0.036
25	275	6,940	199	2.96	2905	3280	375	12.9	0.136
60	300	34,600	2,300	3.02	2270	2415	145	6.4	0.048
60	375	6,600	240	4.30	2600	2965	365	14.0	0.098
60	400	44,000	2,300	3.53	2250	2525	275	12.2	0.069
60	500	2,200	608	3.75	3240	4060	820	25.3	0.164
"	"	38,100	7,500	2.90	2960	3115	155	5.25	0.031
"	"	14,450	2,500	3.26	2570	3265	695	27	0.139
"	"	14,450	2,500	3.29	2565	3145	580	22.6	0.116
"	"	13,200	2,550	3.83	2240	2860	620	27.7	0.124
"	"	15,000	2,300	2.15	2800	3060	260	9.3	0.052
"	"	13,200	2,530	1.58	2090	2270	180	8.63	0.036
"	"	6,600	2,300	2.45	2860	3095	235	8.22	0.047
"	"	11,000	2,300	3.01	2525	3260	735	29.1	0.147
"	"	14,000	2,440	3.34	3055	3340	285	9.35	0.057
60	500	23,000	6,600	2.83	3570	4040	470	13.2	0.094
"	"	25,087	2,500	4.68	3420	3890	470	13.7	0.094
60	600	6,930	3,300	2.48	3740	3780	40	1.07	0.007
"	"	33,000	13,860	3.36	2800	3380	580	20.7	0.097

TABLE I—*Continued.*

Fre-quency cycles	Rat-ing kv-a.	Rated voltage		Imped-ance volts in per cent	I^2R watts	Imped-ance watts	Stray loss watts	Stray loss in per cent of I^2R	Stray loss in per cent of rated kv-a.
		High-tension	Low-tension						
40*	750	22,000	600	3.85	4900	5850	950	19.4	0.127
60	750	10,000	3,450	3.18	3965	4760	795	19.9	0.106
″	″	4,000	2,300	3.23	3380	4640	1260	37.4	0.168
″	″	6,600	2,300	4.14	4250	5570	1320	31.1	0.176
″	″	35,475	2,035	4.23	3100	3855	755	24.3	0.101
″	″	60,000	480	4.88	4325	5025	700	16.2	0.093
60	900	23,000	2,300	3.51	5140	6170	1030	20.0	0.113
25	1000	60,000	11,000	4.15	8460	9040	580	6.85	0.058
60	1000	22,000	2,300	2.13	5500	5980	480	8.74	0.048
″	″	21,000	2,500	2.90	5785	6565	780	13.5	0.078
″	″	110,000	22,000	5.00	7070	7300	230	3.25	0.023
″	″	110,000	22,000	5.00	7165	7330	165	2.58	0.017
60	1500	33,000	2,300	4.41	6570	8200	1630	24.8	0.109

* Transformers equipped with internal magnetic shunts.

NOTE.—Each item above represents an average of from one to six transformers.

I. STRAY LOSS

Magnitude of Stray Loss. Table I shows the value of the stray loss, in a number of commercial transformers selected at random. It will be seen that there is apparently no definite relation between impedance voltage and stray loss, or rated voltage and stray loss, although in general the loss increases with increase in impedance voltage, and also for a given size transformer, increases as the low-tension voltage decreases.

Table II gives the results of some impedance tests on an experimental core type transformer, rated 60 cycles, 165 kv-a., 11,000 volts high-tension and 430 volts low-tension. This transformer has four high-tension coils of 308 turns each (two coils per leg) and two low-tension coils of 24 turns each (one coil per leg). The high-tension conductor is 0.215 by 0.055 in. (5.46 by 1.4 mm.) copper, edge-wound; the low-tension conductor consists of ten strips of 0.5 by 0.095 in. (12.7 by 2.4 mm.) copper, arranged five wide and two high.

The first set of readings was taken under normal conditions at 60 cycles. Then some bundles of laminated steel (20 bundles per leg, each containing 18 sheets) were placed horizontally between high-tension and low-tension coils, to form a magnetic shunt, and the remaining three sets of readings were taken at 25, 40 and 60 cycles. The introduction of the shunt has the tendency to exaggerate the losses and therefore to give better readings. The results are plotted in the curves of Fig. 1.

This transformer requires 4 per cent of the normal voltage

TABLE II

IMPEDANCE TESTS—VARIATION WITH CURRENT AND FREQUENCY. TRANSFORMER 60 CYCLES, 165 KV-A., 11,000 VOLTS TO 430 VOLTS

Low-tension (inner) winding short-circuited. (See Fig. 1.)

Frequency cycles	Impedance volts	Per cent normal volts	Amperes	Per cent normal amperes	Impedance watts	I^2R watts	Stray loss in per cent I^2R	Remarks
60	209	1.9	7.51	50.1	317	294	7.83	
"	312	2.84	11.20	74.6	705	662.5	6.42	
"	423	3.85	14.9	99.4	1270	1177	7.8	
"	529	4.81	18.8	125.2	2050	1839	11.48	
"	633	5.75	22.5	150	2916	2648	10.08	
25	254	2.31	3.75	25	82	73.5	11.55	With magnetic shunts
"	526	4.79	7.55	50.4	387	294	31.6	
"	730	6.64	11.25	75	839	662.5	11.54	
"	876	7.97	15.1	100.8	1500	1177	28.3	
"	985	8.95	18.85	125.8	2190	1839	19.1	
"	1076	9.78	22.6	150.8	3040	2648	14.8	
40	427	3.88	3.75	25	109	73.5	48.3	With magnetic shunts
"	835	7.6	7.55	50.4	459	294	56.1	
"	1167	10.6	11.25	75	945	662.5	42.7	
"	1393	12.68	15.1	100.8	1680	1177	42.8	
"	1570	14.28	18.85	125.8	2505	1839	36.3	
"	1713	15.6	22.6	150.8	3405	2648	28.6	
60	580	5.28	3.75	25	—	73.5	—	With magnetic shunts
"	1256	11.4	7.55	50.4	561	294	90.8	
"	1750	15.9	11.25	75	1143	662.5	72.5	
"	2090	19.0	15.1	100.8	1880	1177	59.8	
"	2330	21.2	18.85	125.8	2650	1839	44.1	
"	2570	23.38	22.6	150.8	3725	2648	40 6	

at 60 cycles to force full-load current through the windings, and its impedance losses are 8 per cent in excess of the calculated $I^2 r$ loss. Equipped with magnetic shunt, the transformer requires 19 per cent of the normal voltage at 60 cycles to force full-load current through the windings. Its impedance losses are 60 per cent in excess of the calculated $I^2 r$ loss.

In this test, the high-tension (outer coils) were excited and the low-tension (inner coils) short-circuited. Considerable difference results if the low-tension is excited and the high-tension short-circuited. See Table III. This is due to the dif-

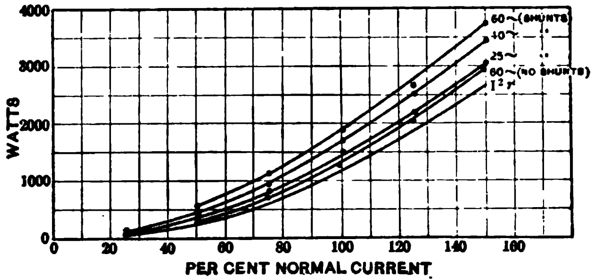


FIG. 1—IMPEDANCE TESTS, 165-KV-A. TRANSFORMER
See Table II.

ferent distribution of the flux in the two cases, and the stray loss is reduced to 50 per cent of the $I^2 r$ loss.

Tests were made on an experimental transformer rated 60 cycles, 8 kv-a., 800 volts primary and 800 volts secondary. Each winding was composed of two coils, one on each leg, of

TABLE III

IMPEDANCE TEST.—TRANSFORMER 60 CYCLES, 165 KV-A., 11,000 VOLTS TO 430 VOLTS

High-tension (outer) winding short-circuited

Frequency cycles	Impedance volts	Per cent normal volts	Amperes	Per cent normal amperes	Impedance watts	I^2R watts	Stray loss in per cent of I^2R	Remarks
60	47.8	11.12	192	50	490	294	66.6	With magnetic shunt
"	66.0	15.35	289	75.3	1080	662.5	63	
"	79.5	18.5	386	100.5	1770	1177	50.4	
"	86.0	20.0	443	115.2	2250	1560	44.3	
"	89.5	20.8	474	123.5	2460	1839	33 8	

160 turns of 0.05 by 0.2 in. (1.27 by 5.08 mm.) copper conductor. The outside coils were excited and the inner coils short-circuited. The tests recorded in Table IV and plotted in Fig. 2 were made, first, with no core; second, with bundles of steel placed vertically between the primary and secondary (two

bundles of 12 sheets each per leg); third, with the laminated steel core in place and no shunts; fourth, with steel core, and magnetic shunts in place between primary and secondary. From these readings, we can separate in a measure the loss in the copper and the core, but only approximately, as the flux distribution is not the same in any two cases.

When a transformer is loaded by the impedance method, the secondary ampere-turns are not equal and opposite to the primary ampere-turns, which is the condition necessary to

TABLE IV

IMPEDANCE TESTS—VARIATION WITH FREQUENCY—TRANSFORMER 60 CYCLES, 8 KV-A., 800 VOLTS PRIMARY, 800 VOLTS SECONDARY

Inside coils short-circuited. (See Fig. 2)

Frequency cycles	Impedance volts	Per cent normal volts	Amperes primary	Amperes secondary	Impedance watts	I^2R watts	Stray loss in per cent of I^2R	Remarks
30	11.25	1.41	10	7.2	79	78.8	0.25	Air core
40	13.10	1.64	10	7.8	83	82.4	0.73	
50	15.10	1.89	10	8.25	87	85.2	2.11	
60	16.90	2.11	10	8.47	90	86.7	3.81	
30	50	6.25	10	3.63	72.2	63.6	13.5	Air core magnetic shunts
40	67	8.37	10	3.96	79	64.6	22.3	
50	84	10.5	10	4.18	85.1	65.3	30.3	
60	98.5	12.3	10	4.27	91.2	65.6	39.0	
30	12.95	1.62	10	9.92	98.5	97.1	1.44	Iron core
40	13.90	1.74	10	9.92	99.5	97.1	2.47	
50	15.90	1.99	10	9.92	100.8	97.1	3.81	
60	17.80	2.22	10	9.92	102.2	97.1	5.25	
30	64	8.0	10	9.85	109.4	96.6	13.25	Iron core magnetic shunts
40	86	10.75	10	9.85	114.3	96.6	18.5	
50	107	13.4	10	9.85	121.3	96.6	25.6	
60	129	16.1	10	9.85	127.5	96.6	32.0	

have no leakage flux. (See third test, Table IV.) This introduces an error in the loss, which, however, is very slight for transformers of ordinary reactance.

Relation between Impedance Watts and Load Losses. To show the relation between the impedance watts and the actual load losses, that is, losses caused by rated current under operating conditions, a series of tests is recorded. The 60-cycle, 165-kv-a., 11,000 volts high-tension and 430 volts low-tension transformer, previously described, with the steel magnetic shunt in place between high-tension and low-tension windings, was used. A so-called "bucking" heat run was made, *i.e.*, with the 165-kv-a. transformer and a 365-kv-a. transformer of the same voltage rating, the high-tension windings being connected in series, bucking, and supplied with sufficient voltage to force full-load current through the winding of the 165-kv-a. transformer, and the low-tension windings being connected in multiple and excited at normal volts. This is the regular manner of performing heat runs on transformers, and its effect is to supply core loss and impedance watts to the transformer. Another run was made with the experimental transformer loaded on a water box, with normal voltage supplied to the high-tension and normal amperes delivered on the low-tension. The results are given in Table V, together with a set of check readings.

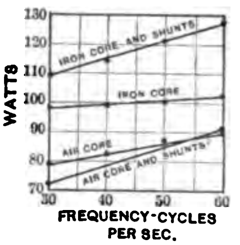


FIG. 2—IMPEDANCE TEST, 8-KV-A. TRANSFORMER
See Table IV.

TABLE V
HEAT RUNS
TRANSFORMER 60 CYCLES, 165 KV-A., 11,000 VOLTS TO 430
Self-cooled

Frequency cycles	Kind of load	Exciting volts	Load amperes	Room temperature	Rise oil	Rise top of tank	Rise bottom of tank	Rise primary	Rise secondary
60	Bucking	430	15	21	39	36	17	37	44
60	Dead	11,000	388	21	36	33	17	36	—
Check run:									
60	Bucking	430	15	20	36	32	28	37.5	44.5
60	Dead	11,000	384	22	36	33	29	38.5	45

Normal amperes: high-tension 15, low-tension, 384.
All rises in deg. cent. above room temperature.
In the first run the thermometer at bottom of tank was placed nearer to bottom than in second run.

The experimental transformer was then placed in a tank containing cooling coils and operated as a water-cooled transformer on both bucking and dead (water box) load. Three runs were made: first, with 125 per cent normal amperes and 1.3 gal. of water per min.; second, with 125 per cent normal amperes and 0.66 gal. of water per min.; third, with 150 per cent normal amperes and 0.5 gal. water per min. The results are given in Table VI.

We may conclude from the results of Tables V and VI that the bucking method of heat run (supplying core loss and impedance watts) gives substantially the same losses as supplied under normal load conditions (as approximated by the water-box run).

To further check this point, tests are recorded of a small transformer rated 60 cycles, 110 volts primary, 50 amperes secondary. This was built on a two-legged core, with one coil on each leg. The coils consisted of 115 turns of 0.24 by 0.12 in. (6.1 by 3.05 mm.) copper, and 48 turns of 0.048 by 0.13 in.

TABLE VI

HEAT RUNS

TRANSFORMER 60 CYCLES. 165 KV-A., 11,000 VOLTS TO 430 VOLTS

Water-cooled

Frequency cycles	Kind of load	Exciting volts	Load amperes	Gal. water per min.	Room temperature	Temperature ingoing water	Rise water	Rise oil	Rise tank
60	Bucking	430	18.8	1.3	23	9.3	11.5	21	17
60	Dead	10,840	483	1.3	26	10	12.0	22	18.5
60	Bucking	430	18.8	0.66	26	9.9	19	26	21.5
60	Dead	11,330	475	0.66	25	8.5	21	28	23.0
60	Bucking	430	22.5	0.50	25.5	9.2	32	37	31
60	Dead	10,700	570	0.50	27.0	8.1	32	38	31

All rises in deg. cent. above temperature of ingoing water.

(1.22 by 3.3 mm.) copper, respectively. The 115-turn coil was used as primary and the other coil as secondary. This gives a transformer with very high reactance and accentuates the difference between the impedance watts and the $I^2 R$ watts. Table VII shows the losses as measured by core loss and impedance tests and also as measured by the input-output method. The $I^2 R$ loss + core loss in this case is 97 watts, but the impedance loss + core loss is 398 watts, which figure is closely checked by the input-output test.

Finally, the tests made on an induction regulator are given. The movable core contained a primary (shunt) winding and a short-circuited winding arranged at right angles to the shunt winding. The secondary (series) winding was on the stationary core. The short-circuited winding consisted of copper bars of large cross-section, thereby naturally giving considerable eddy current losses.

Tests were made with the rotor in two different positions, first the position of maximum boost, which is obtained when the rotor is at one of the limit positions, second at the position of maximum impedance volts, which was found by trial. Table VIII gives the readings of core loss and impedance loss, calculated $I^2 R$ loss, loss by the input-output method and results of heat runs in the two positions. The heat runs were made

TABLE VII

COMPARISON OF LOSSES BY INPUT-OUTPUT AND IMPEDANCE METHODS SMALL HIGH-REACTANCE TRANSFORMER, RATED 60 CYCLES, 110 VOLTS PRIMARY, 50 AMPERES SECONDARY

Core loss			Impedance			Total loss
Volts	Amperes	Watts	Volts	Amperes	Watts	
110	21	48	60	21	350	398
Input			Output			
Volts	Amperes	Watts	Volts	Amperes	Watts	
110	21.8	2017	37.25	50	1630	387
Check 110		2020		50	1644	376
					Average	382

I^2R loss = 49.1 watts
Core loss = 48 "

Total 97.1 "

with the regulator operating as a transformer, the shunt winding being excited and the series winding loaded on a water box.

The difference in heating with the two connections is practically proportional to the difference in total loss.

We may conclude from the tests recorded that the total losses present when the transformer is under load are found by the addition of the no-load losses at normal volts to the impedance loss at normal current, the difference between the impedance losses and the $I^2 R$ loss representing the stray loss.

With transformers of ordinary reactance, the power factor of the impedance reading is sufficiently high (20 to 60 per cent) to leave no question of the accuracy of the wattmeter reading. In the high-reactance transformers, the power factor in some cases is under 10 per cent, and here the wattmeter readings may be somewhat inaccurate.

The loss due to eddy current in the copper is given by a formula of the form

Eddy current loss = (frequency)^2 × (density of leakage flux)^2 × (width of conductor)^2 × a constant.

That is, the eddy current loss is proportional to the square of

TABLE VIII

LOSSES AND HEAT RUNS

INDUCTION REGULATOR, 55 KV-A., 2200 VOLTS PRIMARY, 125 AMPERES SECONDARY, OPERATED AS A TRANSFORMER

Losses	Maximum boost position	Maximum impedance position
Core loss	877	877
Impedance loss	705	1060
	1582	1937
Primary I^2R at 25 deg. cent.	302	110
Secondary " 25 " "	393	393
Short-circuit winding I^2R	0	160
Core loss	877	877
	1572	1540
Total loss, input-output method	1550	2000
Heat run		
Rise primary above 25 deg. cent.	34	39
" secondary " " " "	30	36
" top oil " " " "	30	35
" tank top " " " "	22	29
" tank bottom " " "	14	18

the frequency, the square of the maximum density of the leakage flux, and the square of the width of conductor at right angles to the leakage field. There is also an eddy current and hysteresis loss in the steel of the core cut by the flux in the impedance test, which is small and is not present as a separate loss when the transformer is under load.

The eddy current loss in the copper can be diminished by reducing the reactance, by reducing the width of copper at right angles to the flux, or by twisting the copper. These losses, however, are bound to exist to a certain extent even in the best-designed transformers, and they are unavoidable in transformers that are purposely designed with high reactance. Their value (plus the other stray losses) may be found, as shown above, by subtracting, from the value of the short-circuit watts, the I^2r watts at the proper temperature.

II. Dielectric Losses

In ordinary power transformers the dielectric losses are usually negligible, but in high-voltage testing transformers they may form a large part of the total no-load losses. For a given dielectric at a constant temperature, an equation of this form gives the losses:

$$\text{Dielectric loss} = \text{frequency} \times \text{electrostatic capacity} \times (\text{voltage})^2 \times \text{a constant},$$

TABLE IX

DIELECTRIC LOSSES

TRANSFORMER 3750 KV-A., 120,000 VOLTS HIGH-TENSION, 12,000 VOLTS LOW-TENSION

High-tension in series			High-tension disconnected			Dielectric loss watts
Kilovolts high-ension by ratio	Amperes low-tension	Watts loss	Kilovolts high-tension by ratio	Amperes low-tension	Watts loss	
60	2.40	9100	60	2.3	8660	440
90	6.75	18450	90	6.6	17850	600
120	23.4	30400	120	24.0	29050	1350
132	43.0	42200	132	41.3	38100	4100

High-tension winding disconnected in middle. Oil temperature 30 deg. cent. (See Fig. 3.)

the constant depending upon the properties of the dielectric. In addition to the above there is also a small loss due to current leaking through the resistance of the insulation.

Table IX and Fig. 3 show the results of a test made on a shell-type transformer rated 60 cycles, 3750 kv-a., 12,000 volts low-tension, 120,000 volts high-tension. Losses were measured on the low-tension side, first with all the high-tension coils in series, and second, with the high-tension winding open at the middle. The difference between the readings represents the greater part of the dielectric loss. If the windings could be further subdivided, a still further reduction in the loss would result. It will be seen that only above the operating voltage does the loss in the dielectric become appreciable.

Some tests were made on a testing transformer rated 60 cycles 100 kv-a., 500/1000/2000 volts low-tension, 200,000 volts high-tension. This transformer is of the type described by Mr. A. B. Hendricks, Jr., in his paper on *High-Tension Testing of Insulating Materials* (TRANSACTIONS, A. I. E. E., 1911, XXX, I, p. 167). A voltmeter coil of 40 turns is placed at the middle of the high-tension winding, and one end of this coil is connected to the neutral of the high-tension winding and to the ground. The coil is provided with taps at the 10th and 20th turns. The voltage across the whole coil is 400 volts when the transformer is normally excited.

An ammeter and wattmeter placed in the high-tension

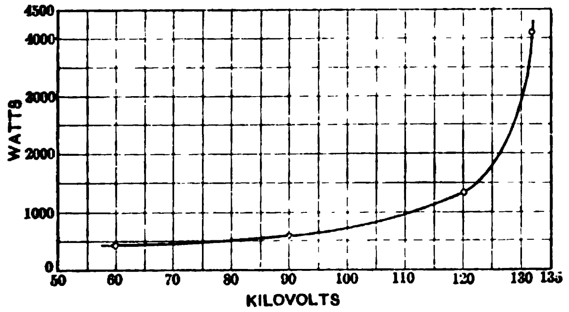


FIG. 3—DIELECTRIC LOSS, 3750-KV-A., 120-KV. TRANSFORMER
See Table IX.

circuit next to the ground enabled a measurement of the high-tension losses to be taken, at least such of them as are caused by the current measured in the high-tension winding. A 0.2-ampere, 30-volt wattmeter, with the potential coil in series with a multiplier and placed across the 10-turn voltmeter coil, was used. The readings obtained were small, and the total multiplying factor high, so that great accuracy cannot be claimed for the results. This method of measuring takes no account of the loss in the insulation between turns, but only that between high-tension and ground. Table X shows the losses at oil temperatures of 23 deg. cent. and 56 deg. cent., respectively. The difference between the total losses and the loss measured on the high-tension side decreases with increase in temperature, much more rapidly than we should expect from a decrease in core loss alone (core loss decreasing about one per cent for each 10 deg. increase in temperature).

Table XI shows another set of readings taken at various air temperatures from −10 to 22 deg. cent. The results of Table X

TABLE X

NO-LOAD LOSS

TRANSFORMER 100 KV-A., 500/1000/2000 VOLTS TO 200,000 VOLTS

Kilovolts high-tension	Amperes low-tension	Total loss watts	High-tension loss watts	Temperature of top oil
100	0.87	682	132	23 deg. cent.
150	1.29	1394	294	
200	1.80	2356	543	
100	1.13	940	440	56 deg. cent.
150	1.68	2086	1174	
200	2.22	3620	2128	

Connected for 2000 volts low-tension, 200,000 volts high-tension. Measurements at 60 cycles. (See Fig. 4)

and the readings at the lowest temperature of Table XI are plotted in Fig. 4.

Table XII gives the results of readings on the low-tension and high-tension at frequencies varying from 47 to 115 cycles

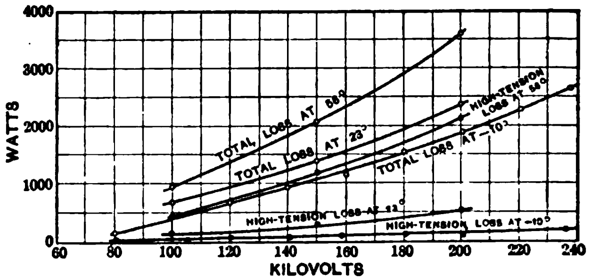


FIG. 4—NO-LOAD LOSSES, 100-KV-A., 200-KV. TRANSFORMER
See Table X.

per sec. These results are plotted in Fig. 5. The high-tension losses increase somewhat with the frequency, but not directly. On account of the preponderance of the core loss, the total losses decrease with increase in frequency, and consequent decrease in flux density in the iron.

TABLE XI

NO-LOAD LOSSES—EFFECT OF TEMPERATURE

TRANSFORMER 100 KV-A., 500/1000/2000 VOLTS TO 200,000 VOLTS

Kilovolts high-tension by ratio	Amperes low-tension	Watts low-tension	Kilovolts high-tension	Amperes high-tension	Watts high-tension	Temperature air deg. cent
80		120	80.5	0.0051	10	—10
101		550	105.6	0.0068	20	
120		650	120	0.008	40	
140		900	140.5	0.0093	60	
160		1150	159	0.0106	90	
180.4		1550	181	0.0124	120	
200.6		1900	201	0.0134	140	
221		2300	221	0.0156	———	
238		2650	237	0.0178	220	
129.6		850	129	0.0087	80	—2
142.6		950	142	0.0095	90	
162.2		1300	162	0.0109	120	
185.4		1650	186	0.0131	150	
203		1900	203	0.0145	200	
226		2450	224.6	0.0165	250	
110		550	111.5	0.0077	50	0
154.4		1100	153	0.0105	110	
200		1850	200	0.0143	180	
50		100	50	—	5	3
64		240	65.5	—	15	
76		260	75.5	0.0052	20	
114		600	113	0.008	80	
135		850	134	0.009	100	
177.4		1450	187	0.0123	160	
192.6		1750	193	0.0136	200	
210.8		2200	210	0.0154	240	
233.2		2600	231	0.0168	280	
80	1.14	400	80	0.005	55	13
100	1.37	620	100.5	0.007	100	
120	1.63	820	121.5	0.008	150	
143.6	1.87	1180	143.5	0.010	210	
162.4	2.05	1450	161.0	0.011	280	
181.0	2.29	1800	181.0	0.013	350	
203	2.69	2200	202.2	0.015	440	
219	3.25	2640	217.0	0.016	540	•
235	4.20	3050	234.0	0.017	660	
158.8	2.10	1500	158.4	0.0117	360	19
181	2.36	1900	180.0	0.0137	480	
202.8	2.78	2340	201.2	0.0152	600	
80	1.18	470	80	0.0057	80	22
90.2	1.28	570	91	0.0065	97	
100.2	1.42	670	100.6	0.0071	110	
120.4	1.65	925	119.3	0.0084	190	
142.4	——	1250	141.0	0.01	270	
160.0	2.15	1550	159.0	0.0117	370	
180.0	2.40	1900	179.6	0.0135	460	
202.2	2.70	2550	201.2	0.0155	600	

Connected 1000 volts low-tension, 200,000 volts high-tension. All measurements at 60 cycles.

(See Fig. 4)

TABLE XII

NO-LOAD LOSSES—EFFECT OF FREQUENCY

TRANSFORMER 60 CYCLES, 100 KV-A., 500/1000/2000 VOLTS LOW-TENSION, 200,000 VOLTS HIGH-TENSION

Frequency cycles per per sec.	High-tension kilovolts by ratio	Low-tension amperes	Low-tension watts	High-tension kilovolts	High-tension amperes	High-tension watts	Air temperature deg. cent.
47	120	1.72	930	120	0.0065	100	12
	140.4	2.11	1200	139	0.0078	180	
	162.2	3.00	1600	162	0.0088	220	
	182.8	4.69	2050	184	0.0101	300	
	201			201	0.011	350	
	211			210	0.012	400	
57	122.2	1.56	880	121	0.0085	100	12
	141.2	1.78	1150	140	0.0091	150	
	162.4	2.00	1430	161.6	0.0107	240	
	181.8	2.33	1800	182.0	0.0126	300	
	203	3.00	2200	203.2	0.0141	350	
	215	3.71	2500	213.6	0.0151	440	
70	120	1.61	770	119	0.0095	120	12
	142.4	1.91	1100	141.4	0.0118	200	
	158.4	2.12	1340	160	0.0135	250	
	180.6	2.32	1650	180.6	0.0155	300	
	203	2.95	2010	203.6	0.0175	400	
	221	2.68	2400	221.6	0.0188	500	
80	122	1.81	770	121.6	0.0113	150	12
	140.4	2.10	990	140.6	0.135	200	
	160.2	2.41	1290	159.6	0.0159	250	
	179.4	2.64	1550	179.0	0.0175	300	
	201	2.96	1900	201.0	0.0193	440	
	225	3.26	2350	223.2	0.0219	550	
90	122.2		700	121	0.13	140	14
	139.2		850	139	0.015	200	
	162.6		1100	162.4	0.018	300	
	180.6		1400	180.6	0.020	400	
	201.0		1750	202	0.022	560	
	221		2200	221.2	0.025	650	
115	121		700	121.6	0.017	140	14
	142.4		850	142.6	0.0195	250	
	160.4		1100	160.0	0.0221	300	
	180.6		1450	181.0	0.0248	400	
	201.0		1650	201	0.0277	550	
	223.2		2200	223.2	0.312	700	

Connected for 1000 volts low-tension, 200,000 volts high-tension (See Fig. 5).

In Table XIII are given the results of an 11½-hour heat run at normal voltage, open circuit. During this time the temperature at the top of the oil rose 35 deg. cent. above its initial

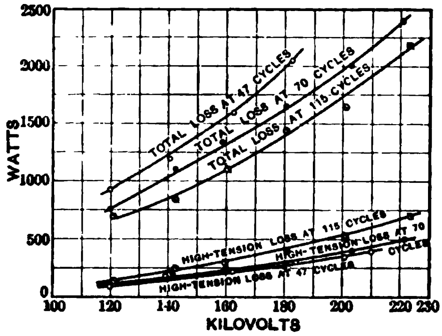


FIG. 5—NO-LOAD LOSSES, 100-KV-A., 200-KV. TRANSFORMER
See Table XII.

temperature, the total losses increased 62 per cent and the high-tension losses increased 263 per cent. Losses are plotted against increase of temperature and increase of high-tension loss against

TABLE XIII
NO-LOAD LOSSES—VARIATION WITH TEMPERATURE
TRANSFORMER 60 CYCLES, 100 KV-A., 500/1000/2000 VOLTS TO 200,000 VOLTS

Hours heat run	Deg. cent. rise oil	Total watts loss	Low-tension amperes	High-tension loss	High-tension amperes	Increase in high-tension loss
0	0	2234	1.74	587	0.02	0
1	4	2268	1.75	587	0.02	0
2	7.2	2346	1.76	660	0.02	73
3	10.8	2424	1.77	734	0.02	147
4	14	2508	1.81	880	0.02	293
5	17	2610	1.83	1027	0.02	440
6	20.2	2774	1.88	1174	0.02	587
7	23	2920	1.92	1321	0.023	734
8	26	3040	1.97	1468	0.023	881
9	28.5	3184	2.0	1614	0.025	1027
10	31	3326	2.1	1761	0.025	1174
11	34	3560	2.2	2055	0.025	1468
11.5	35	3620	2.22	2128	0.025	1541

Rises above 21 deg. cent., initial oil temperature.
Transformer connected for 2000 volts low-tension, 200,000 volts high-tension. (See Fig. 6.)

increase of temperature, in Fig. 6. It will be seen from this that the loss goes up about as the square of the deg. cent. increase in temperature.

A series of tests was conducted on a testing transformer, No. 630263, rated 60 cycles, 500 kv-a., 1100/2200 volts low-tension, 750,000 volts high-tension. To eliminate the effect of variations in temperature, the oil was kept circulating and maintained at a constant temperature of 70 deg. cent. A 500-kw., 60-cycle generator was used, the connections being changed at various points on each curve, in order to operate the machine at nearly normal density and prevent distortion of the wave shape. When the connections were changed and readings repeated, an average of the two readings was plotted.

Phase meter and oscillograph tests showed the current to

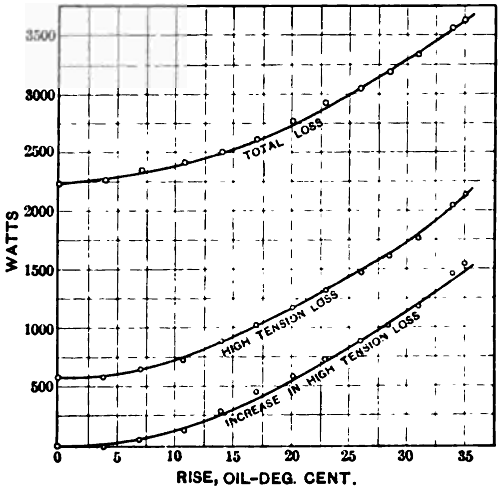


FIG. 6—NO-LOAD LOSSES, 100-KV-A., 200-KV. TRANSFORMER
See Table XIII.

be leading at all voltages, except the very low ones (under 100 kv.) where the power factor approaches unity.

Losses were measured on the low-tension side only. The following curves were taken:

1. Without leads
   a. Middle of high-tension winding grounded.
   b. High-tension not grounded.
   c. One end of high-tension grounded.
2. With leads.
   a. Middle of high-tension grounded.
   b. High-tension not grounded.
   c. One end of high-tension grounded.

The results are given in Tables XIV to XIX inclusive, and plotted in Fig. 7. As the losses with the winding not grounded are practically the same as the losses with the middle grounded, the latter tests are not plotted. With one terminal grounded the losses are considerably increased, as the end of the winding in this case is at double the potential above ground that it is in the first two cases. When the leads are connected, the losses are increased, due to corona on the terminal choke coils and to

TABLE XIV

NO-LOAD LOSSES—WITHOUT LEADS AND WITH HIGH-TENSION TERMINALS FREE FROM GROUND

TRANSFORMER 60 CYCLES, 500 KV-A., 1100/2200 VOLTS TO 750,000 VOLTS NO. 630,263

Kilovolts high-tension by ratio	Volts low-tension	Amperes low-tension	Watts	Generator connection
50	146	2.34	308	440-volt
100	295	4.5	1,185	
150	441	6.2	2,510	
150	441	5.98	2,510	1150-volt
200	586	7.87	4,220	
300	881	11.4	9,110	
350	1028	13.3	12,370	
375	1100	14.1	14,180	
400	1173	15.2	16,180	
450	1320	17.1	20,450	
375	1100	14.7	14,220	2300-volt
400	1173	15.5	16,200	
450	1320	17.4	20,400	
500	1467	19.4	25,100	
550	1614	21.7	30,150	
600	1760	24.8	36,100	
650	1910	29.0	42,800	
700	2060	36.3	50,100	
750	2200	46.5	59,200	

Temperature of oil 70 deg. cent. Connected on low-tension for 2200 volts. (See Fig. 7.)

the boost in high-tension voltage caused by the capacity of the leads. Even without leads, there is considerable boost in voltage, as may be judged from the fact that with center of high-tension winding grounded, a voltage of 650 kv. by ratio caused a discharge across a needle spark gap of 75.5 in. (192 cm.), equivalent approximately to 750,000 volts; and with one terminal grounded, a voltage by ratio of 375,000 caused the discharge of a 50.5-in. (128-cm.) gap, equivalent to 500,000 volts

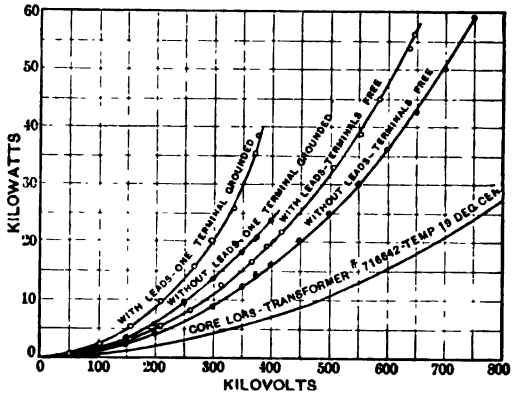


FIG. 7—NO-LOAD LOSSES, TEMPERATURE 70 DEG. CENT., 500-KV-A., 750,000-VOLT TRANSFORMER, NO. 630263
See Tables XIV to XIX.
(Shows core loss of transformer No. 716642 at 19 deg. cent.)

TABLE XV
NO-LOAD LOSSES—WITHOUT LEADS AND WITH ONE HIGH-TENSION TERMINAL GROUNDED
TRANSFORMER 60 CYCLES, 500 KV-A., 1100/2200 TO 750,000 VOLTS. NO. 630,263

Kilovolts high-tension by ratio	Volts low-tension	Amperes low-tension	Watts	Current high-tension to ground	Generator connection
	Left-hand leg, facing high-tension, grounded				
50	146	3.29	416	—	440-volt
100	292	7.58	1,526	—	
150	442	11.9	3,500	—	
150	442	12.3	3,490	—	1150-volt
200	588	16.5	5,980	0.053	
250	732	20.9	9,240	0.084	
300	881	25.1	13,510	0.091	
350	1028	29.1	18,390	0.107	
375	1100	31.2	20,900	0.115	
400	1175	32.8	23,800	0.122	
450	1320	Arcs to ground			
	Right-hand leg, facing high-tension, grounded				
300	881	26.1	13,820	0.093	1150-volt
350	1028	30.3	18,900	—	
375	1100	32.3	21,400	0.112	
400	1173	34.7	24,600	0.118	
425	1247	Arcs to ground			

Temperature of oil 70 deg. cent. (See Fig. 7)

Some tests were also made on a companion transformer, No. 716642, which is identical with the transformer just described except that the low-tension voltage is 2500. The loss was first measured with the low-tension coils only on core, thus giving the core loss alone. The high-tension coils were then assembled on core, but left totally disconnected from each other. This high-

TABLE XVI

NO-LOAD LOSSES—WITHOUT LEADS AND WITH MIDDLE OF HIGH-TENSION WINDING GROUNDED

TRANSFORMER 60 CYCLES, 500 KV-A., 1100/2200 to 750,000 VOLTS. NO. 630,263

Kilovolts high-tension by ratio	Volts low-tension	Amperes low-tension	Watts	Generator connection
50	146	2.35	313	440-volt
100	295	4.44	1,240	
150	441	6.30	2,550	
150	441	6.03	2,520	1150-volt
200	586	7.94	4,280	
250	732	9.78	6,520	
300	881	11.7	9,300	
350	1028	13.5	12,590	
375	1100	14.3	14,310	
400	1173	15.2	16,300	
450	1320	17.0	20,700	
500	1467	18.8	25,700	
300	881	12.0	9,230	2300-volt
350	1028	13.8	12,510	
375	1100	14.8	14,220	
400	1173	15.4	16,320	
450	1320	17.2	20,500	
500	1467	19.1	25,250	
550	1614	21.7	30,800	
600	1760	24.8	36,700	
650	1910	29.4	43,100	
700	2060	36.8	50,400	
750	2200	45.7	59,300	

Temperature of oil 70 deg. cent.

tension winding consists of 134 coils, with a total of 26,000 turns. The volts per coil are about 2850 and the volts per layer 28½. The loss in each case was measured on the low side, and is therefore the core loss plus the dielectric loss. It is evident that the greater part of the dielectric loss is present, even with all the coils disconnected.

TABLE XVII

NO-LOAD LOSSES—LEADS CONNECTED AND HIGH-TENSION WINDING UNGROUNDED

TRANSFORMER 60 CYCLES, 500 KV-A., 1100/2200 TO 750,000 VOLTS. NO. 630,263.

Kilovolts high-tension by ratio	Volts low-tension	Amperes low-tension	Watts	Generator connection
52.2	153	2.73	415	440-volt
104.4	306	5.29	1,560	
157.0	460	8.14	3,360	
157	460	7.92	3,270	1150-volt
209	613	10.7	5,540	
261	766	13.2	8,320	
314	920	15.8	12,500	
367	1074	18.3	16,600	
392	1150	19.5	19,050	
418	1226	20.9	22,700	
506	1484	25.1	33,000	1535-volt
554	1625	27.0	38,800	
586	1720	29.0	44,900	
636	1864	32.4	53,700	
646	1895	33.2	56,100	

Temperature of oil 70 deg. cent. (See Fig. 7)

TABLE XVIII

NO-LOAD LOSSES—LEADS CONNECTED AND ONE HIGH-TENSION TERMINAL GROUNDED

TRANSFORMER 60 CYCLES, 500 KV-A., 1100/2200 TO 750,000 VOLTS. NO. 630,263

Kilovolts high-tension by ratio	Volts low-tension	Amperes low-tension	Watts	Generator connection
52.2	153	5.19	638	440-volt
104.4	306	11.4	2,470	
157	460	18.1	5,550	
157	460	18.7	5,450	1150-volt
209	613	25.1	9,820	
266	772	32.7	15,700	
297	875	37.2	20,100	
335	982	41.9	25,700	
370	1087	46.1	35,400	
376	1102	47.5	38,300	

Temperature of oil 70 deg. cent. (See Fig. 7)

TABLE XIX
NO-LOAD LOSSES—LEADS CONNECTED AND MIDDLE OF HIGH-TENSION WINDING GROUNDED
TRANSFORMER 60 CYCLES, 500 KV-A., 1100/2200 to 750,000 VOLTS. NO. 630,263

Kilovolts high-tension by ratio	Volts low-tension	Amperes low-tension	Watts	Generator connection
177.5	497	8.91	3,350	1150-volt
327	916	16.9	11,420	
333	978	18.5	13,010	
417	1222	24.4	20,870	
502	1472	28.45	30,600	

Temperature of oil 70 deg. cent.

The transformer was next tested completely assembled and ready for service. The difference between this loss and the core loss, we may take as the dielectric loss under normal conditions. Another set of readings was taken with one high-tension terminal grounded. These curves are plotted in Fig. 8, and the readings are given in Tables XX to XXIII, inclusive.

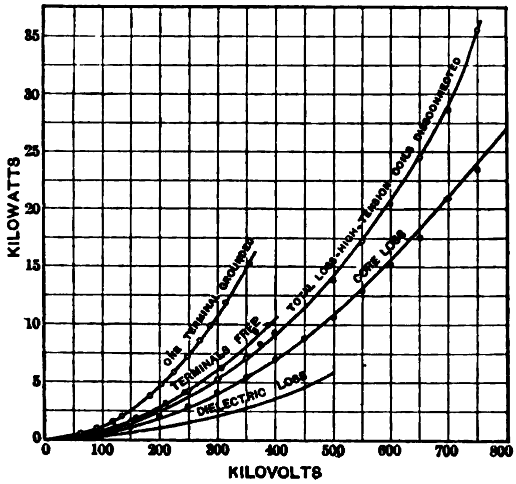


FIG. 8—NO-LOAD LOSSES, 500-KV-A., 750,000-VOLT TRANSFORMER, No. 716642
See Tables XX to XXIII.

For comparison the core loss curve of Fig. 8 is reproduced in Fig. 7. The difference between the total loss curves of Fig. 7 and the core loss curve is greater than the difference between the same curves of Fig. 8, due to the higher temperature in the first case.

TABLE XX

CORE LOSS—LOW-TENSION COILS ONLY ON CORE

TRANSFORMER 60 CYCLES, 500 KV-A., 2500 TO 750,000 VOLTS. NO. 716,642.

Low-tension volts	Kilovolts high-tension by ratio	Amperes low-tension	Watts	Generator connection
166	50	2.24	165	440-volt
333	100	3.04	607	
499	150	3.70	1,234	
499	150	3.71	1,234	1150-volt
666	200	4.35	2,025	
833	250	4.95	3,030	
1000	300	5.64	4,180	
1166	350	6.45	5,360	
1166	350	5.45	5,350	1535-volt
1335	400	7.29	7,070	
1500	450	8.34	8,830	
1670	500	9.45	10,550	
1835	550	11.5	12,900	
1835	550	11.5	12,900	2300-volt
2000	600	13.7	15,150	
2165	650	17.0	17,760	
2330	700	21.5	20,950	
2500	750	27.5	23,600	
2680	804	37.4	28,100	

Temperature of air 19 deg. cent. (See Fig. 8)

TABLE XXI

NO-LOAD LOSSES—HIGH-TENSION COILS ALL DISCONNECTED

TRANSFORMER 60 CYCLES, 500 KV-A., 2500 TO 750,000 VOLTS. NO. 716,642

Low-tension volts	Kilovolts high-tension by ratio	Amperes low-tension	Watts	Generator connection
166.5	50	2.36	196.5	440-volt
333	100	3.30	720	
500	150	4.08	1,520	
666	200	4.84	2,480	
666	200	4.80	2,470	1150-volts
833	250	5.66	3,790	
1000	300	6.54	5,280	
1165	350	7.60	7,100	
1334	400	9.00	9,550	
1250	375	8.37	8,430	2300-volt
1334	400	8.79	9,210	
1500	450	10.4	11,350	
1666	500	12.2	13,800	
1835	550	14.3	17,250	
2000	600	17.2	20,660	
2170	650	20.7	24,560	
2330	700	25.0	28,710	
2500	750	30.7	35,720	

Temperature of air 19 deg. cent. (See Fig. 8)

TABLE XXII
NO-LOAD LOSSES—TERMINALS FREE FROM GROUND
TRANSFORMER 60 CYCLES, 500 KV-A., 2500 TO 750,000 VOLTS. NO. 716,642

Low-tension volts	Kilovolts high-tension by ratio	Amperes low-tension	Watts	Generator connection
200	60	1.66	322	384-volt
242	72.5	2.02	466	
252	75.5	2.11	500	
292	87.5	2.55	648	
314	94.2	2.7	748	
324	97.2	2.91	791	
346	104	3.18	900	
420	126	3.99	1250	767-volt
540	162	5.28	1965	
705	212	7.1	3160	
762	229	7.86	3662	
814	244	8.35	4145	
823	247	8.5	4120	1150-volt
1026	308	10.7	6360	
1223	366	13.0	8830	
1291	388	13.7	9860	

Temperature of air 25.5 deg. cent. (See Fig. 8)

TABLE XXIII
NO-LOAD LOSSES—ONE HIGH-TENSION TERMINAL GROUNDED
TRANSFORMER 60 CYCLES, 500 KV-A., 2500 to 750,000 VOLTS. NO. 716,642

Low-tension volts	Kilovolts high-tension by ratio	Amperes low-tension	Watts	Generator connection
212	63.6	4.71	524	384-volts
255	76.5	6.06	740	
312	93.6	7.60	1,102	
400	120.0	10.2	1,740	
455	136.5	11.85	2,235	
608	182.5	16.4	3,880	767-volts
675	202.0	18.5	4,890	
750	225.0	20.8	5,950	
825	248.0	23.3	7,250	
758	228	21.3	6,080	1150-volts
900	270	26.3	8,720	
960	288	28.5	9,860	
1050	315	31.0	11,900	
1180	354	35.5	15,300	

Temperature of air 22.5 deg. cent. (See Fig. 8)

As the insulation losses in all cases are included in the no-load readings, no especial determination of them is necessary, but it is apparent that it is essential to measure the no-load losses at the operating temperature.

III. Recommendations

It is recommended that Sections 156 to 159 of the Standardization Rules be amended as follows:

The losses in Transformers are:

1 No-Load Losses, including Core Loss and Dielectric Loss, measured with open secondary circuit, at rated frequency and rated (full-load) voltage minus $I\,r$, where I = rated current, and r = resistance of primary circuit. There is also a small resistance loss due to exciting current in the primary.

a. Core Loss, including hysteresis loss and loss due to eddy currents in the steel.

b. Dielectric Loss, energy loss in the insulation due to electrostatic stress.

2 Load Losses, including Resistance Loss and Stray Loss, practically equal to the impedance losses which are determined by short-circuiting the secondary and impressing upon the primary a voltage sufficient to send rated load current through the transformer.

a. Resistance Loss, the sum of the I^2r losses in the primary and secondary windings of a transformer, or in the two sections of the winding of an auto-transformer, where I = rated load current in the winding or section of winding, and r = resistance. In the primary, I = resultant of load current and exciting current.

b. Stray Loss, the eddy current loss in the copper, and the stray losses in the neighboring iron, due to the stray or leakage flux caused by the current at rated load.

3 On account of the variation with temperature, all losses should be measured at the operating temperature.

A paper presented at the Midwinter Convention of the American Institute of Electrical Engineers, New York, February 27, 1913.

STRAY LOSSES IN TRANSFORMERS

BY C. FORTESCUE AND W. M. McCONAHEY

(1) Introduction

The object of this paper is to obtain a convenient method of evaluating the actual losses in the copper of a transformer under operating conditions, and of figuring the regulation.

The losses in the copper of a transformer are influenced by several conditions, namely,

a. Frequency and wave form
b. Dissymmetry of winding
c. Permeability of iron
d. Temperature.

a. The portion of the losses in the copper that depends on frequency is included in what is termed "eddy-current loss in the copper," analogous to "skin effect" in line conductors, and is practically eliminated in careful designs. Higher harmonics in the wave form of impressed e.m.f. have also some effect on these losses, as would naturally be expected, since they are dependent upon frequency.

b. The losses due to dissymmetry of winding are confined to transformers with parallel connections when the different circuits forming the parallel-connected secondary are not similarly disposed with regard to the primary circuit and one another. These losses are independent of frequency and are of little importance, since they will be negligibly small in good designs.

c. The losses in the copper may be affected by the permeability of the iron. If eddy-current losses are present these will be affected by any change in the leakage induction due to a change of permeability. The permeability will affect the value of the exciting current and thereby the copper loss under load. These effects are, however, usually so small as to be negligible except in special cases.

d. Temperature will have the effect of increasing the true I^2R losses and decreasing the eddy-current losses. Where a transformer has large eddy-current losses its copper loss should be measured if possible at the temperature on which guarantees are based. A first approximation to the effect of temperature on eddy-current losses may be obtained by considering them directly proportional to the conductivity of the copper and correcting for temperature accordingly.

(2) Theoretical Study of Copper Losses in Transformers

The theory of the transformer depends on that of two mutually inductive circuits. A clear understanding of the actions that take place in transformers cannot be obtained without a careful study of the theory of such a pair of circuits. It must not be supposed, however, that a system of this kind can be made to represent a transformer exactly; on account of the peculiar characteristics of iron this is impossible. It furnishes, however, a model sufficiently close to be accurate enough for all practical purposes. The resistances and inductances of the two circuits will be influenced by frequency, temperature, etc. in the same way as the windings of a transformer. Results obtained mathematically for such a pair of circuits will apply with almost equal accuracy to transformers.

Let the resistance and inductance of the primary circuit be R_1 and L_1 and those of the secondary R_2 and L_2; let the mutual inductance be M and the resistance and self-inductance of the load on the secondary circuit be R_0 and L_0; let e_1 be the value of the impressed e.m.f. The differential equations of the two circuits are

$$L_1 \frac{d i_1}{d t} + M \frac{d i_2}{d t} + R_1 i_1 = e_1 \qquad (1)$$

$$(L_2 + L_0) \frac{d i_2}{d t} + M \frac{d i_1}{d t} + (R_2 + R_0) i_2 = 0 \qquad (2)$$

From (2) we have

$$i_2 = - \frac{M \frac{d}{d t}}{(R_2 + R_0) + (L_2 + L_0) \frac{d}{d t}} i_1 \qquad (3)$$

Substituting this in (1)

$$\left(L_1 \frac{d}{d\,t} - \frac{M^2 \dfrac{d^2}{d\,t^2}}{(R_2 + R_0) + (L_2 + L_0)\dfrac{d}{d\,t}} + R_1 \right) i_1 = e_1$$

or

$$\left\{ R_1 - \frac{M^2 \dfrac{d^2}{d\,t^2}}{(R_2 + R_0)^2 - (L_2 + L_0)^2 \dfrac{d^2}{d\,t^2}} (R_2 + R_0) \right.$$

$$\left. + \left(L_1 + \frac{M^2 \dfrac{d^2}{d\,t^2}}{(R_2 + R_0)^2 - (L_2 + L_0)^2 \dfrac{d^2}{d\,t^2}} (L_2 + L_0) \right) \frac{d}{d\,t} \right\} i_1 = e_1 \qquad (4)$$

If e_1 be a harmonic function of time it may be represented by the real part of the complex variable function

$$\Sigma \; \{(a_n + j\, b_n)\, e^{j n p t}\}$$

The final or steady value of i_1 will then be the real part of the similar expression afforded by the particular integral of equation (4); or, denoting the complex variable function representing in its real part the value of i_1 by $\check{I}_1$ we have equations (5) and (6).

$\check{E}_1$ represents the complex variable function the real terms of which give the instantaneous value of e_1 and $\hat{Z}_a$ represents the operation $R_a + j\,n\,p\,L_a$ on each successive term of the quantities $\check{I}_1$ and $\check{E}_1$, the value of n being the same as that in the index of the base of the Napierian logarithm contained in the term operated on. The values of R_a and L_a will be

$$R_a = R_1 + \frac{n^2 p^2 M^2}{(R_2 + R_0)^2 + n^2 p^2 (L_2 + L_0)^2} (R_2 + R_0) \qquad (7)$$

$$L_a = L_1 - \frac{n^2 p^2 M^2}{(R_2 + R_0)^2 + n^2 p^2 (L_2 + L_0)^2} (L_2 + L_0) \qquad (8)$$

$$\check{I}_1 = \Sigma \left\{ \frac{(a_n + j\, b_n)\, e^{j n p t}}{R_1 - \dfrac{M^2 \dfrac{d^2}{d\,t^2}}{(R_2 + R_0)^2 - (L_2 + L_0)^2 \dfrac{d^2}{d\,t^2}} (R_2 + R_0) + \left(L_1 + \dfrac{M^2 \dfrac{d^2}{d\,t^2}}{(R_2 + R_0)^2 - (L_2 + L_0)^2 \dfrac{d^2}{d\,t^2}} (L_2 + L_0) \right) \dfrac{d}{d\,t}} \right\}$$

$$= \Sigma \left\{ \frac{(a_n + j\, b_n)\, e^{j n p t}}{R_1 + j\, n\, p\, L_1) + \dfrac{n^2\, p^2\, M^2}{(R_2 + R_0)^2 + n^2\, p^2\, (L_2 + L_0)^2} \left\{ (R_2 + R_0) - j\, n\, p\, (L_2 + L_0) \right\}} \right\} \quad (5)$$

$$= \frac{\check{E}_1}{\hat{Z}_a} \quad (6)$$

This gives the effective resistance and inductance of the primary circuit with loaded secondary to the *n*th harmonic component of the impressed e.m.f.

Using the same notation, equation (3) may be put into the form

$$\check{I}_2 = -\frac{jnpM}{(R_2 + R_0) + jnp(L_2 + L_0)}\check{I}_1 \tag{9}$$

If the load resistance and inductance be made zero we shall have the effective resistance of the system under short circuit, that is,

$$R_b = R_1 + \frac{n^2p^2M^2}{R_2^2 + n^2p^2L_2^2}R_2 \tag{10}$$

$$L_b = L_1 - \frac{n^2p^2M^2}{R_2^2 + n^2p^2L_2^2}L_2 \tag{11}$$

Similarly, if the primary winding be short-circuited and the current circulated through the secondary, the effective resistance and inductance will be

$$R_c = R_2 + \frac{n^2p^2M^2}{R_1^2 + n^2p^2L_1^2}R_1 \tag{12}$$

$$L_c = L_2 - \frac{n^2p^2M^2}{R_1^2 + n^2p^2L_1^2}L_1 \tag{13}$$

The secondary terminal e.m.f. under load is

$$\check{E}_2 = \check{I}_2\hat{Z}_0 \tag{14}$$

The secondary e.m.f. at no load is

$$\check{E}_0 = -\frac{jnpM}{R_1 + jnpL_1}\check{E}_1 \tag{15}$$

by (6) this is

$$\check{E}_0 = -\frac{jnpM}{R_1 + jnpL_1}\check{I}_1\hat{Z}_a$$

and by (**9**) this is

$$\check{E}_0 = \frac{(R_2 + R_0) + j\,n\,p\,(L_2 + L_0)}{R_1 + j\,n\,p\,L_1}\,\check{I}_2\,\hat{Z}_a \qquad (16)$$

From (**14**) and (**16**),

$$\check{E}_0 - \check{E}_2 = \check{I}_2\left\{\frac{(R_2 + R_0) + j\,n\,p\,(L_2 + L_0)}{R_1 + j\,n\,p\,L_1}\,\hat{Z}_a - \hat{Z}_0\right\}$$

or, substituting for $\hat{Z}_a$ and $\hat{Z}_0$,

$$\check{E}_0 - \check{E}_2 = \check{I}_2\left\{(R_2 + j\,n\,p\,L_2) + \frac{n^2\,p^2\,M^2}{R_1^2 + n^2\,p^2\,L_1^2}\,(R_1 - j\,n\,p\,L_1)\right\} \qquad (17)$$

This latter expression gives the basis on which to calculate the regulation of transformers. For air coils having mutual inductance it gives exactly the same effective resistance and inductance as obtained with short-circuited primary and current circulated through the secondary. [See (**12**) and (**13**)]. With transformers where L_1 and L_2 are functions of the induction it gives the effective short-circuit resistance and inductance under load conditions, and these may be slightly different from those obtained with short circuit.

From (**1**) we obtain another expression for $\check{I}_1$

$$\check{I}_1 = \frac{\check{E}_1}{R_1 + j\,n\,p\,L_1} - \frac{j\,n\,p\,M}{R_1 + j\,n\,p\,L_1}\,\check{I}_2 \qquad (18)$$

$$= \check{I}_m - \frac{j\,n\,p\,M}{R_1 + j\,n\,p\,L_1}\,\check{I}_2 \qquad (19)$$

The first term is the open-circuit exciting current; the second term is the current obtained in the primary when it is short-circuited and current I_2 is sent through the secondary winding. If $|I_m|_n |I_1|_n$ and $|I_2|_n$ denote the effective values of the nth term of the series representing each of the quantities $\check{I}_m$ $\check{I}_1$ and $\check{I}_2$, and if φ_n be the phase displacement between the nth harmonic of $\overset{\vee}{E}_1$ and $\overset{\vee}{I}_2$, we have from (**19**)

$$I_1^2 = \Sigma \left\{ |I_1|_n^2 \right\}$$

$$= I_m^2 + \Sigma \left\{ \frac{n^2 p^2 M^2}{R_1^2 + n^2 p^2 L_1^2} |I_2|_n^2 \right.$$

$$\left. + 2 \frac{n\, p\, M}{\sqrt{R_1^2 + n^2 p^2 L_1^2}} |I_m|_n |I_2|_n \sin \varphi_n \right\} \quad (\mathbf{20})$$

The true copper loss is

$$I_1^2 R_a - I_2^2 R_0 = I_1^2 R_1 + I_2^2 R_2$$

$$= I_2^2 R_2 + \Sigma \left\{ \frac{n^2 p^2 M^2}{R_1^2 + n^2 p^2 L_1^2} |I_2|_n^2 R_1 \right\}$$

$$+ 2\, \Sigma \left\{ \frac{n\, p\, M}{\sqrt{R_1^2 + n^2 p^2 L_1^2}} |I_m|_n |I_2|_n R_1 \sin \varphi_n \right\} + I_m^2 R_1 \quad (\mathbf{21})$$

The first two values on the right-hand side of equation (**21**) is the copper loss as obtained from (**18**), which, as will be shown later, may be obtained by measurement. It will also be shown to be practically the same as the loss obtained under short circuit. The fourth term is constant for a given impressed e.m.f. and is included in the iron loss measurement. The third term is the correction to be made to the loss as obtained by (**17**) or the short-circuit method, to obtain the correct value of the copper loss. In practical application of this correction the quantity

$$\frac{n\, p\, M}{\sqrt{R_1^2 + n^2 p^2 L_1^2}}$$

may be taken to be equal to n_2/n_1 where n_1 and n_2 are the primary and secondary turns.

(3) Method of Measuring Impedance and Short-Circuit Loss under Load

It has been shown how the true copper loss in the case of two mutually inductive circuits may be obtained by a correction to be added to the copper loss obtained under short circuit. In transformers the old method of obtaining the short-circuit loss has been subjected to criticism on the ground that the condition of the iron when the transformer is under load as regards permeability is very different from its condition when one winding is short-circuited and full-load current circulated through the other. Equation (**17**) furnishes us with a theoretical basis for obtaining the value of the short-circuit losses with conditions of induction obtained under operation. As regards mutually inductive circuits in air, the difference between the open-circuit voltage of the secondary and the full-load voltage as shown by (**17**) is the mathematical equivalent of the impedance voltage obtained with the primary short-circuited and full-load current circulated in the secondary winding. With transformers there is a difference between these two quantities, due to the permeability being differ-

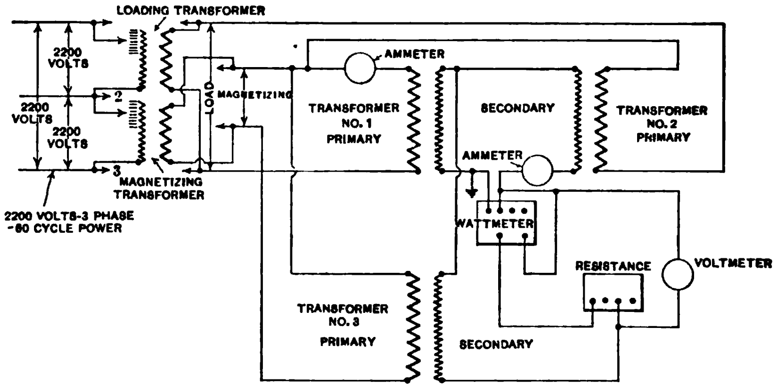


Fig. 1 —Scheme of Connections for Measuring Copper Loss and Impedance Volts of Transformer under Load (Opposition) at Different Power Factors.

ent at high and low induction. Thus for transformers (**17**) gives the short-circuit impedance and short-circuit loss with the same induction in the iron as that obtained under load, while (**12**) and (**13**) give the resistance and inductance obtained under short circuit when the induction is very low.

With a view to determining whether the conditions in the iron produce an appreciable change in the values of the short-circuit losses of a transformer, the following method, based on equation (**17**), of measuring impedance volts and short-circuit losses with load conditions, was used. Two transformers were loaded by the opposition method. A third transformer of like characteristics was connected with its primary in multiple with that one of the two loaded transformers of which the short-circuit losses and impedance under load condition was to be measured. The secondaries of the two transformers were connected so that the electromotive forces were opposed, and a wattmeter, the series coil of which was in the secondary circuit of the two loaded transformers, had its shunt energized by the difference of the secondary electromotive forces of the loaded and unloaded transformers. The wattmeter then, with proper corrections, reads the short-circuit loss corresponding to the condition of induction in the iron at the load that is given. The power factor of the load may be changed by exciting the loaded transformer from the different phases of a three-phase generator. (See Fig. 1.)

(4) Test Results

Tests were made on a 150-kv-a. 60-cycle transformer to ascertain if there were any variations in the values of the short-circuit losses under different power factors of load and change in excitation. The results obtained are tabulated. (See Table I.)

The first six results given in the table were obtained with exciting voltages 0, 500, 1000, 1500, 2000 and 2500; the losses being respectively 1164.4, 1164.4, 1164.4, 1169.4, 1176.4 and 1176.4 watts. The maximum variation in these values is a little over 1 per cent. The first value should be identical with the loss obtained by the short-circuit test, which is 1136 watts; it is seen to be 2½ per cent higher. This difference is partly due to a difference in temperature of the windings at the time the two tests were made and partly to a slight difference in the magnetic characteristics of the loaded and unloaded transformers. The remaining results show a difference in the measured values at full load, with power factors varying from 31 per cent to 99 per cent, of a little over 2 per cent. This difference is due entirely to the rise in temperature of the windings during test. This also is the cause of the difference between the value at zero excitation and that at maximum excitation and 31 per cent power factor. The principal results of these tests are given below.

Short-circuit loss obtained by standard method........	1136
" " " by method described, exciting volts zero.........................	1164
" " " maximum excitation, 31 per cent power factor......	1175.4
" " " " " 99 per cent power factor......	1189.4

Tests were made on a transformer of smaller size with similar results.

(5) Practical Formulas

The following formulas are based on the preceding investigation.

Regulation

$$I\,R = \frac{\text{short-circuit losses}}{\text{rated output}} \times 100$$

$$I\,X = \frac{\text{reactance volts}}{\text{scondary rated voltage}} \times 100$$

$P.F.$ = Power factor of load.

Per cent regulation = $(P.F.)\,I\,R + \sqrt{1 - (P.F.)^2}\,I\,X$ **(22)**

The following correction may be added where a higher degree of accuracy is required.

$$\text{Correction} = \frac{\{(P.F.)IX - \sqrt{1-(P.F.)^2}\,IR\}^2}{200} \qquad \textbf{(23)}$$

$I^2\,R$ and Stray Losses in Copper

If we define:

I_m = effective value of primary no-load, exciting current
I_2 = effective value of secondary load current
n_1 = number of primary turns
n_2 = number of secondary turns
R_1 = effective resistance of primary winding,

$$p = \frac{n_1\,I_m}{n_2\,I_2}$$

$$q = \frac{\left(\frac{n_2}{n_1}\,I_2\right)^2 R_1}{\text{Short-circuit loss}}$$

Total loss in copper

$$= \text{short-circuit loss} \times (1 + 2\,p\,q\sqrt{1-(P.F.)^2}) \qquad \textbf{(24)}$$

To illustrate this by an example: suppose a transformer has an exciting current of 10 per cent and its short-circuit loss is 2 per cent, what will be the correct copper loss at 80 per cent power factor, supposing that the portion of the short-circuit loss in the primary circuit is 60 per cent of the total value?

Per cent copper loss = per cent short-circuit loss $\times$ (1 + 2 $\times$ 0.1 $\times$ 0.6 $\times$ 0.6) = 2.14 per cent.

The above formula omits the effect of the hysteresis angle, which will slightly increase the correction, but the amount added thereby is too small to be of any importance. It will be noted that at 100 per cent power factor the correction is zero, and since efficiencies are normally figured at this power factor, it will be necessary to make a correction only when efficiencies are required at low power factors.

TABLE I.

SHORT-CIRCUIT LOSS AND IMPEDANCE VOLTS OBTAINED BY METHOD SHOWN IN FIG. 1, WITH VALUES OF IMPRESSED VOLTAGE RANGING FROM ZERO TO NORMAL RATED VALUE, AND WITH POWER FACTORS FROM 99 PER CENT DOWN TO 31 PER CENT. FREQUENCY 60 CYCLES.

Primary volts	Primary amperes	Secondary amperes	Power factor of load	Watts loss	Impedance volts
0	60.0	13.63	30.9	1164.4	276
500	60.2	13.63	30.9	1164.4	277
1000	60.7	13.63	30.9	1164.4	278
1500	60.9	13.63	30.9	1169.4	277
2000	61.3	13.63	30.9	1176.4	278
2500	62.6	13.63	31.6	1176.4	277
2500	58.0	13.63	*32.4	1176.4	276
2500	59.2	13.63	65.6	1189.4	276
2500	61.8	13.63	*69.0	1189.4	279
2500	61.4	13.63	99.2	1189.4	278
2500	59.4	13.63	*99.3	1179.4	277

The differences in the wattmeter measurements given in this table are due to the increase in temperature of the windings during the time the test was in progress. Satisfactory measurements of resistance were not obtained and therefore no correction has been made in the readings.

*In this case the transformer was receiving power at the secondary terminals, instead of at the primary terminals.

(6) Recommendations

In view of the test results given in Section 4 and others that have been obtained from time to time, the authors feel justified in recommending that the old standard method of obtaining short-circuit losses in transformers be retained. In special cases the method described in this paper may be used, but on account of the inconvenience of applying it, its general use is not recommended. It is recommended that the copper loss and regulation be calculated from the short-circuit loss and impedance voltage. The correction to the short-circuit loss to obtain the copper loss under load may be made according to the practical formula (**24**) which is derived from equation (**21**). The regulation may be figured according to formula (**22**), the correction for the quadrature component given by formula (**23**) being used where required. This well-known formula is based on equation (**17**) and gives very nearly exact values when the correction for the quadrature component is used. It is very much simpler than other less accurate formulas that are in common use.

List of Symbols used in Theoretical Discussion

L_1 = Primary open-circuit inductance
L_2 = Secondary open-circuit inductance
L_0 = Inductance of load
M = Mutual inductance between primary and secondary
R_1 = Primary resistance
R_2 = Secondary resistance
R_0 = Resistance of load
i_1 = Instantaneous value of primary current
i_2 = Instantaneous value of secondary current
e_1 = Primary impressed e.m.f.
p = $2\pi \times$ frequency
$\check{I}_1$ = Complex variable expression for primary current
$\check{I}_2$ = Complex variable expression for secondary current
$\check{I}_m$ = Complex variable expression for primary open-circuit current
$\check{E}_1$ = Complex variable expression for primary impressed e.m.f.
$\check{E}_2$ = Complex variable expression for secondary normal e.m.f. under load.
$\check{E}_0$ = Complex variable expression for secondary no-load e.m.f.
I_1 = Effective or root-mean-square value of primary current
I_2 = Effective or root-mean-square value of secondary current
I_m = Effective or root-mean-square value of primary open-circuit current
$|I_1|_n |I_2|_n |I_m|_n$ represent the effective value of the nth harmonic of the quantities $\check{I}_1$ $\check{I}_2$ $\check{I}_m$

$\hat{Z}_a$ = For simple sine waves, the effective impedance of the primary circuit under load. For any periodic wave it represents an operation equivalent to impedance on each harmonic of the wave, the value of the impedance changing with each harmonic.

$\hat{Z}_0$ = Similar operation to $\hat{Z}_a$ but representing impedance of load.

$R_a\ L_a$ = Resistance and inductance corresponding to Z_a

$R_b\ L_b$ = Resistance and inductance corresponding to short-circuit impedance.

$R_c\ L_c$ = Resistance and inductance corresponding to impedance of secondary with primary short-circuited.

ϕ_n = Phase angle for nth harmonic.

The symbols used in Section 5 of this paper are defined in the text.

A paper presented at the Midwinter Convention of the American Institute of Electrical Engineers, New York, February 27, 1913.

DETERMINATION OF LOAD LOSS CORRECTION FACTORS FOR ROTATING ELECTRIC MACHINES

BY E. M. OLIN AND S. L. HENDERSON

It is well known that certain of the losses occurring in rotating electric machines can be accurately determined from no-load measurements. Some of the losses, however, cannot be so determined, as, owing to conditions which develop, as load is applied, a gradual increase in these losses takes place.

The difference between the total losses under load and the sum of the separate losses as determined from no-load measurements is commonly known as "load loss" or "stray loss."

This paper will present data relating to the load losses of certain classes of machines and will describe the methods employed to secure the data.

The classes considered are the following:

a. Direct-current motors and generators.

b. Alternating-current generators and synchronous motors.

c. Synchronous converters.

At any given load the only losses occurring in these types which cannot be accurately determined from no-load measurements are the so-called "core loss" and the loss due to eddies set up in the armature conductors by the stray fields of useful currents flowing in these conductors.

In a paper* previously prepared by one of the present writers it was proposed to apply empirical correcting factors to the values of core loss and armature copper loss as determined from no-load measurements in order to arrive at their true values under load.

**Determination of Power Efficiency of Rotating Electric Machines*, E. M. Olin, TRANS. A. I. E. E., 1912, Vol. XXXI, Part II, p. 1695.

Two constants were suggested, the first to be applied to the measured no-load core loss to compensate for the increased loss due to field distortion under load, the second to be applied to the calculated armature I^2r loss to compensate for eddies in the conductors due to the application of load. These eddies include the secondary losses of commutation as well as those set up by useful currents flowing in the conductors.

Since that paper was written, tests have been conducted with a view to establishing the values of these constants. Very early during the progress of these tests it was found that it would not be practicable to separate the increased loss due to load into its two component parts, namely the increase in core loss due to field distortion, and the increase in copper loss due to eddies in armature conductors.

In other words, two separate constants could not be determined, as no satisfactory method could be worked out for arriving at their values. It was found, however, that in each type of machine a fairly uniform ratio exists between the sum of these losses as determined from no-load measurements and their sum as determined from actual load tests.

This relation may be expressed thus: At any given load,

$$\textit{actual core loss} + \textit{actual armature copper loss}$$
$$= \lambda\,(\textit{measured no-load core loss} + \textit{calculated armature } I^2r \textit{ loss}),$$

where λ is a constant depending upon the type of machine and upon the magnitude of the load.

In order to determine the values for this constant, special tests were conducted on each type of machine. In these tests every possible refinement was employed. The most approved methods were used, and the most expert men. All instruments were specially calibrated and great care was used to locate them where they would not be influenced by stray fields. To avoid fluctuations special machines were used to generate the power. Each reading is the average of a series of ten readings taken at intervals of ten seconds. These tests will be referred to throughout as " laboratory " tests to distinguish them from the ordinary commercial tests recorded herein, a large number of which were examined with a view to checking further the values for the constants.

Direct-Current Machines

A convenient way of determining the load losses of direct-current generators or motors is by measuring the power supplied from outside sources to a pair of coupled machines loaded one against the other by the well-known " pumping back " method. In this test the shafts of two similar machines are rigidly coupled together mechanically, the armatures are connected in parallel electrically and current is made to flow between them by adjusting the field strengths to get the required difference in e.m f's.

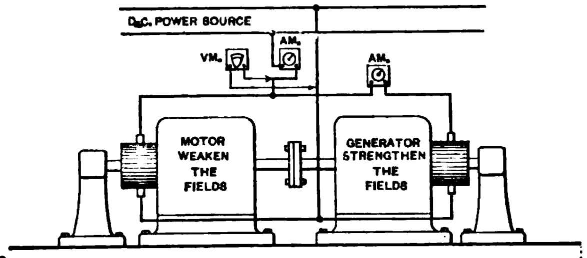


FIG. 1—" LOADING-BACK " TEST OF DIRECT-CURRENT MACHINES—LOSSES SUPPLIED ELECTRICALLY

There are two ways of making the test. In one the losses are supplied by direct electrical connection to the armature circuit of the machines under test, as in Fig. 1; in the other the losses are supplied mechanically by means of a calibrated driving motor, as in Fig. 2.

In either case the power supplied from outside sources rep-

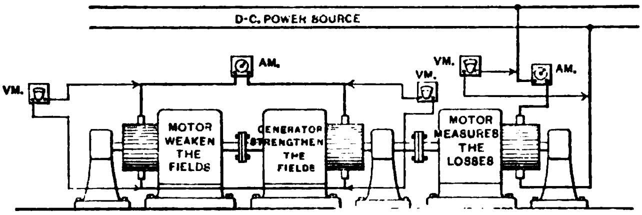


FIG. 2—" LOADING-BACK " TEST OF DIRECT-CURRENT MACHINES—LOSSES SUPPLIED MECHANICALLY

resents the combined losses of the two machines. By a comparison of these losses with the separate losses as calculated from no-load measurements, the increased loss under load due to field distortion and eddies in armature conductors can be determined. If possible, machines exactly alike in characteristics should be used to get the best results.

Referring to Fig. 1, it is to be noted that the machine which operates as a motor will take a greater armature current than that operating as a generator by the amount of loss current supplied from the outside source. Due allowance must be made for this when computing the separate losses. The internal voltage of each machine is calculated at any load from the known armature current flowing, the resistance of windings and brushes and the known terminal voltage.

The no-load core loss of each machine corresponding to these internal voltages is found by referring to core loss characteristics previously determined by the separate driving motor method. The I^2r losses are computed from the measured resistances and the known current flowing in each machine. The frictional

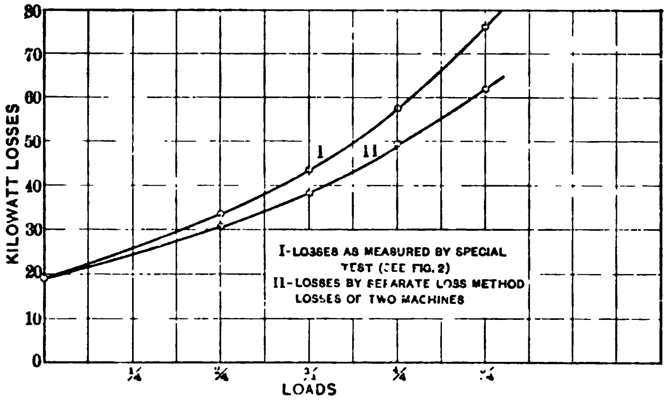


FIG. 3—470-H.P., 500-VOLT D-C. MOTOR, COMMUTATING POLE, 415 REV. PER. MIN.

losses can be measured by the separate driving motor method or computed from a comparison of the no-load power input reading and the core loss characteristics.

When the losses are supplied mechanically, as shown in Fig. 2, the same current circulates in the armature circuit of each machine, but the internal voltages are somewhat different. The separate losses are computed as before.

Laboratory tests were conducted on a pair of 470-h.p. direct-current commutating-pole motors of the most modern design, according to the method described above, with the losses supplied mechanically as shown in Fig. 2. The results of this test are shown in Fig. 3, and in Table I.

In addition to the test described above, the readings taken on

TABLE I

LABORATORY TESTS-DETERMINATION OF CONSTANT λ—DIRECT-CURRENT MACHINES

Values for two machines								Values for one machine	
Machine	Fractional loads	*A* Losses supplied by driving motor kw.	*B* Total losses from no-load readings kw.	*C* Load loss $A - B$ kw.	*D* No-load core loss kw.	*E* Calculated armature I^2r loss kw.	λ $\frac{C + D + E}{D + E}$	Efficiencies by separate losses: Without constant λ	Efficiencies by separate losses: With constant λ
470 h.p. motors, 550 volts	2/4	33.1	30.96	2.04	13.7	4.22	1.11	91.4	90.9
commutating pole 415 rev. per min.	3/4	43.15	38.57	4.58	14.0	8.61	1.20	92.3	91.6
(See Fig. 2)	4/4	57.3	49.34	7.96	13.85	15.34	1.27	92.8	91.7
for special test	5/4	76.2	61.44	13.76	13.75	23.43	1.37	92.5	91.1

a pair of 3750-kw. commutating-pole generators which happened to be on the test floor at the same time, were carefully gone over with a view to getting further information as to the values of the constant.

These generators were tested by the " pumping back " method with electrical loss supply, as shown in Fig. 1. Conditions were not as favorable as in the case of the 470-h.p. motors, owing to fluctuations of the power circuit supplying the losses, due partly to the inertia of the heavy rotors. Hence we have not termed this a " laboratory " test. Nevertheless, fairly satisfactory readings were obtained at full load and the value of the correction factor was found to be 1.32.

A number of commercial input-output tests were then examined. Some of these were of motors, with the mechanical output measured by Prony brake. Others were of motor-

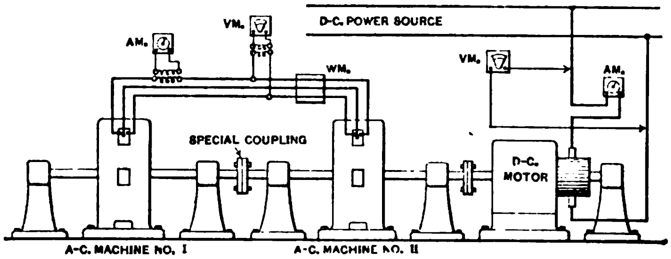


FIG. 4—" LOADING-BACK " TEST OF ALTERNATING-CURRENT MACHINES

generators, where both input and output were measured electrically. The input-output efficiency was in each case checked against the efficiency by separate losses corrected by the factor λ, using values of this constant as found in the tests just described. Table II shows the results of this comparison.

ALTERNATING-CURRENT MACHINES

The load losses of alternating-current generators and synchronous motors may be conveniently arrived at as follows: Two machines made from the same specifications (and therefore of the same wave form) are rigidly connected together and driven from a calibrated motor, as shown in Fig. 4. The rigid connection between the a-c. machines is made by means of a special coupling so arranged that the position of the rotating elements with respect to each other can be varied. There will be certain positions of the rotors when the armature circuits can be paralleled

TABLE II

DIRECT-CURRENT MACHINES—COMMERCIAL TESTS

Machine	Fractional loads	*A* Core loss at no-load	*B* Calculated armature I^2r loss	λ as determined by special test	$\lambda (A + B)$	Total losses from no-load readings: Without correction factor	Total losses from no-load readings: With correction factor λ	Kilowatts output or input	Efficiency by losses: Without correction factor	Efficiency by losses: With correction facto:	Efficiency by input-output
* 30-h.p., 230-volt, 975 rev. per min. d-c. commutating pole motor	2/4	0.433	0.155	1.1	0.646	1.64	1.70	12.97	87.4	86.7	86.3
	3/4	0.428	0.310	1.2	0.885	1.95	2.10	19.06	89.8	89.1	88.2
	4/4	0.418	0.552	1.3	1.26	2.31	2.60	25.08	90.7	89.6	89.2
	5/4	0.405	0.856	1.4	1.74	2.82	3.30	31.12	90.9	89.4	89.8
*35-h.p., 230-volt, 1150 rev. per min. d-c. commutating pole motor	2/4	0.783	0.150	1.1	1.081	2.02	2.07	15.60	87.0	86.2	83.7
	3/4	0.770	0.322	1.2	1.205	2.4	2.55	22.70	89.4	88.5	86.3
	4/4	0.755	0.572	1.3	1.72	2.77	3.16	29.70	90.6	89.3	87.9
	5/4	0.740	0.892	1.4	2.28	3.35	4.00	36.80	91.0	89.3	88.8
*60-h.p., 230-volt, 650 rev. per min. d-c. commutating pole motor	2/4	1.50	0.134	1.1	1.8	3.57	3.74	25.96	86.4	85.6	86.3
	3/4	1.48	0.29	1.2	2.12	3.90	4.25	28.10	89.8	88.9	88.2
	4/4	1.46	0.50	1.3	2.55	4.32	4.91	50.20	91.4	90.1	89.2
	5/4	1.44	0.765	1.4	3.08	5.75	6.63	62.0	92.3	90.8	90.2
†160-kw., 400-volt, 750 rev. per min. d-c. commutating pole generator	4/4	1.98	4.39	1.3	8.28	12.16	14.07	160.0	93.0	92.0	92.3
†260-h.p., 550-volt, 750 rev. per min. d-c. commutating pole motor	4/4	3.25	3.14	1.3	8.30	12.00	13.91	187.0	93.5	92.6	92.8
†175-kw., 550-volt, 600 rev. per min. d-c. commutating pole generator	5/4	8.5	4.11	1.4	17.63	19.71	24.73	219	91.7	90.0	89.5

TABLE II—*Continued.*

Machine	Fractional loads	A Core loss at no-load	B Calculated armature I^2r loss	λ as determined by special test	λ (A + B)	Total losses from no-load readings: Without correction factor	Total losses from no-load readings: With correction factor λ	Kilowatts output or input	Efficiency by losses: Without correction factor	Efficiency by losses: With correction factor	Efficiency by input-output
†260-h.p., 220-volt, 600 rev. per min. d-c. commutating pole motor	5/4	2.4	9.26	1.4	16.30	21.74	26.38	271.8	92.0	90.3	90.0
200-kw. syn. conv., as direct-current generator, 575-volt, three-phase, 60 cycles, 1200 rev. per min. (See Fig. 12.)	2/5	8.05	0.50	1.09	9.32	15.7	16.5	85.1	84.5	83.9	84.0
	2/4	8.10	0.97	1.10	10.1	16.7	17.7	112.5	87.1	86.4	86.5
	3/4	8.20	1.70	1.20	11.88	17.8	19.8	148.0	89.4	88.3	88.5
	4/4	8.30	2.60	1.30	13.73	19.0	21.8	183.1	90.5	89.3	89.3
	5/4	8.35	3.58	1.30	15.75	20.2	24.0	214.5	91.4	90.0	89.7
*20-h.p., 230-volt, 650 rev. per min. d-c. commutating pole motor	2/4	0.244	0.166	1.1	0.451	1.15	1.19	8.76	86.9	86.3	85.2
	3/4	0.240	0.39	1.2	0.756	1.45	1.58	12.77	88.7	87.7	87.6
	4/4	0.230	0.685	1.3	1.19	1.85	2.03	17.00	89.2	87.5	87.9
	5/4	0.218	1.044	1.4	1.70	2.45	2.89	21.50	88.7	86.2	86.9
*20-h.p., 115-volt, 650 rev. per min. d-c. commutating pole motor	2/4	0.270	0.166	1.1	0.48	1.36	1.40	8.87	84.7	84.1	84.1
	3/4	0.263	0.374	1.2	0.765	1.64	1.77	13.04	87.3	86.5	85.8
	4/4	0.248	0.674	1.3	1.20	2.12	2.40	17.0	87.7	86.2	86.3
	5/4	0.230	1.05	1.4	1.791	2.83	3.34	21.73	87.0	84.8	85.9
*25-h.p., 115-volt, 825 rev. per min. d-c. commutating pole motor	2/4	0.450	0.151	1.1	0.662	1.65	1.71	11.23	85.3	84.6	83.0
	3/4	0.440	0.236	1.2	0.881	2.0	2.18	16.38	87.8	86.9	85.4
	4/4	0.420	0.591	1.3	1.314	2.55	2.85	21.75	88.4	87	85.7
	5/4	0.400	0.956	1.4	1.898	3.23	3.77	27.36	88.3	86.4	85.1
3750-kw., 275-volt, commutating pole d-c. generator 180 rev. per min. (See Fig. 1.)	4/4	50.0	65.8	1.3	150.5	239.9	274.6	3750	94.0	93.2	

* Mechanical output by Prony brake. † Part of motor-generator.

without a flow of cross currents between the machines, in other words, when a condition of synchronism exists. If now the two rotors are shifted slightly with respect to each other, current will tend to flow between the machines, depending in amount on the degree of angular displacement between the rotors.

Any desired load, at practically unity power factor, can be obtained by this method. The power delivered at the shaft of the coupled machines by the calibrated driving motor represents their losses under load conditions.

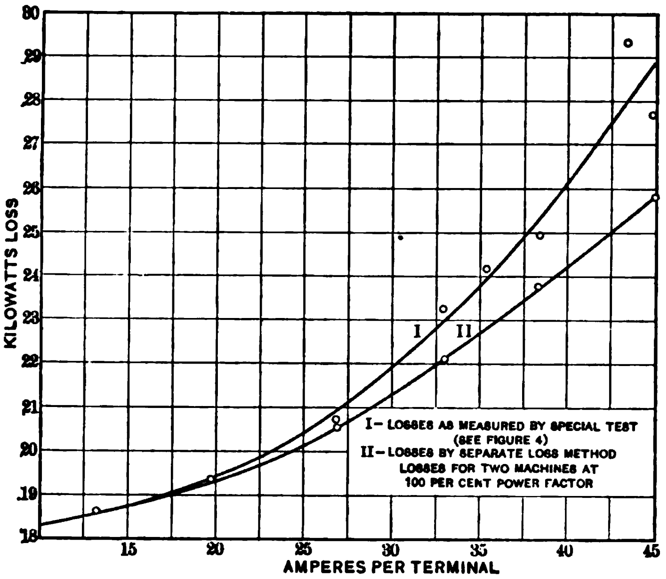


FIG. 5—ALTERNATING-CURRENT GENERATOR, 150 KV-A., 2400 VOLTS, THREE-PHASE, 60 CYCLES, 900 REV. PER. MIN.

By comparing the separate losses as computed from no-load measurements with the losses as shown in the tests described above, the increased loss due to the application of load can be determined.

The machines selected for investigation were two 150-kv-a. 2400-volt 60-cycle belted generators built from the same specifications.

The tests were conducted as described above. (See also Fig. 4.) Laboratory methods were employed. The results are as shown in Table III and Fig. 5.

TABLE III—LABORATORY TESTS

DETERMINATION OF CONSTANT λ—ALTERNATING-CURRENT MACHINES. (SEE FIG. 4)

Machine	Fractional loads	Two machines						One machine	
		A	B	C	D	E		Efficiency by losses	
		Losses supplied by driving motor kw.	Total no-load losses kw.	Load loss $= A - B$ kw.	No-load core loss kw.	Calculated armature I^2r loss kw.	λ $\frac{C+D+E}{D+E}$	Without correcting factor	With correcting factor
150 kv-a., three-phase, 2400-volt, 60-cycle a-c. generator	2/4	19.09	19.07	0.02	8.66	1.14	1.002	88.7	88.68
	3/4	20.94	20.67	0.27	8.66	2.55	1.024	91.7	91.6
	4/4	24.19	22.97	1.22	8.66	4.55	1.092	92.9	92.5
	5.4	28.98	25.88	3.10	8.66	7.10	1.197	93.5	92.8

Synchronous Converters

The methods previously described for determining the load losses of direct- and alternating-current motors and generators are not well adapted for synchronous converters. Our experience indicates that the introduction of the necessary boosters in the armature leads complicates the situation to such an extent that the results cannot be relied upon.

For this class of machines the straight input-output method was used, with the following modification. Two identical synchronous converters were used, one operating d-c. to a-c. to feed a second machine operating as a straight converter. The advantages are: first, the wave forms of the power circuit (the d-c. to a-c. converter) and of the other converter (the a-c. to d-c. converter) are identical; second, the ove-rall efficiency of the two synchronous converters can be computed by a comparison of the direct-current input with the direct-current output. This is an advantage, for d-c. measuring instruments are more reliable and give steadier readings than the a-c. instruments. Fig. 6 shows the scheme of connections used for this test.

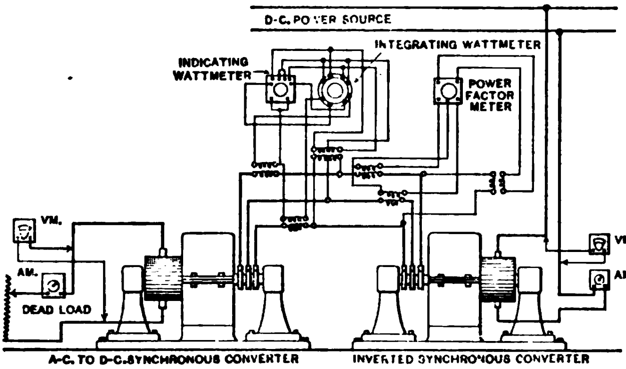


Fig. 6—Input-Output Test of Synchronous Converters

The laboratory tests were made on 60-cycle non-commutating-pole synchronous converters. Two different pairs of synchronous converters were tested and the results are shown in Tables IV and V and in Figs. 7, 8, 9 and 10. At each load, ten readings were taken at ten-sec. intervals and an average taken for each point. It will be seen that the results in the two tests check very well together and go to show that the load loss is small, up to and including full load. On the heavy overloads serious sparking at the brushes occurs and the load loss factor increases rapidly.

TABLE IV

LABORATORY TESTS—DETERMINATION OF CONSTANT λ—SYNCHRONOUS CONVERTERS

(SEE FIG. 6)

Machine	Fractional loads	A Losses from input-output kw.	B Total losses from no-load readings kw.	C Load loss A — B kw.	D No-load core loss kw.	E Calculated armature I^2r loss kw.	λ $\frac{C + D + E}{D + E}$ kw.	Efficiency by separate losses	Efficiency by input-output
200-kw., 625-volt, three-phase, 60 cycles 1200 rev. per min. (See Fig. 6)	2/4	13.19	13.0	0.19	4.36	0.51	1.04	88.5	88.3
	3/4	14.18	13.82	0.36	4.39	1.12	1.07	91.6	91.4
	4/4	15.6	14.9	0.70	4.41	1.99	1.10	93.1	92.9
	5/4	17.6	16.4	1.2	4.44	3.12	1.16	93.8	93.4
	6/4	20.0	17.99	2.01	4.47	4.49	1.22	94.3	93.8
200-kw., 575-volt, three-phase, 60 cycles 1200 rev. per min. (See Fig. 6)	2/4	16.35	16.03	0.32	8.0	0.43	1.04	86.2	85.9
	3/4	17.58	16.8	0.70	8.1	0.95	1.08	90.0	89.6
	4/4	19.22	17.92	1.18	8.15	1.71	1.12	91.8	91.3
	5/4	21.16	19.22	1.89	8.2	2.74	1.17	92.9	92.3
	6/4	23.2	20.73	2.47	8.3	3.84	1.25	92.5	92.8

TABLE V

LABORATORY TESTS—DETERMINATION OF CONSTANT λ—SYNCHRONOUS CONVERTERS—INVERTED

(SEE FIG. 6)

	Fractional loads	A Losses from input-output kw.	B Total losses from no-load readings kw.	C Load loss $A - B$ kw.	D No-load core loss kw.	E Calculated armature I^2r loss kw.	λ $\frac{C + D + E}{D + E}$	Efficiency by separate losses	Efficiency by input-output
200 kw., 625-volt,	2/4	12.35	12.17	0.18	4.3	0.73	1.04	90.5	90.4
three-phase, 60 cycles	3/4	13.69	13.17	0.52	4.35	1.44	1.09	92.6	92.3
1200 rev. per min.	4/4	15.5	14.52	0.98	4.42	2.42	1.14	93.8	93.3
	5/4	17.88	16.16	1.72	4.48	3.61	1.21	94.3	93.7
(See Fig. 6)	6/4	20.45	18.00	2.45	4.57	5.05	1.26	94.7	94.0
200 kw., 575-volt	2/4	16.2	15.9	0.3	8.3	0.67	1.03	87.9	87.6
three-phase, 60 cycles	3/4	17.64	17.0	0.64	8.5	1.30	1.07	90.7	90.3
1200 rev. per min.	4/4	20.0	18.21	1.79	8.6	2.16	1.17	92.3	91.7
	5/4	22.82	20.2	2.62	9.0	3.25	1.21	93.2	92.5
(See Fig. 6)	6/4	25.7	21.85	3.85	9.1	4.39	1.29	93.7	92.6

The results from a number of commercial tests of 60-cycle synchronous converters are shown in Table VI. The efficiencies by input-output in all cases check very closely with those computed from the separate losses, using the correction factor 1.1 at full rated output, the value indicated by the laboratory tests.

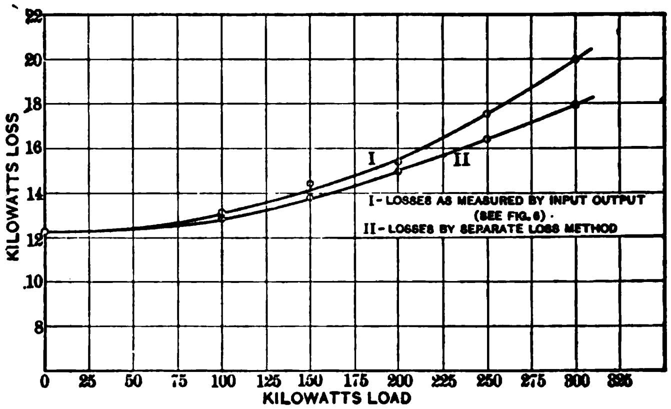


FIG. 7—SYNCHRONOUS CONVERTER, 200 KW., 625 VOLTS, 60 CYCLES, THREE-PHASE, 1200 REV. PER. MIN., NON-COMMUTATING POLE

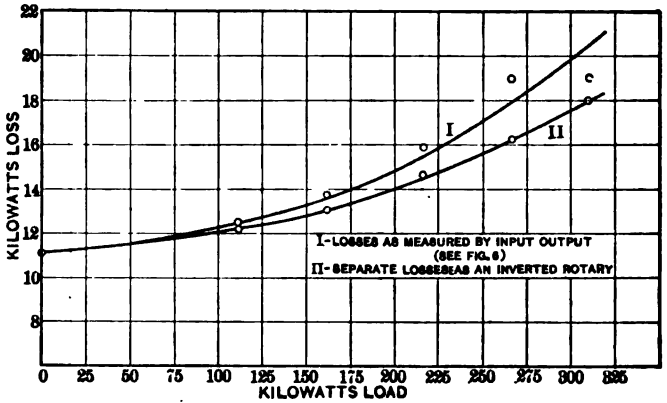


FIG. 8—SYNCHRONOUS CONVERTER INVERTED, 200 KW., 625 VOLTS, 60 CYCLES, THREE-PHASE, 1200 REV. PER. MIN., NON-COMMUTATING POLE

No laboratory tests were made on 25-cycle synchronous converters or on commutating-pole converters of either frequency. Results from a number of commercial tests shown in Table VII seem to indicate, however, that if there is any load loss on 25-cycle converters it is so small that it may be neglected.

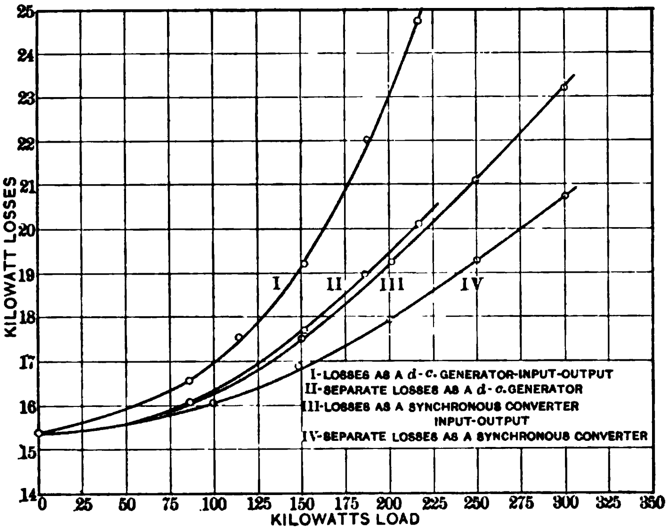


FIG. 9—SYNCHRONOUS CONVERTER, 200 KW., 575 VOLTS, 60 CYCLES, THREE-PHASE, 1200 REV. PER. MIN., NON-COMMUTATING POLE

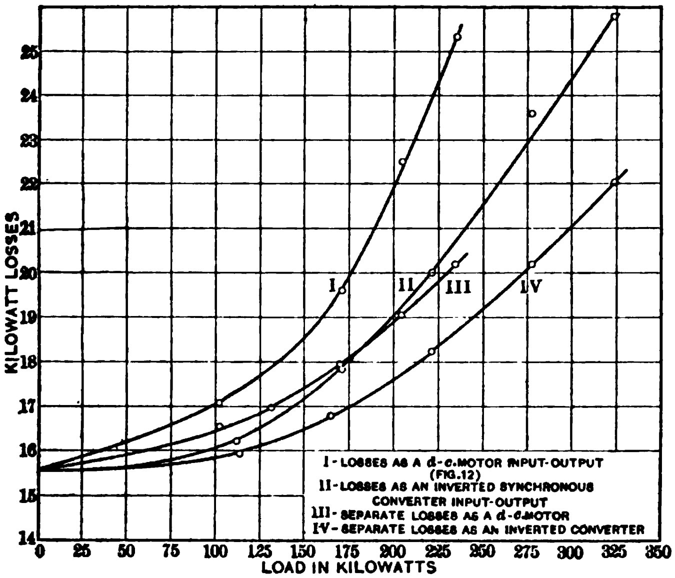


FIG. 10—SYNCHRONOUS CONVERTER INVERTED, 200 KW., 575 VOLTS, 60 CYCLES, THREE-PHASE, 1200 REV. PER. MIN.. NON-COMMUTATING POLE

TABLE VI

60-CYCLE SYNCHRONOUS CONVERTERS—COMMERCIAL TESTS—FULL-LOAD READINGS

Machine	Core loss at no-load	Calculated armature I^2r	λ as determined by special test (See Fig. 6)	$\lambda(A + B)$	Kw. output	Total losses from no-load readings		Efficiency by separate losses		Efficiency by input output
						Without correction factor	With correction factor	Without correction factor	With correction factor	
200-kw., 275-volt, three-phase, 1200 rev. per min.	6.0	1.22	1.10	7.95	200.7	16.25	16.98	92.5	92.0	92.0
300-kw., 250-volt, three-phase, 514 rev. per min.	6.0	2.14	1.10	8.95	298.4	21.07	21.88	93.5	93.2	93.3
300-kw., 250-volt, two-phase, 900 rev. per. min.	7.30	1.47	1.10	9.65	299.9	20.56	21.44	93.4	93.3	93.1
500-kw., 250-volt, six-phase, 720 rev. per min.	5.95	4.16	1.10	11.12	504	25.92	26.93	95.0	94.7	94.6
1000-kw., 600-volt, six-phase, 600 rev. per min.	11.3	10.0	1.10	23.42	961	60.28	62.40	94.1	93.9	93.9
1000-kw., 600-volt, six-phase, 600 rev. per min.	10.1	8.21	1.10	20.15	1010	61.66	63.50	94.2	94.0	93.9

TABLE VII

25-CYCLE SYNCHRONOUS CONVERTERS—COMMERCIAL TESTS—FULL-LOAD READINGS

	Core loss at no-load	Calculated armature I^2r loss	Total loss from no-load readings	Kw. output	Efficiency by losses	Efficiency by input-output	Difference in efficiency
200-kw., 600-volt, three-phase, 750 rev. per min.	2.60	2.17	10.16	199.2	95.1	93.1	—2.0
300-kw., 600-volt, three-phase, 750 rev. per min.	2.4	4.92	13.0	293	95.7	96.4	+0.7
300-kw., 650-volt, three-phase, 750 rev. per min.	3.48	4.3	13.46	296.4	95.6	95.2	—0.4
300-kw., 650-volt, three-phase, 750 rev. per min.	3.95	4.59	14.78	319.7	95.6	94.8	—0.8
300-kw.,650-volt,three-phase, 750 rev. per min.	3.70	4.30	14.10	300.3	95.5	96.1	+0.6
300-kw., 650-volt, three-phase, 750 rev. per min.	4.20	4.21	15.11	299.3	95.2	93.5	—1.7
300-kw., 600-volt, three-phase, 750 rev. per min.	3.10	4.42	13 36	295	95.7	96.2	+0.5
500-kw., 600-volt, six-phase, 500 rev. per min.	10.6	2.04	21.43	523	96.1	95.6	—0.5
500-kw., 600-volt, six-phase, 500 rev. per min.	9.0	1.97	18.81	500.8	96.4	97.9	+1.5
500-kw., 600-volt, six-phase, 500 rev. per min.	13.4	2.06	25.37	525.2	95.4	96.5	+1.1
500-kw., 600-volt, six-phase, 500 rev. per min.	8.8	2.04	17.12	498	96.7	95.2	—1.5

TABLE VII—*Continued*

	Core loss at no-load	Calculated armature I^2r loss	Total loss from no-load readings	Kw. output	Efficiency by losses	Efficiency by input-output	Difference in efficiency
500-kw., 600-volt, six-phase, 500 rev. per min.	11.2	2.04	28.1	521.5	95.9	95.0	—0.9
500-kw., 600-volt, six-phase, 500 rev. per min.	8.8	2.04	25.8	538	95.4	94.3	—1.1
500-kw., 1200-volt, three-phase, 750 rev. per min.	5.76	8.1	23.32	499.5	95.5	95.3	—0.2
500-kw., 1200-volt, three-phase, 750 rev. per min.	4.05	8.88	19.81	501.4	96.2	97.0	+0.8
500-kw., 1200-volt, three-phase, 750 rev. per min.	4.90	8.65	22.11	500.6	95.8	96.6	+0.8
500-kw., 1200-volt, three-phase, 750 rev. per min.	4.50	8.95	21.24	499.6	95.9	96.7	+0.8
500-kw., 1200-volt. three-phase, 750 rev. per min.	4.98	8.82	22.76	500.4	95.7	96.5	+0.8
1000-kw., 650-volt, six-phase, 500 rev. per min.	11.50	5.57	36.72	1007	96.5	97.3	+0.8
2000-kw., 600-volt, six-phase, 375 rev. per min.	13.85	18.8	64.6	2008	96.9	98.0	+1.1

*Average difference in efficiency, 0.02.

Readings were taken at different loads on a 25-cycle commutating-pole machine during a temperature test while the meters were swinging somewhat due to variations in the power supply. The results from these readings are plotted in Fig. 11. This curve further indicated that load loss in the case of 25-cycle converters is a negligible quantity and that the efficiency calculated from the separate loss without correcting factors is sufficiently accurate for all commercial purposes.

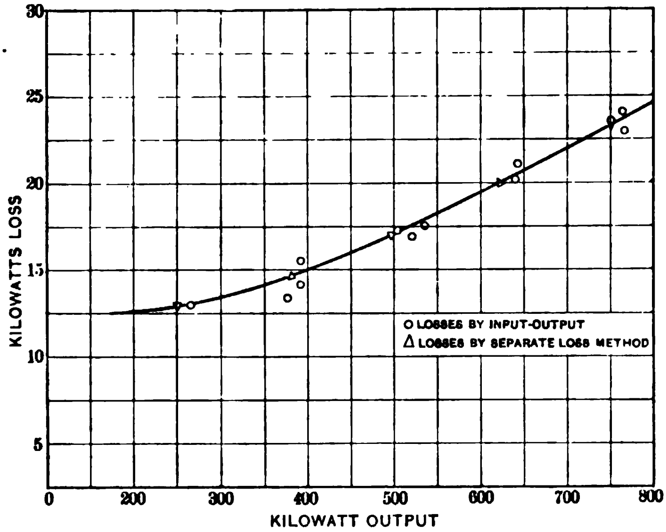


FIG. 11—SYNCHRONOUS CONVERTER, 500 KW., 600 VOLTS, SIX-PHASE, 25 CYCLES, 750 REV. PER. MIN., COMMUTATING POLE—COMMERCIAL TEST

SYNCHRONOUS CONVERTER AS D-C. GENERATOR

In addition to the straight input-output test as described above, a second test was made on the same synchronous converters, operating them as direct-current machines. (See Fig. 12).

The shafts of the two converters were coupled together mechanically and the pair of machines was operated as a d-c. to d-c. motor-generator. A direct comparison of the performance of this type as a synchronous converter and as a d-c. generator was thus obtained. The results are given in Figs. 9 and 10 and show very well the increase in load loss when operating as direct-current machines.

TABLE VIII. MOTOR-GENERATOR SETS—COMMERCIAL TESTS—FULL-LOAD READINGS.

λ for direct-current generator = 1.3 λ for synchronous motor = 1.1

Machine	Synchronous motor: A Core loss at no-load	B Calculated armature I^2r loss	$\lambda (A+B)$	Kw. input	Total losses from no-load reading, Correction factor: Without	With	Direct-current generator: C Core loss at no-load	D Calculated armature I^2r loss	$\lambda (C+D)$	Kw. output	Total losses no-load readings, Correction factor: Without	With	Set: Over-all efficiency by separate losses, Correction factor: Without	With	Efficiency by input-output
200-kw., 250-volt, commutating pole generator 290-h.p., 2200-volt, three-phase, 25-cycle Synchronous motor 750 rev. per min.	3.2	2.6	6.4	23.5	10.03	10.63	5.6	3.62	12	200	18.92	21.70	87.1	86.0	85.1
Three-piece set Two 250-kw., 650-volt, commutating pole d-c. generators 720-h.p., 13,200-volt three-phase, 60-cycle Synchronous motor 720 rev. per min.	15.5	7.9	25.78	570	31.4	33.6	5.6 5.5	4.88 4.95	13.61 13.59	248.5 248.5	20.13 20.10	23.27 23.24	87.4	86.1	87.3
400-kw., 250-volt, commutating pole d-c. generator 580-h.p., 220-volt, three-phase, 30-cycle Synchronous motor 600 rev. per min.	20.2	1.61	24	465.6	31.06	33.24	6.6	5.63	15.9	400	33.16	36.83	86.1	85.1	85.8

TABLE VIII—*Continued.*

Machine	Synchronous motor						Direct-current generator						Set		
	A Core loss at no-load	B Calculated armature I^2r loss	$\lambda (A+B)$	Kw. input	Total losses from no-load reading, Correction factor		C Core loss at no-load	D Calculated armature I^2r loss	$\lambda (C+D)$	Kw. output	Total losses no-load readings, Correction factor		Over-all efficiency by separate losses, Correction factor		Efficiency by input-output
					Without	With					Without	With	Without	With	
400-kw., 600-volt, commutating pole d-c. generator 580-h.p., 2200-volt three-phase, 60-cycle Synchronous motor 720 rev. per min.	9.0	4.2	14.52	453	21.3	22.6	4.0	6.06	13.08	400	23.86	26.88	89.8	88.9	88.3
1000-kw., 275-volt, commutating pole d-c. generator 1440-h.p., 5500-volt, two-phase, 60-cycle Synchronous motor 514 rev. per min.	36.0	8.6	49.1	1159	66.5	71	24.9	9.71	45	1000	74.51	84.9	87.7	86.6	86.3
1000-kw., 550-volt commutating pole d-c. generator 1440-h.p., 2200-volt, two-phase, 60-cycle Synchronous motor 514 rev. per min.	16.7	11.05	30.5	1124	46.26	48.96	11.60	13.35	32.4	1000	62.27	69.75	90.2	89.4	88.9

TABLE VIII—*Continued.*

Machine	Synchronous motor						Direct-current generator						Set		
	A Core loss at no-load	B Calculated armature I^2r loss	$\lambda (A+B)$	Kw. input	Total losses from no-load reading, Correction factor: Without	Total losses from no-load reading, Correction factor: With	C Core loss at no-load	D Calculated armature I^2r loss	$\lambda (C+D)$	Kw. output	Total losses no-load readings, Correction factor: Without	Total losses no-load readings, Correction factor: With	Over-all efficiency by separate losses, Correction factor: Without	Over-all efficiency by separate losses, Correction factor: With	Efficiency by input-output
1000-kw., 600-volt, commutating pole d-c. generator 1440-h.p., 4600-volt, two-phase, 60-cycle Synchronous motor 514 rev. per min.	18.0	7.85	28.45	1143	50.11	52.7	32.2	11.3	56.5	1000	82 47	95.51	88.3	87.2	87.5

125 PER CENT LOAD READINGS

λ—direct-current generator = 1.4 λ—synchronous motors = 1.2

Machine	A	B	$\lambda (A+B)$	Kw. input	Without	With	C	D	$\lambda (C+D)$	Kw. output	Without	With	Without	With	Efficiency by input-output
400-kw., 600-volt, commutating pole d-c. generator 580-h.p., 2200-volt, three-phase, 60-cycle Synchronous motor 720 rev. per min.	8.9	6.1	18	561	25.25	28.25	4.1	8.8	18.07	500	27.45	32.6	90.5	89.2	89.1
240-kw., 500-volt, commutating pole d-c. generator 370-h.p., 370-volt, three-phase, 60-cycle 1200 rev. per min.	5.2	4.3	11.4	342	14.8	16.7	4.6	7.05	16.3	300	22.8	27.5	88.9	87.1	87.8
150-kw., 250-volt, commutating pole d-c. generator 225-h.p., 440-volt, three-phase, 60-cycle 900 rev. per min.	8.3	2.8	13.32	226.5	15.0	17.22	6.2	4.4	14.86	187.5	19.1	23.35	84.6	82.2	82.7

Motor-Generator Sets

A number of motor-generator sets were examined and the constants determined from the laboratory tests were applied to the sum of the no-load core losses and calculated armature copper losses of each machine. The over-all efficiency of the set from the corrected losses was then calculated and compared with the over-all efficiency from the input-output readings taken during commercial tests. The results are shown in Table VIII.

While an examination of these results shows some discrepancies, it must be remembered that the input-output tests are merely of the commercial variety and are probably as nearly correct as average tests of that description.

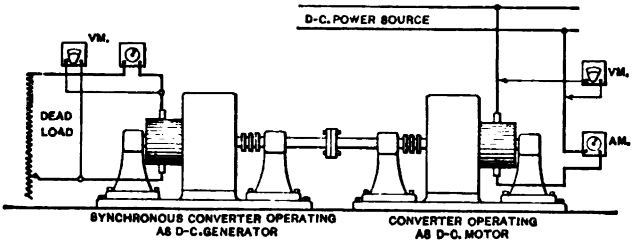


Fig. 12—Input-Output Test of Synchronous Converters as Direct-Current Machines

Recommendations

As a result of our investigation we suggest the use of correction factors for computing from the separate losses, the efficiencies of the rotating electric machines discussed in this paper, namely—a-c. generators, synchronous motors, d-c. generators, d-c. motors, and synchronous converters. The said correction factors to be applied to the sum of the no-load core loss and the armature I^2r loss, as calculated from no-load measurements of resistance. We submit the following values as being approximately correct for well-designed apparatus of the types mentioned:

Fractional loads	Value of constant 2/4	3/4	4/4	5/4
Direct-current generator, Direct-current motor	1.1	1.2	1.3	1.4
Alternating-current generator, Synchronous motor — *Stranded conductors	1.01	1.03	1.10	1.2
†60-cycle synchronous converter	1.04	1.07	1.10	1.2
All 25-cycle synchronous converters	1	1	1	1

* Does not apply to machines with deep strap conductors, in which the eddy current loss in the copper may be large.

† Non-commutating-pole type. Tests on commutating-pole type not completed.

These values apply only to well-designed machines, as shown by moderate losses, low temperatures and satisfactory behavior at the commutator.

In the case of machines having undue temperature elevations or those showing faulty commutation, the load losses may be of considerable magnitude and the above constants will not hold good. Such machines should be investigated individually, according to the method described herein, if the true efficiency is to be determined.

A paper presented at the Midwinter Convention of the American Institute of Electrical Engineers, New York, February 27, 1913.

LOAD LOSSES OF ALTERNATING-CURRENT GENERATORS

BY W. J. FOSTER AND EDGAR KNOWLTON

About twenty years ago it began to be recognized by designers of alternators that certain losses existed under load that were not included among the " segregated losses " taken into consideration in determining the efficiency. It was natural to give to these losses the name " load losses," since they are due to reactions set up by the current in the armature under the conditions of load, and cannot be reproduced on open circuit or calculated directly from measured current and resistance, as can the $I^2 R$ of armature and field. The current in the armature corresponding to that of any load could be easily established by simply short-circuiting the armature terminals and introducing the proper exciting current into the field winding. Hence, for the lack of something more accurate, it became customary to measure the losses on short circuit in the same manner as core losses were determined, and take these as indicative of the losses that would exist under actual load. Obviously, the reactions on short circuit are exaggerated, hence, it has been considered fair to take a fraction of the short-circuit losses as the load losses. The present A. I. E. E. rule, Section 117, specifies one-third as the proper amount.

The method of determining the efficiency by the " segregated losses " method is undoubtedly deficient in at least two particulars.

1. The determination of core losses at the normal potential (corrected though it may be for internal resistance and reactance) can be considered as only an approximation of that existing under the influence of armature reaction when the flux is distorted and of varying density across the magnetic circuit.

2. No satisfactory method has yet been evolved of measuring the eddy currents that may be produced directly by the current in the armature under the load conditions.

However, it may be said that any alternator that has high load losses under normal load conditions, will have high losses under short circuit. Another good criterion of the design in the matter of load losses is the static impedance test with field removed, using wattmeters to measure the energy input.

That load conditions affect the hysteresis losses, becomes very apparent when we consider the case of an unbalanced condition of load between the phases or the exaggerated condition of single-phase load, where the pulsating m.m.f. produces a flux of double frequency, thereby introducing losses in parts of the magnetic circuit which are free from losses in the open-circuit condition of uniform flux. That load conditions have a tendency to increase eddy current losses has been shown by the high temperatures of many alternators in actual service, where the temperature increments are greater than they should be if only what may be called legitimate losses existed. In some cases temperatures have produced charring of insulation and rapid deterioration where the heating, due to the I^2r alone, should be conservative, and where the determination of segregated losses, and possibly heat runs on open-circuit under what was taken to be equivalent heating, gave no indication of trouble from excessive temperatures.

Load losses may be defined as all losses due to the presence of current in the armature windings in excess of the following:

Open-circuit core loss at rated $+Ir$ potential.

I^2r of armature and field due to the actual resistance of the circuits.

The load losses may be regarded as made up of two components; the necessary or legitimate, and the illegitimate. The first of these is small and hardly worth considering in its effect on the efficiency. The second may be so large as to materially lower the efficiency and seriously impair the life of the machine. On the other hand, the second component may be appreciable, and one to be reckoned with in determining the efficiency, and yet be desirable to introduce into the design. For instance, a solid is often preferable to a sectionalized conductor, since the additional eddy current loss may be compensated for by greater net cross-section. Even though this compensation be partial, no greater heating may result, due to the better transfer of heat to the surface, and the use of the solid conductor is justified by the cheaper construction, greater mechanical strength and ease of repairs.

Determination of Load Losses

In a well-designed machine the magnitude of the load losses is so small compared with the capacity that it is impracticable to determine it by direct measurement of input. This has been shown by numerous trials.

A method has been proposed for machines of the enclosed type where the greater part of the losses are removed by the air passing through the machine. This consists in measuring the volume and temperature rise of the air and estimating the losses from the specific heat of the air. In this method there are the following sources of error. The percentages of error given apply to the ordinary commercial test:

Transfer of heat to the room through the frame (amount unknown).

Difference in the specific heat of air due to the presence of moisture (inappreciable).

Measurement of the volume of air (at least 5 per cent error).

Determining the average temperature of the ingoing and outgoing air (at least 5 per cent error).

With these errors it is evident that load losses of considerable magnitude would remain undiscovered.

A modification which eliminates most of the errors is to run the machine at some no-load condition in which the losses can be easily determined; also run the machine under load. The difference in the temperature rises of the air under the two conditions represents a certain loss which can be calculated from the no-load results. The load losses may then easily be estimated. By making such a test carefully, any abnormal load losses should be detected. The authors, however, have had no opportunity to make a trial of this method.

To obtain more satisfactory results there has been devised a number of other methods based on the following plan:

The alternator is run in such a manner that all magnetic and current densities and all reactions shall be the same as under load and the energy measured be only that due to the losses. The excess of the losses thus determined over that obtained by the usual segregated loss method will represent the true load losses. Several tests of this nature have been made with more than usual care, for the purposes of this paper. A general description of the tests follows and a comparison of the results is given in Table III.

Segregated Loss Method

The A. I. E. E. method of determining the total losses consists in obtaining the various losses under partial load conditions and taking their sum as the total loss, making certain allowances for the load losses. The following A. I. E. E. rules relate to the losses:

102 Bearing Friction and Windage. The magnitude of bearing friction and windage (which may be considered as independent of the load) is conveniently measured by driving the machine from an independent motor, the output of which may be suitably determined.

106 Core Losses. In machines these losses should be determined on open circuit and at a voltage equal to the rated voltage $+ Ir$ in a generator, and $-Ir$ in a motor, where I denotes the current strength and r denotes the internal resistance of the machine. They should be measured at the correct speed and voltage, since they do not usually vary in any definite proportion to the speed or to the voltage.

107 Note. The total losses in bearing friction and windage, brush friction, magnetic friction and eddy currents can, in general, be determined by a single measurement by driving the machine with the field excited, either as a motor, or by means of an independent motor.

108 Retardation Method. The no-load iron, friction, and windage losses may be segregated by the Retardation Method, in which the generator should be brought up to full speed (or, if possible, to about 10 per cent above full speed) as a motor, and, after cutting off the driving power and excitation, frequent readings should be taken of speed and time, as the machine slows down, from which a speed-time curve can be plotted. A second curve should be taken in the same manner, but with full field excitation; from the second curve the iron losses may be found by subtracting the losses found in the first curve.

109 The speed-time curves can be plotted automatically by belting a small separately excited generator (say 1/10 kw.) to the generator shaft and connecting it to a recording voltmeter. When the retardation method is not feasible, the frictional losses in bearings and in windage, which ought, by definition, to be included in determining the efficiency, may be excluded; but this should be expressly stated.

110 Armature-Resistance Loss. This loss may be expressed by $p\,I^2r$: where r resistance of one armature circuit or branch, $I =$ the current in each armature circuit or branch, and $p =$ the number of armature circuits or branches.

114 Load Losses. The load losses may be considered as the difference between the total losses under load and the sum of the losses above specified.

116 Estimation of Load Losses. While the load losses cannot well be determined individually, they may be considerable and, therefore, their joint influence should be determined by observation. This can be done by operating the machine on short circuit and at full load current, that is, by determining what may be called the "short-circuit core loss." With the low field intensity and great lag of current existing in this case, the load losses are usually greatly exaggerated.

117 One-third of the short-circuit core loss may, as an approximation, and in the absence of more accurate information, be assumed as the load loss.

In order to have a comparison of the value of the short-circuit core loss and the other armature losses, there have been prepared Tables I and II on 25- and 60-cycle generators, respectively. The accuracy of the individual determinations may be questioned, but they are given, as it is probable that the average of all the values is approximately correct. The data on horizontal lines

TABLE I.

OPEN-CIRCUIT, SHORT-CIRCUIT, AND I^2r LOSSES OF 25-CYCLE ALTERNATING-CURRENT TWO- AND THREE-PHASE GENERATORS.

	1	2	3	4	5	6	7
			Losses in per cent of kv-a.				
	Kv-a.	Volts	Core loss			I^2r arm.	Total 3 +5 + 6
			Open cir.	Short cir.	⅓ of s.c.		
1	270	2300	1.74	0.00	0.00	2.08	3.82
2	360	2300	3.17	0.29	0.1	0.90	4.17
3	937	2300	0.95	0.24	0.08	0.95	1.98
4	937	3300	1.47	0.71	0.24	1.04	2.75
5	937	6600	1.05	0.60	0.20	0.94	2.19
6	1111	220	2.07	1.46	0.49	0.68	3.22
7	360	13200	2.39	0.88	0.29	1.16	3.84
8	1667	2300	1.88	0.80	0.27	0.70	2.85
9	1667	6600	1.07	0.49	0.16	0.50	1.73
10	2222	2300	1.21	0.79	0.26	0.83	2.30
11	2342	480	1.40	1.00	0.33	0.80	2.53
12	430	480	1.56	0.71	0.24	1.67	3.50
13	420	240	2.05	1.01	0.34	3.08	5.47
14	1270	9000	1.33	0.54	0.18	0.82	2.33
15	840	480	1.58	0.59	0.20	1.70	3.48
16	720	480	1.64	0.81	0 27	2 09	4.00
17	1800	2300	2.11	0.29	0.1	1.24	3.45
18	9000	6600	1.00	0.28	0.09	0.44	1.53
Average	1516	3416	1.65	0.64	0.21	1.2	3.06

No. of machines above the average short-circuit core loss, 9.
No. of machines below the average short-circuit core loss, 9.

3, 4, 5, 6, 8, 9, 10, and 11 in Table I, and 2, 3, 4, 5, 6, 7, 8, 9, 11, 14, 15, 16, 19, 20, 29, 33, 36, 39, 40, 42 and 43 in Table II, are on steam turbo-generators and the core losses were taken by the retardation method with turbine wheels assembled; consequently the results are not especially accurate. The core losses on the other machines were taken by driving with a direct-current motor and are more reliable.

TABLE II. OPEN-CIRCUIT, SHORT-CIRCUIT, AND I^2r LOSSES OF 60-CYCLE ALTERNATING-CURRENT TWO- AND THREE-PHASE GENERATORS.

	1	2	3	4	5	6	7
			Losses in per cent of kv-a.				
			Core losses				
	Kv-a.	Volts	Open cir.	Short cir.	⅓ of s.c.	I^2r arm.	Total 3+5+6
1	318	2300	1.41	0.52	0.17	1.18	2.76
2	625	2300	1.59	0.71	0.24	1.10	2.93
3	1875	2300	1.38	1.68	0.56	0.38	2.22
4	937	600	1.60	0.96	0.32	0.66	2.58
5	937	2300	1.45	1.18	0.06	0.44	2.28
6	420	2300	1.37	0.18	0.06	1.10	2.56
7	1250	2300	1.55	0.90	0.30	0.92	2.77
8	1563	480	1.31	0.83	0.28	0.69	2.28
9	1563	2300	1.31	0.83	0.28	0.51	2.10
10	480	600	1.63	1.06	0.35	1.19	3.17
11	2000	2300	1.04	0.29	0.10	0.62	1.76
12	635	2300	1.48	0.43	0.14	1.06	2.68
13	793	2300	1.51	0.72	0.24	0.55	2.33
14	2500	480	1.30	0.25	0.08	0.40	1.78
15	2500	2300	1.82	0.43	0.14	0.41	2.37
16	2500	6600	1.85	0.09	0.03	0.33	2.21
17	600	600	1.98	0.50	0.17	1.18	3.33
18	600	480	2.63	1.32	0.44	1.34	4.41
19	3125	2300	1.63	0.99	0.33	0.40	2.36
20	3125	2300	1.66	0.34	0.11	0.35	2.12
21	635	4150	1.99	0.64	0.21	1.18	3.38
22	480	11000	2.53	0.26	0.09	1.65	4.27
23	370	600	1.49	0.22	0.07	1.52	3.08
24	720	600	2.87	1.15	0.38	0.94	4.19
25	600	11000	2.53	0.25	0.08	1.47	4.08
26	540	220	2.28	0.95	0.32	1.27	3.87
27	1920	2300	1.61	1.32	0.44	0.54	2.59
28	720	600	1.54	0.25	0.08	1.15	2.77
29	6250	4000	0.96	0.43	0.14	0.31	1.41
30	1275	11000	1.51	0.74	0.25	1.00	2.75
31	1200	10000	2.73	0.38	0.13	0.98	3.84
32	1440	2300	1.83	1.25	0.42	0.73	2.98
33	8333	2200	1.05	0.53	0.18	0.31	1.54
34	1200	2300	1.50	0.49	0.16	1.34	3.02
35	6000	2300	0.91	0.51	0.17	0.53	1.61
36	9000	4600	1.00	0.13	0.04	0.30	1.34
37	3000	2300	1.33	0.47	0.16	0.75	2.26
38	12000	6600	1.18	0.24	0.08	0.32	1.58
39	14000	4600	0.87	0.05	0.02	0.31	1.20
40	14000	6000	1.45	0.09	0.03	0.24	1.72
41	15000	4150	0.70	0.48	0.16	0.36	1.22
42	15000	7000	0.91	0.15	0.05	0.25	1.21
43	15000	11431	1.45	0.34	0.11	0.20	1.76
44	7800	4000	1.28	0.55	0.18	0.50	1.96
45	9800	6900	1.58	0.72	0.24	0.40	2.22
46	12000	6600	1.72	0.20	0.07	0.32	2.11
47	6600	4000	1.89	0.43	0.14	0.53	2.56
Average	4111	3670	1.58	0.58	0.19	0.72	2.49

Number of machines above the average short-circuit core loss, 18.
Number of machines below the average short-circuit core loss, 29.

A description of Tables I and II follows:

Column 1. Kv-a. rating. This is on the "continuous" or "single rating" basis, which is being considered in the present revision of rules.

" 2. Potential.

" 3. Open-circuit core loss at rated + Ir volts (in per cent of kv-a. rating).

" 4. Short-circuit core loss at rated current (in per cent of kv-a. rating.

" 5. One-third of the values in column 4.

" 6. Calculated I^2r loss of armature at rated current in per cent of the kv-a. rating.

" 7. Total of columns 3, 5, and 6.

In addition to the data given in Tables I and II there were a few accurate tests made, the results of which are included in Table III.

Modified Segregated Loss Method

This method differs from the segregated loss method in that the reactive drop of the armature current is taken into account. The core loss is taken at a voltage corresponding to rated volts plus IR drop plus IX drop. The IX drop is determined as follows:

Let A = Ampere-turns field to pass full-load current through the armature when it is short-circuited.

R = ampere-turns of armature reaction.

ϕ = angle of lag between volts and current.

X = IX drop which is the volts from the no-load saturation curve corresponding to $A-R$.

V = rated volts.

V_m = volts at which the core loss is taken.

$$V_m = \sqrt{V^2 + X^2 + 2\,VX \sin\phi}$$

See Table III for results of tests.

Phase Characteristic Method

This method of testing consists in running an alternating-current generator as a synchronous motor without mechanical load at rated voltage and current. The energy input is taken for the above conditions of voltage and current with field under- and over-excited. From the average of these two quantities there is subtracted the I^2r of the armature winding and the energy input at unity power factor and rated voltage.

See Table III for results of tests.

Circulating Energy Method

This test consists in coupling two alternators mechanically and electrically, and belting a direct-current motor to the combination to supply the losses. The angle of the two rotors is shifted by means of slots in the coupling so that rated current will flow with rated volts at unity power factor.

The losses of the two machines are determined by the input from the driving motor.

See Table III for the results of tests.

TABLE III.
RESULTS OF CAREFUL TESTS BY SEVERAL DIFFERENT METHODS OF DETERMINING LOAD LOSSES. 60-CYCLE ALTERNATORS.

	1	2	3	4	5	6	7
				Losses in per cent kv-a.			
	Phase	kv-a.	Rev. per min.	Seg.	Mod. seg.	Phase char.	Circ. energy
1	3	860	720				
2	Excess loss				0.035		
3	⅓ short circ. core loss			0.151	0.151		
4	Total load losses			0.151	0.186	0.302	0.49
5	1	600	720				
6	Excess loss				0.025		
7	⅓ short circ. core loss			1.300	1.300		
8	Total load losses			1.300	1.325		3.82
9	3	300	600				
10	Excess loss				0.063		
11	⅓ short circ. core loss			0.099	0.099		
12	Total load losses			0.099	0.162	0.150	0.29
13	3	110	900				
14	Excess loss				0.055		
15	⅓ short cir. core loss negligible						
16	Total load losses				0.055		0.359

Description of Table III

Column 1. Number of phases.
" 2. Capacity in kv-a.
" 3. Revolutions per minute.
" 4. Losses in per cent of kv-a. by the segregated method. Note that all losses are in actual per cent. For instance the loss for line 3 in this column is 1.3 kw.
" 5. Losses by modified segregated method.
" 6. Losses by phase characteristic method.
" 7. Losses by the circulating energy method.

Line 1. Rating of the alternator.
" 2. Excess core losses. The open-circuit core loss at (rated + $I r$ + $I x$, potential) – the open-circuit core loss at (rated + $I r$, potential).
" 3. One-third of short-circuit core loss.
" 4. Total load losses as determined by the various methods.

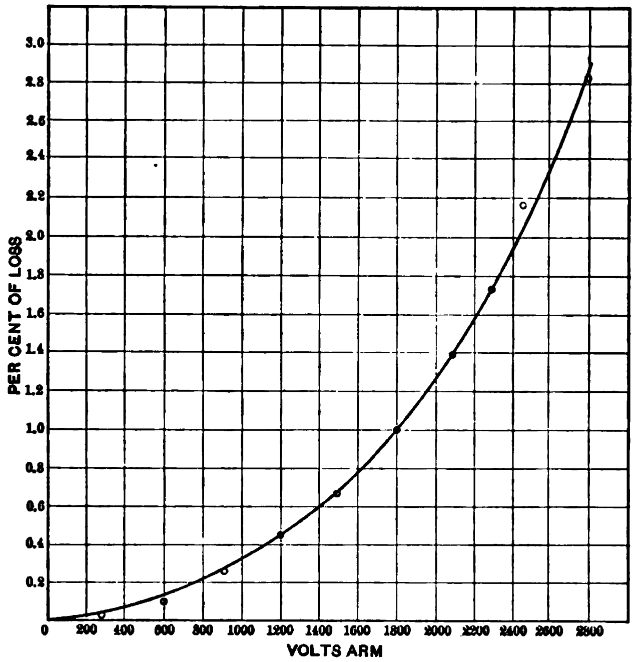


FIG. 1—OPEN-CIRCUIT CORE LOSS.
Three-phase, 300-kv-a., 60-cycle, 2300-volt, 600-rev. per min. generator.

Lines 5 to 8 inclusive: These give the same relative data as lines 1 to 4 inclusive; the difference being that lines 1 to 4 refer to three-phase, and 5 to 8 to single-phase operations of the same generator.

Lines 9 to 12 inclusive: These give the same relative data on a 300-kv-a. alternator as lines 1 to 4 inclusive.

Lines 13 to 16 inclusive: These give the same relative data on a 110-kv-a. alternator as lines 1 to 4 inclusive.

As a matter of interest there have been included Figs. 1 to 11 inclusive, which relate to the characteristics of the machine tested.

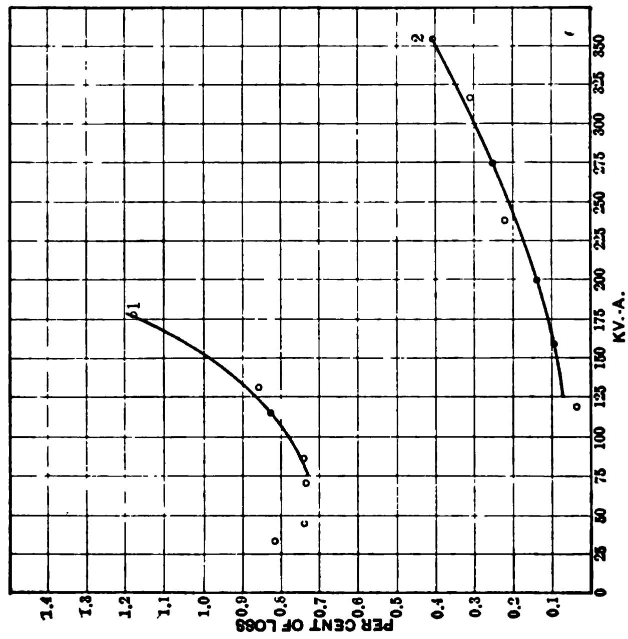


FIG. 2—SHORT-CIRCUIT CORE LOSS.

Three-phase, 300-kv-a., single-phase, 150-kv-a., 60-cycle, 2300-volt, 600-rev. per min. generator

Curve 1—Short-circuit core loss, single-phase.

Curve 2—Short-circuit core loss, three-phase.

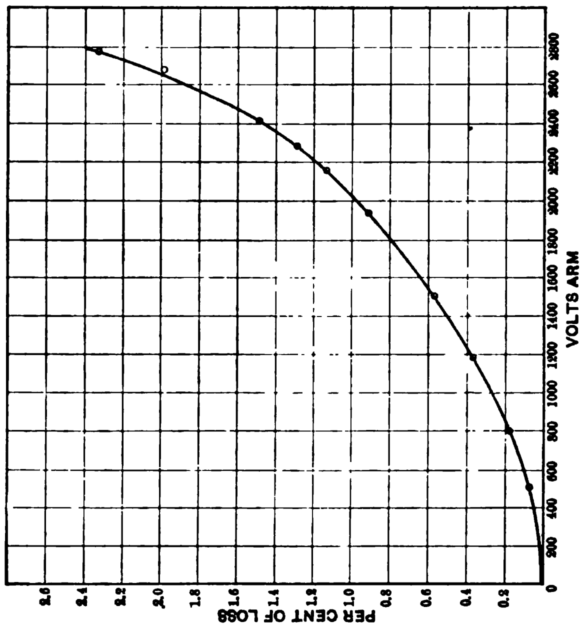


FIG. 3—OPEN-CIRCUIT CORE LOSS.

Three-phase, 860-kv-a., 60-cycle, 2300-volt, 720-rev. per min. generator.

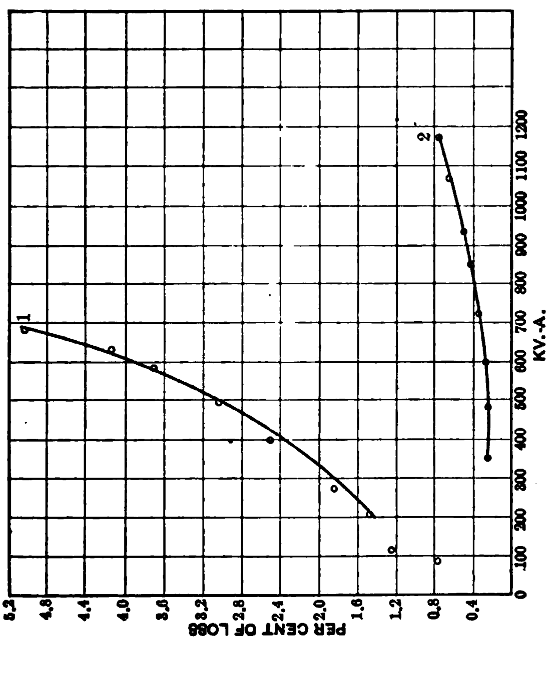


FIG. 4—SHORT-CIRCUIT CORE LOSS.

Three-phase, 860-kv-a., single-phase, 600-kv-a., 60-cycle, 2300-volt, 720-rev. per min. generator

Curve 1—Short-circuit core loss, single-phase.
Curve 2—Short-circuit core loss, three-phase.

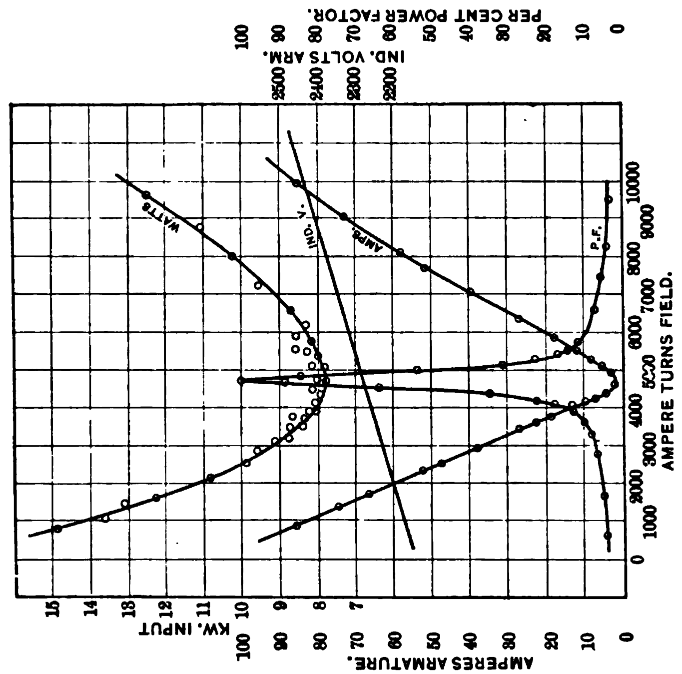


FIG. 5—PHASE CHARACTERISTICS.

Three-phase, 300-kv-a., 60-cycle, 2300-volt, 600-rev. per min. generator.

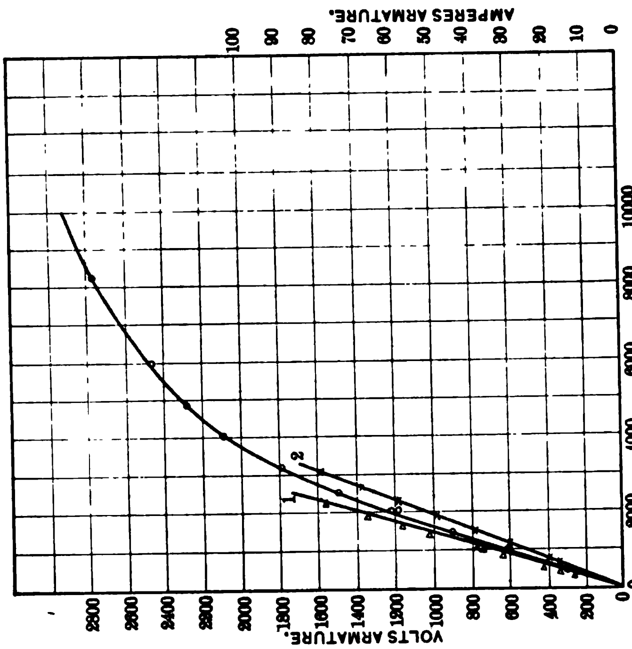


FIG. 7—SATURATION AND SYNCHRONOUS IMPEDANCE.
Three-phase, 300-kv-a., single-phase, 150-kv-a., 60-cycle, 2300-volt, 600-rev. per min. generator

Curve 1—Single-phase synchronous impedance
Curve 2—Three-phase " "

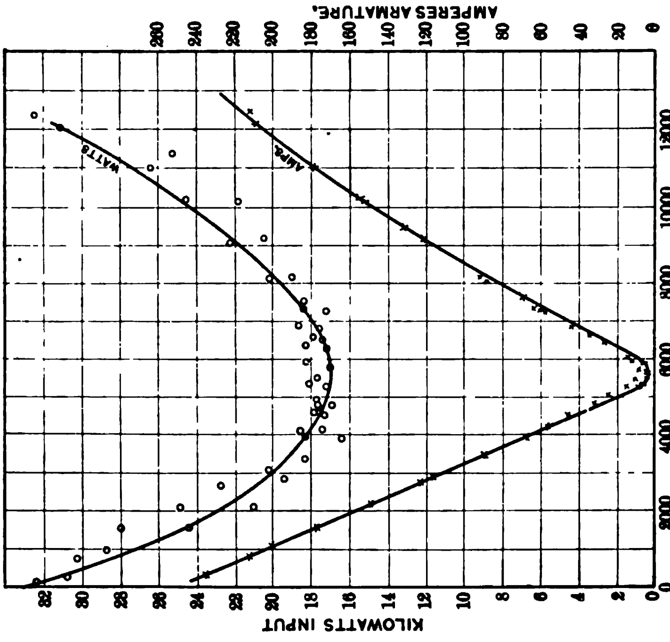


FIG. 6—PHASE CHARACTERISTICS.
Three-phase, 860-kv-a., 60 cycle, 2300-volt, 720-rev. per min. generator.

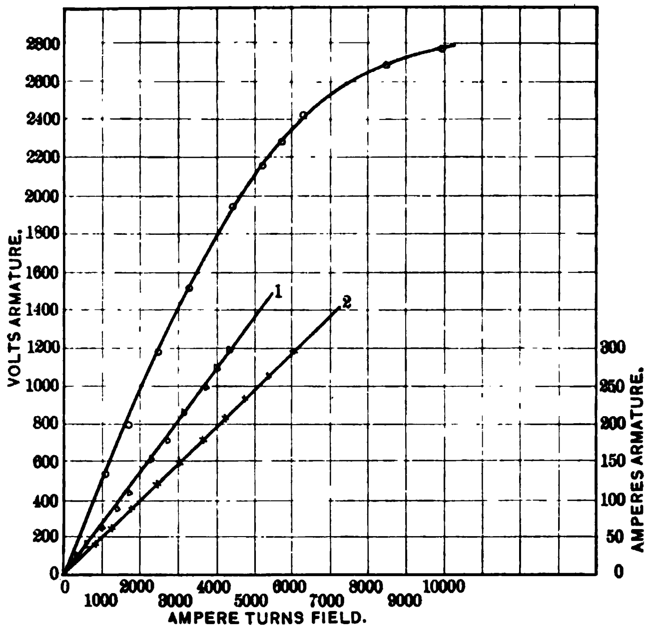


Fig. 8—Saturation and Synchronous Impedance.
Three-phase, 860-kv-a., single-phase, 600-kv-a., 60-cycle, 2300-volt, 720-rev. per min. generator
Curve 1—Single-phase synchronous impedance
Curve 2—Three-phase " "

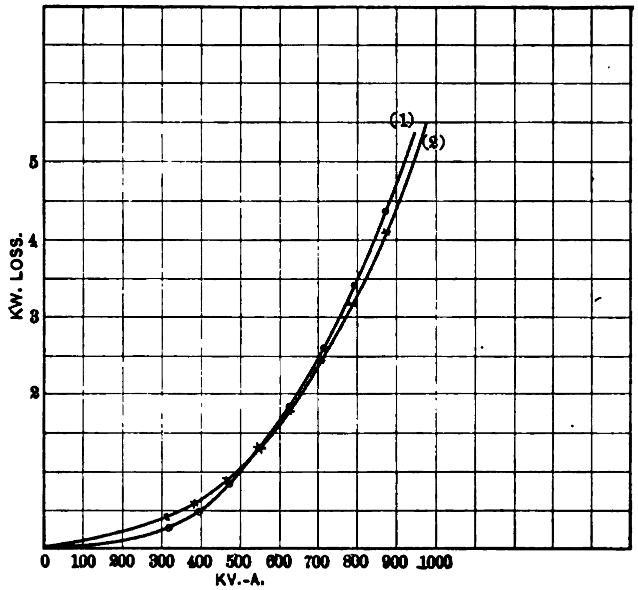


Fig. 9—Load Losses.
Three-phase, 860-kv-a., 60-cycle, 2300-volt, 720-rev. per min. generator
Curve (1) (0)—Load loss (circulating energy method).
Curve (2) (x)—Short-circuit core loss

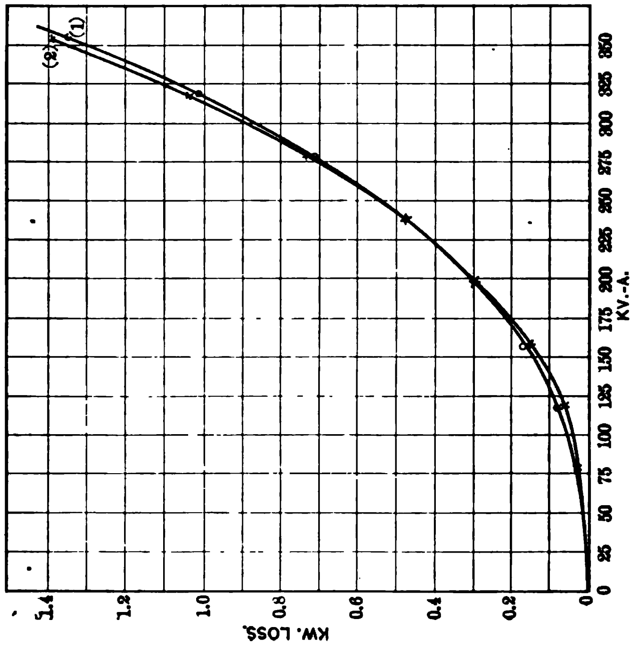


FIG. 10—LOAD LOSSES.

Three-phase, 300-kv-a., 60-cycle, 2300-volt, 600-rev. per min. generator
Curve (1) (o)—Load loss (circulating energy method).
Curve (2) (x)—Short-circuit core loss.

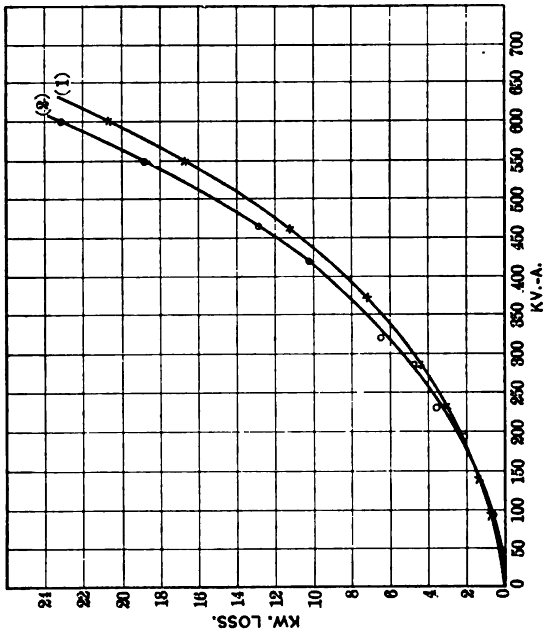


FIG. 11—LOAD LOSSES.

Single-phase, 600-kv-a., 60-cycle, 2300-volt, 720-rev. per min. generator.
Curve (1)—Single-phase load loss (circulating energy method).
Curve (2)—Single-phase, short-circuit core loss.

Tests have recently been made on two 60-cycle, 300-kv-a. 3600-rev. per min., 2300-volt steam-turbine-driven generators. These machines have laminated cylindrical rotors, wound with six coils per pole. The air gap is 0.3125 in. (8 mm.), the armature slots are 0.58 in. (14.7 mm.) and the field slots 0.50 in. (12.7 mm.) wide.

The losses were determined by the circulating energy method. An average of several careful tests gave:

	Watts	Per cent of kv-a. Rating
Load Loss	3450	1.15
Short-Circuit Core Loss	2500	0.83

In estimating the load loss the core loss was taken at rated + *Ir* volts, according to present A. I. E. E. Rule 106, on page 506 of this paper.

In this case the load loss is 38 per cent greater than the short-circuit core loss.

Summary

The circulating energy method is a form of input-output test which eliminates the errors due to the measurement of energy equal to the capacity of the machine. When two duplicate machines are available, the authors consider it the best of the four methods.

The phase characteristic method is faulty, due to the difficulty of obtaining accurate wattmeter readings at the low power factors.

The *total* short-circuit core loss agrees closely with the load loss by the circulating energy method for the 860-kv-a. and the 300-kv-a. three-phase generators. It is to be regretted that careful short-circuit core loss and circulating energy tests could not be made on several polyphase machines having a large short-circuit core loss, such as those on lines 6 and 11, Table I; and lines 3, 5, and 27, Table II. A single-phase short-circuit core loss taken on the 860-kv-a. three-phase generator mentioned above, showed unexpectedly high values. This led to the taking of a careful circulating energy test with a single-phase load. The close agreement of the two is shown in Fig. 11.

The small number of careful tests which it was possible to make for this paper does not warrant any recommendation for the determination of load losses when but one machine is available. Investigations should continue until a rule can be established to cover such cases.

A paper presented at the Midwinter Convention of the American Institute of Electrical Engineers, New York, February 27, 1913.

NOTES ON STRAY LOSSES IN SYNCHRONOUS MACHINES

BY F. K. BRAINARD

Since the Institute rule of determining " load loss," which may more properly be called stray loss, seems in error, an analysis of short-circuit tests of synchronous machines was made with the idea of determining, if possible, the nature of this loss and so the probable relationship between stray loss with actual load on the machine, and the measured loss on short-circuit which cannot be accounted for by armature I^2R.

These losses are probably due almost entirely to the following causes:

a. Eddy current loss in the armature conductors, or rather extra loss due to the unequal distribution of current in the conductor. See *Eddy Currents in Large Slot-Wound Conductors*, A. B. Field, TRANSACTIONS A. I. E. E., 1905, XXIV, p. 761.

b. Eddy current and hysteresis loss due to flux from the armature coils getting into parts of the machine not intended to carry magnetic flux, such as coil supports, end shields, etc.

c. Extra eddy current and hysteresis loss due to the change in distribution of flux in the magnetic circuit.

The eddy current factor for each machine was computed according to Field and the tests were plotted upon logarithmic paper to determine the law of variation of short-circuit loss with armature current.

In the case of slow or moderate-speed machines it was found that the loss varied practically as the square of the armature current and that the stray loss unaccounted for by cause (a) mentioned above was small and usually negligible. The loss due to cause (b) will be small in this case on account of the relatively short ends of the coils. Hence it would appear that the stray loss due to cause (c) in low-speed machines is negligible or can practically be eliminated by proper construction of armature cores.

In the case of high-speed turbo-alternators the loss unaccounted for by eddy currents in the conductors is large, and short-circuit tests made with and without end shields indicate that cause (b) previously mentioned is mainly responsible. In one case removal of the end shields reduced the loss on short-circuit 28 per cent. Hence it would seem that even in this case there is very little stray loss in the core of a well-built machine.

For any synchronous machine the loss due to the "Field effect" is probably practically the same on load as on short-circuit for the same armature current. The same undoubtedly applies to the loss due to flux from the armature coils getting into the unlaminated parts of the machine. However, the loss due to change in distribution of the magnetic flux is probably considerably greater on short-circuit than with the same armature current at full voltage; but since this part of the stray loss can probably be reduced to a negligible amount by proper design and workmanship, it would seem that the Institute rule should be modified to make stray loss equal to, say, one half of the difference between the measured short-circuit loss and the calculated I^2R loss, instead of one third of this difference as at present.

Then the total losses in any synchronous machine would be determined as follows:

1. Core loss, windage and friction, determined by driving the machine with a separate motor at normal speed and normal voltage with the armature open-circuited. This to be assumed constant at all loads.

2. Armature resistance loss and stray loss, determined by driving the machine at normal speed with the armature short-circuited and with sufficient excitation to cause full-load armature current to flow. Of course the power required to drive the machine unexcited should be deducted so as not to include windage and friction twice.

3. Field excitation loss to be calculated from the field resistance hot, the amperes excitation being determined from the regulation curves.

It has been suggested that in the case of turbo-alternators there is an additional loss under load which does not appear on either open-circuit or short-circuit, due to the increased leakage flux from the field resulting from the greater exciting current. However, no attempt has been made, to the writer's knowledge, to measure this loss, but the probable existence of it should serve as an additional argument for taking more than one third of the "short-circuit core loss" as stray loss.

The following table gives the results of a number of short-circuit tests on various machines. The column headed "*n*" is the exponent in the equation kw. $= aI^n$ obtained by plotting upon logarithmic paper, where kw. = total loss on short-circuit, I = armature current and a and n are constants.

Rating of generator					Calculated armature copper* loss at full load	Measured short-circuit loss with full load current	*n*	Short-circuit loss / Calc. I^2R	Eddy current constant
Kv-a.	Volts	Phases	Cycles	Rev. per min.					
2100	2300	3	25	107	22.4	25.7	2.16	1.15	1.29
1000	2300	3	25	94	17	19	1.86	1.12	1.12
250	1100	2	60	120	5.8	5.7	2.15	0.98	1.00
250	480	3	60	120	8.2	8.2	1.95	1	1.03
375	600	3	60	120	9.9	10.5	2.07	1.06	1.00
600	240	3	25	125	8.1	11.7	2	1.44	1.30
600	2300	3	60	150	7.6	9.2	2.07	1.21	1.19
300	4150	3	60	600	3.1	4.2	1.61	1.35	1.00
300	4150	3	60	600	3.1	3.1	1.95	1	1.00
400	7500	3	30	150	9.9	9.5	1.99	0.96	1.00

* The calculated copper loss is based upon 50 deg. cent. coil temperature. As the temperature of the stator coils was not read during the short-circuit tests, a part of the lack of agreement between the calculated and measured short-circuit loss is undoubtedly due to this.

A paper presented at the Midwinter Convention of the American Institute of Electrical Engineers, New York, February 27, 1913.

STRAY LOSS IN DIRECT-CURRENT COMMUTATING MACHINES

BY H. F. T. ERBEN AND H. S. PAGE

During the past two years there has been considerable discussion on the subject of load loss and many misunderstandings have arisen as to its nature and magnitude. To one not familiar with the accepted definition of the term, load loss might imply all of the losses occurring in a machine other than the no-load losses. In order to designate more clearly the subject under discussion the term defined in Section 114 of the Standardization Rules of the A. I. E. E. will be referred to in this paper as " stray loss " instead of " load loss."

In direct-current commutating machines, stray loss may be divided into two elements: first, " commutation loss," consisting of such losses as arise from the improper reversal of current in the coils undergoing commutation, and second, " loss due to flux distortion," resulting from the action of armature flux on main field flux.

Perfect commutation demands that the current in the coils be reversed in such a manner that it falls from a positive maximum to zero, and continuing, risesto a negative maximum uniformly as successive segments pass under the brush, and the magnitude of commutation loss depends upon the extent of divergence from this ideal condition. This latter condition is only obtained in cases where the coils undergoing commutation are passing through such a flux that an e.m.f. is generated in them whose instantaneous values are always equal to and opposite in direction to their own self-induction. Such a condition is most closely obtained in the modern commutating-pole machines and in consequence the so-called commutation loss can be neglected. In machines of the non-commutating pole type, where it is most difficult to obtain the correct commutating flux, it is obvious that commutation loss may be of considerable magnitude, as frequently the current is not properly reversed during commutation. Even if the correct commutating flux is obtained for a a given load on such a machine it does not vary in such proportion to the current as to be correct for other loads. Non-commutating pole machines will, therefore, have one element of the stray loss which is at a maximum at some fixed load but which will increase very rapidly at different loads.

A fair example of flux distortion is shown in Fig. 1. The symmetrical curve shown in solid line represents the flux distribution at no-load as obtained by the oscillograph. The corresponding broken line curve shows flux distribution at full load. It will be seen readily that with the iron worked through an entirely dif-

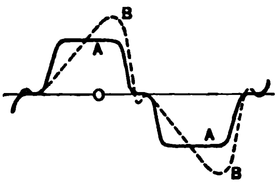


Fig. 1

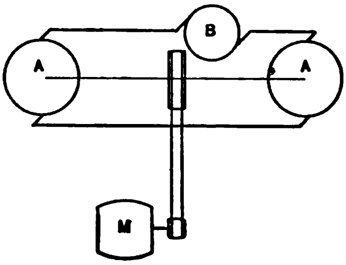


Fig. 2

ferent cycle at full-load than at no-load the hysteresis and eddy current losses will be altered. The maximum flux density in the teeth at no-load represented by $O\ A$ is raised 30 per cent to 40 per cent as represented by $O\ B$, thereby increasing the hysteresis loss and causing additional leakage through the slots with a resultant increase in eddy current loss in the copper.

In order to make efficiency tests taking stray losses into account it is necessary to load the machine to its full capacity, carefully observing either the input and output, or, if two duplicate machines are at hand, the total losses may be observed directly by the following method. In Fig. 2 the machines under test have the fields separately excited. At zero load no power is supplied from B, and M supplies power for all the losses except excitation. As load is applied, B furnishes power to circulate the armature current, the counter e.m.f. of machines A-A being kept alike, and M supplies additional power to compensate for the stray loss, thus allowing both machines to be fully loaded. Instruments indicating the losses are entirely independent of those measuring the load, consequently losses can be observed with a minimum error.

This paper will present data showing the magnitude of stray loss occurring in various types of "direct-current machines" and "synchronous converters," and from these data it will probably be possible to derive approximately correct formulas for general use in determining this loss. From facts later presented it will be seen that the actual value is such that quite an error in its estimation is permissible.

Input-output tests for efficiency are very expensive and frequently give inconsistent results. Errors of observation directly affect the determination of efficiency by this method, and as an example of the possibilities of error the following table is given, showing results on 37 motors tested by the brake method.

H.p. output	H.p. stray loss	
2.01	+ 0.0080	
2.03	— 0.0264	
3.1	— 0.0403	
3.1	+ 0.034	
2.99	+ 0.0508	
3.00	— 0.117	
4.97	+ 0.0249	
4.93	+ 0.045	
4.94	+ 0.1475	
4.95	+ 0.0638	
7.7	— 0.0231	
7.72	— 0.0077	
7.4	+ 0.0074	
7.4	+ 0.0444	
10.06	+ 0.0201	
10.23	+ 0.113	
9.62	— 0.279	
9.62	— 0.192	
14.9	+ 0.313	
15	— 0.166	
15.15	+ 0.273	
15.07	— 0.181	
20.5	— 0.0615	
20.5	— 0.1025	
2.00	+ 0.084	
1.99	+ 0.0139	
3.05	+ 0.0061	
2.95	— 0.0678	
4.92	— 0.0541	
4.84	— 0.0921	
7.67	+ 0.0077	
7.4	— 0.126	
10.06	— 0.040	
9.82	— 0.167	
14.96	— 0.06	
15.12	+ 0.0605	
20.05	+ 0.0802	
Total 311.75	3.6689	Total +
	1.8047	Total —
	1.8642	Net total + = 0.6 %

The resultant efficiencies were compared with efficiency figures obtained by the segregated loss method and the difference in each case considered as stray loss. The instruments were calibrated just before tests were made and all observations carefully checked. Many of the stray loss values appear as negative, and in spite of the care taken to have correct observations made, the results indicate a probable error greater than the stray loss itself. In these thirty-seven motors, ranging in capacity from 2 h.p. to 20 h.p., the total net stray loss shown was 1.86 h.p. for a total capacity of 312 h.p., giving a stray loss of 0.6 per cent. Inconsistencies which appear in these figures show that the results are of value only as an indication of the average stray loss and cannot be taken as correct for any one machine. This table is not given as an example of the most accurate results obtainable but rather as an example of results which will be obtained unless what might be termed " laboratory methods " are resorted to.

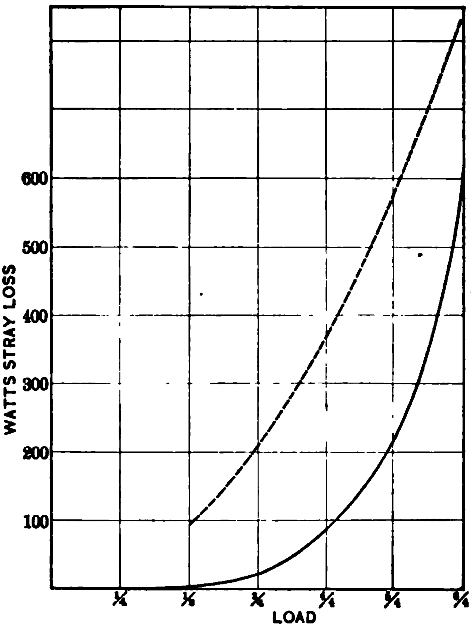


FIG. 3—FOUR-POLE, 20-KW.,1000-REV. PER MIN. GENERATOR.
I^2R armature 1060 watts. Iron and eddy current loss 360 watts

St-ay loss values on five machines tested by the method described in connection with Fig. 2 are shown in Figs. 3 to 8 inclusive. In these tests the utmost care was taken in every detail, the losses were observed directly and the results are much more reliable than tabulated results given for the 37 small motors. The 150-kw. and 400-kw. generators, also the 500-kw. converter, were not fitted with commutating poles, consequently they show a stray loss at no-load due to the initial brush shift.

I^2R loss in the armature copper and iron plus eddy current loss as determined at no-load are given in each case.

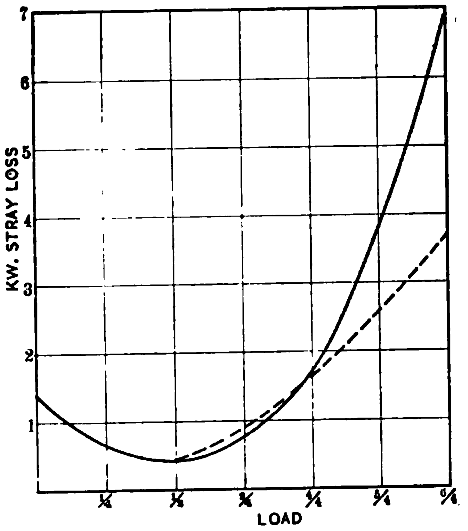


FIG. 4—SIX-POLE, 150-KW., 225-REV. PER MIN. GENERATOR.
I^2R armature 4700 watts. Iron and eddy current loss 1600 watts

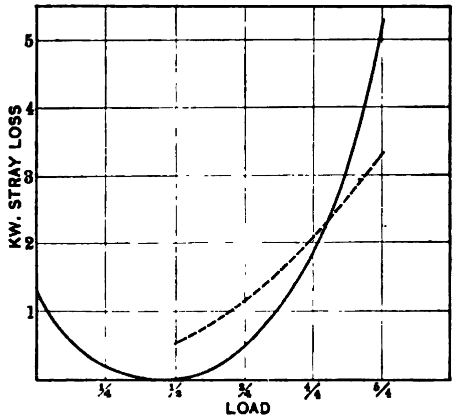


FIG. 5—FOUR-POLE, 500-KW., 750-REV. PER MIN., SYNCHRONOUS CONVERTER RUN AS 300-KW. GENERATOR.
I^2R armature, 3750 watts. Iron and eddy current loss 4400 watts.

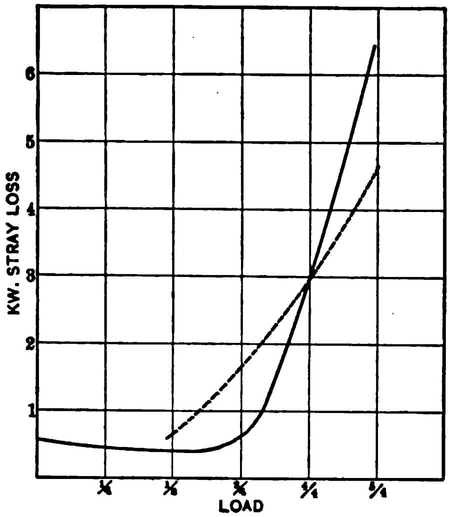


FIG. 6—EIGHT-POLE, 400-KW., 240-REV. PER MIN. GENERATOR
I^2R armature 7680 watts. Iron and eddy current loss 3720 watts

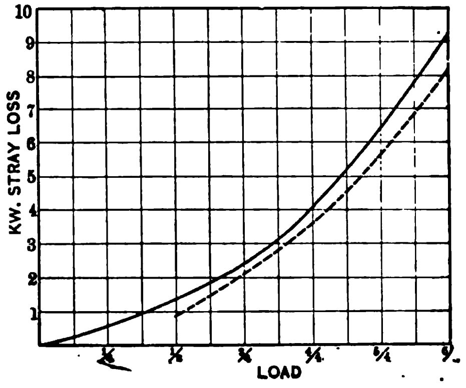


FIG. 7—FOUR-POLE, 500-KW., 720-REV. PER MIN. GENERATOR
I^2R armature 6400 watts. Iron and eddy current loss 7400 watts

Dotted curves shown in Figs. 3 to 8 are based on the assumption that stray losses at normal load are equal to 26 per cent of the no-load iron loss plus full-load armature copper loss and vary as the square of the load. This factor of 26 per cent, chosen to suit the average stray loss of the four larger machines, is evidently too high a multiplying factor for the 20-kw. generator.

Fig. 8 shows the curves of Figs. 3 to 7 combined with the percentage stray loss in each, plotted against load. The two dotted

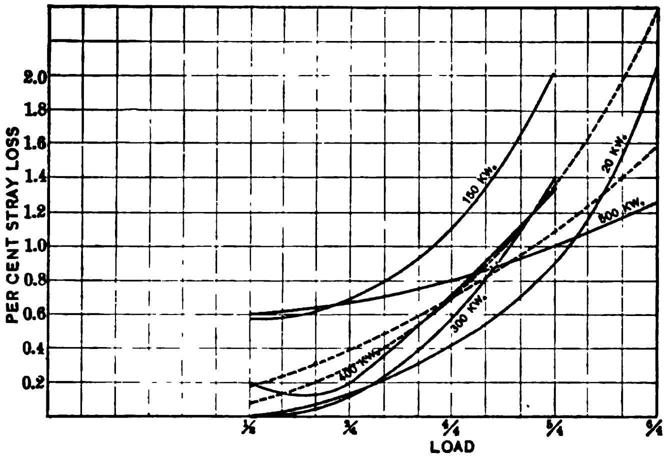


Fig. 8

curves show stray loss values varying as the cube and square of the load, 0.7 per cent stray loss being arbitrarily chosen at the normal load point.

In this paper no definite conclusions are drawn as to a suitable multiplying factor for indicating stray loss. Results of further tests made on different apparatus, together with a recommendation as to suitable multiplying factors, will be presented in the discussion of these papers at the Midwinter Convention of the Institute.

A paper presented at the Midwinter Convention of the American Institute of Electrical Engineers, New York, February 27, 1913.

THE DETERMINATION OF STRAY LOSSES FROM INPUT-OUTPUT TESTS

BY L. T. ROBINSON

It is usual in ordinary testing, even after exercising considerable care, to get results not better than the general order of one per cent.

The purpose of the present paper is to show that input-output tests can be made, using commercial measuring instruments and under shop conditions, that will give, if extreme care is taken throughout, efficiency values within one part in 500, and that with such results, a general idea of the stray losses may be obtained within about 0.2 of one per cent of the full-load output.

With this degree of precision, the stray loss curve expressed as per cent of full-load output, and plotted against the output of the machine, can be shown as a curved belt, 0.4 per cent wide, thus giving the stray losses definitely to a varying degree of approximation depending on the amount of such loss.

In computing stray losses from efficiency tests by input-output method, the fact that some of the points obtained do not lie within the prescribed belt is felt to be due more to the fact that certain components of the losses which are determined under no-load are variable than to any failure of the efficiency determinations to come within the prescribed limits. For example, in one instance, Curve 3, Table XIV, the brush and bearing friction alone would, if varied about 25 per cent in the right direction, reduce the stray losses to zero, and it is felt that some such effect caused the losses on 100 and 125 per cent load on this set to fall below what would ordinarily be expected.

If this point had been more fully appreciated during the progress of the tests, certain components of the no-load losses that enter largely into the final results would have been determined at frequent intervals during the tests, instead of once for all before the efficiency runs. Careful examination of the final tests shows that the difficulty is not so much to be found in obtaining a trustworthy measure of the efficiency as in obtaining the true stray losses, by substracting from the definitely known total losses, under load, the somewhat indefinitely known no-load components corresponding to each full-load determination.

In the examples chosen, the stray losses do not exceed the general order of 1 per cent of the full-load output, and hence the stray losses, assuming that the no-load losses remain constant, may be definitely known within 20 per cent. Obviously, the approximation will be 10 per cent if the stray losses are 2 per cent of full-load output, and 40 per cent if the stray losses are ½ per cent of full-load output.

The accuracy obtainable in this way is, of course, decidedly inferior to that obtainable if the losses could be obtained by direct measurement, using only ordinary care in obtaining the results. However, direct methods are not always known for determining stray loss which may be applied to any and all types and sizes of machines to which input-output testing is applicable, and the development and acceptance of any such methods must depend, in the final analysis, on some over-all efficiency measurement by input-output tests or by similar means.

If it has been shown that the attainable precision is of the general order of 1 in 500, the presumption is that directly determined stray losses falling within the belt would represent more definitely the magnitude of the losses sought, than the center of the belt, and also more correctly the change in such losses with change of load. On the other hand, it is always possible to omit from consideration some conditions of testing when stray losses are directly determined, and determination of over-all efficiency within 0.2 per cent precludes the possibility of large stray losses, or any that would be of interest to the user, being overlooked.

Besides the degree of precision possible in obtaining the efficiency in terms of the test instruments employed, must be considered the calibration and testing of the instruments used, so that the input and output may be known in the same terms.

In all the cases considered, the input was alternating current, and the voltage and current components were of such magnitude that current and pressure transformers were required in the measurements. The output was direct current.

In estimating the permissible average deviation in results the following were considered:

1. Determination of input and output in terms of the instrument employed.
2. Precision of wattmeters used for input.
3. Precision of instrument transformers used.
4. Resistance of shunts used for output in connection with millivoltmeters.
5. Precision of millivoltmeters used with shunts for measuring output current.
6. Voltmeters for measuring pressure of d-c. output.

No attempt will be made to give in conventional style a complete discussion of the probable error in relation to the six things involved, or to establish the correctness of the conclusions reached, except by reference to the tabulated results or to the curves which follow. The contention is that the results obtained usually indicate that the precision estimated is more often attained than not. All the points determined from averages are given, and enough of them lie outside the estimated belt to indicate, as would be expected, that not every point will fall within the probable region.

Taking up the various headings in order:

1. The efficiency, or ratio of instrument readings, output divided by input, was determined in connection with each complete observation, and the deviation of each efficiency or ratio so computed from the average, was used as a basis for determining the average deviation as follows:

$$\text{Average deviation of mean} = \frac{\Sigma d_1 \ldots d_n}{R\, n \sqrt{n}}$$

where $d_1 \ldots$, and $d_n \ldots$, are the individual variations from the average ratio, n is the number of observations taken and R is the ratio of input to output. The number of observations is increased until the average deviation is about 0.15 per cent.

In determining average deviation of mean it is preferable to follow the procedure of Tables V to XV and use the ratio of input reading to output reading, rather than to complete each separate efficiency. The labor is much less and the opportunity is entirely removed to be unconciously influenced by preconceived notions of what efficiency may be expected.

2. Wattmeters were checked by comparison with usual standards at several points very close to the respective values to be

determined, and these checks were compared carefully with previous similar sets of observations to determine with greater accuracy the true values to be used. The average deviation of these values at any point did not exceed 0.07 per cent.

3. Instrument transformers used were checked by potentiometer methods of comparison giving average deviation of 0.05 per cent for current and the same for pressure transformers.

4. Resistance of shunts used for measuring the d-c. output was not considered in connection with the precision estimates. It is possible, either by a single determination using a great deal of care, or by averaging a number of independent determinations, to know the resistance of these shunts within limits so small that they would not appreciably affect the accuracy of the results.

5. Millivoltmeters used on these shunts for measuring d-c. were felt to be somewhat more capable of accurate comparison with standards than the wattmeters, therefore the value 0.05 per cent was taken.

6. Voltmeter errors were estimated to be the same as millivoltmeter, 0.05 per cent.

Effects of temperature variation were eliminated by carrying out all the tests to completion when the series was started, and disturbing influences of surrounding machinery and circuits were very carefully guarded against. In addition to this, shielded instruments were used in every instance.

In must be borne in mind quite clearly that no attempt is being made to state that instruments can be ordinarily relied upon, based on the usual single comparison, to within anything like the accuracies here estimated. It is usual to take only one reading at a point, instead of a closely bunched set of three or five readings, and to neglect entirely previous comparisons at the same point made on the instrument. The average deviation in connection with all the items referred to was handled in the same way as that given under (1).

For a final precision measure we get

$$\sqrt{0.15^2 + 0.07^2 + 0.05^2 + 0.05^2 + 0.05^2 + 0.05^2} = 0.19 \text{ per cent}$$

or roughly, as previously stated, 0.2 per cent. The only significance that was attached to this very elementary computation, was to draw from it the conclusion that, by using very unusual care in all the work, the desired degree of precision could be obtained, and it is believed that the results amply justify this conclusion. This estimate refers to efficiency alone, and, for reasons previously given, cannot be extended, in all cases, to cover the stray loss determinations as well.

It is usually customary to translate values obtained in a manner similar to those referred to above into "probable error" by multiplying by about two-thirds, but this was not done, in order to be on the safe side.

Conclusions

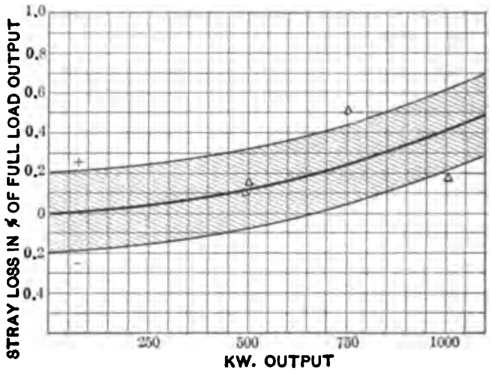
Tests like the following, resulting in efficiency determination within 0.2 per cent, may be successfully made with commercial instruments and on commercial circuits by using unusual care and with a considerable expenditure of time in arranging the tests and taking the observations. The labor of tabulating and computing the results, together with the labor in connection with the checking of instruments and accessories, is far more than the labor of obtaining the observations or setting up the tests.

In the checking of the instruments and computing the test results, a high order of skill and reliability is demanded, such as can be obtained only by training average operators to do the work with the necessary degree of care by keeping them continuously employed at it for some time, or as an alternative, utilizing several high-priced men for computing and arranging for such tests. In either case the cost of the work, if it is done well enough to be of value, will be so great that it is not permissible except in connection with large sets of comparatively high cost and as a check method for demonstrating the reliability of other methods better suited to commercial needs. The method, is, therefore, clearly one that can be considered only in connection with special investigations.

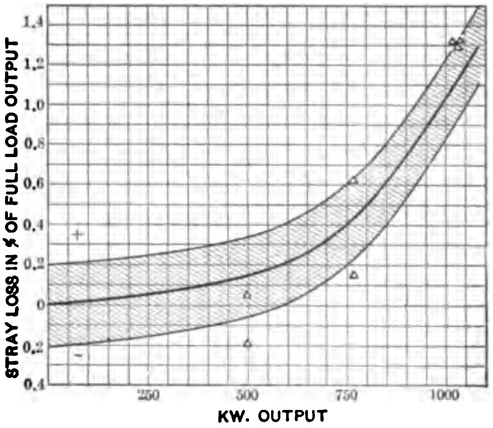
The test observers need not be especially skilled if computation of deviations from average are carried on simultaneously with the test and the number of separate observations increased until the desired precision limits are obtained. Such unskilled observers may increase the number of readings necessary from 9 or 10 per point to three or four times that number, or even more, with the corresponding increase in labor of computing, etc.

The same effect may occur with unsteady current supply.

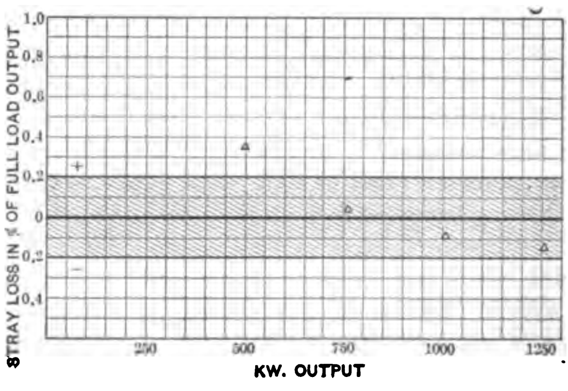
An occasional observer may be found who cannot record the indications of his instruments even with impartial inexactness. Such a one must be located and replaced before the test can be satisfactorily concluded.



CURVE 1



CURVE 2



CURVE 3

Reference to Curve 1 will show that redeterminations at 50 per cent load made on succeeding days checked very closely because presumably the conditions of bearing and commutator did not differ. This is also shown on Curve 2, at 100 per cent load.

These points are referred to simply to emphasize the fact that the load loss values could, as stated, be brought closely within the belts, if the losses determinable at no-load and which vary within relatively wide limits from time to time, had been ascertained at the same time that the efficiency point was taken.

TABLE I

THREE-UNIT SET—1135-KV-A., 6600-VOLT, SYNCHRONOUS MOTOR DIRECT-CONNECTED TO TWO 500-KW., 125-VOLT DIRECT-CURRENT GENERATORS.

50 per cent load	Obs. No.	Kw. Input	Kw. Output	Per cent efficiency	Deviation from mean
	1	614.2	502	81.8	1.1
	2	583.3	486.8	83.5	0.6
	3	562.4	464	82.6	0.3
	4	599.8	497	83.1	0.2
	5	598.2	496	83	0.1
	6	590.8	494.3	83.7	0.8
	7	584.7	490.5	83.8	0.9
	8	602.3	493.5	82	0.9
	9	588.8	489.5	83.1	0.2
	10	602.6	495.5	82.3	0.6
Average		592.71	490.91	82.9	$\frac{0.57}{\sqrt{10} \times 82.9} = \pm 0.22\ \%$

Test output		Seg. loss		Seg. loss input	
490.91	+	100.8	=	591.71	$\frac{490.91}{591.71} = 83\%$ seg. loss eff.

Test input		Seg. loss input		Stray loss
592.71	—	591.71	=	+ 1.0 kw. = +0.10% of normal load output.

TABLE II

THREE-UNIT SET—1135-KV-A., 6600-VOLT SYNCHRONOUS MOTOR DIRECT CONNECTED TO TWO 500-KW., 125-VOLT, DIRECT-CURRENT GENERATORS

50 per cent load	Obs. No.	Kw. Input	Kw. Output	Per cent eff.	Deviation from mean
	1	589.3	470.5	80	2.8*
	2	611.3	508	83.2	0.1
	3	597	494.5	83	0.1
	4	605.6	505	83.5	0.4
	5	612.2	512	83.8	0.7
	6	599.8	495.7	82.6	0.5
	7	604.4	499	82.7	0.4
	8	602.4	499	83	0.1
	9	598.4	498.5	83.2	0.1
	10	611.4	507	82.9	0.2
Average	(without 1)	604.72	502.07	82.8	$\frac{0.3}{\sqrt{9} \times 82.8} = \pm 0.12\%$

Test output 502.07 + Seg. loss 101.2 = Seg. loss input 603.27 . $\frac{502.07}{603.27}$ = 83.2% seg. loss eff.

Test input 604.72 — Seg. loss input 603.27 = Stray loss +1.45 kw. = +0.145% of normal load output

* Values in these tables marked with an asterisk rejected on account of being over three times greater than the average.

TABLE III

THREE-UNIT SET—1135-KV-A., 6600-VOLT, SYNCHRONOUS MOTOR DIRECT-CONNECTED TO TWO 500-KW., 125-VOLT, DIRECT-CURRENT GENERATORS

75 per cent load	Obs. No.	Kw. Input	Kw. Output	Per cent eff.	Deviation from mean
	1	879.92	766.5	87.1	1.1
	2	881.8	760	86.2	0.2
	3	942.9	728	77.3	8.2*
	4	892.8	770.3	86.4	0.4
	5	881.9	760.9	86.3	0.3
	6	896.8	766.1	85.4	0.6
	7	896.8	771.1	85.9	0.1
	8	877.4	758.8	86.5	0.5
	9	873.8	752.8	86.2	0.2
	10	891.3	762.3	85.6	0.4
	11	912.1	781	85.6	0.4
	12	879.9	750.4	86.5	0.5
	13	896.8	766.3	85.4	0.6
	14	875.7	744.8	85.1	0.9
	15	853.5	742.4	87	1
Average	(without 3)	885.03	761	86.05	$\frac{0.5}{\sqrt{14} \times 86.05} = \pm 0.15\%$

Test output 761 + Seg. loss 119 = Seg. loss input 880 $\frac{761}{880}$ = 86.5% seg. loss eff.

Test input 885.03 — Seg. loss input 880 = Stray loss + 5.03 kw. = +0.503 % of normal load output

TABLE IV

THREE-UNIT SET—1135-KV-A., 6600-VOLT, SYNCHRONOUS MOTOR DIRECT CONNECTED TO TWO 500-KW., 125-VOLT, DIRECT-CURRENT GENERATORS.

Full load	Obs.	Kw. Input	Kw. Output	Per cent eff.	Deviation from mean
	1	1140	1003	87.8	0.2
	2	1120	1000	89.5	1.9
	3	1157	1003	86.7	0.9
	4	1196	1009	84.2	3.6*
	5	1142	993	87	0.6
	6	1172	1009	86	1.6
	7	1156	1016	87.5	0.1
	8	1142	1000	87.5	0.1
	9	1138	1028	90.5	2.7*
	10	1138	1035	90.9	3.1*
	11	1167	1041	89.2	1.6
	12	1156	1018	88	0.4
	13	1146	1003	87.5	0.1
	14	1169	1022	87.5	0.1
	15	1145	996	87.1	0.5
	16	1161	1016	90.6	2.8*
	17	1120	987	88.2	0.6
	18	1128	984	87.2	0.4
	19	1195	1029	86	1.6
	20	1157	1014	87.6	0.0
	21	1117	993	89	1.4
	22	1120	986	88.7	1.1
	23	1160	1022	88.3	0.7
	24	1139	1012	88.9	1.3
	25	1142	1012	87.6	0.0
	26	1198	1025	85.6	2
	27	1169	1019	87.2	0.4
	28	1140	1003	88	0.4
	29	1150	1009	87.6	0.0
	30	1157	1016	87.8	0.2
	31	1142	1004	87.9	0.3
	32	1145	1006	87.8	0.2
V	33	1161	1001	86.5	1.1
Average	(without 4, 9, 10, 16)	1150	1007.8	87.6	$\frac{0.69}{\sqrt{29} \times 87.6} = \pm 0.14\ \%$

Test output		Seg. loss		Seg. loss input	
1007.8	+	140.5	=	1148.3	$\frac{1007.8}{1148.3}$ = 87.76% seg. loss eff.

Test input		Seg. loss input		Stray loss
1150	—	1148.3	=	+1.7 kw. = +0.17% of normal load output.

Brush and bearing friction including windage = 23.98 kw.

TABLE V

TWO-UNIT SET—1120-KV-A., 6600-VOLT SYNCHRONOUS MOTOR CONNECTED TO A 1000-KW., 250-VOLT, DIRECT-CURRENT GENERATOR

50 PER CENT LOAD

Obs. No.	Input Wm. + $\frac{\text{kw. field}}{\text{ratio trans.}}$	Output millivolts × volts	Ratio	Deviation from mean
1	342.8	10600	30.91	0.213
2	339.8	10420	30.72	0.023
3	340.8	10450	30.65	0.047
4	339.3	10390	30.61	0.087
5	339.3	10380	30.60	0.097
6	338.8	10350	30.58	0.117
7	343.8	10610	30.92	0.223
8	344.8	10530	30.59	0.107
Average	341.2	10466	30.69	$\frac{0.1146}{\sqrt{8} \times 30.69} = \pm 0.132\%$

Ratio of trans. 1789.4

Mult. fact. of shunts 48.6

Kw. 610.1 508 $\frac{508}{610.1} = 83.3\%$ eff. input-output.

Test output + Seg. loss = Seg. loss input

508 + 104 = 612 $\frac{508}{612} = 83\%$ eff. seg. loss.

Test input — Seg. loss input = Stray loss

610.1 — 612 = −1.9 = −0.19% of normal load output.

TABLE VI

TWO-UNIT SET—1120-KV-A., 6600-VOLT, SYNCHRONOUS MOTOR CONNECTED TO A 1000-KW., 250-VOLT DIRECT-CURRENT GENERATOR

50 PER CENT LOAD

Obs. No.	Input Wm. $+\frac{\text{kw. field}}{\text{ratio trans.}}$	Output millivolts × volts	Ratio	Dev. from mean
1	342.25	9980	29.15	0.188
2	346.25	10200	29.43	0.092
3	341.25	10080	29.55	0.212
4	340.25	9990	29.35	0.012
5	337.25	9850	29.21	0.128
Average	341.65	10020	29.388	$\frac{0.1264}{\sqrt{5} \times 29.388} = \pm 0.193\%$

Ratio of trans. 1784

Mult. fact of shunts 50.6

Kw. 608.9 504.4 $\frac{504.4}{608.9} = 82.9\%$ eff. input-output.

Test output		Seg. loss		Seg. loss input	
504.4	+	104	=	608.4	$\frac{504.4}{608.4} = 83\%$ eff. seg. loss.

Test input		Seg. loss input		Stray loss	
608.9	—	608.4	=	+0.5	= +0.5% of normal load output.

TABLE VII

TWO-UNIT SET—1120-KV-A., 6600-VOLT SYNCHRONOUS MOTOR CONNECTED TO A 1000-KW., 250-VOLT, DIRECT-CURRENT GENERATOR

75 PER CENT LOAD

Obs. No.	Input $\text{Wm.} + \frac{\text{kw. field}}{\text{ratio trans.}}$	Output millivolts × volts	Ratio	Dev. from mean
1	496.4	15700	31.60	0.145
2	494.4	15650	31.70	0.045
3	501.4	15910	31.78	0.035
4	499.4	15890	31.80	0.055
5	499.4	15850	31.79	0.045
6	495.4	15730	31.70	0.045
7	495.4	15750	31.78	0.035
8	494.4	15710	31.81	0.065
Average	497.02	15774	31.745	$\frac{0.0587}{\sqrt{8} \times 31.745} = \pm 0.065\%$

Ratio of trans. 1778.6

Mult. fact. of shunts 48.6

Kw. 884.1 768 $\frac{768}{884.1} = 86.8\%$ eff. input-output.

Test output 768 + Seg. loss 114.6 = Seg. loss input 882.6 $\frac{768}{882.6} = 87\%$ eff. seg. loss.

Test input 884.1 — Seg. loss input 882.6 = Stray loss +1.5 = +0.15% of normal load output.

TABLE VIII

TWO-UNIT SET—1120-KV-A., 660-VOLT, SYNCHRONOUS MOTOR CONNECTED TO A 1000-KW., 250-VOLT, DIRECT-CURRENT GENERATOR

75 PER CENT LOAD

Obs. No.	Input $Wm. + \frac{kw. field}{ratio trans.}$	Output millivolts × volts	Ratio	Dev. from mean.
1	494.6	15110	30.59	0.134
2	494.6	15100	30.57	0.114
3	489.6	14850	30.39	0.066
4	492.6	14900	30.23	0.226
5	493.6	15040	30.50	0.044
Average	493	15000	30.456	$\frac{0.1168}{\sqrt{5} \times 30.456}$ = ±0.171%

Ratio of trans. 1777.3

Mult. fact. of shunts 50.4

Kw. 876.4 756 $\frac{756}{876.4}$ = 86.4% eff. input-output.

Test output 756 + Seg. loss 114.2 = Seg. loss input 870.2 $\frac{756}{870.2}$ = 86.9% eff. seg. loss.

Test input 876.4 — Seg. loss input 870.2 = Stray loss +6.2 = +0.62% of normal load output.

TABLE IX

TWO-UNIT SET—1120-KV-A., 6600-VOLT, SYNCHRONOUS MOTOR DIRECT-CONNECTED TO A 1000-KW., 250-VOLT, DIRECT-CURRENT GENERATOR

NORMAL LOAD

Obs. No.	Input $Wm. + \frac{kw. field}{ratio trans}$	Output millivolts × volts	Ratio	Dev. from mean
1	675.3	21800	32.23	0.165
2	647.3	20780	32.10	0.035
3	656.3	21000	32	0.065
4	658.3	21110	32.09	0.025
5	657.3	21070	32.03	0.035
6	656.3	21030	32.01	0.055
7	655.3	21030	32.10	0.035
8	653.3	20970	32.11	0.045
9	655.3	21000	32.01	0.055
10	663.3	21150	31.89	0.175
11	657.3	21040	32.01	0.055
12	660.3	21200	31.99	0.075
13	663.3	21400	32.29	0.235
14	658.3	21190	32.19	0.125
15	661.3	21100	31.92	0.145
Average	658.57	21125	32.065	$\frac{0.0884}{\sqrt{15} \times 32.065} = \pm 0.071\%$

Ratio of trans. 1775.3

Mult. fact. of shunts 48.63

Kw.	1169.2	1027.1	$\frac{1027.1}{1169.2} = 87.85\%$ eff. input-output.
Test output	Seg. loss	Seg. loss input	
1027.1 +	129.2 =	1156.3	$\frac{1027.1}{1156.3} = 88.8\%$ eff seg. loss.
Test input	Seg. loss input	Stray loss	
1169.2 —	1156.3 =	+12.9	= +1.29% of normal load output.

TABLE X

TWO-UNIT SET—1120-KV-A., 6600-VOLT, SYNCHRONOUS MOTOR DIRECT-CONNECTED TO A 1000-KW., 250-VOLT DIRECT-CURRENT GENERATOR

NORMAL LOAD

Obs. No.	Input Wm. + kw. field / ratio trans.	Output millivolts × volts	Ratio	Dev. from mean
1	659.9	21570	32.70	0.58*
2	662.9	21220	32.05	0.00
3	653.9	20940	32.09	0.04
4	658.9	21990	32.19	0.14
5	654.9	20980	32.03	0.02
6	655.9	21080	32.12	0.07
7	657.9	21200	32.21	0.16
8	662.9	21060	31.08	0.25
9	659.9	21180	32.09	0.04
10	656.9	20960	31.90	0.15
Average (without No. 1)	658.2	21179	32.05	0.097 / $\sqrt{9} \times 32.05$ = ±0.107%

Ratio of trans. 1775.3
Mult. fact. of shunts 48.63

Kw. 1170.1 1027.7 $\frac{1027.7}{1170.1}$ = 87.8% eff. input-output.

Test output		Seg. loss		Seg. loss input	
1027.7	+	129.2	=	1156.9	$\frac{1027.7}{1156.9}$ = 88.6% eff. seg. loss.

Test input		Seg. loss input		Stray loss	
1170.1	—	1156.9	=	+13.2	= + 1.32% of normal load output.

TABLE XI

TWO-UNIT SET—1120-KV-A., 6600-VOLT, SYNCHRONOUS MOTOR, DIRECT-CONNECTED TO A 1000-KW., 250-VOLT, DIRECT-CURRENT GENERATOR

NORMAL LOAD

Obs. No.	Input Wm. + kw. field / ratio trans.	Output millivolts × volts	Ratio	Dev. from mean
1	654.9	20250	30.98	0.09
2	647.9	20100	31	0.11
3	650.9	20210	31.02	0.13
4	650.9	20160	30.95	0.06
5	650.9	20150	30.95	0.06
6	655.9	20166	30.75	0.14
7	654.9	20140	30.75	0.14
8	654.9	20250	30.95	0.06
9	654.9	20220	30.90	0.01
10	653.9	20210	30.95	0.06
11	651.9	20120	30.85	0.04
12	656.9	20320	30.90	0.01
13	654.9	20280	30.98	0.09
14	658.9	20300	30.82	0.07
15	655.9	20320	30.95	0.06
16	652.9	20100	30.80	0.09
17	655.9	20240	30.85	0.04
18	652.9	20100	30.80	0.09
19	649.9	20000	30.79	0.10
20	649.9	20050	30.82	0.07
Average	653.5	20184	30.89	$\frac{0.076}{\sqrt{20} \times 30.89} = \pm 0.055\%$

Ratio of trans. 1775.3

Mult. fact. of shunts 48.63

Kw. 1159.7 1017.3 $\frac{1017.3}{1159.7} = 87.7\%$ eff. input-output

Test output		Seg. loss		Seg. loss input	
1017.3	+	129.2	=	1146.5	$\frac{1017.3}{1146.5} = 88.7\%$ eff. seg. loss.

Test input		Seg. loss input		Stray loss	
1159.7	—	1146.5	=	+13.2	= +1.32% of normal load output.

Brush and bearing friction including windage 28.87 kw.

TABLE XII

THREE-UNIT SET—1450-KV-A., 2300-VOLT, SYNCHRONOUS MOTOR DIRECT-CONNECTED TO TWO 500-KW., 1200-VOLT, DIRECT-CURRENT GENERATORS

50 PER CENT LOAD

Obs. No.	Input			Output kw.	Ratio	Deviation from mean
	W_1	W_2	$W_1 + W_2$			
1	172	67	239	496.37	2.074	0.024
2	172	70	242	513	2.118	0.020
3	170	70	240	493.46	2.052	0.046
4	170	67	237	499.80	2.108	0.010
5	174	69	243	511.65	2.182	0.076*
6	171	68	239	502.82	2.101	0.003
7	170	66	236	493.71	2.088	0.010
8	171	67	238	503.94	2.115	0.017
9	168	65	233	491.14	2.106	0.008
10	170	67	237	502.88	2.120	0.012
Average (Without 5)	170.47	67.44		499.66	2.098	$\frac{0.0167}{\sqrt{9} \times 2.098} = 0.265\%$

Phase angle correction factor	0.9982	0.9705			
C. T. ratio	120.4	120.3			
P. T. ratio	19.88	19.90	Kw. input		
Kw	407.3	156.69	563.99		$\frac{499.66}{571.47}$ = 87.43% eff. input-output
Sep. exc. field			7.48		
Total input			571.47		
Total output				499.66	

Test output 499.66 + Seg. loss 68.42 = Seg. loss input 568.08 $\qquad \frac{499.66}{568.08}$ = 87.95% eff seg. loss

Test input 571.47 — Seg. loss input 568.08 = Stray loss +3.39 kw. = +0.34% of normal load output.

TABLE XIII

THREE-UNIT SET—1450-KV-A., 2300-VOLT, SYNCHRONOUS MOTOR DIRECT CONNECTED TO TWO 500-KW., 1200-VOLT, DIRECT-CURRENT GENERATORS

75 PER CENT LOAD

Obs. No.	Input			Output kw.	Ratio	Deviation from mean
	W_1	W_2	W_1+W_2			
1	253	103	356	770.17	2.162	0.005
2	250	98	348	745.05	2.14	0.017
3	250	99	349	751.56	2.15	0.007
4	250	97	347	755.58	2.175	0.018
5	250	98	348	748.82	2.147	0.010
6	250	97.5	347.5	751.79	2.164	0.007
7	249	97.5	346.5	743.78	2.143	0.014
8	249	97.5	346.5	745.96	2.152	0.005
9	250	98	348	757.77	2.176	0.019
10	250	97.5	347.5	751.10	2.163	0.006
Average	250.1	98.3		752.158	2.157	$\frac{0.0108}{\sqrt{10}\times 2.157}$ $= \pm 0.1586\%$

Phase angle corr. factor	0.99804	0.9719				
C. T. ratio	120.1	120				
P. T. ratio	19.88	19.9	Kw. input			
Kw.	596.1	228.13	724.23			
Sep. exc. field			8.085		$\frac{752.158}{832.315}$	= 90.37% eff input-output
Total input			832.315			
Total output				752.158		

Test output		Seg. loss		Seg. loss input		
752.158	+	79.871	=	832.029	$\frac{752.158}{832.029}$	=90.508% eff. seg. loss.

Test input		Seg. loss input	Stray loss
832.315	—	832.029	= +0.286 kw. = +0.286% of normal load output.

TABLE XIV

THREE-UNIT SET—1450-KV-A., 2300-VOLT, SYNCHRONOUS MOTOR DIRECT-CONNECTED TO TWO 500-KW., 1200-VOLT, DIRECT-CURRENT GENERATORS

NORMAL LOAD

Obs. No.	Input			Output kw.	Ratio	Deviation from mean
	W_1	W_2	W_1+W_2			
1	338	132	470	1021.8	2.178	0.001
2	330	124	454	992.7	2.183	0.004
3	331	130	461	1001	2.172	0.007
4	333	137.5	470.5	1026.3	2.181	0.002
5	330	137	467	1020.1	2.183	0.004
6	330	136	466	1014.9	2.179	0.000
7	330	137	467	1020.9	2.183	0.004
8	326	131	457	994.7	2.173	0.006
9	330	131	461	1003.3	2.176	0.003
10	330	131	461	1002.8	2.175	0.004
11	330	132.5	462.5	1010.8	2.187	0.010
12	332	131	463	1004.9	2.173	0.006
13	334	134	468	1020.2	2.182	0.003
14	330	132	462	1005.6	2.179	0.000
15	329	132	461	1016	2.204	0.024*
16	331	134	465	1011.6	2.175	0.004
17	330	132.5	462.5	1005.1	2.175	0.004
18	329	128	457	1000.7	2.190	0.011
19	330	131.5	461.5	998.7	2.162	0.017
20	330	131.5	461.5	1001.2	2.189	0.011
Average (without 15)	330.74	132.37		1008.28	2.179	$\frac{0.0053}{\sqrt{19} \times 2.179} = \pm 0.574\%$

Phase angle corr. factor	0.99804	0.9719	
C. T. ratio	119.8	119.8	
P. T. ratio	19.88	19.9	Kw. input
Kw.	785.98	306.55	1092.53

Sep. exc. field 10.832

Total input 1103.36

Total output 1008.28

$$\frac{1008.28}{1103.36} = 91.59\% \text{ eff. input-output}$$

Test output 1008.28 + Seg. loss 96.068 = Seg. loss input 1104.348

$$\frac{1008.28}{1104.348} = 91.34\% \text{ eff. seg. loss.}$$

Test input 1103.36 — Seg. loss input 1104.348 = Stray loss − 0.988 = —0.0988% of normal load,

Brush and bearing friction including windage = 18.55 kw,

TABLE XV

THREE-UNIT SET—1450-KV-A., 2300-VOLT, SYNCHRONOUS MOTOR DIRECT-CONNECTED TO TWO 500-KW., 1200-VOLT, DIRECT-CURRENT GENERATORS

125 PER CENT LOAD

Obs. No.	Input			Output kw.	Ratio	Deviation from mean
	W_1	W_2	$W_1 + W_2$			
1	407	165	572	1259.69	2.200	0.024
2	410	168	578	1259.32	2.181	0.005
3	407	168	575	1250.79	2.178	0.002
4	410	168	578	1249.87	2.164	0.012
5	405	167	572	1246.75	2.178	0.002
6	405	167	572	1267.54	2.216	0.036*
7	412	169	581	1267.25	2.181	0.005
8	414	170	584	1252.96	2.148	0.028
9	406	167	573	1248.80	2.180	0.004
10	412	169	581	1262.16	2.171	0.005
Average (without 6)	409.2	167.9		1255.29	2.176	$\frac{0.0097}{\sqrt{9} \times 2.176} = 0.148\%$

Phase angle corr. factor	0.9980	0.9719	
C. T. ratio	119.6	119.6	
P. T. ratio	19.88	19.90	Kw. input
Kw.	970.96	388.37	1359.33
Sep. exc. field			12.338
Total input			1371.668

$$\frac{1255.29}{1371.668} = 91.512\% \text{ eff. input-output}$$

Total output 1255.29

Test output 1255.29 + Seg. loss 118.025 = Seg. loss input 1373.315

$$\frac{1255.29}{1373.315} = 91.409\% \text{ eff. seg. loss.}$$

Test input 1371.668 — Seg. loss input 1373.315 = Stray loss —1.647 kw. = —0.165% of normal load output.

A paper presented at the Midwinter Convention of the American Institute of Electrical Engineers, New York, February 27, 1913.

SOURCES OF ERROR IN THE EFFICIENCY DETERMINATION OF ROTATING ELECTRIC MACHINES

BY ELMER I. CHUTE AND WILLIAM BRADSHAW

The efficiency of any apparatus is the ratio of its net power output to its gross power input. According to the Standardization Rules of this Institute this may be determined with certain limitations by either of two methods, by the input-output method or by the summation of separate losses.

By the first method the input and output are to be measured directly, either mechanically or electrically as the case may be; by the second the separate losses are to be determined either individually or collectively, and, in the case of generators, added to the output to give the input, or, in the case of motors, subtracted from the input to give the output. In either method the efficiency equals the output divided by the input.

To determine the loss values, measurements of resistance, core and frictional losses are involved, quite elaborate tests being usually required to determine the latter two. In the input-output test two measurements only are required, which to the uninitiated seem as though they should be obtained without any special preparation whatever, especially where both values to be measured are electrical, as will be assumed in this paper.

If all measurements could be taken with perfect accuracy there could be no discussion whatever on this subject, or even if all inaccuracies affected the final results in equal proportion, one would be as reluctant to take separate loss measurements for efficiency calculations as some are now to take input-output tests. But this is far from the case.

Inaccurate readings of power input to the driving motor, while determining the core and frictional losses, are practically the only sources of error in loss measurements. These errors, moreover, enter into the result only indirectly. In a paper* presented at the annual convention at Boston, it was shown from data on a large number of machines that even in the most extreme case when the effect is largest, an error of five per cent in the actual readings taken affects the efficiency by only one-half of one per cent. The effect in the average case is less than two tenths of one per cent. The real variation is probably much less than this, for the five per cent error assumed is a very high value seldom occurring even in commercial practise.

In the input-output test, on the other hand, all errors in measurement enter directly into the result. The five per cent above assumed (and it is as logical to assume it in one case as in the other, under equal conditions) would probably affect the efficiency at least five, if not ten per cent, depending on whether the errors in measurement of the output and input were cumulative or tended to neutralize. The probability of this may be more apparent from the following analysis of all the incorrect conditions that might exist.

Input	correct,	output	high	—	Effect	directly	proportional	to	error.
"	"	"	low	—	"	"	"	"	"
"	high	"	correct	—	"	"	"	"	"
"	low	"	"	—	"	"	"	"	"
"	high	"	low	—	"		"	to sum of error	
"	low	"	high	—	"	"	"	"	"
"	low	"	low	—	Errors	tend to	neutralize		
"	high	"	high	—	"	"	"		

It is apparent, then, that if efficiencies as consistent as those determined by the loss method are to be obtained, every precaution conceivable must be taken that the observations made, or readings taken, are as little in error as possible. It is this necessity that offsets the otherwise apparent simplicity of the input-output method, and renders the test far more elaborate and costly to conduct than any other method of efficiency measurement.

A brief examination of the sources of error will bring out some of the precautions that must be observed. These may be classified as follows:

**Determination of Power Efficiency of Rotating Electric Machines* E. M Olin, TRANS. A. I. E. E., Vol. XXXI, p. 1695.

a. Instrument errors.
   1. An unsuitable selection of meters or associated apparatus.
   2. Inaccuracies of calibration.
   3. Improper location or use of the instruments.

b. Observation errors.
   1. Personal errors.
   2. Errors due to fluctuation of meter indicators, depending on character of circuit.

c. Errors due to varying conditions of operation.
   1. Change in constant losses.
   2. Comparison with separate loss values not made under same conditions.

Instrument errors may be avoided to a large extent in any first class laboratory or manufacturing plant, but become of vital importance in isolated stations where suitable instruments are not available, or in plants when apparatus is much crowded or where very heavy currents are the rule.

Measuring instruments must be selected with the conditions thoroughly in view. Well damped indicating instruments have proved the best in practise. Watt-hour meters placed on both the incoming and outgoing lines would naturally seem to be the best solution of the problem, but repeated tests with the best types of available instruments have not proved as satisfactory as those taken with indicating meters.

The accurate calibration of the meters used is essential. For ordinary work the accuracy of one-half of one per cent of the full scale reading at any point on the scale, which is the usual guarantee, is sufficient, but in an input-output test even this error, considering the number of meters used, may lead to an error of two per cent or more, in the efficiency of the apparatus under test. Meters if used with care can be depended on to hold this accuracy very well, but to make certain that no change in calibration has occurred during a test it is always advisable to check them at the end as well as the beginning of the test.

The proper locating of meters, especially those of the moving coil type, must be given careful consideration. They should rest on a firm, rigid support and be so located as to be free from any external field effects. It must not to be assumed that even meters of the shielded types can be exposed indiscriminately to stray fields. As a general rule the same precautions should apply to them as to others.

When one considers the large number of meters necessary on such tests, the necessity of eliminating or accounting for even the most minute errors in them is evident. In the simplest case, when the incoming and outgoing power are each direct current, four meters are necessary. In the case of synchronous converters, or motor-generators other than d-c. to d-c., the number is increased to at least seven and possibly ten, all of which enter directly into the result. In addition to the measuring instruments a number of series and potential transformers are usually required. These, however, may be very accurately calibrated provided the proper equipment is available, and can be depended on to hold their calibration well. The name-plate ratio in the case of current transformers when used at from 70 to 100 per cent of their rated load is only accurate within one-half of one per cent. Potential transformers when properly compensated may be considered accurate to three-tenths of one per cent.

Errors in reading indicating instruments vary, depending to a large extent on the variations in the circuit in which they are connected. The ideal condition, of course, is where the characteristics of the circuit permit the indicator of the meter to move at once to its proper location, and remain there as though fixed. In such cases the error is due to the inability of the average reader to locate the indicator closer than to possibly 0.1 of a division. This would introduce an error of approximately 0.13 per cent in the reading if taken at about the middle of a scale of 150 divisions. Such an error could readily be neglected if occurring in one meter only, but if such an error should exist in each of the meters used and the effect were cumulative with respect to the final result, it is evident that the error would soon be of considerable magnitude.

This ideal condition, however, may be said never to exist, the nearest approach to it being in cases where the source of power is not more than one step removed from a well regulated prime mover on which there is no variable load, and liberally designed grid resistances or a water rheostat used to absorb the power output of the apparatus under test. Under this condition the pointers of the meters will swing back and forth over a small part of the scale, perhaps only a division or two. The effort of estimaing the position of the pointer is added to the former effort to divide up the space correctly. The liability to error therefore is increased materially.

However, under the usual conditions met with in commercial work, power must be taken from generators serving many other machines, the load on which is likely to be quite variable. Loading these machines, especially the larger sizes, on water rheostats or resistances is not a desirable economical proposition. Loading-back methods, therefore, are ordinarily resorted to, so that only the losses have to be supplied from the prime mover. Under such conditions the swinging of meter pointers will be greatly increased. It not infrequently occurs that meters must be read while their indicators are wandering more or less irregularly over as much as 25 per cent of the scale. That errors of reading increase much more rapidly than the increase in length of the indicator swing is not at all improbable, though no accurate data are available as to the probable law.

If there were no compensation of errors, consistent results from data obtained under such conditions would be out of the question. It may be proved by certain laws of mathematics that the probable variation of the average of a number of readings differing widely among themselves is but little from the true but unknown value. However, the accuracy is not increased in direct proportion to the number of readings, but in proportion to their square root, so that very little is gained by taking more than ten or fifteen readings. In view of this it is customary to take readings in series of ten, a simultaneous reading being taken of all meters involved, at intervals of about ten seconds, until ten are obtained. The average of these is considered as the nearest approach to the true value. In a certain case where the maximum variation from the mean value of ten readings was 1.5 per cent, the variation, as computed from the mathematical laws previously referred to, was less than 0.2 per cent from the true, but unknown, value. This would all have been very well if the average of ten other such readings taken under as nearly as possible the same conditions had not given 1.7 per cent difference in efficiency, and according to the same laws was also accurate to within 0.2 per cent. As this is the condition that is continually arising, it is evident that even the laws of chance can give us very little help in the solution of the problem.

It would seem as though an actual change in the efficiency of the apparatus had taken place, due possibly to a change in operating conditions, or in the constant losses of the machines. But when one stops to analyze the magnitude of any such possible change one finds a considerable discrepancy still unaccounted for. However, such changes must not be ignored, especially in the case of tests on synchronous converters, where the total losses are but a very small proportion of the output of the machine. To make sure that no change of any consequence has taken place it is always well to check the frictional losses both before and after the test. This also insures that the determination of efficiency by separate losses, which is the best criterion available as to the accuracy of the input-output test, is made under identically the same conditions.

It is conceded that the efficiency by the " summation of losses " is not the true operating efficiency of the apparatus, as no account is usually taken of the so-called " load " or " stray " losses. It has been proposed to account for this discrepancy by a system of factors that depend for their magnitude on the type of apparatus. As this additional loss under load consists of additional core and copper losses, due to distorted field effects and eddy currents, it is suggested that the factor required be applied to their sum. In another paper* at present before this Institute, the magnitude and determination of this constant have been discussed and its limits experimentally determined to some extent. It is interesting to compare the probabilities of error of this method with those already existing.

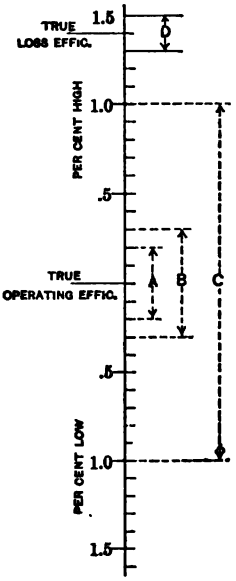


FIG. 1

Assume a concrete case of a 500-kw. direct-current generator the losses of which total 10 per cent or 50 kw., and that they are measured with an accuracy of 2 per cent. Of this 50 kw., 70 per cent., or 35 kw., will be assumed as a fair value of the sum of the core and armature copper losses. The value of the load factor for normal load, as suggested in the paper previously referred to, is about 1.3 for direct-current machines. It seems that this can be determined within ± 2 per cent or, in other words, may vary between 1.27 and 1.33.

Using the values assumed above, it will be found that the efficiency by losses at normal load will be between 91.6 and 91.8

*_Determination of Load Loss Correction Factors for Rotating Electric Machines_, by E. M. Olin and S. L. Henderson; page 479 of this volume.

and after applying the load factors, allowing for the above-mentioned variation, the efficiencies will be between 90 and 90.6 per cent.

This difference depends chiefly on the ratio of the sum of the core and the copper losses to the total loss. As this ratio decreases with the larger machines, or in those whose total losses are a small proportion of the output, the variations are reduced. The input-output test shows just the opposite tendency, for the same error is liable with small losses as with large ones and therefore becomes a much larger factor in the final efficiency.

The foregoing conditions may be represented graphically as in Fig. 1, in which *A* represents the probable limit of error in an input-output test, conducted on stable circuits and liberally designed water rheostats or grid resistances, in which all possible precautions have been taken. The limits represented by *C* cover the probable limits of error where this test is taken on commercial circuits, but where first-class testers are reading the meters and conducting the test. Under other conditions the results are entirely too variable to be represented in such a diagram. The limits covered by *B*, mark the probable limit of error of efficiencies determined from losses, in connection with correcting factors, in which the correcting factors have been determined from input-output tests as conducted in the manner *A*, or from other more accurate methods whenever possible. *D* represents the possible variation in the loss efficiency.

It is apparent that input-output tests under the best conditions can be taken with a considerable degree of accuracy, but does the additional accuracy gained over the simple method suggested warrant the industry incurring the larger additional cost of equipment and labor necessary to conduct such tests?

Efficiency values, while they should represent average operating conditions as nearly as possible, are of especial value for the comparison of the product of different manufacturers. Methods, then, that can be standardized and easily duplicated should be used in the determination of this value, so that the product of all will be on the same basis.

A paper presented at the Midwinter Convention of the American Institute of Electrical Engineers, New York, February 27, 1913.

BRUSH FRICTION AND CONTACT LOSSES

BY H. F. T. ERBEN AND A. H. FREEMAN

The rapid development of modern electrical apparatus has necessitated most careful selection of brushes to meet the varying conditions of current, voltage, speed, etc., which in turn has required a very thorough study of their characteristics and losses.

The present Standardization Rules (Sections 103, 111, 112) fail adequately to cover this most important subject, and appreciating the necessity for more definite methods of determining brush losses, an exhaustive series of tests has been conducted during the past year, results of which are herewith presented.

The two sources of loss to be considered, either on commutators or on collectors, are brush friction and brush contact drop.

Friction. The friction of two bodies in sliding contact is governed by—1st, the nature of the materials, 2nd, the pressure forcing them together. The actual value of this friction (expressed in watts or h.p.) can only be determined by trial. Theoretically, the magnitude of the friction loss between two materials varies directly with the pressure and, therefore, the ratio of the force tending to retard the relative movement of the two bodies to the force pressing them together (coefficient of friction) should remain constant for all pressures less than that producing crushing or grinding of the materials.

Determination of Friction. To determine the coefficient of friction, a commutator was mounted on a shaft directly driven by an adjustable speed motor of such size that the maximum total load was less than the rated capacity of the motor. As many brushes as possible were assembled on the commutator in order that the brush friction might be a large part of the total load. Tests were made on numerous grades of brushes, in various types of brush holders, operating in both directions of rotation and through wide ranges of speed and pressure. The data thus obtained give the friction of brushes when carried in commercial brush holders. We are not yet prepared to give the full and complete results of these tests, as many of the results are now being verified. We show, however, on Curve 1 the coefficient of friction obtained at various speeds with one type of graphite brush operated at an angle of 37½ deg. leading. Fig. 1 shows what is meant by various angles, leading and trailing.

Test was also made to show the effect of temperature on coeffi-

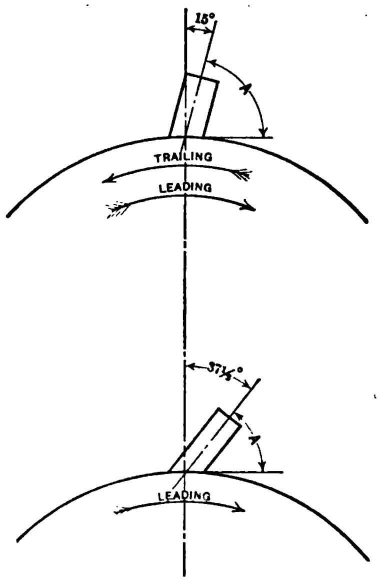


FIG. 1

cient of friction by enclosing a commutator in an asbestos-lined box, placing resistance grids under the commutator for a source of heat and piping a blower to the box to regulate and hold the temperature at various values. Tests were made at various speeds and at 40, 60, 75 and 100 deg. cent., and it was found that the change in input to motor over wide ranges in temperature was so slight as to be negligible in commercial applications.

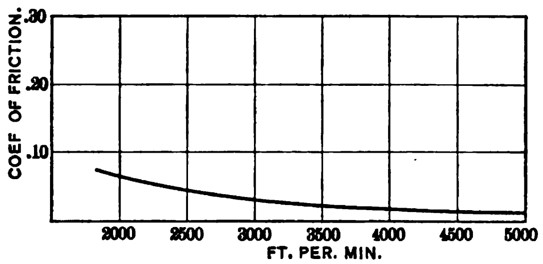
We will later present similar curves showing coefficient of friction values with various types of brushes when operated at different angles.

Application of Data on Brushes. Assuming the coefficient of friction as shown on curve to be applicable to the type of brush holder used, the friction should be calculated, as follows:

$$\frac{P \times S \times V \times F \times 746}{33{,}000} = W$$

When W = Watts loss, brush friction.
P = Total applied pressure on brushes.
V = Velocity, commutator or ring, in ft., per. min.
F = Coefficient of friction.
S = Sine of angle A (Fig. 1).

For example, on a commutator 25 in. (63.5 cm.) in diameter running 2500 ft. (763 m.) per minute with 60 grade S brushes



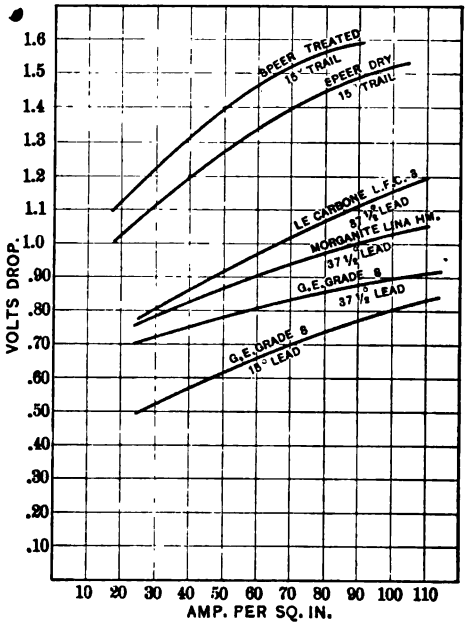
CURVE 1.—COEFFICIENT OF FRICTION, GRADE S BRUSHES
Angle 37½ deg. leading. Approx. 2 lb. per brush applied pressure

at an applied pressure of 2 lb. (0.907 kg.) per brush operating at 37½ deg. angle.

F = 0.05 (Curve 1).
S = 0.783.

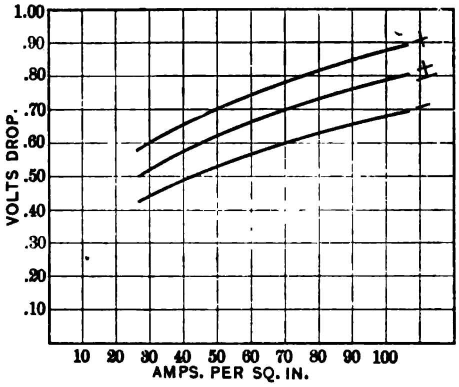
$$W = \frac{60 \times 2 \times 0.793 \times 2500 \times 0.05 \times 746}{33{,}000} = 270 \text{ watts}$$

Contact Resistance. Contact drop varies with the characteristics of the materials, with temperature, speed and pressure. Generally, the volts drop under the brush which is positive to the rotating element (negative brush on a d-c. generator) is greater than under the brush which is negative to commutator (positive brush on generator.) Curve 2 shows average volts drop with



CURVE 2.—BRUSH CONTACT DROP—GROOVED COMMUTATOR

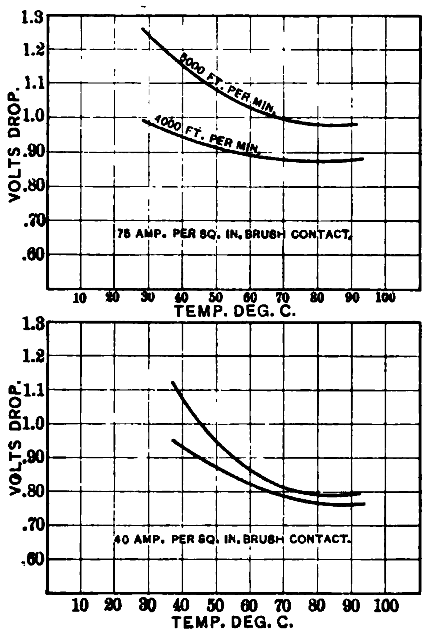
Average of positive and negative.
Average speed 2500 to 5000 ft. per min. Uniform spring pressure.



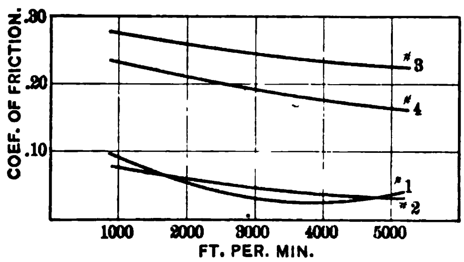
CURVE 3.—BRUSH CONTACT DROP—GRADE *S* BRUSHES—GROOVED COMMUTATOR—65 DEG. CENT.

Average speed 2500 to 5000 ft. per min. 15 deg. lead.
\+ = Current from brush to copper.
— = Current from copper to brush.

carbon and graphite brushes on grooved commutator. Curve 3 shows volts drop, positive and negative, and average between them. Curve 4 shows combined effect of temperature and speed



Curve 4.—Effect of Temperature and Speed on Brush Contact Drop—Graphite Brush, Grooved Commutator



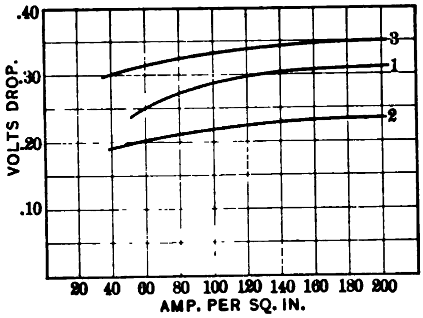
Curve 5.—Coefficient of Friction, Copper-Graphite Brushes

1—Metite grade *L*.
2—Lecarbone Metal, No. 2.
3— " *K K* 3.
4—Morganite *C M* 3.

on contact drop with graphite brushes. Curve 5 shows coefficient of friction of copper-graphite brushes such as metite, morganite CM-3, etc. Curve 6 shows contact drop with copper-graphite brushes or same grades. The data on contact drop shown on accompanying curves give the average of the drop under brushes of both polarities. The watts loss from contact resistance will then be

$$C\,V = W$$

in which W = Watts loss.
C = Current in amperes.
V = Total volts drop (twice volts drop shown on curve).



CURVE 6.—BRUSH CONTACT DROP—COPPER-GRAPHITE BRUSHES
Average of positive and negative.
1—Metite, grade *L*.
2—Morganite *C M* 3.
3—Lecarbone *K K* 3.

SUMMARY

It is apparent from the foregoing that the niceties of adjustment found necessary in obtaining coefficient of friction show the hopelessness of determining the true friction loss by the method outlined in the existing Standardization Rules. It is evident that in measuring brush friction by reading the input to the machine with brushes up and brushes down there is a great element of error due to the fact that the value of brush friction is such a small percentage of the total input. It would, therefore, seem obvious that the Standardization Rules should be so amended as to allow the determination of brush losses by calculation from data which have been obtained by the most reliable methods and approved by the Institute.

A paper presented at the Midwinter Convention of the American Institute of Electrical Engineers, New York, February 27, 1913.

METHODS OF DETERMINING BRUSH LOSSES DUE TO CONTACT AND FRICTION

BY H. R. EDGECOMB AND W. A. DICK

1—Determination of Contact Voltage Losses

I. *Introductory.* Losses in voltage at the brushes of an electric machine are caused primarily by high resistance contact between the brushes and the rotating element (slip ring or commutator). Augmenting this contact resistance voltage loss there is, as stated by F. W. Carter, in *Electrical World*, June 29, 1912, an ionization voltage induced at the point of contact between the brush and the rotating part.

The value of the voltage loss resulting from these two causes is affected by: (a) composition and physical characteristics of brush; (b) composition and surface of rotating part; (c) fit of brush in holder; (d) brush pressure; (e) peripheral speed of rotating part; (f) specific resistance of brush; (g) design conditions which control reversal of current under brush; (h) temperature; (i) humidity; (j) current density; and (k) lubrication. The first seven of these may be made constant for a given test, the last four are more or less variable. It is the fact that the test readings indicate the composite effect of these variables that makes them so difficult to analyze. This has led certain brush manufacturers to state that they are fortunate if they can check results within 25 per cent. Our experience bears out this statement..

II. *Apparatus.* The ideal test apparatus for determining voltage losses due to contact should have facilities for varying pressure, speed, composition of rotating part, temperature, humidity, current density and lubrication, and for accurately measuring degree of variation. Specific resistance, composition of brush and design conditions are of course constant for any given test. The range of variation should be as follows: brush pressure, 0 to 10 lb. per sq. in. (0 to 703 g. per sq. cm.); peripheral speed, 500 to 10,000 ft. (152 to 3048 m.) per. min.; composition of rotating part, copper, brass, bronze, steel and cast iron for collector rings and copper-mica, plain and undercut, for commutators; temperature, up to 100 deg. cent.; humidity up to 100 per cent saturation; current density up to the glowing point of the brushes being tested. Amount of lubrication may be measured by weight. It is assumed that apparatus for these tests must be sufficiently massive to prevent vibration and that the electric circuits should be of ample cross-section. Apparatus should be direct-connected to a source of power in preference to being belted, as the variation in speed and the vibration due to the belt sometimes seriously affect the test.

Most testing outfits are arranged to provide for measuring all these variables and for controlling all except humidity and temperature. Humidity has such an important part in voltage losses that unless it is controlled the record of a test will be seriously affected, and as far as we know, no attempt to do this has been made up to this time. For the control of temperature, however, Dr. Arnold has used an electrically heated collector ring and his researches as recorded in *Elektrotechnische Zeitschrift*, March 21, 1907 have established a very useful set of facts concerning effect of temperature on commutation.

The apparatus used in getting the results later referred to (Figs. 1 and 2,) consists of a d-c. generator, the commutator of which has its bars short-circuited by a number of turns of copper wire soldered around the neck. Six ingoing and six outgoing brushes were used. These were well plated and shunted. Fine copper wire was bound tightly into a groove around the brush near the contact point and insulated leads were brought out along a beveled corner of the brush. A power-operated controller connected the brushes consecutively with a graphic voltmeter making records of about 15 sec. duration for each brush. The return was through a copper leaf brush bearing on the shaft which was grounded to the commutator. The records consisted of a series of dots, and indicate quite accurately the range of travel of the voltmeter needle.

Temperature measurements were made with the potentiometer, using thermocouples inserted in the brushes near the contact surface. Current was measured by an indicating ammeter. Humidity was measured by a hygrodeik.

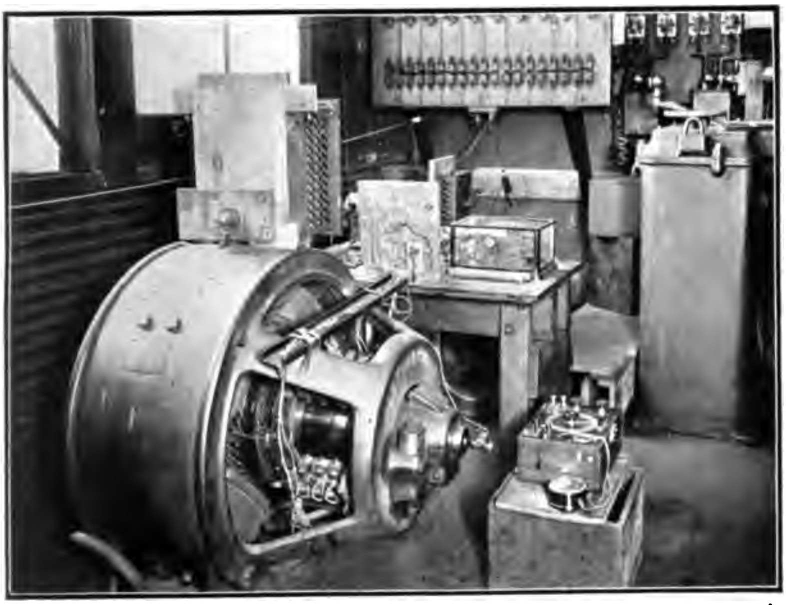


FIG. 1 [EDGECOMB AND DICK]

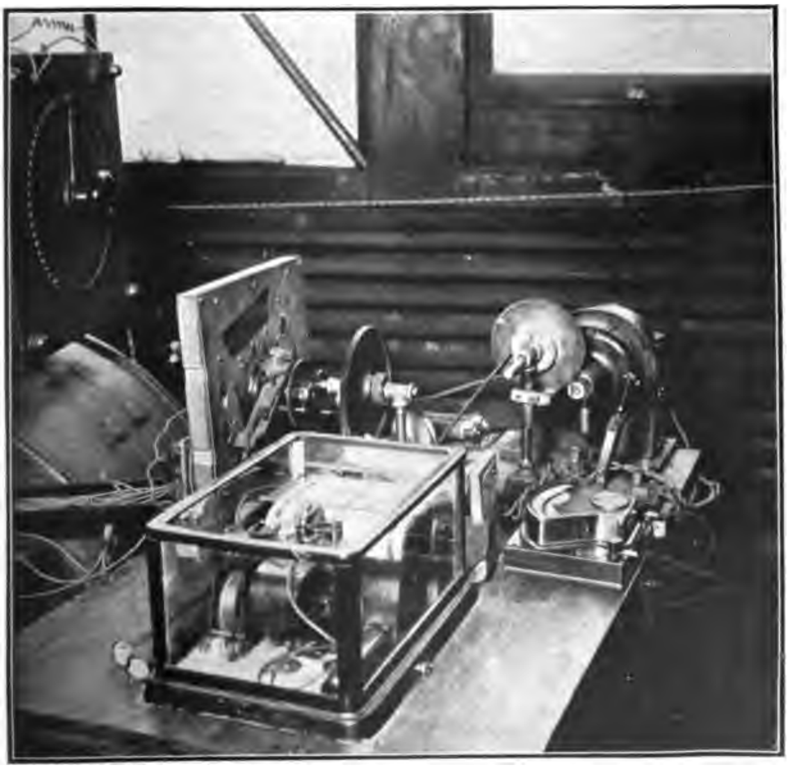


FIG. 2 [EDGECOMB AND DICK]

The graphic meter was frequently calibrated and on its record strip was entered the log of the test, including current density, voltage drop for each brush, temperature, humidity and individual brush pressure.

III—*Surface Variations*. In addition to the variations in brush drop due to causes previously mentioned, the graphic meter has identified two other sources from either of which there might be at least 50 per cent variation on voltage loss.

At the beginning of the test the brushes were fitted as carefully as possible and the machine was run for a day without load to insure a good brush contact. The current was then applied and after a steady temperature was attained it was noted that the voltmeter showed relatively small momentary fluctuation,

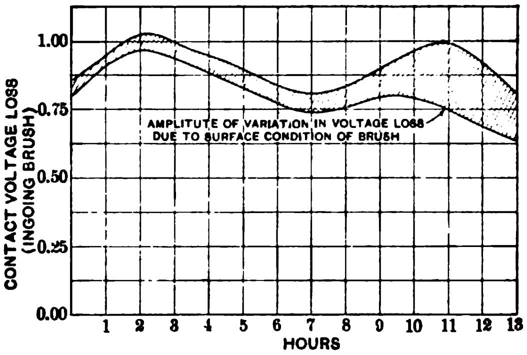


FIG. 3

about 6 per cent (Fig. 3.) The test was continued for 13 hours and during this time the fluctuation increased to 29 per cent. This was checked several times by running the commutator without current for about ten hours and repeating test with practically the same results. During these tests nothing was done to the surface of the commutator.

It is evident that this fluctuation in voltage drop is related to the surface condition of the brush due to the passage of current, as it was controlled by polishing the brush without voltage. We may assume that a disintegrating action takes place, due to the passage of current, which either reduces the actual surface contact or which momentarily separates the brush from the commutator, or which causes migration of the points of contact over paths of greater or less resistance.

The second source of variation has to do with the surface condition of the commutator. After a week's run with current on, when conditions have become as stable as possible the commutator was wiped with clean filter paper and the removal of the surface coating caused an instantaneous increase in the voltage drop, particularly at the ingoing brush. Return to normal conditions occurred in about an hour. Analysis of the material removed showed it to be all carbon, none of the copper oxide having been removed.

As a probable explanation of this phenomenon it is suggested that the fine carbon dust which adheres to the commutator serves as a plastic medium between brush and commutator, adjusting itself to each brush, giving better contact and less voltage loss.

These two causes of variation are brought out because they must be reckoned with in any test for contact voltage loss. It is essential to accurate results that all tests be made under the same conditions and this implies the avoidance of the unstable periods referred to.

Another effect, perhaps of minor importance, was recorded by the graphic meter. At one time the room was filled with dust, and this caused a very marked amplitude in the voltage fluctuations, probably due to the introduction of the particles of dust under the brush and its consequent separation from the commutator.

IV. *Variation Due to Materials and Operating Conditions.*

a. Composition and physical characteristics of the brush: Relative density, proportion of graphitic to plain carbon, the percentage of metal, the percentage of non-conducting abrasive material in the composition, and the temperature, time and manner of firing, all play an important part in determining the contact voltage loss.

b. Composition and surface of rotating part: Voltage loss will be less for material which does not form a non-conducting oxide on its surface. It was noted that brass collector rings containing zinc became coated with an oxide which increased the contact resistance. The oxide was removed with acid and the resistance reduced. Voltage loss is probably increased by the glaze or polish which results from operating a machine.

c. Fit in holder: Continuous contact between brush and rotating part is seriously affected by the inaccuracies of fit of the brush in the brush holder.

d. Brush pressure: It is the practise of brush manufacturers to make contact drop tests at the pressure considered most suitable for the brush being tested, and for this reason very few data on the effect of brush pressure are available. It is known, however, that contact voltage loss varies inversely with brush pressure and as increased pressure causes increased friction, the resulting temperature rise should further decrease the voltage loss.

e. Peripheral speed: Contact voltage loss will increase with peripheral speed because the contact between the brush and rotating part will be made less intimate by the increased jumping of brush due to irregularities on the surface of rotating part.

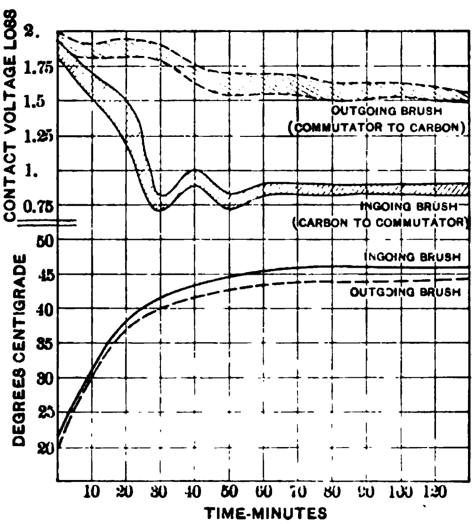


FIG. 4

f. Specific resistance of brush: Contact voltage loss may be expected to vary with the specific resistance of the brush.

g. Design conditions: This feature is not considered, as it is outside the scope of this paper.

h. Temperature: The temperature effect accompanying the passage of current through a brush-commutator circuit is shown in Fig. 4. The maximum efficient temperature appears to be determined either by the glowing point of the carbon or by the amount to which the specific resistance of the brush can be reduced by heating and not cause excessive sparking due to short-circuited coils under the brush. The most efficient point may, however, be higher than it is desirable to use in actual practise.

i. Humidity: Fig. 5 shows a somewhat remarkable relation between contact voltage loss and humidity. In a run of fifty hours the contact drop curve rises and falls with the humidity curve. The fact that the amounts of increase and decrease are not always proportional can be credited to other definite causes such as temperature, surface variation, etc.

j. Current density: Voltage loss will increase with current density.

k. Lubrication: The use of local lubrication is not viewed with favor on account of the resulting gumming of the commutator. Some manufacturers add lubricants to the brushes by impregnation and graphitic brushes are made to depend upon their graphite for lubrication. The effect of a lubricant upon contact voltage loss is clearly a question of the conductivity or

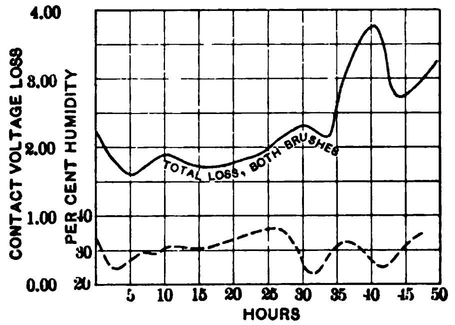


Fig. 5

non-conductivity of the lubricant used. Graphite should be a less harmful lubricant than paraffin for this reason.

V. *Conclusions and Recommendations.* When the tests which have been recorded in this paper were begun, it was with a conviction that unless the variables which have been the despair of investigators could be eliminated or controlled, very little profit would result from further tests. With means for identifying and controlling these variables, we believe results should check within 5 per cent instead of 25 per cent, and if brush manufacturers have means for accurately determining the characteristics of the brushes now being produced, they will certainly be better able to develop new brushes having better characteristics.

To arrive at a definite figure for the voltage loss due to contact which may be expected from each brand of brush, it is essential that tests be made under uniform .conditions, particularly as to brush pressure, peripheral speed, surface conditions, composition and diameter of rotating parts, temperature, humidity and current density.

Fig. 6 presents a typical set of curves for recording the data pertaining to each kind of brush. These include the more important characteristics: current density, temperature, pressure and contact voltage loss. Suitable multipliers are to be used to cover composition of rotating part, speed and humidity.

Because of the limited time for making tests, the actual voltage loss values for the principal types of brushes cannot be presented in this paper. A complete statement of these figures may be expected at a later date.

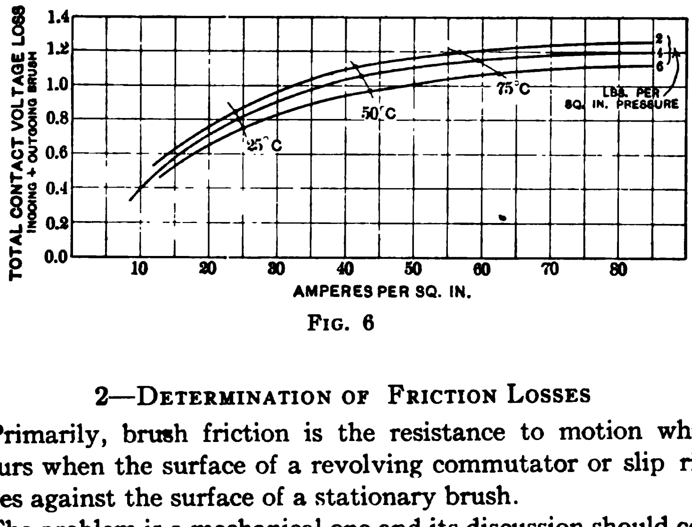


FIG. 6

2—DETERMINATION OF FRICTION LOSSES

Primarily, brush friction is the resistance to motion which occurs when the surface of a revolving commutator or slip ring slides against the surface of a stationary brush.

The problem is a mechanical one and its discussion should consider:

I. The friction formula with reference to modifications for dry surfaces.

II. Test methods and apparatus.

III. Convenient units for expression of brush friction values.

IV. Effect on brush friction of composition of revolving part of brush, and angle of brush, of surface condition of revolving part, of peripheral speed, of brush pressure and of contact area of brushes.

V. Proposed formulas and typical curves for use of designer.

I. The friction formula for perfect contact conditions is expressed as follows:

$$K = \frac{F}{N} \text{ or } F = K\,N$$

in which K = coefficient of friction.
F = force required to make one body slide on another.
N = normal pressure.

In the case of dry surfaces, F will be less than $K\,N$ up to a certain value for N, after which F will rapidly increase until seizure occurs. This feature as it affects seizure does not enter into usual brush tests because the pressures are all well below the critical point.

II. Tests for brush friction have been made in two ways: 1st, electrically, by measuring watts expended in overcoming friction of brushes, and 2nd, mechanically, by weighing directly the friction resistance of brushes pressing against a rotating element.

When the electrical method is used the machine under test is connected to a source of power with facilities for measuring the power used. The difference between the power required to maintain rotation with brushes resting on rotating element and the power required when the brushes are removed is the result desired. This method is used in all commercial tests. Too many variables, however, render it unfit for use in investigating the laws of brush friction. We have noted variations of 100 per cent within a few minutes. For this reason the comparison of commercial tests on a large number of machines gives very little reliable information.

In the mechanical method the brushes are supported in a delicately balanced cage, mounted on ball bearings and provided with an arm attached to a spring. This spring resists the tendency to revolution due to friction of brushes and its extension is measured in desired terms on a dial, the direct reading of which eliminates calculation errors. An apparatus of this kind, as used by a leading brush manufacturer, is shown in Fig 7.

If a strict comparison of the coefficients of friction of a number of brush samples is desired, more accurate results will be obtained if the rotating part is a solid ring, preferably copper, with an accurately polished surface. It must be remembered, however, that the behavior of the brush on this nearly ideal surface will be different from its behavior on a commutator having mica segments. It is suggested that this difference be accounted for

PLATE X
A. I. E. E.
VOL. XXXII, 1913

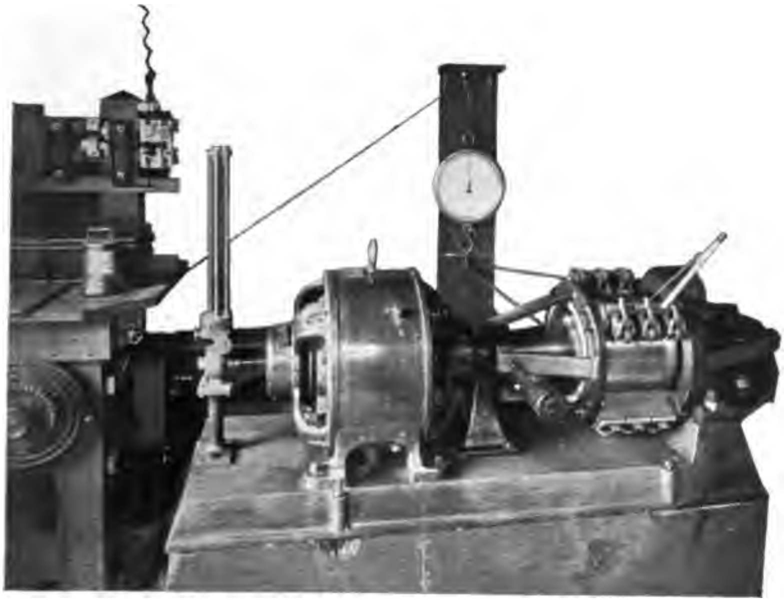


FIG. 7 [EDGECOMB AND DICK]

by a system of multipliers as follows; (a) a set of constants representing the difference between the surface of the test apparatus ring and the surface of cast iron, steel, bronze, and brass rings and of copper commutators, all assumed to be as well polished as the test ring; (b) a set of constants representing the difference between the surface of a " green " or freshly turned and sandpapered rotating part and the surface of a well polished rotating part; (c) a set of constants representing the difference in friction due to the angle of the brush.

III. The results of brush friction may be expressed in three ways: (a) coefficient of friction, the value of which may be computed from readings obtained electrically or which may be read directly from the dial in the mechanical method. (b) watts consumed due to brush friction, which is obtained, as stated, by the first test method, also by calculations to be mentioned later; (c) percentage of total input, calculated from (b).

IV. The effect of materials and operating conditions may be detailed as follows: When two materials slide on each other, their coefficients of friction vary with the nature of either or both of the materials. Each of a large number of carbon brush compositions gives a different coefficient when bearing on the same revolving part. Each of the materials used for slip rings and commutators, such as iron, steel, bronze, and copper, gives a different coefficient on the same brush. In the case of brush composition, friction increases with the hardness and the amount of abrasive, and decreases with increase of graphite and with the addition of other lubricant. Metal-carbon brushes have more friction than plain graphite and about the same as the harder carbon brushes, the friction increasing with the percentage of metal in the brush. Slip ring friction is assumed to be the greatest for cast iron and least for copper, values for the metals generally used decreasing in the following order: cast iron, steel, bronze, brass and copper.

Friction is lowest when brushes are mounted at an obtuse angle with the approaching surface of the rotating part. It is greatest when they are mounted at an acute angle with this approaching surface. Some of this effect is due to the contact of the brush with the walls of brush holder when the box type is used.

Friction is greater when the surface of the rotating part is rough and less when it is well polished. Increasing peripheral speed decreases the coefficient of friction, and friction generally increases with brush pressure.

V. In the preparation of formulas and curves for the use of the designer we must consider the six variables presented under section IV, as they all affect the value of the friction coefficient. A series of diagrams is proposed, one for each brand of brushes. These diagrams should have separate curves for the various pressures to which the brush is adapted such as the set of curves for one brand shown in Fig. 8; they should show the variation in coefficient due to change in peripheral speed, and they should give the factors used in modifying coefficients to agree with angle of brush, material and surface condition of rotating part.

With a dependable value for the coefficient of friction the power consumption expressed in watts may be calculated, using the formula:

$$\text{watts consumed} = 0.0226\, P\, A\, V\, K$$

in which 0.0226 represents the ratio $\frac{746}{33000}$

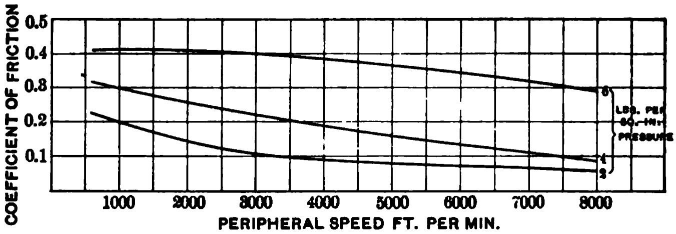


FIG. 8

P = normal brush pressure in lb. per sq. in.,
A = total contact area of brushes in sq. in.
V = peripheral speed of rotating part in ft. per min., and
K = coefficient of friction (adjusted to suit conditions).

In this connection it is interesting to note that a great many formulas are extant, mostly empirical, for determining watts input. Eight of these now in use were applied to a concrete case, with results varying from 910 to 3450 watts. It is likely that the tolerance of these widely varying formulas is explained on the ground that they will all check with actual tests depending on the operating conditions existing at the time of the test.

It is possible that other values might be profitably substituted for "Coefficient of friction," in Fig. 8. For example,

" watts consumed per pound pressure per inch diameter of rotating part " would put the values into convenient form for use on the testing floor.

Because of the limited time for making tests, the actual friction coefficients for the principal types of brushes cannot be presented in this paper. A complete statement of these figures may be expected at a later date.

General Conclusions

In discussing brush losses due to contact and friction we have done little more than indicate the avenues along which investigation should proceed and the nature of the results which will be useful to designing engineers. Some troublesome variables have been pointed out, the controlling of which should materially increase the accuracy of results.

Because of the complexity of these tests and the length of time and expense of apparatus necessary to produce dependable results it may be argued that commercial tests, those made on machines actually in course of manufacture and awaiting shipment, should be sufficient to give all necessary information. Our experience in attempting to correlate results of such tests and establish laws therefrom has been exceedingly discouraging, and we are convinced that improvements in the design of electrical apparatus as related to brushes and in the quality of the brushes themselves must result from the establishment of more accurate values for the losses due to brush contact and friction.

There is also every reason to believe that accurate values thus established will be found so much more reliable than actual performance tests that they will be substituted for such tests in the majority of cases.

A paper presented at the Midwinter Convention of the American Institute of Electrical Engineers, New York, February 27, 1913.

COMMUTATION AND BRUSH LOSS

BY C. E. WILSON

Even when made on slip rings, or on special short-circuited commutators, brush loss tests are hard to duplicate and results of tests under similar conditions vary greatly. A number of things are known to affect the brush loss at a given current density. Among these, the most important are: the direction of current in the brushes, the glaze or polish of the collector, the surface and fit of the brushes, oil or any foreign material on the collector, the temperature of the parts and the peripheral speed of the collector.

In direct-current machines, there are still other reasons for variations in brush loss. It is likely to be increased by high mica, unequal division of current among the brushes and brush arms, vibration or chattering of the brushes, imperfect spacing of them, and, above all, by the commutation and the strength of the magnetic fields in which the short-circuited coils are commutated. In fact, tests show that one machine may have two or three times the brush drop of another of the same type, although the grade of brushes and the apparent current density in them are the same. Even in the same machine, with the same current output, the brush drop measured at the different brush arms and at different times, may vary 100 per cent or more.

Table I gives the tests on four adjustable-speed motors of the same type, at the maximum and minimum speeds for which they were designed. These were chosen at random from a large number of tests made on this line of machines, all of which were equipped with the same grade of brushes.

The voltages *A*, *B* and *C* were taken at different points on the brush and commutator, as shown in Fig. 1, and were measured

TABLE I
TESTED BRUSH DROPS ON ADJUSTABLE SPEED MOTORS

Rating			20 amperes per sq. in.						40 amperes per sq. in.						60 amperes per sq. in.					
H.p.	Volts	Rev. per min.	Volts *A*		Volts *B*		Volts *C*		Volts *A*		Volts *B*		Volts *C*		Volts *A*		Volts *B*		Volts *C*	
			+	−	+	−	+	−	+	−	+	−	+	−	+	−	+	−	+	−
20	115	300	1.1	0.9	0.7	0.7	0.8	0.7	1.5	1.3	1.1	0.9	1.5	1.2	2.2	1.3	1.6	1.2	2.2	1.0
		1200	1.3	1.5	0.5	0.6	−0.5	0.0	1.6	1.8	1.1	0.9	0.7	0.9	1.4	0.7	1.3	1.0	1.8	1.7
25	230	300	0.9	0.6	0.7	0.6	0.4	0.7	0.9	0.7	0.5	0.9	0.1	1.3	0.7	1.8	1.1	1.1	1.7	2.1
		1200	0.4	0.4	0.3	0.3	−0.1	−0.1	0.5	0.4	0.5	0.5	0.3	0.7	0.5	0.6	1.0	1.3	2.3	2.2
25	115	400	0.4	0.5	0.5	0.1	0.3	0.2	0.6	0.9	0.7	0.2	0.6	0.3	0.8	0.8	0.7	0.3	0.8	0.6
		1200	0.9	1.00	0.5	0.5	0.2	0.1	1.1	1.1	0.7	0.7	0.5	0.5	0.7	1.2	0.9	0.9	0.7	1.0
30	230	350	0.3	0.4	0.1	0.3	0.0	0.2	0.4	1.4	0.2	0.9	0.1	0.7	0.3	1.3	0.2	1.0	0.5	1.2
		1050	0.6	0.5	0.1	0.1	−0.6	−0.5	0.5	0.7	0.1	0.2	−0.3	−0.3	0.4	0.6	0.2	0.3	0.1	0.2

in the usual way with a low-reading direct-current voltmeter and a pair of pencil points making contact at 1-1, 2-2 and 3-3, as shown. The current densities given correspond to approximately one-half, full load and 50 per cent overload on the motors.

The data recorded in Table I show, principally, the extreme variations in brush drop found in commercial testing. These variations can usually be explained by one of the causes previously given. The overshadowing influence of the commutation and the commutating field on the brush drop is also shown clearly. As far as measuring the loss is concerned, the product of the average brush drop and the load current is such a rough approximation that it is often practically worthless. In some cases the loss obtained in this way is less than the loss measured on slip rings and short-circuited commutators. It is hard to see how this can

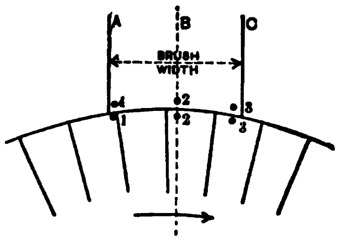


Fig. 1

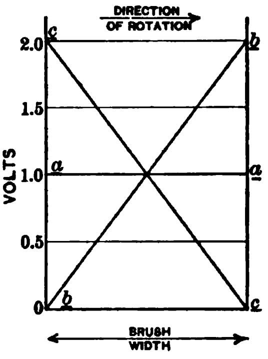


Fig. 2

possibly be true, as all the things which affect the loss in machines tend to increase it.

It should be remembered that the primary function of a brush is to commutate and collect current. Brushes are selected for their commutating qualities, primarily, and the brush loss is of secondary importance. The commutation is first made right and then the question of loss is considered. A machine which has a high short-circuit or reactance voltage demands a high-resistance brush, especially in the case of a machine without compensating windings.

In the case of an ordinary commutating pole motor, a reasonable value for the total inherent short-circuit voltage* across the

* Figured according to the method given in Mr. B. G. Lamme's paper, *A Theory of Commutation and its Application to Commutating Pole Machines*, Trans., A. I. E. E., 1911, Vol. XXX, p. 2359.

brush at full load, is 15 volts. Assume that, in order to neutralize this voltage, the ampere-turns on the commutating pole must be 30 per cent in excess of the armature ampere-turns, or a total of 130 per cent. With this value of ampere-turns, and with a commutating field of the proper shape, the theoretical curve *a* in Fig. 2 will be obtained. If the ampere-turns on the commutating pole are decreased to 126 per cent of the armature ampere-turns, the neutralizing voltage will be decreased to 13 volts and the motor will be two volts under-compensated, and a brush curve such as *b* in Fig. 2 will be obtained. A local current will then flow in at one brush tip and out at the other. This curve assumes constant brush resistance. However, the actual curves approximate these theoretical ones with sufficient accuracy for purposes of illustration. If the ampere-turns on the commutating pole should be increased to 134 per cent of the armature ampere-turns, the motor will be two volts over-compensated, and curve *c* in Figure 2 will be obtained. Curves *b* and *c* show two volts drop at the trailing and leading brush tips respectively. On the assumption of constant brush resistance, this drop of two volts means double current density in the tips of the brushes. Experience shows that this drop and current density are about the maximum permissible with ordinary carbon brushes, if the machine is designed to carry a reasonable overload with perfect commutation.

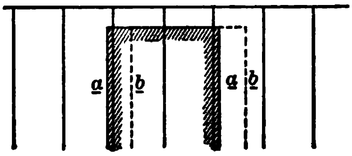


FIG. 3

To obtain this desirable adjustment in such a machine, the commutating pole ampere-turns must be correct to four parts in 130, or within approximately 3 per cent. If, for any reason, the effective ampere-turns on the commutating pole vary independently of the armature ampere-turns in excess of 3 per cent, commutation trouble is likely to be experienced.

The voltage across the brush is usually measured with a direct-current voltmeter. If an alternating-current meter which would read correctly on high frequency were used, an entirely different reading would be obtained in the majority of cases. This is due to the variation in the short-circuit voltage which occurs during the period of commutating one bar, and in some cases, during the commutation of the bars on one slot. This variation in short-circuit or reactance voltage cannot be correctly compensated by a commutating pole which will only give the proper *average values.* It is quite apparent that a different condition is encountered when the brush is in position *a* than when in position *b* in Figure 3; likewise, when the brush passes from bar *a* to bar *b* and from bar *b* to bar *c* in Fig. 4. This is one of the reasons why the brush drop, as measured on machines, is so variable that tested values are practically meaningless and worthless so far as measuring the loss is concerned.

In the case of a non-commutating pole machine, it is still more necessary to have a high-resistance brush if the short-circuit voltage is high. It is common practise to shift the brushes of a non-commutating pole machine from the mechanical neutral, in order to induce a voltage in the short-circuited coils, due to the main field which will neutralize the short-circuit voltage. A fair limit for the short-circuit voltage across the brush of a non-

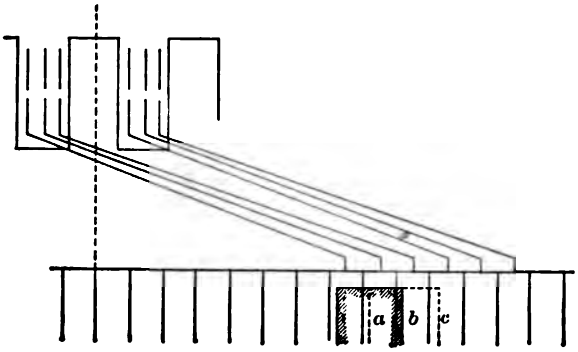


Fig. 4

commutating pole machine at full load is six volts. Assuming high-resistance carbon brushes with constant brush resistance and a drop of 1¼ volts at a nominal current density of 35 to 40 amperes per square inch (6.45 sq. cm.), the brushes may be shifted into an active field which will give five volts at no-load, and a current density in the brush tips of twice the normal value. A brush curve similar to *a* in Fig. 5 will then be obtained. At full load, this five volts will neutralize all but one volt of the short-circuit voltage and curve *b* will be obtained. At 50 per cent overload, curve *c* would be obtained with approximately three times current density in the trailing brush tip and the condition of poor commutation. The probable limit for commutation on such a machine would be 25 per cent overload. The brush loss in this machine at no-load would be considerable, and this is often found to be the case, as shown by tests. Often the no-load losses are decreased much more by raising the brushes than can be accounted for by the decrease in loss due to the absence of brush friction. Many tests have been made to measure this no-load brush loss, and it has been found to be comparatively large. In the case of curve *c* at 50 per cent overload, Fig. 5, the loss in the tip of the brush would be approximately nine times normal, and would undoubtedly cause glowing and honey-combing of the brushes.

The commutation of non-commutating pole direct-current machines has often been improved by narrowing the brushes and

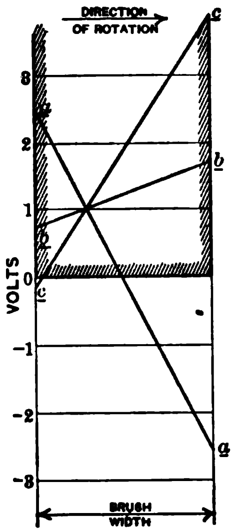


FIG. 5

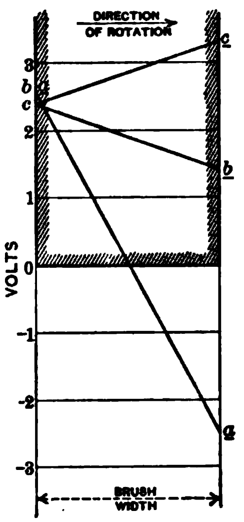


FIG. 6

increasing the apparent current density in them. For example, consider the brush in Fig. 6 to be reduced from $\frac{3}{4}$ in. (18.9 mm.) to $\frac{1}{2}$ in. (12.7 mm.). The short-circuit voltage at full load will then be approximately four volts, and curves *a*, *b* and *c*, Fig. 6, will be obtained at no-load, full load and 50 per cent overload respectively. With this narrower brush the loss in the tips of the brushes is the same as before, but the total loss is less, because the section of the brush has been reduced. The loss at full load is practically the same as before, but the loss at 50 per cent overload has been reduced so that commutation might be satisfactory. Actually the improvement is greater than indicated, due to the fact that the brush does not have a constant resistance, but has a resistance which decreases with increase in current density.

The writer recently had this relation between brush drop and the commutating characteristics of a machine forcibly illustrated by his experience with a line of small six-volt motors. In order to improve the efficiency, very low resistance brushes were used. In a given case, the short-circuit voltage was 0.75 volt across the brush at full load. The commutation was perfect with brushes having a nominal brush drop of 0.25 to 0.3 volt at a current density given by full load on the motor. To improve the efficiency, other brushes were tried having a drop of 0.1 to 0.15 at this same current density. The efficiency by brake was practically the same for both cases, showing that the low-resistance brush had not decreased the total brush loss. Also, the commutation with the second brush was poor and unsatisfactory. This same brush, having a drop of 0.1 to 0.15 volt and working at approximately the same apparent current density, gave entire satisfaction on another motor which had a short-circuit voltage of only 0.35 volt across the brush at full load.

On the previous assumption of constant brush resistance, and neglecting the increase in loss due to variations in short-circuit voltage during the period of commutation, the total loss which occurs at the brush can be calculated from brush curves.

Let I_1 = Amperes per unit area.
E_1 = Brush drop at current density I.
E_2 = Short-circuit voltage across the brush.
A = Area of brushes.

Then the brush loss on a short-circuited commutator would be

$$W = E_1 I_1 A$$

When a local current, caused by the short-circuit voltage E_2, is present in addition to the useful current, the loss can be shown to be

$$W_t = E_1 I_1 A \left(\frac{4 E_1^2 + \frac{1}{3} E_2^2}{4 E_1^2} \right) \text{ or, if}$$

$$a = \frac{E_2}{E_1},\ W_t = E_1 I_1 A \left(\frac{12 + a^2}{12} \right) \text{ and the increase}$$

in loss will be $8\frac{1}{3}\ a^2$ per cent.

Referring to curves *b* and *c* in Fig. 2, the increase in loss due to the local currents caused by the two volts across the brush will be 33⅓ per cent. Actually the increase in loss with such a brush curve as *b* or *c* will be considerably more than 33⅓ per cent, due to the decreasing brush resistance with increasing current, as previously mentioned. This decreasing brush resistance allows a larger local current to flow, which, of course, increases the loss.

Referring to curve *a* in Fig. 5, the brush loss at no-load would be 133⅓ per cent of the loss at normal full-load current density and no local currents.

There appears to be no way of measuring this increase in brush loss, or, as it may be called, commutation loss. In the average commutating pole machine, this increase in loss probably does not exceed 50 per cent, but it may be very much greater in some cases. Probably the most satisfactory method of dealing with this total brush loss is to obtain a value for what may be called the true brush loss from data obtained by tests on short-circuited commutators, and to regard any increase in loss which may occur, as commutation loss or part of the load losses.

GROUP II PAPERS

(Pages 423 to 584)

METHOD OF DETERMINING LOSSES IN APPARATUS

(a) INDUCTION MOTORS

Induction Motor Load Losses, by H. G. Reist and A. E. Averrett.
Stray Losses in Induction Motors, by A. M. Dudley.
Notes on Induction Motor Losses, by R. W. Davis.

(b) TRANSFORMERS

Losses in Transformers, by W. W. Lewis.
Stray Losses in Transformers, by C. Fortescue and W. M. McConahey.

(c) GENERATORS, A-C. AND D-C.

Determination of Load Loss Correction Factors for Rotating Electric Machines, by E. M. Olin and S. L. Henderson.
Load Losses of Alternating-Current Generators, by W. J. Foster and E. Knowlton.
Notes on Stray Losses in. Synchronous Machines, by F. K. Brainard.
Stray Loss in Direct-Current Commutating Machines, by H. F. T. Erben and H. S. Page.

(d) ERRORS OF TESTS

The Determination of Stray Losses from Input-Output Tests, by L. T. Robinson.
Sources of Error in the Efficiency Determination of Rotating Electric Machines, by Elmer I. Chute and William Bradshaw.

(e) BRUSH LOSSES

Brush Friction and Contact Losses, by H. F. T. Erben and A. H. Freeman.
Methods of Determining Brush Losses Due to Contact and Friction, by H. R. Edgecomb and W. A. Dick.
Commutation and Brush Losses, by C. E. Wilson.

Discussion on Group II Papers (Methods of Determining Losses in Apparatus), New York, February 27, 1913.

(*a*) Induction Motors

A. E. Averrett: Regarding the separation of copper losses, there seems to be a tendency on the part of some users to want bar-wound stators on account of the ease of repair. You can make a bar-wound stator with rather deep bars, but if you have a rotor that is bar-wound also and assume that the losses are practically all in the rotor, these will disappear at synchronous speed and you will apparently have a more efficient machine than you really have, if all the losses are taken into consideration. But you can actually get the loss by taking out the rotor and measuring the impedance of the stator alone by wattmeters, which will show up the losses correctly if the tests are made carefully.

It seems to me that the present method of summation of losses will apply only where the impedance-ampere curve is a straight line proportional to voltage (which shows no saturation in the iron) and where there are no eddy losses in the stator copper, which can be shown up by the wattmeter measurement at impedance. There is one feature about a deep rotor bar where we neglect the losses; there is a reduction in power factor due to skin effect in deep rotor bars which does not show at standstill test.

B. A. Behrend: I want to make a plea for the adoption of the term " stray losses " instead of the term " load losses " or losses incidental to operation. We understand by " load losses," as the term is used, a great many losses, including certain losses which occur at no-load, losses which occur in certain cases at full load, and losses which occur at both no-load and full load, and I think the adoption of the term " stray losses " would be a little more logical. We have a certain stray loss at no-load, we have a certain stray loss at full load, and instead of the use of the term " load loss," which is used in some of these papers as a loss occurring at no-load, I think we should use the term " stray loss." I believe that Mr. Hobart, who suggested my making these remarks, agrees with the substitution.

C. P. Steinmetz: We are particularly interested in the magnitude of the additional stray losses which we have at load, and we are only in a less measure interested in the stray losses at no-load. These latter need rarely be segregated from the total losses at no-load. We need a satisfactory name for those losses which appear at load, and are not accounted for by the usual methods of measuring individual losses. " Load loss " is rather a poor name, because there are many other losses which are accounted for in a separate test; but "stray loss" does not cover it exactly either, and we should have some additional name.

James Burke: Following Dr. Steinmetz's remarks, would it be consistent to speak of "stray no-load losses" and "stray load losses?" We have both kinds of stray losses. These papers show that in some kinds of motors the stray load losses may be very considerable. In other types they may be very small. Generally, the reduction in core losses due to the drop in the stator windings, resulting in lower total magnetization, compensates for the usual load stray losses, so that there is not very much to be taken into consideration if the free core losses are used rather than the corrected core losses after the reduction in the magnetism in the machine. It would seem, however, that in formulating any new rules, we must take proper recognition of the stray load losses, because from these papers it is evident that we may have very considerable stray load losses, or they may be negligible, and that fact would seem to make it necessary to formulate some plan by which they would be properly taken into consideration when they exist.

H. M. Hobart: We ought to have four components in this proposition. The four components I have in mind are: first, output; second, no-load loss; third, the load loss (by which we mean the legitimate load loss), increasing as the square of the current; and fourth, what we might call "stray losses," that is, the losses which, at full load, we must add to these other three losses in order to get the input at full load. This fourth part, which we have been apt to ignore in the past, and which we shall probably have to take into account in the future, could be given the name "stray losses," instead of, as in some of the papers, being termed "load losses." I should prefer to reserve the term "load losses" for what we call the legitimate losses coming on with the load.

Leo Schuler: Does Mr. Hobart propose to call losses due to distortion of the field, stray losses as well? They certainly are not stray losses.

H. M. Hobart: For practical purposes I should have input, minus output, minus no-load losses, minus I^2R losses, coming on with the load, which I should call stray losses.

Leo Schuler: You then include losses which are not stray losses.

Perhaps it will interest you if I say that in Germany we have the no-load losses, which include also stray losses if there are any, and then speak of "additional losses" simply. Additional loss means the loss which does not come on at full load, and which is not accounted for by what you call the load losses.

B. G. Lamme: Stray losses?

Leo Schuler: As far as I understand, load loss is better than stray loss.

H. M. Hobart: I do not care what it is called, if we do not confuse it with other losses. "Extra losses" would be a good term.

B. A. Behrend: The objection is to the use of the term "load loss." It is not implied that the expression "stray" is such a general term, but it might be as good a one as any other. An objection to using the term "load losses" is that in an equivalent test it might mean legitimate losses or extra losses.

B. G. Lamme: The term "load loss," as Mr. Behrend says, is a very misleading one, because practically all our secondary copper loss is a load loss. Practically all of our primary copper losses are load losses, and when we speak of load losses we should include all these, and yet what we are after is the additional extra loss which we call the "stray loss." I do not think, whatever other term we use, that we can dispense with the use of that term—after we have described our "load losses," we will still have use for that term "stray losses," or some other similar term.

A. E. Averrett: I believe load losses are included in the legitimate copper losses and legitimate iron losses, plus excess losses. On induction motors, these losses occur only on the load running light. There is no excess loss.

R. E. Hellmund: The present Institute rules are not quite right, in so far as they give the losses in too general a way. These papers give suggestions as to the method of determining the losses for practically all cases, with the possible exception of machines with entirely closed slots. As has been said, such cases are comparatively rare. All other cases can be taken care of, if the Committee would adopt the suggestions made in the paper of Messrs. Reist and Averrett, and the paper of Mr. Dudley.

It might appear at first sight that Mr. Dudley's suggestion of finding the true I^2R loss by testing the motors for different frequencies is impracticable, but it must be considered that almost any factory has generators available with at least two or three frequencies. Since the method is on the other hand the only method proposed for finding the I^2R losses in large squirrel-cage motors, I would advocate that the Institute adopt this method as one of its standard methods.

C. J. Fechheimer: Messrs. Reist and Averrett make the following statement: "Tests have been made which verify the above statements; round wires or rectangular strips of one cm. or less for a maximum dimension, apparently do not show any appreciable loss." This refers to eddy current losses in copper conductors. I would call attention to the fact that eddy currents are a function of the frequency and increase very rapidly with an increase in frequency. At standard frequencies the loss is negligible with the depth of conductor given, but it would be possible to raise the frequency high enough to cause a very large eddy loss with the same size conductor.

We believe that the method which is mentioned briefly at the end of this same paper, that is, of obtaining the losses at various frequencies, would be the proper one to determine the extent of the eddy losses. If we go down low enough with the impressed frequency the eddy current loss becomes negligible and we then have a direct means of measuring these losses with accuracy.

R. E. Hellmund: We first have to find out what the extra losses are at standstill and then separate them. Mr. Dudley proposes to find out what they are by measuring at different frequencies and then separating them, either by the rule of making them proportional to the square of the depth of the conductor, or by taking the rotor out of the stator and testing the stator separately.

There are two steps to be taken, and I think they are very proper steps, in large machines, where it is not possible to get the secondary losses by slip readings. In smaller machines I think the method proposed by the first paper is very desirable, that is, to get the secondary losses by slip readings and the primary losses by considering the I^2R losses, or, in the case of heavy conductors, by making the additional test of the stator without the rotor.

Comfort A. Adams: I want to say a word in favor of the German name translated as "additional losses". The term "stray losses" has been for many years much used to represent the no-load losses, that is, the friction and windage losses, and losses measured when the machine is running absolutely light. Thus in order to avoid confusion it seems to me that "additional losses," a perfectly distinct name, would be more appropriate. Referring to Mr. Fechheimer's question as to iron slot bridges and the reduction of the total flux at full load, it is obvious that if you mean by total flux the useful working flux, plus the leakage flux, it is practically constant at constant impressed voltage, differing therefrom by only the IR drop, which is small. But the part of this total flux which is increased by the bridges, namely, the leakage flux, is in a magnetic circuit of relatively short length, and involves a much smaller volume of iron, although at much higher density. It is thus impossible to say, definitely, whether the bridges would increase or decrease the total core losses.

James Burke: I want to answer Professor Adams's point regarding the difference in core losses. Very often on motors below 10 h.p. there is a 5 per cent reduction of e.m.f. due to 5 per cent drop, which makes about 5 per cent difference in core loss. On larger motors this is much less, but in motors of 10 h.p. or less a large IR drop in the stator generally exists.

Comfort A. Adams: Mr. Burke's statement is undoubtedly correct, but if the resistance drop is constant for any given load, say full load, the effect of the slot bridges is not to change the total flux, but to increase the leakage flux at the expense of the main flux, as above described.

B. A. Behrend: It seems to me extremely dangerous to draw an inference from the excess over an assumed rate of increase. We are familiar with the fact that the core loss in induction motors rises at a greater rate than the square of the impressed voltage, and, therefore, any inference based on the law of increase as suggested, would be, to borrow a phrase used by Mr. Burke, " mind reading " rather than good engineering.

A method of that sort seems most dangerous to me, and in this connection I wish to second the remarks made by Mr. Schüler and Professor Adams on the use of the term " indeterminate losses." It seems from the discussion that the use of the term " indeterminate " would be excellent, because no one knows how to determine them.

C. P. Steinmetz: In connection with Mr. Behrend's remarks, I wish to say that I have never seen any core losses go up faster than the square of the voltage, or even as fast as the square of the voltage, except in those cases where there was saturation somewhere. Practically any core loss, if you run the voltage high enough, will begin to rise abruptly at the point where saturation is reached, and then the increase goes up according to powers ranging from the square to the cube and sometimes even at still greater rates. The abrupt increase of core losses beyond the quadratic rate at high voltages is an indication that saturation is the cause.

In view of the general experience with core losses in commutating machines, and in motors, especially in the commutating induction motor, we must expect that as soon as saturation is passed anywhere, we will have an abnormal rise of core loss, but below saturation the core loss does not go up as the square.

B. A. Behrend: I do not question the approximate truth of the 1.6th power law of Dr. Steinmetz's—but this is not a practical condition obtaining in any generators or motors, and therefore the core loss does increase more rapidly than the 1.6th power or even the square, at high inductions.

C. J. Fechheimer: I plotted a number of core loss curves on generators far below the saturation point, plotted them on logarithmic paper to determine the exponent, and I found in a number of cases that the exponent was higher than 2. There were a couple of cases below 2, but the general average was around 2, below saturation, and I have never been able to account for it.

Comfort A. Adams: The reason why the core losses behave so erratically in the case of induction motors is that they are not losses which occur, as in transformers, under conditions of fairly uniform density and single frequency. There are at least five different kinds of core losses in an induction motor, or five ways in which they occur, so that if the calculations could be made it would require five separate calculations. There are losses at fundamental frequency behind the teeth, at fundamental frequency in the teeth, at tooth frequency in the teeth, at tooth frequency in certain portions of the core back of the teeth, due to the tooth and slot groupings on the two sides of the gap, wave losses in the faces of the teeth, and, finally, illegitimate losses due to the breakdown of the insulation between laminations. It is thus not a simple matter to compute these losses, and not strange that they do not behave according to the manner of the core losses of well-behaved transformers. The increase more rapid than the square of the voltage is due partly to the wave losses in tooth faces and partly to a progressive breakdown of lamination insulation as the eddy e.m.fs. increase.

B. G. Lamme: In some cases the so-called iron losses increase very rapidly with the load, and also very rapidly with the induction. I have found many cases in which an abnormal increase in apparent iron loss occurs with only slightly increased inductions. But in many of these cases, if the copper was removed from the slots, the loss did not go up nearly so fast; that is, the so-called iron loss was, in reality, largely eddy current loss. The confusion comes from the fact that where iron losses are referred to, in most cases the term "core loss" should really be used. Part of the extra losses found may be true iron losses, but are eddy currents in the iron due to burred edges of the laminations in the slot, due to filing, etc.; they also may be due to contact between the plates. Such losses, if located in the armature teeth, may go up much more rapidly than the square of the induction, for they may be a function of the tooth saturation. I have found cases where the loss went up to as high as the 5th power of the induction, but this was largely eddy current loss in the teeth and armature copper, which is dependent upon the degree of saturation of the armature teeth. In fact, therefore, before arriving at any conclusions regarding the variation of the iron losses with the voltage, we should first find what really is iron loss and what is something else.

R. B. Williamson: Mr. Lamme has mentioned the paper by Mr. Field on eddy currents, in which the eddy current losses in conductors were shown to be due to the cross flux in the slot. In slots that have been filed or drifted, the laminations become more or less connected together, and the cross flux may set up considerable loss in the side walls of the teeth. A machine that shows high core loss on open circuit will, in many cases, also show a high short-circuit loss; that is, there is a connection between the two, and I believe a great deal of this loss is due to eddy currents in the teeth, or in the side walls of the teeth.

B. A. Behrend: As to the discrepancies in the opinions expressed by Dr. Steinmetz and myself, let me say that core losses are not iron losses, as I have used the term, and as I believe most of us are using it. Core losses also contain indeterminate losses. The iron loss is one thing, the copper loss is another, and the core loss may contain copper losses and iron losses and losses due to the filing of cores and bad workmanship, as well as losses due to stray fields from magnetic fields in the end plates or anywhere in the machines. I think that as we measure core losses, and not iron losses, it is not rational to talk of iron losses, except in calculations. This seems to me, from twenty years of experience, a matter of course, but I felt constrained to point it out, as the iron losses, if they could be separated and isolated, would not increase at a rate greater than the square of the induction. The core losses, however, do show such increase.

C. P. Steinmetz: I would say that transformers and induction motors for 60-cycle circuits, are two classes of apparatus in which good practise keeps the magnetic densities below saturation, or certainly does not let them go beyond saturation. Consequently, changes in voltage are not associated with changes in the flux path. As long as there is no change of flux path, all the losses must be dependent on the voltage or the current. The eddy losses would change with the square, the iron losses probably less than the square, so that the combined effect cannot exceed the square of the voltage, except where, by saturation, the flux path is affected.

B. A. Behrend: I believe there are about three million h.p. of induction motors in operation, with the design of which I have been concerned in one form or another, and I believe almost all of these induction motors carried the saturation above the bend of the saturation curve in the teeth of the rotor, and also in parts of the stator, and I am a little at a loss to understand the remarks of Dr. Steinmetz in this connection, because saturation is almost invariably used in the teeth of the rotors of induction motors, and not infrequently in other parts of the magnetic circuit.

C. P. Steinmetz: I think the confusion exists in the indefinite meaning of the term saturation. I mean such densities of saturation that the m.m.f. consumed in the iron is of the same order of magnitude per unit of length of the magnetic circuit as the m.m.f. consumed in the air gap. That, naturally, would not be desirable. Exceeding the bend of the saturation curve is really not yet saturation, in the meaning of the term as I have used it. You can go beyond that for a little way and still not seriously increase the proportionality between the exciting current and the voltage.

H. M. Hobart: Another circumstance, as showing our ignorance of this subject, could be mentioned. If in the laboratory you measure the specific resistances of two samples of iron, one of high and the other of low specific resistance, the eddy current loss may be as great in the one with high specific resistance as in the one with low specific resistance. Pending an explanation of this, we must admit that we do not know much about eddy current losses.

L. T. Robinson: We do not know very much about these things. An encouraging sign is that we begin to take an interest in them, with the object and hope of finding out more about them.

R. E. Hellmund: It has been brought out that the core losses, as a rule, rise with the square of the voltage impressed on induction motors, more so than in the case of transformers. That is partly due to high densities in certain parts, but I take it that most of it is due to the fact that the largest part of the losses in induction motors, of practical design, are eddy current losses, and only the smallest proportion of the losses are hysteresis losses. In considering transformer iron, with about 60 cycles, the hysteresis loss is pretty large as compared to the eddy loss. In the case of the induction motors the hysteresis losses are in many motors only about 20 per cent, while all the rest of the losses are eddy current losses, caused by the higher frequencies in the teeth. Now, as we all know, the eddy losses go up with the square of the voltage, and since the eddy losses in most motors are more than half of the total losses, it is not surprising that the curve follows the law of the eddy loss rather than that of the hysteresis losses. It is therefore really not surprising that the core loss curve is nearly the curve of the squares, even for low densities.

B. A. Behrend: Once more to the subject of saturation, and the iron losses, where hysteresis alone may be considered. Dr. Ewing, who first brought out the general principles of induction in iron and other metals, suggested to Professor Baily of London certain physical research work, the results of which are published in the *Transactions* of the Royal Society. The paper of Professor Baily is fundamental, and he proves that rotative hysteresis diminishes at high induction. With the induction plotted as an ordinate and the loss plotted as an abscissa, Professor Baily's researches show that at saturation, viz., $4 \pi I$ in the relation $\mathbf{B} = \mathbf{H} + 4 \pi I$ having become a constant, the loss diminishes. Whatever induction we have below or beyond that point is the induction, **B**, which is the sum of the air field plus the iron field. This was experimentally demonstrated in Professor Baily's paper. I also carried on some experiments in our own laboratories about eight or ten years ago, and I found an approximation to a similar result. We eliminated eddy currents as nearly as possible, and we found that the loss diminished at high inductions.

M. G. Lloyd: Mr. Behrend is quite right in his reference to the experiment of Professor Baily, and further work along the same line has been done by others, especially by Professor Weiss, of Zürich. The curve between watt loss and magnetic flux density has been found to come down almost to zero at sufficiently high values of the flux density, but this applies only to a case where you have a purely rotary magnetic field, constant in intensity and simply changing its direction in the magnetic material. I do not believe that such a condition can be found in any kind of a machine such as an induction motor. You always have there a combination of a rotary effect with a reversing effect, that is, you have pulsation of the magnetic field in both time and space. In a case like that the law illustrated by Mr. Behrend's curve no longer holds.

H. M. Hobart: In view of our demonstrated ignorance in these matters of hysteresis and eddy current losses, we must base our reasoning on the results of tests, and we should refrain from basing any conclusions on deductions from old-fashioned conventional alleged truths which have been shown to be not only inadequate but utterly misleading.

R. E. Hellmund: In considering the field distribution in induction motors, we must not only consider the effect of the primary, but also the effect of the secondary. The secondary, if short-circuited, has a correcting effect upon the field. This is especially the case in squirrel cage motors. For instance, a squirrel cage rotor with an infinite number of bars will always correct the field so it will have a sinusoidal distribution, no matter what the initial distribution of the field as set up by the primary winding. In actual practise, the correcting effect of the secondary with a limited number of slots, will be such as to cause the field distribution to be very nearly sinusoidal. We must therefore say that the chording of the primary has little influence upon the actual field distribution, but that it will have a big influence upon the correcting currents in the secondary.

In other words, we may say that the correcting secondary currents and the losses caused thereby are the smaller, the more the initial field set up by the primary approaches a sinusoidal field.

The initial primary field in three-phase motors with full pitch is not quite ideal in this respect. By introducing a chording sufficient to have two of the phases overlap half-way, we obtain an almost sinusoidal primary field distribution. By chording the coils considerably more than that, the field becomes worse again and will be the same as in the case of full pitch, if two of the phases overlap entirely. In two-phase motors, the initial primary field with full pitch windings is considerably worse than in three-phase motors, but also in this case phases overlapped about one-half improve the initial primary field considerably.

It follows from the above that while the chording of the winding has no great influence upon the actual field distribution, it is, if properly chosen, an advantage with regard to the operation of the motor.

James Burke: That also happens in the loaded conditions?

R. E. Hellmund: It is pretty hard to say just exactly what happens in the loaded condition, but my previous statements certainly do apply to the synchronous condition.

Comfort A. Adams: Mr. Hellmund's statements are quite in line with my ideas in this matter, but he undoubtedly assumed one condition, which he did not mention; that is, that the correcting effect of the secondary is only complete under the assumption of zero resistance and reactance in the secondary.

R. E. Hellmund: Yes, this assumption has to be made in order to get ideal correcting effect.

Comfort A. Adams: In most squirrel cage motors this condition is, of course, nearly fulfilled. If you had zero resistance and reactance, evenly distributed, it would wipe out all kinds, no matter what the e.m.f. condition was. As you reduce the coil pitch below two-thirds in a three-phase motor, say to 50 per cent, you might assume that there would result the same perfect m.m.f. distribution as for ⅔ pitch, but this is not so, because the currents in the overlapping phases have larger phase differences and yield quite different amplitudes in adjacent belts. The phase differences of the resultant currents in the various belts and the number of belts (12) is the same as for ⅔ pitch, but when you go down to 50 per cent pitch, you may have what Mr. Lamme has indicated, an actual local reversal of the flux.

(*b*) TRANSFORMERS

J. M. Weed: The two papers on transformer losses are in a sense complementary to each other, but after both papers are read, there are some discrepancies apparent which need to be harmonized, and some points which still need to be brought out to clear up the subject.

The paper by Messrs. Fortescue and McConahey mentions the losses due to eddy currents in the copper, and those due to circulating or unbalanced currents in unsymmetrical parallel windings, but dismisses them with the statement that they are negligible in careful designs. In the remaining part of the paper these losses are ignored, the total copper loss being classified as I^2R loss and stray loss (see section V of the paper). The term stray loss is confined to that part of the loss in the primary winding due to the resultant of load current and exciting current which is in excess of that which would be caused by the load current alone plus that which is caused by exciting current alone. In equation (**24**) the term " short-circuit loss " is used as an alternative term for I^2R loss, and is intended to refer to the loss which would be caused by load current alone.

On the other hand, Mr. Lewis calls the loss due to the resultant of load and exciting currents the I^2R loss, and classifies the extra losses due to magnetic leakage, which include the losses due to eddy currents, as stray loss. Moreover, the extensive tabulations of tests given by Mr. Lewis show that these losses are often far from negligible, varying as they do from a small value to more than 50 per cent of the I^2R loss.

This discrepancy in the use of terms is no doubt the result of efforts on the parts of the authors of both papers to use in a rational and discriminating way the old stereotyped terms " load losses " and " stray losses," which have been used rather indiscriminately to cover a multitude of sins. This effort has taken different directions in the two cases.

Messrs. Fortescue and McConahey have confined the term " stray loss " to a rational significance, that of a loss which escapes measurement. This applies to the extra loss due to combining the load and exciting currents, but not to the losses due to magnetic leakage.

On the other hand, Mr. Lewis has made the term " load loss " to include the I^2R loss, which, in so far as it is due to load current, certainly is a load loss. But he has made the I^2R loss of the primary to include the total loss due to the resultant of exciting current and load current. This loss all might properly be called I^2R loss, but not all load loss, since that element of it due to exciting current alone is included in the open-circuit or " no-load loss."

Probably the losses which Mr. Lewis has designated as stray loss would be more properly designated as losses due to magnetic leakage, or extra losses due to loading, these losses being included with the I^2R losses due to loading, under the general term of load losses, as Mr. Lewis has suggested.

I would make a similar suggestion with respect to rotating machines also, where the same difficulties have appeared in the rational classification of losses. The resistance loss in the armature winding does not seem to be logically excluded from the term load loss. Moreover, the losses due to magnetic leakage, which are properly included under the general term of load losses, may not properly be designated as stray losses, since they are included with the armature resistance loss, windage and friction, in the short-circuit core loss. The results of tests given in the papers by Messrs. Foster and Knowlton, and by Mr. Brainard, seem to indicate that these losses, as measured by this method, are not " greatly exaggerated," as stated in section 115 of the present rules, but that they are approximately the correct losses for the normal load condition.

Returning now to losses in transformers, an independent discussion of the various elements of loss and their classification may be useful. I will begin this by summarizing these elements, thus: The total losses of the transformer may be divided into those due to excitation, called no-load losses by Mr. Lewis, and those due to load, which he calls load losses. The former, ordinarily referred to as open-circuit or core loss, includes, besides hysteresis and eddy current losses in the core, a small element of resistance loss in the primary winding due to magnetizing current and a dielectric loss in the insulation which is very small in low-voltage transformers, but may be quite large in very high voltage transformers. These losses should be measured with open secondary, at rated frequency and rated sine wave voltage plus IR (instead of rated voltage minus IR). This correction of sign will be approved at once when it is considered that the rating of the transformer is based upon its output, and that the current rating is universally calculated from the name-plate voltage. The nameplate secondary voltage must, therefore, be taken as the full-load voltage, while the primary applied voltage is supposed to be in excess of the name-plate primary voltage by an amount equal to the transformer drop. The excitation of the core corresponding to full load at 100 per cent power factor, is, therefore, the name-plate voltage plus the percentage of I^2R drop in the secondary winding.

The load losses include the losses due to magnetic leakage plus the I^2R loss due to load current and the measured resistance plus the extra loss due to the combination of load current and exciting current in the primary winding. This does not include the loss due to exciting current alone, which was measured with the open-circuit loss. The losses due to magnetic leakage and the I^2R loss due to load current are included in the measured short-circuit or impedance loss, but not the extra or stray loss due to exciting current. This loss is usually very small, but may be included by a correction, based not on the total short-circuit loss, as stated in equation (**24**) by Messrs. Fortescue and McConahey, but on the I^2R loss due to load current. Moreover, the total exciting current does not enter into this correction, but only the fundamental component of the exciting current. The correction may then be made exact, including the effect of the hysteresis angle, by the formula:

Total loss in copper − loss due to magnetizing current alone = extra loss due to magnetic leakage + resistance loss due to load current × $[1 + 2\,pq\,(\cos\theta\,\cos\theta' + \sin\theta\,\sin\theta')]$*
where

$$p = \frac{n_1\,(I_e)_1}{n_2\,I_2}$$

and

$$q = \frac{\left(\frac{n_2}{n_1}\,I_2\right)^2 R_1}{\text{total resistance loss due to load current}}$$

*The derivation of the formula given above for correcting I^2R loss to include stray loss due to exciting current is obtained as follows:

Referring to Fig. 2, page 602, the fundamental component of exciting current,

$$/I_e/_1$$

may be separated in two components a and b, the former in phase with the load current, the latter at right angles to it. Represent the load current in the primary winding

$$\frac{n_2}{n_1} I_2 \text{ by } c.$$

The total loss due to these currents is

$$[a^2 + (b + c)^2]\,R_1 =$$
$$(a^2 + b^2 + c^2 + 2bc)\;R_1$$

The loss due to the fundamental component of exciting current alone is $(a^2 + b^2)\,R_1$. The loss due to the harmonics of exciting current must be added to this, but these components do not affect the extra loss due and where

$$\cos\theta = \text{power factor of the load}$$

and

$$\cos\theta' = \text{power factor of } (I_e)_1$$

$$(I_e)_1 = \text{fundamental component of exciting current}$$

With 10 per cent exciting current, which is the maximum value that should occur in practise, the fundamental component would probably be about 8 per cent, so that $p = 0.08$. Assuming a core loss of one per cent, we have $\cos\theta' = 0.125$, and sine $\theta' = 0.993$. Assuming equal resistance losses due to load current in the primary and secondary windings, we have $q = 0.5$. Now, for full load at 100 per cent power factor, the correction for stray loss due to magnetizing current becomes

$$I^2R \times 2 \times 0.08 \times 0.5 \times 0.125 = 0.01\ I^2R$$

and for full load at 80 per cent power factor,

$$I^2R \times 2 \times 0.08 \times 0.5\ (0.8 \times 0.125 + 0.6 \times 0.993) = 0.056\ I^2R$$

Thus the maximum correction that would be made for 100 per cent power factor load is about one per cent of the resistance loss due to load current, and this becomes about 5½ per cent for 80 per cent power factor load. It appears that this correction is hardly worthy of consideration in questions of efficiency. This is particularly true if we remember that the rise in temperature of the windings while the losses are being measured may produce an increase in the loss as great as or greater than the correction which we are considering.

---

to the combined exciting and load currents. The total I^2R loss in primary, minus the loss due to the exciting current alone, is

$$(c^2 + 2bc)\ R_1$$

Now

$$b = /I_e/_1 \cos(\theta' - \theta)$$

$$= p\ \frac{n_2}{n_1}\ I_2 \cos(\theta' - \theta)$$

$$= p\ c \cos(\theta' - \theta)$$

whence we have: Total I^2R loss in primary loss due to exciting current alone

$$= c^2\,[1 + 2\,p \cos(\theta' - \theta)]\ R_1.$$

Adding the loss due to load current in the secondary, and remembering that $c^2 R_1 = q \times$ total loss due to load current, we have:

Total loss due to load current, plus stray loss due to exciting current $=$ total loss due to load current $\times\,[1 + 2pq\,(\cos\theta\cos\theta' + \sin\theta\sin\theta')]$

This increase in the copper loss due to temperature rise in the windings while the loss measurements are being made, seems to deserve more detailed consideration. Full-load current must be sent through the windings for this measurement, and at the start, all of the heat produced is stored up in the copper itself. As the temperature of the copper rises, it throws more and more heat out through the insulation into the oil, until the heat thrown out is equal to the heat generated, when the temperature ceases to rise, except as the temperature of the oil rises. The initial rate of temperature rise is therefore fixed by the thermal capacity of the copper and the rate at which heat is generated. The thermal capacity of copper is about 177 joules per pound and the average loss in large transformers is about 10 watts per pound. This gives an initial rate of temperature rise of one deg. in 17.7 seconds, or 3.4 deg. per minute, and an increase in the copper loss of $3.4 \times 0.4 = 1.36$ per cent.

This effect of temperature rise in compensating for the stray loss is illustrated by reference to Table I, showing the results of tests, at the end of the paper by Messrs. Fortescue and McConahey. We see here that the measured loss is larger at high power factors than at low, whereas the correction for stray loss would indicate that it should be smaller. (The stray loss was, in this case, included in the measurement by the method of the test.) This is explained by the fact that the high power factor readings were taken last, and that the loss was increased due to increase of temperature during the tests more than it was decreased by increasing the power factor. These results would have looked very different if the measurements had been taken in the reverse order.

The temperature tests recorded by Mr. Lewis under the heading "Relation between Impedance Watts and Load Losses," in Tables VII and VIII, are, of course, not comparative tests between impedance watts and load losses, but between loads of different power factors, since the exciting current is present in both cases, the only difference between the two cases being that of phase relation between the exciting current and the load current.

Although, as we have seen, the stray loss due to magnetizing current is not ordinarily important from the standpoint of efficiency, still it may be sufficient to account for a considerable difference in temperature rise between two transformers tested together by the opposition method, when this test is made with loading voltage and exciting voltage of the same frequency. The power factor of the load, or circulating current, depending as it does upon the relative amounts of inductance and resistance in the combined impedance of the transformers, is always low, giving almost the maximum correction for stray loss. The correction for one transformer will be positive and that for the other negative, since, while the current lags in one, it leads in the other, the difference in phase of the circulating current in the two transformers with respect to the exciting current being 180 deg. The difference between the copper losses in the two transformers is thus twice the correction for one. There is, of course, a slight compensation for this difference in copper losses, due to the fact that the core loss is somewhat increased in the transformer having the smaller copper loss, and vice versa. The net result, however, may be a difference of two or three degrees in the temperature rises of the two transformers.

Equality of losses may be obtained by using a frequency for loading which is different from that used for exciting. This difference in frequency may be very slight, if desired, maintaining practically the rated frequency both for the excitation and for the load. This is, of course, a necessity for normal core loss, and it is also a necessity for normal copper loss when the eddy current losses are appreciable. The extra loss due to the combination of exciting current and load current will not be present under these circumstances, but only the sum of their respective independent losses.

The mathematical discussion given by Messrs. Fortescue and McConahey, under the second heading, " Theoretical Study of Copper Losses in Transformers," is correctly applicable to mutual inductive circuits in air (with no iron core). However, the values of equivalent resistance and equivalent inductance, given in equations (**7**) to (**13**) inclusive, are not completely expressed. The expressions given are the equivalent values for the nth harmonic only. The equivalent values which must be taken in connection with the value of the total resultant current depend upon the wave form of the current, *i.e.*, upon the relative values of the various harmonics of current. These values of resistance and inductance cannot, therefore, be expressed independently of the current. The complete expression for equations (**7**) and (**8**) would be

$$R_a = R_1 + \frac{\sum\left(\frac{n^2p^2M^2}{(R_2+R_0)^2+n^2p^2(L_2+L_0)^2}(I_1)_n^2\right)}{I_1^2}(R_2+R_0)$$

and

$$L_a = L_1 - \frac{\sum\left(\frac{n^2p^2m^2}{(R_2+R_0)^2+n^2p^2(L_2+L_0)^2}(I_1)_n^2\right)}{I_1^2}(L_2+L_0)$$

The remaining part of the discussion, if completely expressed, as it must be for any practical application, will be more complicated than it appears in the paper.

I do not agree with the authors that the results obtained in this manner, based upon the assumption that L_1 and L_2 and M are constants, apply with almost equal accuracy to transformers, where these values vary, not only for different values of maximum inductance, but throughout the cycle for any given value of maximum inductance. This introduces additional harmonics in both current and voltage not taken into account in this treatment, the object of which is to cover the effects of harmonics.

The authors have made no practical application of this mathematical discussion. They state that formulas given in Section 5 of the paper are based upon it, but these formulas take no account of harmonics, and are very easily derived from considera-

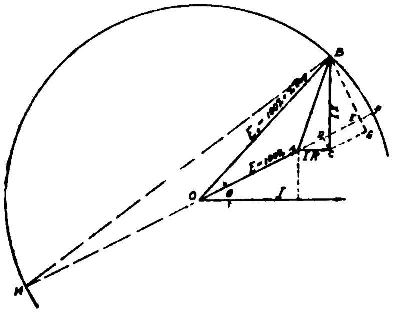


FIG. 1

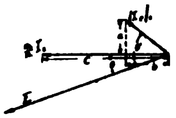


FIG. 2

tions based upon a simple sine wave. Thus, referring to Fig. 1, the formula for regulation is derived as follows:

E_0 is the no-load voltage of the transformer and E the full-load voltage at the secondary terminals, so that

$$\text{regulation} = \frac{E_0 - E}{E} = \frac{AD + DF + FG}{E}$$

$$= \frac{I\,R\cos\theta + IX\sin\theta + FG}{E}$$

$$\text{But } FG = \frac{(FB)^2}{FH} = \frac{(IX\cos\theta - IR\sin\theta)^2}{2E + 2\,IR\cos\theta + 2\,IX\sin\theta + FG}$$

whence

$$\text{regulation} = \frac{IR\cos\theta}{E} + \frac{IX\sin\theta}{E} + \frac{\left(\dfrac{IX\cos\theta}{E} - \dfrac{IR\sin\theta}{E}\right)^2}{2 + \dfrac{2\,IR\cos\theta}{E} + \dfrac{2IX\sin\theta}{E} + \dfrac{FG}{E}}$$

and, multiplying by 100,

$$\text{per cent regulation} = \% IR \cos\theta + \% IX \sin\theta + \frac{(\% IX \cos\theta - \% IR \sin\theta)^2}{200 + 2\% IR \cos\theta + 2\% IX \sin\theta + \frac{FG}{E}}$$

Neglecting the last three terms in the denominator of the third term, we have the formula given in the paper, which, though sufficiently accurate for practical purposes, is thus seen to be inexact, apart from any consideration of harmonics.

Mr. Lewis has done the Institute an important service by including in his paper a large amount of data on the extra losses in low-voltage transformers, due to the magnetic leakage field, and in high-voltage transformers, due to the dielectric field. It is possible that the values given for extra loss due to magnetic leakage (called stray loss by Mr. Lewis) are, in general, too large, being the total measured loss minus the loss calculated from the measured value of resistance and the current. The increase in temperature during the measurement of this loss gives ordinarily too high a value, as explained above. Moreover, many of the worst cases are seen to be those of magnetic shunt transformers, in which the losses in the shunts themselves are included.

While these losses may be kept to a minimum value by careful design, and are practically eliminated in many cases, yet Messrs. Fortescue and McConahey must agree that commercial requirements as to rating and service, and conditions of design, are often such as to make it practically impossible to avoid quite large losses due to eddy currents.

These cases may be recognized, however, and the calculation of these extra losses usually can be made with sufficient accuracy for a basis of efficiency guarantee. In my opinion, therefore, the common practise of excluding these losses from efficiency guarantees is not necessary, nor desirable.

C. Fortescue: When the paper referred to by Mr. Weed was written, the authors had in mind both the eddy current losses in the copper, which are easily measured, and the portion of the I^2R loss due to the exciting current which is not included in any of the usual measurements. A portion of the I^2R loss due to the exciting current is included in the core loss measurements, and properly so, since it is practically constant at all loads. It is necessary to apply a correction to the short-circuit measurement for the other portion of the I^2R loss, but only when the exciting currents are very high, since otherwise this correction is negligible.

Mr. Lewis in his paper tabulates the stray losses and impedance voltages in a number of commercial transformers, and points out that there is apparently no definite relation between these two quantities. I have stated in my paper that in a good design the eddy loss in the conductor can be reduced to a very small value. It might be well to modify this statement by adding that when high impedance is required, the cost of keeping down eddy currents to a low value sometimes becomes excessive. In transformers for furnaces, in which very large secondary currents are required, it becomes particularly difficult to keep this stray loss down. In the ordinary run of transformers, however, this loss can be controlled without materially adding to the cost.

James Burke: I simply desire to call attention to one feature of the paper by Mr. Lewis, which recommends the measurement of the copper losses hot, whereas the universal practise, I think, in this country now on transformers is to take the copper loss at 25 deg., and I simply wish to call attention to the fact that the recommendation of Mr. Lewis is a departure from the existing commercial practise, which is in use in connection with a very large number of transformer manufacturers.

I would like to bring to the attention of the Standards Committee another feature which might be considered in the new rules, and that is, sometimes manufacturers are asked to guarantee the volt-ampere efficiency of transformers, and I have seen specifications asking for the volt-ampere efficiency at 100 per cent power factor, 90 per cent power factor, and 80 per cent power factor, and I think it would be of advantage if the Standards Committee introduced some simple methods of bringing these factors into consideration rather than calling for specific tests under each of these conditions for acceptance tests.

E. A. Wagner: I think Mr. Burke's point about the volt-ampere efficiency is well taken. There has been a very pronounced demand, especially in the railway signal service, for figures giving volt-ampere efficiency. There is nothing really specific in the present standardization rules showing how this shall be done, and I think that the Standards Committee should mention this matter, and state absolutely clear definitions of the meaning of volt-ampere efficiency and also decide as to the effect of the excitation current on these volt-ampere efficiencies.

In regard to these recommendations at the end of Mr. Lewis's paper, I do not think that the exciting current should be deducted and segregated from the core loss measurement, that is, the effect of the exciting current on the copper losses. In a transformer designed for high exciting current, necessarily the core losses have been affected, due to that design, and as the exciting current is really a function of the core loss design, I think it should be included in the heading "no-load losses," or else a separate definition made as to the no-load losses which will include the effects of exciting current. Where the no-load losses are to be taken on the primary circuit, according to these recommendations, then it is going to make considerable difficulty in measuring losses on high-voltage transformers. We do not measure the core losses on 100,000-volt transformers on the primary side, we always measure them on the secondary side. The same holds true in the case of commercial transformers, 2200 volts to 110. We measure them directly with a 110-volt wattmeter, and do not attempt to step-up and then step-down.

The last recommendation reads: "On account of the variation with temperature, all losses should be measured at the operating temperature." If the Standards Committee adopts this recommendation, it should be specific as to a definite temperature. As a matter of fact, core losses go down with the increase in temperature, so that it affects the total loss in that way; but it is difficult to hold the temperature steady at any predetermined point if any time is consumed in making the measurements of these losses, and I think, therefore, it is better to specify a room temperature, and take the iron loss or no-load loss at that temperature, and if the full-load losses, including copper losses, are wanted, they can be corrected for the rise in temperature.

Charles P. Steinmetz: When the first Standardization Rules were established in 1899, it was stipulated that all losses should be measured at the full-load operating temperature. Transformer losses were included in this rule.

Before that time they had been measured by the manufacturers at the room temperature and the efficiency guaranteed on that basis, and these guarantees were wrong. That question has frequently arisen, and transformer designers have come before the Standards Committee and requested that the Rules should be changed to read: "In all apparatus the losses shall be measured at the full-load operating temperature, except in the case of transformers, where they shall be measured at the room temperature." It is not desirable to make a specification of this kind for transformers. There is no reason whatever to be seen why this should be done. The efficiency you want to know is that at which the transformer is running, which is at the operating temperature, and not at some fictitious temperature. It is true that it means that you must lower the efficiency guarantees a little, but that cannot be helped. What you guarantee when measuring the I^2R losses at the room temperature, is not the efficiency of the apparatus, but a higher value than is actually there.

The I^2R loss of the exciting current should be separated and not included in the core losses. The I^2R loss of the exciting current changes with the load. For instance, if the exciting current is 10 per cent of full-load current, then at no-load the I^2R loss of the exciting current is 1 per cent of the primary I^2R loss, and conversely the primary I^2R loss at full non-inductive load is 100 times the I^2R loss at no-load. But at full inductive load the exciting current increases the load current by 10 per cent, and so gives an additional primary I^2R loss of 21 per cent, and that means at full inductive load the primary I^2R loss is 121 times as high as at no-load. Obviously, therefore, the I^2R loss due to the exciting current must be considered separately and not included in the no-load losses.

Leo Schuler: I call your attention to the fact that when you measure the efficiency of the transformer at room temperature, instead of working temperature, it does not mean necessarily a very great difference in the efficiency, because the copper losses, of course, are lower, but the iron losses are higher, especially with alloyed iron.

James Burke: I want to correct any misunderstanding of what I said. I am not advocating measuring the transformer losses at room temperature. I was simply calling attention to the difference between the commercial practises of today and the recommendations.

Charles F. Scott: In regard to the word "stray," if I am not mistaken, laboratory manuals present the "stray power" method of measuring efficiency which is taught to the student as a very definite thing. Stray power is about everything that I^2R does not include. The power to drive a motor is measured at no-load, and what is not I^2R is stray power.

I asked this morning one of the designing engineers of one of the large companies, whether the term "stray power" was used in his organization. He said he did not know that it was. We come here and find the word "stray" used indiscriminately to cover about everything which cannot be accounted for.

The moral of this is that one of the chief duties of our Standards Committee is in the matter of definitions. Here is a term "stray," that certainly needs to be defined.

J. M. Weed: I think Mr. Wagner's objection to measuring the core loss from the primary side is based upon an erroneous definition of the primary side. The primary side is often the low-voltage side, as the transformer may be used either for raising or for lowering the voltage.

J. E. Saunders: I notice the recommendation is that transformer losses are to be measured at operating temperature. I want to know what this operating temperature is. We are hanging transformers where it is 130 deg. fahr. in the shade in the summer time, and they are hanging out all summer, and we are hanging them in another place where it goes as low as 30 to 40 deg. fahr. below zero, in the winter time, and those are the operating temperatures, so I want to know why we should not substitute room temperature for operating temperature.

C. Fortescue: The operating temperature, I think, is defined as the temperature at which the transformer will operate at the standard room temperature of 25 deg. with full load, at 100 per cent power factor. In transformers we base our rise on the average temperature of the coils as measured by the rise of resistance method. We do not indicate the temperature of the hot spot. In transformers there is a very slight difference between the average temperature of the coil and the maximum temperature, on account of the small temperature gradients through the insulation, and coils, etc.

J. M. Weed: There is one point which I think should be recognized by the Standards Committee in laying out their rules, and it may be that some of the members present might wish to discuss the matter if it is brought out specifically, and that is the question of cooling after the load is taken off the transformer. This, of course, applies to all electrical apparatus. It is practically impossible to get the resistance measurements at once, of course, and it is a fact that the apparatus is cooling after the load is taken off until such time as the resistance can be taken. It is possible, as has been brought out in some of these papers, to make corrections, but this matter of correction is always an uncertain matter, and will involve complications, and it would possibly be better to specify a time limit within which the resistance measurement should be made, recognizing the fact that the actual temperature at the instant that the load is taken off is a little higher, but considering it on the same basis as the hot spot that we cannot find.

A Member: There exists a method for finding the real temperature which is much easier than to find the hot spot. You have only to know exactly the moment when you really take the load off the transformer. Say that you press a stop watch at this moment, and then take two or three readings afterward, plotting a time-heat curve, and extrapolate that curve, you get the correct value the moment that you really took the load off the transformer.

L. T. Robinson: In connection with the remarks of the last speaker, I would say if those who are interested will refer to the paper which was read here yesterday by Mr. Chubb, they will see that the extrapolated value will not always be correct. Under some conditions the part where you are taking the temperature by resistance goes down after a while, and then it begins to go up by transfer of heat from some iron part or something else that is hotter. It may be useful to use extrapolation sometimes, but it must be used with caution, otherwise you may get a large error.

J. M. Weed: In determining the temperature by extrapolation the results are very uncertain, for the reason that slight inaccuracies in the readings modify the shape of the curve very much. The point that you decide upon as the initial temperature is very much influenced by errors in the first two or three points of the curve.

C. Fortescue: I agree with Mr. Weed that the method of extrapolation by means of a curve is very uncertain and brings in a large personal factor. It may be necessary in some cases to use such methods, but it would be better if some quicker methods could be devised for measuring the resistance of the windings than those at present in use. One thing that can be done in using the Kelvin bridge or the Wheatstone bridge is to have them set for the expected reading; in this way the final adjustment can be made very quickly. A measurement made in this manner is so quick that I believe only one or two degrees are lost. A matter of one or two degrees has but little effect on the operation of a transformer, and therefore I think measurements obtained in this way are sufficiently accurate.

Paul M. Lincoln: A definite question has been asked as to what temperature shall be used in calculating the copper losses of transformers and other apparatus, and I think this question is one which requires a definite answer and one which should be fixed by the Standardization Rules when promulgated. It seems to me that it is best answered by adopting the suggestion made in Mr. Lewis's paper, namely, to base it upon the operating temperature. But that operating temperature should be taken above a standard air temperature, and in my opinion that standard air temperature should be taken at 25 deg. cent. Therefore, the copper loss in a transformer should be calculated at the temperature at which it will actually operate when the surrounding air is 25 deg. cent.

W. C. Smith: In connection with the question of losses just brought up, and also referring to the statement of Dr. Steinmetz this morning, in which he said that the customer is most interested in losses at the operating temperature, there is another question regarding efficiencies which should be touched upon by the Institute, and that is the question of efficiencies at fractional loads. The present rules imply that the efficiency at a given load should be based on the operating temperature at that load. In the case of fractional loads, this imposes a serious burden on the manufacturer, one that is not complied with nowadays or asked for. Fractional-load efficiencies are guaranteed at the same temperature as the full-load efficiencies, so I believe that the new Institute Rules should state clearly that all efficiencies, both for full load and fractional loads, should be based on the full-load operating temperature.

J. M. Weed: I would suggest that it ought to be satisfactory to specify the efficiencies of the transformers at temperatures at which they are guaranteed to operate at full load; that is, with standard room temperature, if you guarantee a transformer for 35 deg. cent. rise, with a room temperature of 25 deg. cent., base the efficiencies of that transformer on the losses at 60 deg., and if you guarantee 50 deg. cent. rise over 25 deg. cent. room temperature, you guarantee your efficiencies on the basis of 75 deg.

W. C. Smith: Inasmuch as I brought up this point, I would like to go on record that I concur fully with the two gentlemen who have just preceded me—in my opinion, all efficiencies should be based on the full-load guaranteed operating temperature.

G. K. Kaiser: Referring to the question of operating temperatures, it appears to me that the regulation as well as the efficiency should be based on the temperature which the apparatus assumes when operating continuously under load. That is, the regulation at various loads and power factors should be determined from the *load losses* at operating temperature and the reactance of the transformer.

(*e*) Brush Losses

W. B. Brady: Referring to the paper by Messrs. Edgecomb and Dick, the brush manufacturers are very much interested in the subject of brush losses and we are very glad the Institute has taken up this matter. We have been working on the development of brush tests for more than six years but we have been handicapped by the lack of apparatus that would give us consistent results. Then, again, as we have developed our own apparatus the results we got were applicable only to that apparatus and only under the exact conditions of that test. This is due to the number of variables and losses that are in a certain combination under the conditions of that test. By having these losses more definitely separated and understood we may be able to get a definite basis to make our results comparable. One of the best ways I know of is to standardize the brush-testing apparatus and I hope this can be done in the very near future. It will be a decided step in brush development which will be of great assistance to engineers.

Leo Schuler: I ask whether any experiments have been made in this country with regard to the influence of the current flowing from the commutator to the brush, in relation to the friction. It is rather surprising, but it is a fact, that friction is somewhat influenced by the current on the brush. I cannot see the reason for it, but there may be a molecular attraction on the surface of the commutator.

B. A. Behrend: In regard to Mr. Schuler's question, whether the coefficient of friction depends on the current in the brush. If an experiment is conducted on an electric motor or generator, very many vitiating factors enter into it, so that it would be impossible to say that the presence of an electric current is responsible for the alteration or change of the coefficient of friction, and I ask Mr. Schuler whether he means that under stationary conditions such change or variation in the coefficient of friction is obtained?

Leo Schuler: I do not know whether in those cases it would affect it or not. What I know is this: With a slip-ring arrangement, driven by a small synchronous motor, just sufficient to drive the slip-ring with the brush on it, that synchronous motor falls out of step the moment you put current on the brush. That seems to indicate an increase of friction due to the current.

There is another reason. When you work a machine idle, that means excited, but no current on, then you hear a certain noise produced by the friction of the brushes on the commutator, and you will notice quite a pronounced alteration of this noise, when you put current on; there seems to be no other explanation for this than that there is another coefficient of friction when the current is on the machine.

B. A. Behrend: May I ask whether it is not possible that this additional power required in your synchronous motor is due to current induced in your slip-rings and in your brush? You would expect such induced currents, would you not?

Leo Schuler: But I cannot see how power is taken from the motor for these eddy currents.

B. A. Behrend: That seems to be the critical point at issue; if there is any possibility of this applied power having to come from the synchronous motor, it would not be charged to coefficient of friction.

Leo Schuler: I think if there are induced currents in the slip-rings then the power to produce these currents can only be taken from the source of current which is going through the slip-rings, but not from the motor driving the slip-ring.

B. A. Behrend: I would not be so ready to endorse that statement; it does not seem to me to be absolutely evident that it is so. Perhaps some one else has thought more about that point, and can give us some information regarding it. I ask Mr. Lamme whether it is not possible that the creation of a magnetic field by an electric current and its consequent passing through the slip ring would not account to some extent for the falling out of step of the synchronous motor driving the device. I do not know that this is the explanation in the specific case cited, but it seems perfectly plausible to assume the existence of such a magnetic field through which the slip-ring has to cut at a certain definite rate of speed which would put an additional load on the driving synchronous motor. I may be mistaken in my explanation, but it is entirely reasonable.

W. B. Brady: Answering the question about the influence of current on the coefficient of friction, Messrs. Martindale and Berkeley in their written discussion indicate that the coefficient of friction decreases very materially with increase of current. We tried that under four different grades of carbon on the slip-ring, simply a band of copper mounted on a pulley, and measured the friction in that way. It decreased very materially with increased current.

A. H. Freeman: We have made no particular tests to determine the effect of current density on coefficient of friction, but my observation has been, on some temperature results, it may have that effect Mr. Brady speaks of, that the coefficient of friction drops slightly with increased current densities. In the paper by Messrs. Edgecomb and Dick, I see that they recommend a set of constants for representing the difference between the surface of the test apparatus and the surface of cast-iron, steel, bronze and various other materials.

Alexander Gray: About a year ago I started some students on experimental work on brush friction and the results they obtained were so erratic that no satisfactory conclusion could be drawn from them. Recently I started up the same apparatus and found that the coefficient of friction could vary 300 per cent in half an hour. The friction force was measured by a spring balance at the end of a beam which was supported on ball bearings concentric with the shaft of the motor and which carried two brushes at opposite ends of a diameter. At the start, the pull on the scale was about one pound; at the end of half an hour the pull had gone up to two pounds, and the temperature of the brush, measured by a thermocouple placed within 1/16 in. of the rubbing surface, also increased. The brushes then began to chatter, the temperature rose very rapidly to several times the previous value and the pull on the scale went up to 3.5 pounds. One of my assistants came into the laboratory at that time; he was smoking and blew a cloud of smoke on the ring; the scale reading went down immediately to 2 pounds, the chattering stopped and the temperature dropped considerably; the chattering did not commence again nor did the pull on the scale reach the value of 3.5 pounds for about one minute. Afterwards, as a matter of curiosity, I ran my finger across the ring surface, the pull immediately dropped to two pounds, the chattering ceased, and both took about one minute to rise again to the original value. A piece of waste was then put in contact with the running ring and was left there for half an hour, at the end of which time chattering had ceased and the scale reading had come down to one pound, and then, although the waste was removed, the pull did not increase in two days, for which length of time the apparatus was kept running continuously. A bunsen flame was then applied to the ring, and although the friction pull decreased, the decrease was very small. A little vaseline was then put on the ring and the scale pull came down to a smaller value and stayed there.

After a thin film had been ground from the brush contact it was possible to reproduce the same cycle of operations.

In case there should be any misunderstanding, I may say that the brush was a modern one, and had a highly polished surface. The film on the ring was not something I put on myself, but was inherent in the brush.

The above results seem to me to be of such importance that I would be tardy in standardizing certain coefficients of friction for certain standard machines, in fact I consider the proposition to be very objectionable. Brush friction depends largely on the design of the holder and on the workmanship put into the brush mechanism. If I buy a machine with a certain guaranteed efficiency I want to measure the efficiency or losses in some way or another, and shall not be satisfied with something calculated by the manufacturer from constants, even although they are standardized by the Institute.

If the brush friction is measured when the brushes are newly ground a certain result is obtained; if the machine is then run for several hours the friction loss may increase 300 per cent, but by running a piece of waste across the commutator surface for some time the friction loss can be reduced again. This being the case, I would suggest that the new rules make some recommendation as to when the friction loss shall be measured, whether before or after the heat run, and also as to whether or not the manufacturer should be allowed to clean the commutator before he measures this loss.

Comfort A. Adams: I think we will all agree that the whole question of satisfactory operation of commutating machines, as to the commutator and brushes, is one which depends upon a very great many details not only of manufacture but also of care and operation. There are many variables involved, and it is difficult to lay down general rules. It is not anything you can theorize much about, one way or the other. I will simply state what I have found to be the most successful method of operating commutating machines from the purely practical standpoint. This applies to modern, good quality commutating machines, with brushes such as are ordinarily supplied. Start with the commutator in perfect condition, as far as it can be made so, and the brushes well fitted; then watch it very carefully for the first few days, or possibly a week. During the first day of its operation be very sure to keep the commutator so clean and in such condition that it will not cut. I assume, of course, that the machine does not spark enough to burn. After the polished condition has been once established, very little care will keep the commutator in good operating condition without any lubrication. In fact, my experience has been that there is more danger with lubrication than without, after the commutator is once thoroughly well polished and is in good working condition. It is not sufficient, in the case of a freshly ground commutator, to clean it off once and let it run for a day or two without any care at all. It is only after it has become thoroughly polished that it can be allowed to go with occasional care, and the principal thing is to keep it clean and dry. There is a little lubrication in these brushes, but much of it is apt to gum up the commutator and a little sparking will cause the coefficient of friction to rise very rapidly.

Alexander Gray: The work presented to us in these papers is excellent and we ought to be grateful to the writers for the amount of work they have put into them. I know what the measurement of the coefficient of friction on brushes means. The point we are concerned with, however, is not the angle of the brushes—we are not going to test for that; we are looking for a suitable performance test for brush friction loss and instructions as to how to make it.

As Professor Adams says, the proper thing to do is to take care of the commutator for two or three days and then let it run; but if you tell the manufacturer you want the machine to run for a week before you test it, he will probably increase the price. The point I want to make is that we might hurry this first part of the business by cleaning off the film which is deposited and so allow the brushes to take the bedding sooner than two or three days. If we can do that, let us standardize it; if we cannot do it, let it alone.

In regard to the suggestion that the smoke was a lubricant, it is ingenious and it may be right. The smoke may also act as an abrasive which cleans off the film of carbon; the same man blew on the commutator when he was not smoking and it made no difference to the friction loss, so that we can eliminate the question of moisture.

B. G. Lamme: Mr. Schuler has raised a point about the effect of current on brush losses. I will say that under some conditions current may have a great effect on the total losses in connection with collector rings. In the case of very heavy rings carrying very heavy currents, the magnetic field set up by such currents, if cut by the collector rings, may be such as to cause very heavy eddy current losses in the rings themselves. In the case of a 2000-kw. unipolar generator with which I am familiar, in the preliminary tests, one-half of the rings on the machine were made of steel, instead of bronze, and on the test with normal full load current under zero voltage conditions, the measured loss was approximately 200 kw. higher than when all the rings were bronze. Our investigation showed that the excess loss was due almost entirely to eddy currents due to magnetic action, and was not due to brush contact. The loss was almost entirely in the rings themselves. With bronze rings there was some loss, but not more than a few per cent of that found with the steel rings. In this case, therefore, the losses varied with the current, and such losses were carried by the driving motor.

Apparently there are two opposite opinions regarding this variation of friction with the current, one claim being that it is reduced with the current and another that it is increased. It seems to me that an action other than the eddy currents above described may also be present, namely the influence of the direction of current on the brush contact itself. Our test with the unipolar generator referred to, showed that where the currents passed from the brushes to the rings, the rings tended to take a good glaze, and but relatively little attention was required in the way of cleaning; whereas, where the current passed from the rings to the brushes there was a continued tendency to burn away the surface of the rings and to increase the resistance of contact. Apparently, one of these actions should give less friction loss than the other, and possibly in the conflicting opinions cited, the different results may have been due to different arrangements of testing which did not take the direction of current into account.

In the question of brush friction losses, especially on new machines, the condition of the commutator itself must be taken into account. Until commutators are well "seasoned," they are liable to show high brush friction losses. The binding material in the commutator mica may ooze out to a very slight extent, and "gum" the commutator slightly. This will increase the friction enormously, but as it is obviously impossible to thoroughly season every commutator before shop test, it is necessary to put the commutator in the best possible shape for such test, with the understanding that in practise, after it is well seasoned, the losses will doubtless decrease materially. That is one of the handicaps on the manufacturer.

Alexander Gray: Should that seasoning be done by the manufacturer or should that seasoning be done by the purchaser? If we adopt certain standard friction coefficiencies, then it will have to be done by the purchaser. If we do not adopt these standards, the manufacturer will be at liberty to get his commutator into any kind of shape he likes, and we will test it when in that shape, and then the manufacturer will have to do the tinkering to the machine. Most of us would be quite satisfied, on an order of twenty machines, to get a test on one; to test all of the twenty would be somewhat of a nuisance. We do not expect elaborate tests on small machines.

R. B. Treat: The efficiency of direct-current machines will in the majority of cases be found to meet the specifications at the time of factory tests. Whatever reduction of brush friction occurs on the commutator after erection accrues to the advantage of the customer.

Commutator seasoning time may vary all the way from zero to six weeks or more, but the manufacturer's brush friction figures are usually based on results he can obtain in factory test.

One disturbing feature of brush tests on collector rings is the chattering. This often results from a slip ring loosening when it becomes warmed up. Some recent tests were conducted with taper shaft and ring fitted with a follow-up spring.

Many cases of very rapid brush wear were cured by the substitution of rings that could not become loose, even where the original ring could not be declared " loose " by hand examination.

Brush friction probably decreases as the current increases. The general result of many tests indicates this conclusion.

W. F. Dawson: I would suggest in explanation of Professor Gray's trouble with brush chattering that the trailing angle was incorrect and that a reduction in such angle would have stopped the chattering. The increased friction coefficient observed was probably due to the brush surface picking copper.

Alexander Gray: I am personally doubtful about a leading brush not chattering so much. My experience has been largely with machines having trailing brushes, and if these are rotated in the opposite direction the brushes will generally chatter.

B. A. Behrend: As Mr. Lamme has stated, there are so many mysterious factors about the commutator and the brushes and in the operation of commutators, that he has no hesitation in frankly saying so. Professor Gray, who has had wide experience in the operation and design of direct-current machines, has come to the same conclusion.

I do not think it is possible to adopt a specific brush angle. We have to run at all commutator speeds up to 8000 ft. These angles have to be adjusted, and it is often necessary to undercut the mica on some commutators; in other words, every possible and suitable means must be resorted to in order to make the commutator run true and to eliminate the chattering of brushes. Chattering of brushes is due to some sort of harmonic disturbance, and it becomes cumulative at certain speeds and under certain conditions. The disturbing periodic force can occasionally be eliminated, and though the natural period of vibration remains, the external cause of chattering having been eliminated, the chattering has been stopped.

Under these conditions it seems to me it would be folly, as we are interested here in the subject of standardization, to lay down rules as to the coefficient of friction, as to the angle of the brushes, and as to a great many of these things which the manufacturer himself does not know anything about as yet. I was once talking to the chief draftsman of one of our large manufacturing companies, and I asked him if he had any standards. He said, " Why, Mr. Behrend, we have thousands of them." We are going to have thousands of standards if we have to lay down such rules at the present time, because we do not know enough about these things. Every day the field changes. The commutator of to-day is a very different piece of apparatus from what it was ten years ago.

F. D. Newbury: I believe Mr. Gray has been the only speaker who has kept to the point at issue in the discussion of the brush loss papers. The work before the Standards Committee, as Mr. Gray said, is determining how to measure brush losses and not how to operate machines.

In regard to brush friction, I do not believe that this measurement presents a very difficult problem, not so difficult as the determination of the other brush losses touched on by Mr. Wilson, losses of commutation, voltage drop, etc. Brush friction can be measured directly on the machine at the time the machine is on test at the manufacturer's plant. If such determinations do not fall within reasonable average values, it means one of two things; that the manufacturer must go to the expense of putting the commutator in first-class shape, which not only means seasoning so that there are no high bars or high mica, but running the machines a sufficiently long time to secure the dark brown glaze every one likes to see on a commutator; or that the customer accepts such average values, appreciating the fact that the commutator on shop test is not in the best condition. In the case of other brush losses, involving voltage drop, I believe we will be forced to an assumption of average drop per brush, or some such method as will be discussed tonight in determining additional losses.

L. E. Underwood: Regarding the running of brushes trailing, I am inclined to think that it is possible to run them trailing with success at slightly greater angles than 15 deg., possibly at 20 deg. or even 25 deg., although that depends a great deal upon the speed and condition of the commutator.

As for running the brushes leading, it is undoubtedly true that for different speeds of commutator it is necessary, in order to get best results, to run a leading brush at different angles. That being the case, the practical application of leading brush holders on the same machine which is used for various and sundry speeds, seems to be quite a problem. I do not see exactly how the problem will be solved.

T. M. McNiece (by letter): The determination of brush losses is exceedingly difficult under any conditions. The actual values or results secured by tests are greatly influenced by the state of the surfaces of the sliding contacts. The duplication of these surface conditions on brushes and commutators is exceedingly difficult and it is a problem to determine whether or not these factors are sufficiently alike in successive tests to warrant definite conclusions.

In the paper by Messrs. Erben and Freeman, attention has been called to the effect on friction of various angles of inclination between the brushes and the commutator. In connection with this, a formula is given for calculating the watts lost through brush friction, and this formula is applied to a specific case.

Some question may be raised in regard to the accuracy of this formula. The effect of varying the angle of inclination of the brush with respect to the commutator may be well illustrated by the use of a force diagram. With holders of the box type, four forces may be said to be acting upon the brushes: (1) the pressure, P, applied to the brush in the direction of its axis; (2) the frictional force, W, at the brush contact, acting in the direction of rotation; (3) the normal pressure, N, between the brush and commutator; (4) the reaction H, of the brush holder. The line of application of this force is normal to the sides of brush and holder. The amount and direction of P and the direction of W are known and the amount of W determined by the test as made. The angle B is the angle between the brush and the normal to the commutator at the point of intersection between the axis of brush and the circumference of commutator.

If a force diagram be now constructed it will be seen that

$$N = \frac{P}{\cos B} + W \tan B$$

The formula in this form applies to those cases in which the brush is trailing. When the brush is leading the formula becomes

$$N = \frac{P}{\cos B} - W \tan B$$

The coefficient of friction may then be calculated from the formula $F = \frac{W}{N}$, where F is the coefficient of friction.

Substituting in this equation the values of N, for both trailing and leading brushes, we find that

$$W = \frac{F\,P}{\sin A - F \cos A}$$

for trailing brushes and

$$W = \frac{F\,P}{\sin A + F \cos A}$$

for leading brushes, where A is the angle between the brush and the tangent to the commutator at the point of intersection of the axis of the brush with the commutator.

When the brush is trailing, the normal pressure is found to be composed not only of the projection of the applied pressure, P, but also of the added projection of the reaction, H, of the brush holder. The forces, P and F, combine to make H of considerable moment in the trailing brush. The normal component of this latter force may be sufficiently great to hold a brush on the under side of the commutator without any spring tension and against its own weight.

The cause of the greater friction in the trailing brush may be said to lie in the greatly augmented normal pressure on account of this wedge action of the brush between the holder and the commutator. As indicated in the formula

$$W = \frac{F\,P}{\sin A - F \cos A}$$

for trailing brushes, there is a certain value of the angle A for each value of F, where theoretically W becomes equal to infinity. This is at the point where the tangent of the angle A is equal to F and the reaction, N, at this instant is infinitely great. Before this point is reached the holder or brush would yield, or the braking action would stop the rotation of the commutator.

In the leading brush, the action of the force W tends to decrease the wedge action of the brush as well as to decrease the applied pressure P. As a result of this, the normal pressure is greatly decreased, with a consequent reduction in friction. The formula in the paper under discussion would therefore take the form

$$W = \frac{P \times F \times V \times 746}{33000\,(\sin A + F \cos A)}$$

Sufficient work has not been done along these lines to enable us to say how closely results may be expected to agree with the theoretical formula, or what correction factors may have to be applied in order to make this equation of working value. A later report will be made as soon as definite results are secured.

There is always a certain degree of eccentricity which may cause a very slight rise and fall of the brushes in the holders and it is possible that the slight friction between sides of the holder and the brush may affect the results to some extent. The clearance between brushes and holders may also exert some effect on these quantities.

Another factor which would undoubtedly have a considerable influence on this point is the rigidity of the holders, especially when the brushes are trailing. The brush holders on a friction testing machine should be rigidly constructed and supported, or they will have a great influence on chattering, which will seriously affect the frictional readings. A study of these conditions leads to the conclusion that trailing brushes demand much more rigid holders than leading brushes.

In view of the form taken by this formula, it is recommended that the characteristic friction tests made on any grade of brushes be made with the brushes in a radial position. The proper friction at any angle can then be calculated from this formula. This will simplify the tests very greatly.

L. R. Berkeley and E. H. Martindale (by letter): There is one phenomenon which has not been mentioned in this paper which is worthy of much investigation, namely the variation of friction with different current densities. The accompanying figures are an average of the results obtained on four different grades of carbon brushes, with comparatively high coefficient.

Amperes per sq. in.	Coefficient of friction
0	0.81
20	0.59
40	0.41
60	0.34
80	0.30
100	0.27

These results were obtained on a copper slip ring at a pressure of 2 lb. per sq. in. (70.4 grams per sq. cm.) and at a peripheral speed of 1000 ft. (305 m.) per minute, with brushes set in a radial position.

From the table it is seen that the coefficient of friction at 100 amperes per sq. in. (15.5 amperes per sq. cm.) is one-third as great as when no current is passing through the brush. The writers believe this may be due to a graphitization of carbon particles by the small electric arc which carries the current between the brush and the commutator.

This phenomenon cannot be explained by change in temperature, as artificial heat will not produce the same effect, in fact, heat usually tends to increase rather than decrease the friction.

After the current is shut off it requires from three to fifteen minutes for the friction to rise to the normal zero current value.

T. M. McNiece (by letter): As stated by Mr. C. E. Wilson, the contact drop losses are so intimately associated with others which may be termed commutation losses, that their accurate separation seems to be practically impossible at this time.

In practise the I^2R loss at the brush contacts is often so much greater than the loss which would be indicated by test on a slip ring or short-circuited commutator at the rated current density, that the tests may be said to give almost no indication of the loss to be expected at this point. It seems very proper to separate such losses from the brush losses if possible, and since they depend entirely on certain details of design and construction of the machines, the pure brush losses should be determined and increased by a certain factor which might be termed a commutating constant. This constant would be peculiar to the type of machine upon which the brushes are to be used.

It seems advisable in making contact drop tests, to make all standard tests upon radial brushes. In view of the great changes produced in the normal reaction between brush and commutator by running the brushes at various angles, and since this normal reaction also has great influence on contact drop, it would seem that these effects might be determined by the use of suitable constants.

The decreased friction secured by operating the brushes in a leading position cannot be said to be a net gain, as it may be assumed that under the decreased effective pressure, the contact losses will be increased to a certain extent.

The variations introduced by running the brushes at different angles are merely those which would be produced by changes in effective pressure and area.

From an analysis of the conditions accompanying the determination of brush losses, it seems that in friction losses as well as voltage losses, the most satisfactory methods for securing these results will be those in which the characteristic tests are applied under the most simple conditions and the results of these standard tests modified by the application of factors to be determined by the actual condition of operation. This system would result in simpler and more effective standardization of testing methods.

(*c*) Generators, A-C. and D-C.; and (*d*) Errors of Tests

John L. Harper: There are statements in the paper by Messrs. Foster and Knowlton which seem to warrant further consideration, reference being made especially to the first paragraph in the summary. The authors describe four methods of determining losses, in each of which it will be noted a different result was obtained.

These losses are measured for the purpose of determining the efficiency of the generators. Now, if the efficiency of a generator could be determined by the use of several different methods, which under the same conditions gave different results, this one generator at the same moment would have several different efficiencies, depending upon the method used for determining the losses or upon the wording of the contract under which it was furnished, or whether the designing engineer had been so incorrect in his computation that it was necessary to boost the efficiency by a choice of methods.

It may be conceded that efficiency might vary with time or conditions, and that errors of measurement of energy and losses may exist. But it is undoubtedly true that a generator can have only one efficiency at one time.

I therefore believe that the Standardization Rules should be explicit and frank in the statement that the efficiency of an apparatus is a fact, and not a variable result depending upon the method of determination.

The gentleman who prepared the articles on efficiency in the old rules probably never intended that a manufacturer should claim the approval of these rules, in requiring a purchaser to accept and pay for apparatus as meeting efficiency requirements when such so-called efficiency determination was based on the shop measurement of certain segregated losses; when this same apparatus after being set up and tested by a method closely resembling the " circulating energy " method (mentioned in the paper as the most correct of the determinations), was shown to have additional losses which brought the efficiency down to about 3 per cent below the specifications.

As one of that part of the membership of the Institute which is not connected with the manufacturing interests, it is not my desire to put forward such methods of measurement of efficiency as will cause undue or unnecessary expense to the manufacturer, thereby increasing over-all expense which is added to prices which customers must pay; and it is far from my intent to assume that it would be right or proper to require small and stock apparatus to be sold under any special efficiency requirements.

It is only my desire to ask that, in the further consideration now being given the Standardization Rules, the word efficiency be considered to mean true efficiency; and that the Institute give its approval, for the purpose of determining efficiency, only to such methods as measure all the loss (within the limits of correctness of measuring instruments), and also such methods as represent a minimum probable error; and that any other commercial methods be accepted only as approximates of efficiency, and be approved only on that basis. What I want to bring out is that methods in which all the losses are not determined should not be used for efficiencies, but should be used in determining commercial approximations of efficiency which are approved on this basis. Therefore, in my opinion the drafters of the new rules should make clear what is intended by the term efficiency, and then so generalize the methods of determining the losses, that facts cannot be evaded, and still full leeway may be obtained for the adoption and use from time to time of more correct methods of determining all the losses in electrical apparatus.

It is unfortunate that the classes of apparatus chosen by Messrs. Foster and Knowlton for these determinations are such that the difference in the losses determined by the segregated loss method and the " circulating energy " method appears to be slight, for in my own experience, gotten in the Niagara Falls district in connection with apparatus from the smallest to the largest sizes, it has come under my observation that in certain classes of machinery a difference of two or three per cent may be found in the losses as determined by the manufacturers according to the segregated loss method, and those found after erection, by methods which measure all the losses with minimum error and of the general form called " circulating energy " methods in the experiments of the above authors.

In general, in small apparatus the exact determination of efficiency is of little interest to the purchaser. In the larger, and often the special apparatus used in the Niagara vicinity, exact determination of efficiency is of vital importance, often representing the payment or non-payment of hundreds of thousands of dollars, and it is hoped that the Institute will not approve such methods of determining losses that the purchasers of this class of apparatus will be obliged to oppose the standards approved by the Institute, but may rather be assisted by them in obtaining correct efficiency determinations of any of their electrical apparatus.

Leo Schuler: In the German Standards Committee we have recently discussed very carefully the question of what we call " additional losses." We have tried to get some method for taking these losses into consideration, but so far we have failed. We have considered the proposition contained in your present rules, which takes one-third of the short-circuit loss, but this, I understand, is only intended for synchronous alternators. You do not give any hint how to check additional losses, in the case of direct-current machines or induction motors.

I have looked forward with great interest to what is to be said about this question here, but as far as I can see, it will be very difficult indeed for the Standards Committee to condense these papers into certain rules to be applied to all kinds of machinery, and I should strongly advise you, if you do not arrive at a very definite method of doing it, and I do not think you will, to leave the additional losses out altogether and do as we have decided to do, that is, call the efficiency measured by the separate loss method the " conventional efficiency " or something of that kind, an efficiency which is somewhat lower than the real efficiency, but you do not know how much lower it is. This proposition is not very good, but I think it will be the best you can do.

R. E. Hellmund: I wish to make a few remarks about the paper on " Losses in Commutating Machines," especially referring to direct-current machines. The hope is expressed in this paper that it will be possible to get a constant percentage which should be added to the I^2R losses in order to take into account the stray losses. This, of course, would get the average efficiency slightly more correct, but it certainly would not show the merits of the various machines. The reason for this is that the stray losses are very numerous in direct-current machines, in fact in all commutator machines.

In the paper referred to, the authors mention only the commutation losses and the loss due to flux distortion. These are some of the most important losses, but there are a large number of others, which in some motors may be rather large. We have first the regular iron core losses, which, of course, are little influenced by the distortion, but even these may be somewhat influenced by the interpole flux which exists only at load. The same is true with regard to the core losses in the teeth. Here, as the paper mentions, the distortion of the main field is of importance, but the interpole flux may also add some losses at load. Further, there may be some extra losses in the surface of the interpoles, which are due to the fluctuations caused by the passing of the armature teeth. Then we have similar losses in the surface of the main poles, which also change with the distortion.

Further, if the relation of the teeth to the pole face has certain proportions, the total reluctance of the magnetic path changes, and this, of course, will tend to set up fluctuations in the main flux which in turn will set up eddy currents in the frame casting, as well as in the short-circuited coils under the brushes; this, of course, again means losses. It has also been shown that considerable losses may occur in the commutator bars, and with very heavy currents and heavy bars this loss may be appreciable in some machines.

Finally, we have some losses which seem to me rather important, and which exist even with ideal commutation. The paper mentions that commutation losses exist if the commutation is not perfect. These losses are important, but even if the commutation is perfect, the current direction in the armature changes; in other words, the current in the armature conductor is alternating current, and we know very well that in alternating machines, with heavy conductors, the losses in the conductors are rather large. There is no reason why we should not have the same losses in the armature conductors of direct-current machines. These losses are due to the fact that the current sets up fluxes across the slot and these fluxes reverse when the coil passes the commutator zone, thereby inducing voltage and consequent eddy currents. Then we have certain losses in the armature conductors under the pole tips, in case the saturation in the teeth is very high. This is due to the fact that the reluctance of the teeth just under the pole is very high, while the reluctance of the teeth next to the pole, with small saturation, is rather low. The difference will cause a certain flux to pass across the slot, and since this is a changing flux there will be eddy losses. These losses were mentioned this morning. Finally we have the losses in the bands and coils.

It can be seen, with such a variation of losses, that it would be hardly fair to assume that the stray losses are similar in all machines, and I think that an attempt should be made to get at these losses separately. This, of course, can only be done by building machines which have only one of these losses at a time to any appreciable amount, and making tests along that direction.

In connection with commutator machines, it seems important for us to consider not only the direct-current commutator machines, but also the single-phase and three-phase commutator machines, for standardization. Some work along this line is especially desirable, because certain losses cannot very readily be tested in case of alternating-current motors, and in order to avoid complications it is always good to have some rules as to how they should be taken into account, when ideal methods of testing are not known. While the influence of the field distortion in the single-phase motor is usually not so important as in the direct-current machine, we have other losses. For instance, we have alternating current in all windings and consequent losses, and also the conditions are rather complicated due to the fact that there is a main flux and a cross or transformer flux. Since it is impossible to get a combination of the two fluxes as they actually exist, without having the load on the machine, there is at the present time no method of testing the core losses at all.

W. J. Foster: The two papers which I wish to discuss are those by Messrs. Olin and Henderson and by Messrs. Foster and Knowlton. The first contains diagrams showing connections of machines that must be considered in connection with the second paper, as that is not as complete as it should be. Now, reference was made this afternoon to the " circulating energy " method which was advocated by Messrs. Olin and Henderson, as a safe, reliable and accurate method, and a practicable one, where two identical alternating-current generators or motors are available. By referring to their paper, you will find a diagram given for connecting together two alternators for an input-output test. It consists in placing the machines back to back, with a coupling so arranged as to give an angle in one direction for one machine, and in the other direction for the other machine, and so that they will operate as generator and motor under certain conditions of load and power factor, and any conditions of load and power factor may be obtained by changing this adjustment. There is a direct-connected motor, preferably a direct-current motor, such as would be used in determining core losses or the friction and windage. It is not necessary to have the motor direct-connected, as it can just as well be belted.

The determination of load losses by this method, I have found, by a number of tests, involves no more difficulty, and is just as accurate as the determination of windage or core losses by the same method. The only objection that might be made to it, possibly, is that one machine is operating as a generator and the other as a motor, and we must assume that practically the same effect is obtained in the motor as in the generator in the matter of the additional losses due to the presence of the current in the primary. In working up the results, the open-circuit core loss in the generator is taken at the normal voltage, plus the *IR* drop, and in the motor, the normal voltage minus the *IR* drop. That method is compared by Messrs. Foster and Knowlton as to the results obtained with two or three other methods that were tried. One of the other methods that the authors undertook with considerable confidence that it would prove the practicable one, was the no-load phase characteristic, for the reason that when an alternator is running with a very weak field, or very strong field, just so as to have full-load current in the armature at the normal voltage, it seems probable that the same conditions exist, as far as producing the stray load losses are concerned, as under normal load. The trouble with that method is the measurements which must be made by wattmeters, first with the lagging current, and then with the leading current, and that the results must be compared with another value that is almost as great, namely, that obtained at the minimum current input. There are many little errors in the readings when you are reading a comparatively large quantity, which may amount to a very decided error when considered with reference to a small quantity that is the difference of two large quantities. All such errors are eliminated in the "circulating energy method."

This particular method of "circulating energy" must not be confused with a form of operation in testing that consists of coupling together two alternating-current generators or motors in phase, supplying the necessary energy to run them from a third generator, and obtaining the current desired by adjustment of excitation. In this method we do not obtain the correct conditions of potential, field excitation, etc., whereas in the one I have just described all conditions are normal, and there is just that one difference between the machines, that one is a motor and the other a generator.

The authors have given a great many data, these data having been available and having been selected out of a great mass of data of tests made on machines extending back for fifteen years. We do not have data available on this new method, because it has not been in use. The data given in these tables were available because the Institute has had a rule that for what it has called load losses, a short-circuited core loss should be determined, and one-third of that taken. By referring to these tables you will see that in some cases the second column under "core losses" shows in certain cases the short-circuited core loss to be as great as 1.5 per cent of the full-load energy of the machine, and would affect its efficiency 1.5 per cent, and that in certain cases the short-circuit core loss is greater than the I^2R loss, and in certain other cases greater than the open-circuited core loss, and in some greater than either; but by taking one-third of that, it will reduce the efficiency from one-quarter to one-half per cent, but you will also notice that the general average of all the machines is rather low, something like one-half of one per cent; and when we take one-third of that, it amounts to only one-sixth of one per cent, so that the machines have not been badly punished in the past, you may say, where the efficiency has been determined by taking into consideration one-third of the short-circuited core loss.

The input-output method was not tried on any one of these three machines, but, in connection with other machines tests were made by the input-output method, as has been referred to by other speakers. Such tests involve great difficulty and do not inspire confidence, since the tests obtained at different times by different men give such widely different results; whereas, with the "circulating energy" method, the results agree with one another every time you make the test, as closely as they do in the open-circuited core losses or determination of windage.

A speaker this afternoon referred to some of the machines given in the tabulation, as having very low short-circuited loss, and that it was unfortunate that machines had not been selected that had high short-circuited loss. We feel the same way, but there did not seem to be available any machine that had a high short-circuited core loss, and, therefore, we ran one of these machines single-phase, and greatly to our surprise, the loss there loomed up large, as you will see by referring to Fig. 11 of Foster and Knowlton's paper. Comparing Fig. 11 with Fig. 9, which refers to the same machine, you see that the short-circuited loss amounts to 3 per cent of the single-phase rating of the machine; whereas, when operating three-phase, it amounts to a little less than one-half per cent.

What we wish to suggest to the Standards Committee is that they do not discard these short-circuited core losses until further data are accumulated, as we think it probable that the entire short-circuited core loss should be taken. Another evidence that we have that the entire short-circuited core loss should be taken is what we have obtained from the heating of the machines. There are now available a great many data on the enclosed type of machines, not only steam turbine generators but water-wheel generators. It is a comparatively easy matter to measure the temperature rise of the air passing through the machine, when it is operating under load; then run the machine on open-circuit at over-voltage at a point where the losses are equal to the combined segregated losses as determined by the ordinary method. The difference in the temperature rise of the ventilating air under the two conditions is the measure of the load losses.

F. D. Newbury: The question before the meeting is not whether all losses should be taken into account in determining the efficiency, but the best method of determining the true efficiency. The operating engineer can have no legitimate objection to any method that takes all losses into account, whether that method is based on the determination of the separate losses, or on the direct measurement of input and output.

Mr. Robinson's paper shows the difficulty, if not the impossibility, of obtaining an accurate, true efficiency by the direct measurement of input and output, even in the most favorable case, where both can be measured electrically. Even with his well-known ability and unexampled facilities, the extra load losses are very elusive. The load losses as shown in curves 1 and 3 depart markedly from the known law of their variation with load. The reasons for this are well brought out in the paper by Messrs. Chute and Bradshaw. One probable reason for the obviously incorrect results shown in curve No. 3 is the unusually high efficiency of this set. With a combined efficiency at full load of 91.3 per cent by separate losses, the efficiencies of the separate machines would have to be 97 per cent for the alternating-current motor and 94.5 per cent for each 500-kw. direct-current generator. These are unusually high efficiencies for machines of this class. The combined efficiency of 88.7 per cent in the 1000-kw., 250-volt set more nearly represents commercial figures.

If the correctness of this statement be granted, then it follows that more accurate efficiencies will be obtained from the separate loss method (including all losses) than by the most careful input-output tests.

Alternating-Current Machines. Three recommendations for estimation of extra load losses are made:

1. That open-circuit core loss and calculated I^2R loss be increased by a factor 1.1 to cover the extra load losses.

2. That the measured short-circuit load loss be taken as the load loss.

3. That one-half measured short-circuit load loss be taken as the load loss.

The relation between the load losses measured under short-circuit conditions and those existing under actual load is too complicated to permit any general statement to be made that will apply to all generators. Consequently, it is very difficult to make one rule apply to all machines. Load losses may be roughly divided into three classes:

1. Increase in armature core loss due to change in flux distribution.

2. Eddy and hysteresis losses in adjacent metal parts due to the stray field from armature current.

3. Eddy current loss in the copper conductors.

In generators having well-divided armature conductors and small flux per pole (including all small generators and all engine type generators) the load losses are, undoubtedly, mainly due to flux distortion. For example, the 150-kv-a., 900-rev. per min. generator referred to by Olin and Henderson; the measured load loss for one generator at full load was 0.61 kw. and at 1¼ load 1.55 kw. The measured short-circuit loss (including I^2R loss) is 2.3 kw. at normal load amperes and 3.7 kw. at 1¼ normal load amperes. The I^2R loss calculated by Olin and Henderson is 2.27 kw. at full load and 3.55 kw. at 1¼ load. The measured short-circuit load loss is, therefore, practically zero at full load and 0.15 kw. at 1¼ load. The calculated I^2R loss is based on an assumed temperature which may or may not have existed during the short-circuit test, as pointed out by Brainard. Comparing the measured load losses at full load with the load losses at short circuit, the actual load losses at full load are seen to be greatly in excess of those at short circuit. The difference is, undoubtedly, due to the increased tooth loss under load, due to armature reaction. This same increase in measured load loss at full load over the corresponding loss at short circuit is shown in the 110-kv-a., 900-rev. per min. generator referred to by Foster and Knowlton.

In large medium-speed generators involving moderate flux per pole, the load losses are probably due to flux distribution and to increased stray field.

In high-speed steam and water turbine generators, the load losses are a combination of all three classes.

Single-phase generators are in a class by themselves, due to the pulsating armature field in distinction to the rotating armature field existing in polyphase generators.

I believe that a combination of the correcting factor advocated by Olin and Henderson and the direct measurement of load losses at short circuit advocated by Foster and Knowlton will be necessary. Moreover, different correcting factors will be necessary for different frequencies and for single-phase generators. The correcting factor is undoubtedly nearer the truth with small moderate-speed machines and low-speed engine type machines. For large moderate-speed and all turbo-generators, the actual load losses are probably equal to the measured losses at short circuit, judging from the data submitted by Foster and Knowlton. For single-phase generators, obviously the measured short-circuit losses are nearer the true load losses than those obtained by a factor based on polyphase machines. I agree with Mr. Foster that we should measure the load losses directly by the "circulating power" method. That method, however, is greatly limited by the fact that two duplicate generators are necessary, and it is an expensive test to make, so that it can only be used where the size of the machine and importance of exact efficiency demands that care and expense in the testing work. In spite of the complicated relation existing between actual load and short-circuit conditions, it is interesting to note the close agreement between load losses calculated by the two methods as presented by Mr. Knowlton in his discussion. It is also interesting to note that the load losses by any method of calculation are small when expressed as per cent of the machine output. In 25-cycle generators, the average is well under one-half per cent, and in 60-cycle generators the average is well under three-fourths per cent.

Direct-Current Machines. Comparing the Olin-Henderson paper and the Erben-Page paper, the results are seen to be in fairly close agreement for the 470-h.p., direct-current commutating pole motor in the former paper and the 500-kw., 720-rev. per min. commutating pole generator in the latter paper. The correction factor for the 470-h.p. motor is 1.27 at full load, while it is 1.29 for the 500-kw. generator at full load; while at 1½ load the former is 1.37 for both machines. In both machines the load losses are very closely proportional to the square of the load.

The non-commutating pole machines follow an entirely different law in regard to the variation of load losses with load, and the same constants cannot be applied to both classes of machines, particularly at light load. Other factors undoubtedly must be used for other classes of machinery.

To conclude:

1. Efficiency will be most accurately determined from the separate measurement of losses, including the extra load losses.

2. The extra load losses may be determined in the case of all machines by direct test when two duplicate machines are available and the importance of the matter warrants the testing expense.

3. They may be determined approximately in synchronous machines by established correcting factors and by the measurements at short circuit. I agree with Mr. Foster that we should not abandon the measurement at short circuit, because whether that does or does not represent full-load conditions, it is certainly a very good indication, and will detect faulty machines, particularly when the extra load loss is an eddy loss in the copper, which is a factor not always foreseen.

4. They may be determined approximately in commutating machines by established correcting factors. Correcting factors will vary with the ratio of no-load and full-load maximum inductions, which may be approximately calculated. That difference in the ratio is well brought out by the different conditions existing in constant-speed machines and variable-speed machines, and the reason for the difference in the correcting factors which Mr. Erben mentioned this afternoon.

5. The determination of additional load losses may be made only within relatively wide limits without causing a variation in efficiency greater than the minimum variation obtained with an accurate input-output test.

E. F. Collins: To those who have not had long and close contact with electrical testing, it may seem that input-output methods are best to adhere to, and that all this discussion concerning the stray load losses, brush friction, contact loss of brushes, etc., should cease. On the other hand, one who has conducted many input-output tests knows that only by the most careful arrangement can correct efficiencies be secured through the use of this method. This includes the best of power conditions, as regards its freedom from fluctuations, and skilled men as observers. Sometimes as many as 20 observers are required to obtain simultaneous observations in a single efficiency test. In order to secure the same accuracy in efficiency determinations very much less error in observations is allowable than where losses alone are being observed directly. Many sets of observations must be taken, so that the probable error will be reasonable. The computation of these observations requires the very greatest attention and diligence on the part of the calculator on account of their multiplicity. Many meters of precision must be used. Heavy currents usually associate themselves with the method, especially in the larger units, and much care and judgment must be exercised to protect against unknown influence of stray fields. Thus one could continue to enumerate, almost indefinitely, precautions which accompany a successful determination of efficiency by input-output methods. Many such sources of error do not exist in using no-load methods, and where they do exist they do not influence final results to the same extent.

Mr. Robinson has shown well by tables and curves what may be done in determining efficiency by input-output methods. He has indicated somewhat the price paid for his results, and this price may be now and then justified. In general testing, however, I think there is little justification for the input-output method.

It has been said this afternoon that if we are to get at the true efficiency by segregated losses, we should include all losses. I agree that we ought to have approved methods of test which will allow of a determination of all losses, as well as a segregation of of such losses when required. I believe that certain methods exist and have been employed that do determine such losses, at least for certain classes of apparatus, such as direct-current rotating apparatus and alternating-current synchronous apparatus.

At the present time, standard methods exist for determining core loss with brushes at electrical neutral. Measurement can readily be made of bearing friction and windage, also brush friction, at no-load. The I^2R loss of the armature and fields may be readily and accurately figured for any load. The contact and brush resistance may be calculated from the contact loss curves, or it may be measured directly under operating conditions, as I will indicate later. Having determined these losses by no-load methods, the question naturally arises whether or not all the load losses are accounted for.

A method which may be relied upon to answer this question for alternating-current synchronous apparatus is the "circulating energy" method described by Messrs. Foster and Knowlton.

The objection to this method is that it involves two duplicate machines. As to its accuracy, however, in accounting for all losses, there is no doubt. It is, of course, desirable to have a method which may be applied with the same accuracy to single machines. Hence, the turning to a consideration of the short-circuit core loss as compared to the stray load loss measured by the circulating method.

I want to refer to the diagram given in Mr. Robinson's discussion, to illustrate that an equally efficient method has been employed for direct determination of stray load losses in direct-current machines. Incidental to this method of determining stray losses, and in view of the discussion this afternoon of brush losses, I want to point out to you that in this method we have actually measured the brush contact I^2R loss under operating conditions, through the use of insulated brushes, as shown. It may be of interest to say that a number of such determinations have been made in this way and they agree closely with standard curves of contact resistance loss that have been derived from tests upon a dead commutator.

I want to call attention also to the fact that if a change in brush friction occurs under load, this method takes account of it. From such tests I have had indications of change of brush friction with current. Some of those discussing the subject this afternoon held the opinion that this friction decreased with current, others that it increased. I am sure from the tests I have made that both are in a measure correct. In some cases I have observed an increase with current, in others an indication of a decrease with current.

It will no doubt require much investigation before practical methods may be devised that will permit of ready use for determining stray load losses for all classes of apparatus. In the meantime, perhaps, the use of certain empirical constants may be necessary in determining true efficiency by segregated loss method. In that event, however, I believe that some such direct method of measurement of these stray load losses should be employed as a basis for the value of such stray load losses, and correcting factors, should identify them with the designs with which they correspond.

B. G. Lamme: I wish to speak on the subject of load losses from the standpoint of a designer, and not as a member of the Standards Committee. Since I first looked over the Institute rules, I have always objected to the methods given in these rules for the determination of load losses, and also the method for determining efficiency, as it was an approximation that did not represent the facts closely enough to suit me.

Now, in comparing any of the methods of making tests for the stray losses, we should not take good machines when making such comparisons, because, in well-proportioned machines, the extra losses may be relatively low. We should pick out machines which we have reason to think have abnormal extra losses, and in that way we may find so large an extra loss that errors in the determination are of smaller proportion.

For many years I have been concerned in the measurement of load losses, and have frequently taken up the calculation of them just to determine the possibilities for the application of correcting factors, which would cover fairly accurately the extra losses. Such calculation is a long and complicated process in many cases, but when carefully carried out, I obtained some astonishingly close results in those cases where I had reasonably accurate data to compare with, but I will say that the accuracy of these results was, in one way, very discouraging; for in some cases, the larger items in these calculated extra losses were such that they apparently bore no relation whatever to the measurable quantities; that is, the data we could obtain by any simple direct measurement had no relation to the extra losses, and therefore our separate measurement of losses gave no direct quantitative indication of the value of these extra losses.

In a very well proportioned machine, I will venture to say this—that I can review the design of that machine, and I can then estimate, off-hand, the load losses as closely as they would be given by any of the methods suggested this evening. Still, these methods may show closer, on individual items of extra loss, yet, in my rough estimate, I would allow for some losses not touched on at all by any of the methods proposed for approximating the extra losses. In taking any of these methods of approximating the losses, you may get results which look very good from the general standpoint, but if you look at the results closely from the design standpoint, you are likely to see something in them that you know is inconsistent with the design—and that throws doubt on the whole result.

Take the method of determining losses by short circuit. In many machines there are extra losses due to saturation of certain parts of the circuit, and these do not appear at all in the short-circuit test, and therefore, in such cases, it is purely an accident if the measured short-circuit loss happens to coincide with the extra losses in the machine.

It looks to me as if all of these methods, including the input-output, under commercial conditions, must be considered as only rough approximations. I have seen a good deal of testing done by the input-output method, and I have never been satisfied with the results unless I knew beforehand what the extra losses ought to be. I have seen tests repeated four or five times until the results happened to give a value that everybody agreed upon as being reasonably correct, but in fact, this particular result may not have been any closer than any of the others. It seems to be largely a question of keeping up the testing until everybody is satisfied or tired out. That is how much confidence I have in it.

We must look at this input-output testing from two different standpoints. The man who tests the machine from the instrument standpoint may have absolute confidence in the instruments and in the men who make the tests for him, but the result which he thinks is satisfactory or correct may appear to be decidedly incorrect to the man who designs the machines, for he may know that the stray losses indicated are inconsistent with his design.

It is possible that some correction factor, even if an assumed one, is better than to do away with any allowance for the extra losses. The great trouble with the old rule of segregated losses was that the engineering public knew that these did not represent the total losses, and did not want to accept the resulting efficiencies, because there was something left out. Now, the total extra loss usually represents only a small value compared with the measurable losses, and even if omitted, the error is ordinarily rather small, although, in extreme cases or under very special conditions, it may become quite appreciable. In some cases, this omission of the load losses has been made up by the direct assumption of a certain per cent reduction in the efficiency, as, for instance, one per cent. Such assumption unquestionably averages closer to the true result than omitting the stray losses altogether. But it seems to me that this is largely a commercial consideration, and, from an engineering standpoint, all we can do is to state what the measurable losses are, and then possibly indicate roughly the total extra losses, but not make them the subject of any guarantee, as we should not guarantee what we cannot measure.

H. F. T. Erben: I wish to say a few words in relation to the possibility of obtaining a correction factor which will be universally applicable to machines of a given type.

I note in Messrs. Olin and Henderson's paper that they recommend certain correction factors, each of which will be applicable to all machines of a certain class. For example, they have a correction factor of 1.3 for direct-current generators and motors; 1.1 for alternating-current generators and synchronous motors; 1.1 for 60-cycle converters, and 1.0 for 25-cycle synchronous converters.

Our tests have shown that different correction factors must be applied to various types of machines comprising a general group, and these factors will vary very considerably. For example, we have shown that the correction factor for a fully compensated generator may be as low as 1.2, for a commutating pole generator about 1.3, and for a non-commutating pole generator it may be as high as 1.5. Again, the correction factor for variable-speed motors will be greater than for a constant-speed motor, due to the distortion of the wave form.

There will also be a different correction factor for commutating pole and non-commutating pole synchronous converters and another correction factor for converters of the synchronous booster commutating pole type. It is therefore evident, that if we intend to apply correction factors we must subdivide any given class of apparatus into its various types in order that we may arrive at the true value of the efficiency. Such a course of procedure might lead to such a multiplicity of correction factors that the whole scheme would become unworkable. However, I am not saying this in any spirit of discouragement, as I believe the various manufacturers should continue the tests which have been so ably carried out during the past few months, and it might be advisable later on for the Institute to appoint a sub-committee to formulate rules based on all the available test data.

I fully believe that, in those cases in which it is impossible to obtain accurate input-output tests, the efficiency should be determined by following the segregated loss method in combination with a suitable correction factor.

B. A. Behrend: All of us who have listened to the thoughtful remarks of Mr. Foster will, I think, be inclined to agree with him absolutely in his statements and recommendations. As I understand Mr. Foster, he desires to represent the full efficiency in his tests, and he suggested for alternating-current generators, of course, also for direct-current generators, the input-output test, which was first suggested in 1885, by the late Dr. John Hopkinson. The cost of that test is, however, frequently prohibitive. On that account it is difficult to carry it out in the power house or in the testing department. From time to time, however, two generators happen to be on the same shaft, either alternating-current generators, or direct-current generators, and then such tests can be made in a very simple and accurate manner, yielding very important data.

In applying these data to other cases, by using the experienced designer's judgment, we can obtain fairly good ideas of the actual efficiencies of machines, which can be used to form a basis for guarantees, even though, perhaps, we know only the individual losses. For example, if we have to guarantee a 25,000-kw. turbo-generator, to operate at 1500 rev. per min., and we have the data on a 10,000-kw. unit, it would not be difficult for a man of Mr. Lamme's experience to make his estimates in such a manner that he would secure fairly accurate results.

The advantage of the use of the short-circuit loss curve, I think, is not to be underestimated. The use of one-third of the total loss is arbitrary and absurd, as Mr. Lamme justly pointed out. Whether or not we shall take the entire short-circuit loss or whether we shall deduct from it the core loss, corresponding to the open-circuit voltage, which corresponds to the short-circuit current, is a matter of judgment.

Mr. Erben's extremely interesting results have shown that in order to coordinate the data on different types of machines it will be necessary to use correction factors, " thousands of them," and we should be worse off at the end than we were at the beginning.

If we should deduct the iron losses from the short-circuit core losses, we would certainly get a more nearly accurate measure of the short-circuit losses than we should have if we included the obvious iron loss in the amount that we use for the correction. The philosophy, I believe, of this whole thing, comes right down to this—that the further our experience carries us, the more we doubt. Those of us who have been through the building of large machines up to 25,000 kw., at very high speeds, appreciate the fact that we continually learn. As Darwin said in the preface to his great work, " The Descent of Man, " " It is more frequently ignorance that begets confidence than does knowledge." It will serve no good purpose to collect a number of utterly worthless correction factors. It is senseless to do it. You had better take the data Mr. Schuler mentioned today, calculate conventional efficiencies, and thus simplify your work. Conditions are constantly changing. You all remember that ten years ago the former Grand Central Station was completed. To-day that station has disappeared, and a new one, which has taken its place, has just been opened. Ten years ago the large 5000-kw. generators which Mr. Lamme designed for the Manhattan Railway in New York City were the leading machines of their class. To-day they are thinking of replacing them with turbo-generators. In ten years we have gone from 75 rev. at 5000 kw. to 1500 rev. at 25,000 kw.

I want to point out again that you are standardizing the past. Probably five years hence the facts will have ruthlessly wiped away the whole cobweb of rules we are laying down here. Your rules must be flexible, so that they can be applied to changing conditions. I plead for this because I feel very keenly the hampering influence of the old rules. It is a matter well worthy of serious consideration, especially by designing engineers. I know Mr. Lamme is in full accord with me. I do not like to say that for him, because he can say it for himself. I am sure I am in full accord with his ideas on the subject. It is extremely dangerous to go too far. The problem before us consists in laying down a few general rules which will not handicap and hamper the designing or consulting engineers. Make the rules general, and prescribe methods of test. For example, say whether the commutator should be seasoned at the factory or outside. If you do not want to say it should be seasoned at the factory, say it should be seasoned outside of the factory; if you do not want to say it should be seasoned outside of the factory, make such provision as you think is proper, but it is most important to do this, and to say what you want.

W. J. Foster: I want to say another word on the matter of the short-circuited core loss. I should hate very much to see correction factors adopted, one-quarter per cent, one-half per cent, or any per cent, that would punish good machines to the advantage of inferior machines. I should prefer to see the entire short-circuited core loss taken, in order that we might proceed at once to reduce these losses. It can be done in nearly every case by better design, and that is what we all want.

The use of $\frac{1}{3}$ the short-circuit loss probably does not approximate the correct quantity that should be added. I think we should take the whole loss, and possibly a little more, rather than a little less. Mr. Behrend will excuse me for pointing out that, unfortunately, the other scheme to which he referred would result in boosting the efficiency of two-thirds of the commercial machines at the present time, for the reason that the excitation that is required to put the short-circuit current through, gives an open circuit, a higher core loss than the entire short-circuited loss on most machines, and, therefore, when you subtract it, you get a negative value, and if you add that negative value to the other losses, you will have your efficiency raised rather than reduced.

B. A. Behrend: I meant to correct the exciting current for the armature reaction. I did not want to go into that detail.

W. J. Foster: Just the reactive drop?

B. A. Behrend: Simply take the reactive drop. I took it for granted that should be taken into consideration.

W. J. Foster: In line with Mr. Lamme's suggestion of correcting the core loss, or the combined loss and I^2R, I have never succeeded in getting anything that was reasonable or that fitted the case; I trust that some positive evidence will be brought forward that the entire short-circuited core loss is the wrong thing to take. We will probably have to wait until some one has a pair of identical machines to test by the " circulating energy " method, which we appear to agree on, and on those same machines have the short-circuited test applied, and find the case where the correction as determined by the short-circuited loss is going to appear absurd when compared with the actual stray load loss, and then we will have some direct evidence.

As stated before, I regret that the particular machines that were available for use in Foster and Knowlton's tests all proved to have small short-circuited losses, as polyphase machines, but does it not seem significant that one of these has very large short-circuit losses as a single-phase machine, and that these losses agree very closely with the load losses as determined? There have been a number of cases in the past with which I have had to do, of single-phase machines, where changes were made after the first tests, in order to reduce temperatures, in connection with which the short-circuit losses were reduced.

Therefore, I consider that the evidence presented by the curves, Figs. 9, 10 and 11, of the paper by Foster and Knowlton, should be taken up and carefully considered by the Standards Committee, and something in the way of positive evidence to condemn them should be obtained before they are rejected entirely.

James Burke: The papers under discussion and the corrective factors introduced should not, perhaps, apply only to the machines, but also to the instruments and to the men, and I think that these tests show that instrument designing is in the same high class as dynamo and motor designing, they show such close agreement with what might be expected from dynamo and motor tests.

It seems to me that the function of the Standards Committee is to standardize and not to average. The suggestions throughout these papers for corrective factors is to average these factors, and that eliminates the individuality of the designing. Now, it is quite possible, and I think most designers will agree on this point, to have negative corrective factors. For example, if we take a direct-current generator and shape the pole pieces with varying air gap so that the distribution of flux is approximately a sine wave under load, which would be quite different free, then our load core losses are less than our free core losses. That is not only a theory, but I have proved that by tests. I know that some European machines have been built that way, in which the air gap is so proportioned that the influence of the armature reaction is such as to produce very nearly a sine wave under load. Now, if a designer introduces such a feature as that, that is a negative corrective factor, he gets no credit for it, and his individuality is removed as soon as the Standards Committee averages these factors. The question of compensated machines, the difference between the 60 per cent compensated machine, which Mr. Erben has referred to, and the 100 per cent compensated machine, would have to be taken into consideration by the Standards Committee, or the punishment due to the average law would apply. In the compensated machine we may have large corrective factors, depending on the distribution of the compensating conductors. If you have two or three compensating conductors per pole, you probably will have quite a different increase in loss under load than when you have a larger number.

To do any good, to get any reasonable consideration for corrective factors, the Standards Committee would have to tabulate on the alternating-current end of motor-generators a great many construction details, whether the slots are closed, or open, or partly closed, the depth of conductors, many factors of that kind, to determine the corrective factors for the alternating-current end, and similar treatment on the direct-current end. Then you would have to introduce factors covering the way that the brush losses were determined—I think we have heard this afternoon that there are a good many different theories on brush losses and different opinions as to the amount of brush losses. Now when it is all finished, all that we have accomplished is the recognition that there may be, and it is to be expected that there will be, some corrective factors on account of loads, but I do not think we have any idea as to how much they should be. Personally, I have conducted a number of tests in which I have found so little corrective factor that I could not say I had found any, and in every case there has been a large difference. I am making a plea to avoid the averaging of these corrective factors and to leave some room for individual effort in telling the story of the individuality of different types of machines.

Charles P. Steinmetz: I can see a hard time ahead for Mr. Lamme and myself as members of the Sub-Committee on Revision when we attempt to designate the exact methods of determining the stray load losses so as to give the operating engineer the exact and true efficiency which he wishes to have, and which the input-output method, as I understand, is incapable of supplying, and which you cannot get by considering the short-circuited core loss, or open-circuited core loss, or any other thing, except by using correction factors, and as many different correction factors as you build machines. Our only hope is that we shall be assisted by the engineers who are now interested in the stray losses, by constructive criticism; that is, when we have drafted a tentative rule we shall submit it to them, and in case of their disagreeing with us in our first attempt, we hope they will send us material for the formulation of a better rule.

In regard to the short-circuit core loss, as shown in the diagram which Mr. Behrend discussed, I wish to say that $\frac{1}{3}$ of the short-circuit core loss was not a mere incident, and while at present the data seem to show that the total short-circuit loss is more correct, we must realize that when the rule was made we had different machines to deal with. The single-phase low-speed alternator was a very common machine then, also other types of machines which we rarely see now, such as the inductor alternator, where the field exciting winding was far away from the armature, separated by solid magnetic material. In these machines the pulsation of single-phase armature reaction reached back throughout the entire field when you ran at the low densities corresponding to the short-circuited core loss; while at the rated voltage, especially in a highly saturated machine such as the inductor alternator, the pulsation due to the single-phase armature reaction is very much less, and we knew, if we used the full short-circuit core loss, there could not be a loss of any such magnitude because the machine would have burnt out in less than no time, and therefore, especially since people were rather horrified when we made the first test of that character, and found these apparently enormous losses, we had to play safe and use a value which we knew was less than the probable true value. However, we were proceeding in the right direction to the extent that instead of neglecting the short-circuit loss altogether, we at least took in a part of it.

Now, the short-circuit loss undoubtedly gives some indication. The residual loss after deducting the armature I^2R loss and the iron loss corresponding to the approximate magnetic density under short-circuit tests, affords an excellent criterion as to the extent of the stray losses at full load. Even if in some cases it leads to ascribing to the machine an efficiency lower than is actually obtained, nevertheless the general adoption of the plan of taking this residual short-circuit loss as a measure of the stray loss at full load, would provide designers with a criterion which would lead to designs of improved quality.

E. M. Olin: Although Mr. Robinson, in drawing his conclusions, in his paper, states that the input-output method of measuring efficiency is " one that can be considered only in connection with special investigations " and " as a check method for demonstrating the reliability of other methods better suited to commercial needs," it is a fact, I think, that a reading of this paper would lead the average engineer, inexperienced in this class of work, to believe that this method is perfectly simple and thoroughly reliable for measuring the efficiency of rotating electric machines. Such a conclusion is so at variance with the experiences of the writer and a number of engineers with whom he has been associated, extending over a number of years, that it seems advisable to point out a few contributing factors not taken into account by Mr. Robinson.

In propounding the mathematical theory which deals with " indeterminate errors " and the average deviation of the mean reading from the true value, for the purpose of illustration, use is often made of an example like the following: A skilled marksman fires a thousand shots at a target under conditions as nearly alike as possible. Experience shows that the shots will be distributed in a manner which at first sight seems entirely irregular, but which, on more careful examination, will be found to be approximately in conformity with a perfectly definite law, the law of chance. The plus and minus deviations of the shots from lines drawn at right angles through the center of the target are about equally frequent, and small deviations occur with much greater frequency than large ones.

It can be shown mathematically that there is a certain definite curve, representing the law of chance, whose equation can be expressed in symbols, and that the average deviation of the mean is as shown by Mr. Robinson.

Now, in this example of the marksman firing at a target which I have just cited, it should be borne in mind that the target is fixed, not swinging.

In input-output tests the mark at which we are firing is the true value of efficiency. However, the true value of efficiency is the ratio of the output as determined from measuring instruments in the output side to the input as determined from certain other measuring instruments in the input side. In other words, during our observations we have two targets and two marksmen or sets of marksmen.

Now if the output is constant for all readings, the target remains fixed, and it would be correct to figure the input and output separately according to Mr. Robinson's method but not the efficiency direct from each observation or set of observations.

In the tests described, however, the output is not constant. In Table II, the output ranges from 470 kw. to 512 kw., a variation of some 40 kw. The target at which the marksmen are firing, then, instead of being fixed, is swinging.

Now the instruments used in the incoming circuits of motor-generators, such as are covered by Mr. Robinson in these data, do not swing synchronously with those in the outgoing circuits, due to the different types of damping used in the various instruments, the introduction of series transformers, etc. This introduces another complication in figuring tests such as described in this paper. The target which should be fixed is not only swinging, but swinging along an irregular curve with a varying time constant. Obviously, the problem is not quite so simple as one might be led to believe from glancing over this paper. It is undoubtedly true, as stated by Mr. Robinson, that certain of the no-load losses may change during the progress of the input-output tests. These should be measured carefully by the separate loss method and due allowance made, when computing efficiency, for such changes, and average values should be used for such factors as frictional losses in brushes and bearings and brush contact losses.

L. T. Robinson: I am willing to admit the extreme difficulty of satisfactory input-output testing, but not its impossibility. Mr. Newbury said that the test results obtained were not right because they departed from well-known laws. We have been all day yesterday and today discussing these well-known laws and there appears to be no definite understanding of them on which definite criticism of the results obtained could be based.

With regard to the shooting at the swinging target, I cannot see that this has any bearing, so long as the individual observations of the output are not far enough apart, so that the efficiency line that joins them does not depart substantially from a straight line. I do not recommend these methods for practical tests. I think that an impartial examination of the evidence presented will indicate that the results are correct within the limits stated. If you cannot look at it that with the input-output test we prove that the circulating loss tests are right, perhaps you will look at it the other way, and say that the circulating loss tests prove the input-output tests are right, because we got the same answer by both methods. After a full appreciation of the variability of the brush friction, etc., came to my notice, we made some more tests, and every point comes within the degree of precision claimed, and beyond which I did not care to go.

As to the No. 3 test in the paper, I think I made it quite clear that that was not very good, and I think the reasons given were quite sufficient. In a machine in which the brush friction is about one to 1.5 per cent of the total output, and with the statements this afternoon, by presumably competent observers, that brush friction varies 300 per cent, why is it any fault of the method or the test if the points do not fall all within the belt? I submit that the evidence is sufficient to establish all the claims made.

C. J. Fechheimer: In the paper by Messrs. Foster and Knowlton, on the last page they state " The phase characteristic method is faulty, due to the difficulty of obtaining accurate wattmeter readings at the low power factors." If we have a three-phase circuit and use single-phase wattmeters and the power factor is less than 20 per cent, both wattmeters will indicate high values, although one wattmeter will read negative. The true power is the difference between the two wattmeter readings. Both wattmeters read with considerable acccuracy and the only inherent errors which are introduced are those due to observation, and the difficulty in obtaining with accuracy the difference between two nearly equal quantities. However, the error that we usually think of in connection with single-phase wattmeters when connected to low power factor circuits does not apply to the three-phase case with the standard two single-phase wattmeter method, because a single-phase wattmeter reads inaccurately when the current and electromotive force are nearly in quadrature, and this condition obtains when the power factor of the three-phase circuit is not far from 50 per cent.

It is our opinion that the phase characteristic method is the most accurate one for measuring the losses within the machine, with possibly the exception of the circulating energy method. This latter, however, can be used so seldom that we must resort to some other means, and usually the phase characteristic method can more frequently be employed, with the equipments in most test departments in manufacturing companies.

In regard to the modified segregated loss method, we believe that all of the short-circuit losses should be included instead of one-third of the short-circuit losses, and that the core loss should be taken at the internal voltage as given by the terminal voltage plus the impedance drop taken in the proper phase relations. The reactance of the armature winding may be measured with the rotor removed, the current being circulated at normal frequency in only one of the phases. The reactance thus determined may then be used for computing the internal voltage.

B. A. Behrend: In the papers that are before us the difficulty of measuring the power in watts or kilowatts has never been discussed. This is rather surprising. Mr. Robinson will appreciate the immense difficulty of obtaining correct wattmeter readings. I remember in the early days it used to be one of the most difficult problems, and the new instruments have by no means solved it. Even the best instruments for the purpose are influenced by the proximity of large currents circulating in cables and by magnetic fields which exist almost everywhere in the shops. Let us take a simple case, and endeavor to determine the power factor of an induction motor at different loads. I have had experience in that direction, and I have had working with me some of the learned professors of our great universities, and have found it necessary to correct power factors of 101 and 102 per cent, to harmonize them with reasonable facts. It is so important a matter that I believe it should not be omitted in our consideration of this complex subject.

I want to say that in a number of cases of high-tension circuits, I have found that the power factor of an induction motor was extremely low, due to the peculiar condenser effects in the shunt windings of the wattmeters. It is an effect which it is very hard to eliminate and very hard to correct. At low power factors this effect is tremendous. Of course, it can be corrected in the well-known academic way, if we care to pile assumption on assumption. This is a matter of importance. Mr. Robinson might give us his views of the accuracy of wattmeters and their use in alternating-current circuits in power houses and on the testing floor. I have personally no faith in them, and I am just as willing to take the no-load current of the induction motor and use the simple circle diagram developed by me in 1896, when stimulated by the difficulty of harmonizing the results of wattmeter measurements with the likely facts, in order to get an idea of the power factor.

L. T. Robinson: You may be interested in my views on what accuracy we can obtain with wattmeters in commercial testing. First we will take the wattmeter as it is, and then we will see how much influence the testing floor has on it. We can build wattmeters, and they are constructed and obtainable commercially, that will measure, with power factors down to 10 per cent, within about 2 per cent of the correct value. That 2 per cent is largely correctable. This refers, of course, to single-phase operation. But you can, I think, get the best results when you want a really close measurement on three-phase circuits at low power factor, by using three instruments, so that you do not have the trouble that you have in subtracting one reading from another, which, of course, when they are pretty near alike, is bound to introduce large errors. You can choose instruments that will go over the scale, perhaps with one-third of the product of the rated volts and amperes of the circuits, and use three, and get very good results. When you introduce larger quantities for volts and amperes you must, of necessity, employ instrument transformers. Now, the phase angle between the secondary current and the primary current in the current transformers is determined within, perhaps, three to five minutes of the equivalent angle, which again brings you substantially within the limits spoken of for the wattmeter alone. The total magnitude of that phase difference can be kept small enough so that correction is reliable.

With regard to the potential transformer, it is possible, practically, in ordinary work, to eliminate them altogether, because the usual transformer is so constructed that, with non-inductive load, at certain loads the phase displacement between primary and secondary voltages is zero, and you have only to find out what that load is and put it on in addition to the wattmeter potential coil to bring the angle to zero, and use it that way. How far that can be applied to the testing floor, I do not know. You have things to deal with there that are difficult to handle.

The difficulty is not great if you can retain the people permanently who have been shown how to do the work. I will admit the trouble in doing such work commercially, but when the real necessity for it arises, I do not think the instruments or the method will be found wanting.

B. A. Behrend: The disturbing effect in the neighborhood of the wattmeter is because the wattmeter is so apt to be influenced.

L. T. Robinson: I feel sure that the instruments to which I refer are entirely free from those disturbing effects.

Carl J. Fechheimer: I always understood that the wattmeter was incorrect, and the current in the measured coil is almost in quadrature with the current in the corresponding coil.

That only occurs at 50 per cent power factor, or approximately 50 per cent power factor.

L. T. Robinson: It does not occur at 50 per cent power factor when you use three instruments. The power factor on each instrument is the power factor of the whole circuit.

Carl J. Fechheimer: Will you have correct results on your two instruments when one wattmeter reads a negative value and the other reads a positive value?

L. T. Robinson: Incorrect, in comparison with the results obtained with three instruments.

Carl J. Fechheimer: Why?

L. T. Robinson: You are substracting one thing from another thing, to which it is very nearly equal, and a very small inaccuracy in the wattmeter becomes a large part of the difference which is shown.

Carl J. Fechheimer: Each wattmeter will read fairly correctly?

L. T. Robinson: Yes.

C. A. Adams: I will answer one part of Mr. Fechheimer's question. In the case of the two wattmeters three-phase power measurement with nearly 50 per cent power factor, where one wattmeter reads zero, or very nearly zero, the per cent error of the low reading wattmeter may be very large but the actual numerical error of the total measurement is exceedingly small, and quite as small as when both wattmetters read high.

James Burke: If I may be permitted to make a suggestion to the Standards Committee, it is that in preparing new rules, they should not overlook the introduction of the necessary rules for the correction of instruments when efficiency tests are made by input-output methods. We have heard quite a little on that this evening, and I think it is very important, since input-output methods do come up occasionally, that the Standards Committee tell us how to introduce corrections on wattmeters and on various other instruments that come into use, and also to correct for the nature of the voltage supplied, whether pure sine wave or a wave that has some other complications in it. I think that is going to be quite important, because, otherwise, input-output methods will be relied upon, without their proper correction factors, and it may be that the correction factors for input-output methods would be as large as on the summation of loss methods, and it is equally important not to overlook them.

B. A. Behrend: Mr. Burke's remarks should be endorsed emphatically and heartily by all of us. We must know how to correct instruments. I have never been able to get accurate readings in a testing department because of the effect of large magnetic fields around the instrument. You cannot shut down a large manufacturing plant, employing twenty thousand men, for the sake of making one test. Night testing and Sunday testing are equally unsatisfactory for other reasons. I want to know if wattmeters can be obtained commercially which are not readily influenced by outside currents and powerful magnetic fields.

L. T. Robinson: Yes.

J. L. Harper: We have heard a great deal of discussion as to corrective factors, and my remarks this afternoon have been referred to by Mr. Newbury as not being pertinent to the discussion, but as I understand it, the necessity for determining all these losses is for the purpose of arriving at efficiency. I do not wish to have it considered that any remarks of mine had reference to any special method of test, or to any test; I merely desired to protect true efficiency from the corruption of a commercial understanding of efficiency; and instead of applying so many corrective factors to the losses, why not apply one corrective factor to the efficiency?

Mr. Schüler spoke of using the word " conventional " efficiency in Germany—why not use " conventional " or " commercial " efficiency for what manufacturers are able to determine as efficiency? You all seem to agree that you can determine certain of these losses with fair accuracy. Why not decide upon these losses and set them apart and let them be determined, and call that a certain kind of efficiency, designate it by some kind of qualifying term that will differentiate it from the true efficiency, and eliminate the necessity for the use of these thousands of corrective factors? That also may assist Dr. Steinmetz in solving that very difficult problem of satisfying the operating engineer in getting the true efficiency, which you all seem to claim is a physical impossibility. That may be so at the present time, but as time goes on the distance that separates the losses that you are able to determine by the segregated loss method, from all losses, I think will decrease, and there will come a time when further characteristic losses will be added to the determinable ones, and the difference between true efficiency and " commercial " or " conventional " efficiency may be decreased. The present rules I believe to be inconsistent, in that they give a fairly clear definition of efficiency, and then say that efficiency may be determined by the measurement of the losses separately.

You have made it very clear to me that it is a physical impossibility to determine these losses separately. Therefore, this efficiency which the rule purports to approve, is not a true efficiency, but some qualifying efficiency which you may call " conventional " or " commercial."

I merely wish to make the suggestion that by incorporating this one corrective factor, (which may be $\pm X$) in the efficiency, you make unnecessary all of these thousands of other corrective factors, and thus simplify both the matters before you in discussing the losses, and also the work of the Standards Committee in endeavoring to satisfy all parties.

E. I. Chute: It is very true, as Mr. Behrend and Mr. Robbins brought out, that if we try to make constants to account for the additional load losses for all types and sizes of apparatus we will have constants as numerous as the sands of the sea. But the case is not as bad as that. There are not very many operating or consulting engineers complaining about the smaller types of apparatus; it is the larger types, where one per cent in efficiency counts in dollars and cents. In these the constants can be simmered down to comparatively few in number, as will be noted in the summary to the paper by Messrs. Olin and Henderson. It must not be thought that these constants are absolute, nor do they need to be. No method of test now in use, even with the help of the laws of chance and Mr. Robinson's shaded belt, will assure us of the true efficiency of a machine, within two or three tenths of one per cent. The additional load loss, being small in proportion to the total losses of any machine, may vary materially, often even 25 to 50 per cent, without affecting the actual efficiency more than a quarter of one per cent, so, even though the additional load losses may vary slightly in the various types of the larger machines, a large number of them may be bunched together under one classification. An average constant for such a classification, applied as suggested, will then give operating efficiencies as accurate as the average engineer demands.

The average input-output test is not acceptable to either the manufacturer or operator, and some other solution of the problem of determining true efficiencies within reasonable limits must be worked out. The sooner we make a start at it the better.

Mr. Robinson's paper serves to bring out very nicely the limitations of the input-output test, even when taken under the best conditions and the calculations made with all the refinements of mathematics. On one curve shown by a previous speaker, two values of efficiency are given at normal load, the difference apparently being due to the exchanging of one instrument for another of similar capacity and accuracy. It was noted that there was a difference between these values of four tenths of one per cent, a greater difference than accounted for in either test by any possible change in constant losses. Why might not this discrepancy have been in just the opposite direction to that shown?

The values of additional load loss obtained in all the tables but IX, X and XI speak for themselves. If these values are correct, why all this agitation? The separate loss method of efficiency determination is good enough. If such results with the input-output method of test are obtained by Mr. Robinson with his reputation for refinement and accuracy, what can the rest of us hope for?

L. T. Robinson: There should be no misunderstanding on that point. The only claim I make is that these points will fall within that belt. And they do. The instruments were interchanged, at one point, and there is no conclusion to be drawn from the result except that the results are within one-quarter of one per cent, because the belt is only that wide and the points fell within the belt.

E. I. Chute: Other unaccounted-for inconsistencies in these tests might be pointed out, and it is my personal opinion that an accuracy of two tenths per cent claimed for them is not warranted. It only goes to show that even when the most elaborate and refined of methods are applied to the input-output tests, the results are very uncertain.

H. M. Hobart: No mention has yet been made of the best method of determining the true efficiency, the method which the Standards Committee would probably be well advised to adopt, and that is a method based on calorimetry. For instance, one can measure the losses at no-load with exactness, while a given quantity of air is being passed through the machine. The temperature of the air at the inlet and at the outlet can be taken. Again the machine can be run at full load and the same quantity of air measured at the inlet and outlet. This gives a simple way of deducing quite exactly the ratio of the losses at full load to the losses at no-load, and consequently of arriving at the magnitude of these stray losses, and consequently, also, of deducing the true efficiency. I believe that by these means we may arrive much more closely at the true efficiency than by any other practical method.

I allude first to large machines, because generally the conditions surrounding the contract for large machines are the more important, but in my opinion, for smaller machines calorimetry also offers the best method. In the case of a small machine, you can house it completely. You can house the motor in a case, through which you pump air, measuring the quantity of air, its inlet temperature and its outlet temperature. The measurement is made first with load. Then the measurement can be repeated at no-load with the addition of suitably disposed rheostats, into which you can introduce an additional loss, which will give you the same inlet and outlet temperature when your machine is running unloaded as was obtained in the first test when running it at its rated load. The amount of the losses in these rheostats will be equal to the load losses, including therein the legitimate and the illegitimate load losses.

Another way is to have your machine in one receptacle and then let the air pass on to still another receptacle in which you have your rheostat, and get the temperature drop across each. It is a thermal-drop process. When the thermal drop across each receptacle is equal, then you know that the loss in the machine is equal to the loss in the rheostat. If the drop is not equal, you can get the loss by taking the ratio of the thermal drops. There is no need to measure the *quantity* of air, but merely to maintain the flow constant.

W. F. Dawson: The method proposed by Mr. Hobart is interesting and replete with possibilities, but it requires much study and care so that new sources of error are not introduced, greater than those contended with previously. The difficulties of successfully measuring the actual amount of air delivered, and charting it so as to get a true average, are very great. I have been recently checking up losses with the thermal capacity of the air passing. There is probably additional loss, by heat convection, that will not be measured by thermal capacity of the air passing through. In respect to turbo-alternators, and particularly those which are provided with definite air passages for inlet and outlet, it will be exceedingly instructive to make these tests and keep them for comparison and reference. I doubt very much if the consulting engineers and the customer will very rapidly embrace the facilities which Mr. Hobart has offered in respect to determining these load losses. Mr. Erben has shown splendidly how these load losses are affected by design. It would be a great mistake and an injustice for the committee to lay down any set of rules and say that " this class of machine shall be considered to have ¼ of 1 per cent additional loss " and " this class of machine shall be considered to have ½ of 1 per cent additional loss," etc., because it will handicap individual initiative. It will put the designer and the manufacturer of inferior machinery, to a certain extent, on the same basis as the designers and manufacturers of machines who have practically eliminated these load losses. I hope the discussions and investigations will go on. There cannot be any question but that this discussion will make for better designs of machines, smaller load losses, or a considerable elimination of them. Mr. Lamme pointed out, and Mr. Erben and Mr. Burke, and others, have also pointed out, that while we may indicate what these losses are, it is going to be an exceedingly difficult thing to standardize them.

H. M. Hobart: I do not propose to measure the quantity of air. That is difficult. We do not measure the quantity of current when we get the drop through two resistances. We keep the current constant. The air flow is kept constant by running the fan or blower at constant speed. There is nothing new in calorimetric methods.

B. G. Lamme: I have listened with a great deal of interest to what Mr. Harper has had to say on the question of load losses. He says that if we cannot measure all the losses, and will state what we can measure, and if we are willing to state the situation plainly, he is willing to accept it. If everybody else would agree with him in this, it would make it very easy for the Revision Committee. Mr. Harper called attention to putting a factor x in the equation. However, it has been suggested by others that such a correction factor puts a penalty on good design. That is true, but you must not forget that virtue is its own reward. The designer who builds with very small extra losses has a correspondingly better machine, because any reduction in such losses reduces the temperature, and therefore allows him correspondingly to increase the output. The designer who builds with high extra losses will eventually lose out, so that the normal tendency is toward the reduction of such extra losses.

Mr. Harper also stated that eventually we would come closer to the true efficiency. I agree with him, but possibly it will not be so much by improved means of measuring as by reduction in the extra losses. I know that, right now, the tendency toward reduction of the extra losses is very strong, and that all losses in the machines are being analyzed and studied very carefully with a view to getting better performance and larger outputs by eliminating all unnecessary or useless conditions.

A paper presented at the Midwinter Convention of the American Institute of Electrical Engineers, New York, February 28, 1913.

COMPARISON OF METHODS OF LOADING LARGE A-C. AND D-C. GENERATORS AND SYNCHRONOUS CONVERTERS FOR FACTORY TEMPERATURE TESTS

BY F. D. NEWBURY

Object

It is obviously impossible to test the larger machines under energy load at the factory. The advisability of making adequate tests before shipment, to eliminate as completely as possible the greater expense and delay of possible constructional changes after shipment, is generally recognized by consulting engineers and manufacturers. These facts make the question of compromise temperature tests well worth the consideration of the Insitute in connection with the work of the Standards Committee. The author believes the Standards Committee should recommend methods of loading for such factory temperature tests for the guidance of consulting engineers and others responsible for the acceptance of machines, and for the protection of manufacturers against unreasonable demands in connection with such tests. This paper has been prepared with the object of bringing before the Institute data on the relative merits of the various methods of loading which have been suggested and found practicable, and of making recommendations for its consideration.

Requirements of a Satisfactory Test

Any adequate test should duplicate as completely as is possible the conditions under which the machine is guaranteed to operate. In addition to this obvious requirement, the factory test should be *more severe* and not *less severe* than actual operating conditions, and the test conditions of current, voltage, etc., should correspond closely with actual load conditions, so that the comparison between the two can be made directly and without the aid of experimental data.

Description of Methods of Loading A-C. Generators

The following methods of loading have been used more or less extensively and will be considered in this paper:

a. Separate open-circuit and short-circuit tests.

b. Alternate open-circuit and short-circuit tests.

c Direct-current open-delta test.

d. Zero power factor test.

a. *Separate Open-Circuit and Short-Circuit Tests.* The generator is first tested with the armature winding on open circuit and the field winding excited to give a higher armature voltage and core loss than normal in order to compensate for the absence of the armature winding loss. Then a second test is made during which the armature winding is short-circuited through ammeters and the field excited sufficiently to circulate more than normal full load current. In the open-circuit test, the armature core is the only part subjected to conditions comparable to actual load. The field winding is subjected to less than the maximum operating field current, but the temperature rise is approximately proportional to the loss, so that the temperature rise and exciting voltage can be corrected to maximum load conditions. The temperature rise of the armature winding, being on open circuit, is no guide whatever to the temperature rise under load. In the short-circuit test, the armature winding is the only part subjected to conditions comparable to actual load. The field-winding loss and core loss are altogether too small to give rise to temperatures that would be any guide to actual load temperatures. The open-circuit test, therefore, is of value in predicting armature core and field winding temperature rises and the short-circuit test is of value in predicting armature winding temperature rises.

The open-circuit and the short-circuit tests have the advantage of simplicity in manipulation and in test equipment. No equipment, other than a relatively small driving motor and measuring instruments, is required. For this reason, it is sometimes the only possible method that can be carried out under factory conditions.

The important disadvantage of this method is the difficulty of properly interpreting the results. This difficulty arises from the fact that the core loss and armature winding loss are not present in the same test, so that the combined effect of the two losses on the core and winding temperatures must be approximated from the separate test results. Experience has shown that in the large majority of machines the normal load core temperature will be lower than the open-circuit core temperature when the open-circuit voltage is approximately 10 per cent higher than normal, and that the normal load armature winding temperature will be lower than the short-circuit test results when the short-circuit current is approximately 25 per cent higher than normal.

These figures can be considered only as rough approximations. It is obvious that the combined effect of the core and copper losses on the temperature of either depends on the relative amount of the two losses, the amount and kind of insulation and the relative temperatures of the core and copper. In high-speed generators, such as steam turbine-driven generators where the core loss is normally three to four times the armature copper loss, the relation between the temperatures resulting from open-circuit short-circuit tests and actual operation will be very different from that in low-speed generators where the core loss is often less than the armature copper loss. In the former class of generators, the core temperature can be predicted with reasonable accuracy from an open-circuit test alone, since the smaller copper loss will have little additional effect, but in the latter case the results from both open-circuit and short-circuit tests must be given serious consideration. The insulation affects the interchange of heat between the core and coils, and for this reason the insulation must be given consideration. Usually, high-voltage machines with their greater thickness of insulation will interchange heat less readily, and a smaller increase in current than 25 per cent for the short-circuit test will give approximately the same results as actual load. The interchange of heat between coils and core will vary in direction, depending on which part has the higher temperature. At normal load, for which the coil temperature will not infrequently be lower than the core temperature, heat will flow from the core to the coils. At overloads, for which the coil temperature will generally be the higher, the flow of heat will be in the opposite direction. In machines such as turbo-generators, in which the problem of ventilation is mainly centered in the core, the flow of heat for all loads may be from the core to the coils.

While these various conditions complicate the interpretation of short-circuit and open-circuit test results, they are subject to simple laws, and the existence of these conditions is not a serious objection to this method of loading, but the results from short-circuit and open-circuit tests are very considerably affected by the efficacy of the ventilation provided in the machine under test, and the correction necessary for this condition cannot be easily nor infallibly applied. In general, in machines in which the core loss is larger than the copper loss the temperature of the core should be relatively low at the end of the short-circuit test. If contrary results are obtained, there is usually some fault in

TABLE I

OPEN-CIRCUIT, SHORT-CIRCUIT AND ACTUAL LOAD TESTS ON 3000-KV-A. 2200-VOLT, THREE-PHASE, 60-CYCLE, 225-REV. PER MIN. WATER WHEEL GENERATORS—DEFECTIVE CORE VENTILATION

	Open circuit	Short circuit	Actual load	Actual load
Length of test in hours	7	8.5	20	12
Volts	2510	0	2280	2520
Amperes per phase	0	985	780	760
Per cent power factor	—	—	98	90.7
Field amperes	161	65.5	157	178
Field volts on rings	164	57.2		
Temperature rises:				
Stator core	32.5	30	51	53.7
Stator coils	10.5	30	27	20.7
Rotor coils	34.5	12.5	35	46.7
Air temperature	20.5	18	26	39.8

Field coil resistance at 25 deg. cent. = 0.818 ohms.
Armature coil resistance per phase at 25 deg. cent. = 0.016 ohms.
Core loss at 2200 volts = 48 kw.
Core loss at 2510 volts = 63 kw.

the ventilation of the armature core, but this is by no means a complete test for proper ventilation, and the dependence of the proper interpretation of the test results upon ventilation constitutes a serious disadvantage of this method of loading.

In Tables I and II, results from short-circuit and open-circuit temperature tests and tests under actual energy load are given, which illustrate the discrepancies which may occur due to defective ventilation. In these 3000-kv-a. generators, it will be noted that the temperature rise in the core on open circuit was 32.5 deg., and the short-circuit test showed a temperature rise of 30 deg. in both armature winding and core. The tests under normal load showed a temperature rise considerably above the open-circuit core temperature and an armature coil temperature somewhat below the short-circuit temperature rise. The excessive core temperature was found on investigation to be due to insufficient armature core ventilation, and the results after this defect was corrected (Table II) showed substantial agreement between the results of the open-circuit short-circuit tests and actual energy load. The discrepancy between the core temperatures in the compromise test and in the actual load tests can be explained only on the assumption that on open circuit the low temperature of the armature winding caused a large part of the heat generated in the core to be conducted from the core to the external air through the armature coils, thereby maintaining the core at a fairly low

TABLE II

OPEN-CIRCUIT, SHORT-CIRCUIT AND ACTUAL LOAD TESTS ON SAME GENERATOR AS IN TABLE I AFTER VENTILATION WAS CORRECTED

	Open circuit	Short circuit	Actual load	D-c. open delta	
Length of test hours	7.5	10.5	10	8.5.	11
Volts	2520	0	2460	2200	2540
Amperes per phase	0	788	795	788	988
Per cent power factor	—	—	88.5	—	—
Field amperes	159	51.5	181	137	161
Field volts on rings	147.5	44	162	127.5	152
Temperature rises:					
Stator core	27	15.5	33.6	26.8	37.8
Stator coils	16	15.5	17.1	14.8	20.8
Rotor coils	28.5	6.5	33.1	19.3	25.8
Air temperatures	18	20	33	17.2	17.2

temperature. During the load tests, the temperature of the copper in the slots was probably not materially different from that of the core, so that this avenue of escape for the core heat was shut off, resulting in high core temperature.

The difference in actual temperature conditions on open circuit and under a compromise load which experience has shown to approximate actual load, is shown in Table III. These results were obtained by indications from thermocouples located in the center of the slots of a 3800-kv-a., 4000-volt, 60-cycle, three-phase 400 rev. per min. generator. These results show the great influence of the core losses on the coil temperatures, or, in other words, the important part the coils play in conducting heat from the interior of the core to the outer cooling air, particularly when the loss generated within the coils is negligible. The true conditions are entirely masked by the usual thermometer readings.

A minor disadvantage of the short-circuit and open-circuit method of test is the inability to test the field winding adequately for temperature and exciting voltage margin. The field current used in the open-circuit test is approximately equal to the full-load 100 per cent power factor field current. To test with a field current even approaching that required for the maximum load and low power factor for which the generator is usually designed would result in a prohibitive core loss and temperature. With this method of testing, the guarantees involving the field winding must be checked by calculation from the relatively low

TABLE III

OPEN-CIRCUIT AND ZERO POWER FACTOR TESTS WITH THERMOCOUPLE READINGS OF TEMPERATURE

	Open circuit 4000 volts 0 amperes	0 Per cent power factor 4000 volts 550 amp.	Open circuit 4800 volts 0 amperes	0 Per cent power factor 4800 volts 550 amp.
Core—tooth 1/2 in. below air gap	36	45	55.5	66
Core—1/2 in. below slot	35	44	53.5	62
Core—1/2 in. inside outer surface of core	32	38	48.5	55
Core—outer surface (by thermometer)	23	31	44	47
Max. armature coil temperature in center of core	28.5	40	45	60
Max. armature coil temperature on ends of coils, by thermometer	7	17	18	26.5

field current used in the test. This disadvantage applies not only to the field coil temperature but to the exciting voltage. It is of considerable advantage in a compromise test that the field excitation required should be a maximum. In that event, the exciting voltage shown by the test record will directly indicate the adequacy or inadequacy of the design in this respect.

b. *Alternate Open-Circuit and Short-Circuit Tests.* As has been pointed out, the difficulty in properly interpreting the results from the separate short-circuit and open-circuit tests would be eliminated if both core and copper losses were present in the same test, as in actual operation. To approximate more closely this condition, it has been suggested to test the generator under a succession of short cycles, each cycle consisting of a short-circuit and open-circuit test. The values of current and voltage during the short-circuit and open-circuit parts of the cycle are determined so that the average core loss during the cycle and the average copper loss during the cycle are equal respectively to the core loss and copper loss during actual load conditions. The average loss during the complete cycle and the loss during normal operation will be equal, and if the length of the cycle is sufficiently short the temperatures should be the same under the test conditions and actual load. The division of time between the open-circuit and short-circuit parts of the cycle may be arbitrarily made within fairly wide limits. If the lengths of time are very different, either core loss or copper loss will have to be abnormally increased to maintain the desired average, so it will be found desirable to divide the cycle into approximately equal parts. If the two losses are sufficiently near together in value, it is of advantage, theoretically at least, to divide the cycle between the two conditions in the ratio of each loss to the total losses. With the time so divided, not only will the average losses during a complete cycle equal the normal load losses during a nequal time, but the core loss during the open-circuit and the copper loss during the short-circuit part of the cycle will equal the sum of the core loss and copper loss during normal operation.

Since this test has only recently been suggested and is probably not as familiar as the other tests discussed, an example will be described in detail. The test described was made on a 6250-kv-a., 6600-volt, three-phase, 60-cycle, 240-rev. per min. waterwheel generator. The core and copper losses under actual load were approximated from separate loss measurements by taking the core loss from the open-circuit core-loss curve at the total induced voltage under normal load and normal voltage and the armature copper loss was taken for normal current from the short-circuit loss curve. This latter loss is greater than that actually existing under load since it includes not only the copper loss but the core loss, and to this extent is an unfavorable assumption. The corresponding favorable assumption would be the use of $I^2 R$ loss only. The numerical values for this generator are as follows:

Losses as determined from separate loss test.	
At 6640 volts (total induced) core loss =	93 kw.
At 547 amperes short-circuit kw. loss =	72 kw.
Total losses at normal load,	165 kw.

165 kw. open-circuit core-loss is obtained at 8200 volts and 280 field amperes.

165 kw. short-circuit loss is obtained at 875 armature amperes and 125 field amperes.

Dividing the time proportionally to the losses:

$$\text{Time on core loss} = \frac{93}{165} = 56 \text{ per cent.}$$

$$\text{Time on copper loss} = \frac{72}{165} = 44 \text{ per cent.}$$

Choosing a 30-minute cycle, the time on open circuit would be 17 minutes and the time on short circuit would be 13 minutes.

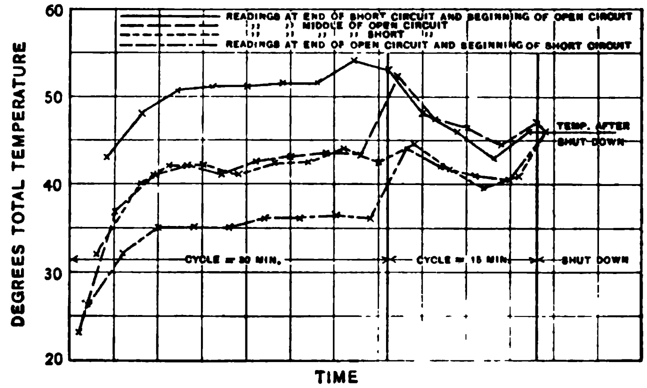


FIG. 1—ALTERNATE OPEN-CIRCUIT AND SHORT-CIRCUIT LOADING—TYPICAL ARMATURE COPPER TEMPERATURES

The value of the field copper loss in kw-hours will be dependent upon the values of open-circuit voltage and short-circuit current assumed. This loss must be compared with the loss under operating conditions and the results interpreted accordingly. In this example, the figures are:

$$\begin{aligned} \text{Field loss per hour} &= 280^2 \times 0.6 \times 0.56 = 26.5 \\ &\ 125^2 \times 0.6 \times 0.44 = \underline{\ 4.15\ } \\ &\ 30.65 \end{aligned}$$

The field loss per hour under normal load, 100 per cent power factor, is 19 kw., and at normal load, zero power factor, is 46.5 kw. Thermometer readings were made at the middle and end of each part of the cycle in order to follow the temperature changes. On a generator of this size, it is not desirable to change from open circuit to short circuit with the full excitation used in the test. The field current was, therefore, reduced to practically zero and brought up again to the short-circuit value in changing from the open-circuit to the short-circuit condition. Typical core and copper temperature logs are shown in the curves, Figs. 1 and 2, and the final temperature results are shown in Table IV.

It will be noted from these results that while the armature and field copper temperatures check fairly well with the results from other methods of loading, the core temperature is much too high.

Results from a similar test on a 150-kv-a., 2400-volt., 60-cycle, 900-rev. per min. belted generator, tested under conditions to

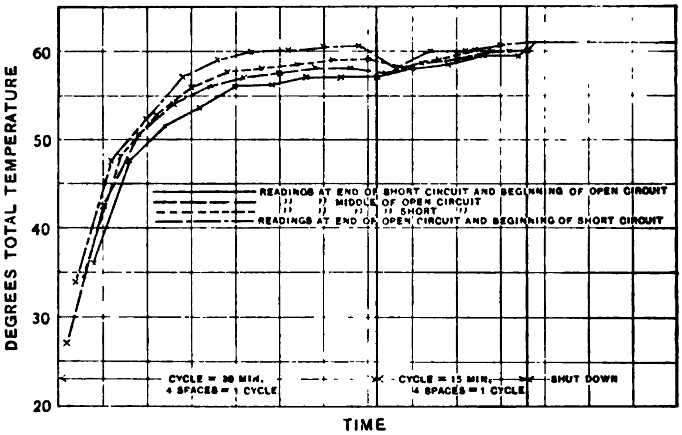


FIG. 2—ALTERNATE OPEN-CIRCUIT AND SHORT-CIRCUIT LOADING—TYPICAL ARMATURE CORE TEMPERATURES

correspond to its normal rating of 150 kv-a., and also under conditions corresponding to 200 kv-a., are shown in Table V.

It will be noted that the results from these cycle tests correspond fairly well with actual load and with zero power factor tests, except in the field copper temperatures.

The temperature logs, Figs. 1 and 2, show:

1. The variation in temperature is much greater in the armature copper than in the armature core, due to the smaller volume of the former and the consequent greater concentration of loss.

2. The longer the cycle the greater this variation. A fifteen-minute cycle appears to be a sufficiently short cycle for a large generator such as the one described.

The high core temperature obtained, in comparison with the core temperatures obtained by other methods of loading, indicates that the losses on open circuit should be reduced possibly to the value found by experience to be correct for the separate open-circuit test. This is the core loss for 110 per cent normal voltage, instead of 124 per cent normal voltage as used in the alternate open-circuit, short-circuit test described. But any such departure from a logical and simple basis to an empirical basis is a decided disadvantage in a standard method of test. While the correct core loss for the test of this generator could be found by comparative tests, the relation so found might or might not apply to another generator of different proportions, depending on all of the

TABLE IV

ALTERNATE OPEN-CIRCUIT AND SHORT-CIRCUIT TEST AND COMPARATIVE DIRECT-CURRENT, OPEN-DELTA AND ZERO PER CENT POWER FACTOR TESTS ON 6250-KV-A., 6600-VOLT, THREE-PHASE, 60-CYCLE, 240-REV. PER MIN. GENERATOR

All tests continued until constant temperatures attained.

	D-C. delta	0 per cent power factor	Cycle
Volts	6600	6600	0/8200
Amperes per phase	547	547	0/867
Per cent power factor		0	
Field amperes	159	278	127/305
Field volts	90.5	174.5	160/380
Rise, stator core	33.4	32.5	41.5
Stator copper, thermometer	26.4	26.5	30.5
Rotor copper, thermometer	17.4	40	33.5
Rotor core	17	14	33
Air temp. room	20.6	22.5	19.5

factors influencing the transmission of heat between the core and winding.

c. *Direct-Current Open-Delta Test.* This method of loading is still closer to the conditions existing under normal operation in that the generator is excited to correspond to normal voltage and the armature winding simultaneously carries current corresponding to normal load. The winding, if not already so connected, is connected in delta, in the case of three-phase winding, or in the equivalent closed winding in the case of two-phase windings, and one corner of the closed winding is opened and direct current is introduced and circulated through the complete winding. Since the alternating voltage is balanced at any corner of the closed winding, no alternating voltage is introduced in the direct-current circuit and a direct voltage equal to the resistance drop to the alternating-current winding is all that is required. The presence of the direct current in the armature winding results in a stationary field with respect to the armature, having a pole for each phase of each pole of the a-c. generator. With chorded armature windings, the value of this field is reduced, until, with a three-phase winding having a throw equal to two-thirds of the pitch, this direct-current field disappears. These statements can be easily checked by plotting the field form due to

TABLE V

COMPARATIVE TESTS ON 150-KV-A., 2400-VOLT, THREE-PHASE, 60-CYCLE, 900 REV. PER MIN. GENERATOR

All tests continued until constant temperatures attained.

	Actual load	Zero power factor	Cycle 1	Actual load	Zero power factor	Cycle 2
Volts	2400	2400	0/	2400	2400	0/3150
Amperes per phase	36.1	39.5	0/60.3	48	48.2	0/63
Per cent power factor	100	0		100	0	
Field amperes	17.3	25.7	14/53	19.4	28	15.8/31
Field volts	62.5	102.8	20/77	72	119.5	63/127
Rise stator core	24.5	24.5	23	27	30.5	31
Stator cop. thermometer	19.5	15.5	16	23.5	28	21
Rotor cop. thermometer	23	45	24	26	64	49.5
Rotor core	13.5	15.5	13	20	25	17.5
Air temperature, room	23.5	19	25	24.5	20	24.5

Cycle (1) Open circuit, 14 minutes.
Short circuit, 6 minutes.
Cycle during last hour of test reduced to 5 minutes.

Cycle (2) Open circuit, 16 minutes.
Short circuit, 14 minutes.
Cycle during last hour of test reduced to 15 minutes.

the distributed armature winding in the same manner as the field form of induction motors is commonly plotted. Since this direct-current field is stationary with respect to the armature, it generates voltages in any solid part of the field magnets, and since it has three times the number of poles of the generator, the losses due to the stationary field may be considerable if there is any considerable volume of solid metal in the field. In a 3000-kv-a., 225-rev. per. min. 60-cycle generator having laminated poles and solid end plates and with non-magnetic wedges between poles to retain the field coils, this additional loss in the rotating field magnets amounted to 7.6 kw. In a larger generator of similar construction, this loss amounted to 17 kw. Losses of this magnitude may easily increase the temperature rise of the field coils and so vitiate the test results. This method of test is, therefore, limited to generators in which the magnet poles are mainly laminated, unless the armature winding is considerably chorded. In any event, the additional power required to drive the generator under test should be observed, with and without the circulating direct current, in order to check the additional losses due to it, before reliance is placed upon test results.

The correct field current to use with this method of loading is very little more than the field current to give normal voltage on open circuit, and no appreciable error is introduced by the use of open-circuit normal voltage field current. Obviously, the,

TABLE VI

COMPARATIVE TEMPERATURE RISES—LEADING AND LAGGING 60 PER CENT POWER FACTOR TESTS

	Lagging	Leading
Length of test in hours	11.5	11.5
Volts	6300	6300
Amperes per phase	344	344
Per cent power factor	60	60
Field amperes	135	48
Field volts on rings	102.5	35
Rise, stator core	27.5	27
Stator copper, thermometer	14.5	14
Rotor copper. thermometer	14	5.5
Air temperature, room	21	21.5

excitation should be such as to duplicate full-load core loss as nearly as this can be duplicated without alternating current in the armature winding. This condition will be attained when the excitation is sufficient to produce the flux required for the induced voltage with normal terminal voltage and normal armature current. This flux is equal to the flux required to produce an open-circuit voltage equal to the vector sum of terminal voltage, the voltage drop due to armature resistance and the voltage drop due to self-induction of that part of the armature winding *outside of the core*. The larger field current actually required by load conditions (accounted for mainly by armature demagnetization) is neutralized by the armature load current and does not result in increased armature flux. That this value of field current is substantially correct is shown by numerous cases where the results of this method of loading have been compared with the results from zero power factor method and from actual energy load.

A further check on this same point is afforded by the comparison of test results made at zero power factor or at higher power factors leading and lagging. Such comparative results are shown in Tables VI and VII for a 2250-kw., 3750-kv-a., 6300-volt, three-phase, 50-cycle, 300-rev. per min., synchronous motor operating at 60 per cent power factor leading and lagging and a 6000-kv-a. generator operating at zero power factor leading and lagging.

It will be noted that in both cases the core temperatures are practically the same at the leading and lagging power factors, which indicates very little difference between actual load flux corresponding to the true induced voltage. The reason armature

TABLE VII

COMPARATIVE LEADING AND LAGGING ZERO POWER FACTOR TESTS 6000-KV-A., 400 REV. PER MIN. GENERATOR

Generator operated at normal load until constant temperatures attained, followed by 25 per cent overload for three hours

	Lagging	Leading
Volts	4000	4000
Amperes per phase	1083	1083
Per cent power factor	0	0
Field amperes	177	2
Field volts on rings	222	—
Rise, stator core	40.5	40 5
Stator copper, thermometer	41	37
Rotor copper, thermometer	28.5	6
Air temperature room	24.5	22

self-induction due to that part of the winding in the slots is not included is that this part of the self-induction, like the armature reaction, does not result in an increased flux and generated voltage but merely produces a flux which must be neutralized by the main flux. A further reason why differences in loading do not produce differences in temperature rise, as commonly measured, is that the differences in loss produced by loading occur mainly in the armature teeth where accurate thermometer temperature measurements cannot be made.

Special armature windings having only one coil per slot may introduce irregular direct-current wave forms which will have an effect on the field-core heating and temperature. Whether this is a serious matter can always be determined by plotting the field form or checking the losses due to the direct current.

To summarize, the test conditions of this method of loading are the same as those existing under actual load, except for the lower field winding loss and for whatever difference results from the use of direct current in the armature winding instead of alternating current. In general, the core temperatures will correspond very closely with those obtained from actual operation. The armature copper temperature will be slightly less than that obtained from actual operation and the field coil temperature and exciting voltage will be considerably less than that obtained from actual operation. The differences in field coil temperature and excitation are not a serious objection to the method of test, since both can be corrected to any desired condition of operation by proportional calculation.

d. *Zero Power Factor Method of Loading*. With this method of loading, the generator under test is operated in parallel with a generator of the same voltage and frequency and of, at least,

TABLE VIII

COMPARATIVE TESTS ON 12,500-KV-A., 6600-VOLT, THREE-PHASE, 50-CYCLE, 300 REV. PER MIN. VERTICAL SHAFT GENERATOR

	Zero power factor	D-C. delta
Volts	6600	6700
Amperes	877	877
Per cent power factor	0	—
Field amperes	281	170
Rise, stator core	35.5	36
Stator copper, thermometer	35	29.5
Rotor copper, thermometer	37.5	17

equal current capacity. The generator under test is over-excited and the generator in parallel with it under-excited until the desired armature current flows. The generator under test, therefore, is operating under normal voltage, normal current and a field current equal to or greater than the maximum value ever required under operating conditions. The test is, therefore, made under more severe conditions than actual full load at any operating power factor.

The one limitation in the application of this method of loading is the necessity for testing equipment equal in kilovolt-ampere capacity to the generator under test. When there are two duplicate generators being built, this limitation obviously does not apply, since one generator can be tested in parallel with the other. This duplicate generator will not always be available and in some cases, and even when it is, the expense for a duplicate test rig may be prohibitive. This is generally the case with large vertical generators where the thrust bearing is supplied as part of the waterwheel. A large number of comparative tests made at zero power factor and under actual load conditions shows the substantial agreement between the zero power factor and actual loading.

Various tests, from which comparisons between actual load, the direct-current circulating method of loading and the zero

TABLE IX

COMPARATIVE TESTS ON 5000-KV-A., 6600-VOLT, THREE-PHASE, 50-CYCLE, 300 REV. PER MIN. GENERATORS

	Actual load	D-C. delta	Actual load	D-C. delta
Volts	6000	6000	5910	6000
Amperes per phase	480	480	600	600
Per cent power factor	100		100	
Field amperes	128.5	118	136	118
Field volts on rings	145	125	155	125
Rise, stator core	34	35	43	42
Stator copper, thermometer	30	25	46	40
Rotor copper, thermometer	26	25 5	27	29

TABLE X

COMPARATIVE TESTS ON 1400-KV-A., 2300-VOLT, THREE-PHASE, 60-CYCLE, 200 REV. PER MIN. GENERATORS

	Open circuit	Short circuit	D-C. delta	0 per cent power factor
Volts	2550	0	2300	2300
Amperes per phase	0	440	352	352
Per cent power factor				0
Field amperes	106.5	53	97	139
Rise, stator core	20.5	10	22	24.5
Stator copper, thermometer	9	7	14.5	17
Rotor copper, thermometer	10	3	8	16.5

power factor method of loading can be made, are given in Tables VIII, IX and X. Comparative results from d-c. open delta and actual load tests are also given in Table II. From Table VIII, it will be noted that in the direct-current circulating test the field excitation was slightly above that required for open-circuit normal voltage and the core temperature was slightly above that obtained on the zero power factor test. This bears out the statement made in connection with the proper value of field current in the direct-current circulating test. It will also be noted that the armature winding temperature rise is somewhat lower on the direct-current test than on the zero power factor test.

CONCLUSIONS

Of the four methods of loading discussed, it is the author's opinion that the zero power factor method more nearly approaches the requirements of a satisfactory method of loading, as outlined in the early part of this paper. The conditions in this method of loading correspond almost exactly with conditions of actual operation, so that no question can arise as to the proper voltage, armature current and field current to use in the test, and whatever differences exist make the test more severe and not less severe than actual load. The tests also gives a direct indication of the field coil temperature and exciting voltage under maximum conditions of operation. If, on account of lack of equipment, this method of loading is not feasible, the author recommends that the direct-current circulating test and the separate short-circuit and open-circuit method be substituted, in the order given. The alternate short-circuit and open-circuit test is not recommended, on account of the wide divergence from actual operating conditions, making the question of correspondence between the conditions of test and conditions of operation entirely one of judgment and experience in which the two parties concerned may not agree. However, as experience with this method of loading is accumulated, it may prove preferable to the separate open-circuit and short-circuit method, as it does more nearly approach actual operating conditions than the separate tests.

METHODS OF LOADING LARGE D-C. GENERATORS AND SYNCHRONOUS CONVERTERS

There has been only one method of loading suggested or used which gives results of any value in comparison with the results from actual operation. This method is the well-known " loading back " method in which two machines of equal capacity are operated together, one as a generator and the other as a motor. The correspondence with actual operating conditions is exact as far as commutation and temperature on steady load are concerned. Where a second machine is not available, the field coil temperature can be checked by an open-circuit test. The core and copper temperatures on such a test are of very little value on account of the absence of the relatively large armature copper loss. The commutation, however, can be checked by operating at zero voltage or reduced voltage and normal current, in case the generator is provided with commutating poles. Such tests are advisable but do not afford any adequate test of temperatures.

It is recommended that, the loading-back method of test be used for d-c. generators and synchronous converters when the necessary apparatus is available at the factory and the generator is too large to test on a resistance load.

A paper presented at the Midwinter Convention of the American Institute of Electrical Engineers, New York, February 28, 1913.

COMPARISON OF METHODS OF MAKING LOAD TESTS ON A-C. GENERATORS AND ON INDUCTION MOTORS

BY E. F. COLLINS AND W. E. HOLCOMBE

The object of this paper is to outline briefly some of the most practical methods that have been employed for obtaining the normal running temperatures of alternators and induction motors under no-load or partial load conditions.

Nearly all the methods treated have been discussed in scientific papers at home and abroad and are known to most electrical engineers. It is our intention to speak of these methods only in a general way and to submit data showing comparative results obtained in regular commercial testing.

The chief reasons for testing by these no-load methods are in order that the cost of testing may be materially reduced, and because machines are being built in such numbers and of such capacity that the manufacturer cannot economically provide power supply and testing equipment for the actual loading of all machines.

I. ALTERNATORS

The methods for making heat runs on alternators, with which this paper deals, will be designated as follows:

1. Zero power factor method.
   - a. Leading current.
   - b. Intermittent leading and lagging current.
2. Open- and short-circuit method.
   - a. Continuous run.
   - b. Intermittent run.
3. Open delta method.
4. Phase displacement method.

1. Zero Power Factor Method

a. *Leading Current.* The machine to be tested is operated as a synchronous motor running free at normal voltage and normal leading current in the armature, obtained by over-exciting the field. As practically the whole armature reaction directly opposes the field, the field current is greater than under any other condition of normal armature voltage and current. The normal field heating can, however, be approximated by reducing the observed temperature rise in proportion to the respective field losses.

This method has given very good results and has been extensively used in testing turbo-alternators. The main error of the method is that the field heating will be greater than occurs when operating under full load.

b. *Intermittent Leading and Lagging Current.* The above method is modified by operating the machine alternately for short periods with leading and lagging currents in the armature. The two periods may be adjusted so that the average loss is equal to normal.

Let I_0 be the field current required for over-excitation, I_u the current required for under-excitation, I_n the normal field and X the per cent of a complete cycle (consisting of a period of over-excitation and a period of under-excitation) during which over-excitation is used, then

$$I_0^2 X + (1 - X) I_u^2 = I_n^2$$

or

$$X = \frac{I_n^2 - I_u^2}{I_0^2 - I_u^2}$$

In this method an attempt is made to compensate for the excessive field heating of the previous case by running the machine intermittently over-excited and under-excited, the time interval to be such as to give normal heating in the field. This method has been found to give excellent results.

The only practical objection to the zero power factor method is that it requires equivalent kv-a. supply, which is not always available.

Tables IA, IB and IC give data comparing results obtained from zero power factor tests with those from actual load on same machine.

TABLE I A

THREE-PHASE, 60 CYCLES, 2500 KV-A., 1800 REV. PER MIN., 2300 VOLTS

Maximum temperature rise	Full load	Zero power factor, leading
Armature laminations	35	35
" ducts	31	29
" coils	29	23
" " (res.)	34	27
Field coils (res.)	41	68
" " " reduced to normal	—	42

TABLE I B

*THREE-PHASE, 60 CYCLES, 715 KV-A., 720 REV. PER MIN., 2300 VOLTS

Maximum temperature rise	Full load	Zero power factor leading	Zero power factor Leading 7 min. Lagging 13 min.
Armature laminations	30	36	29
" ducts	23	36	28
" coils	30	35	26
" " (res.)	28	44	30
Field coils	26	54	27
" " reduced to normal	—	19	—
" " (res.)	38	69	38
" " " reduced to normal	—	24	—

TABLE I C

* THREE-PHASE, 60 CYCLES, 257 KV-A., 720 REV. PER MIN., 2530 VOLTS

Maximum temperature rise	Full load	Zero power factor leading	Zero power factor. Leading 3.5 min. Lagging 5.5. min.
Armature laminations	22	22	19
" ducts	20	23	18
" coils	16	20	15
" " (res.)	31	18	23
Field coils	32	77	32
" " reduced to normal	—	36	—
" " (res.)	42	102	44
" " " reduced to normal	—	48	—

* Location of thermometers was the same for these runs

2. Open- and Short-Circuit Method

a. *Continuous Run.* This method consists in running the machine a certain percentage above normal voltage on open circuit until temperatures are constant, then a certain percentage above normal current on short circuit until temperatures are constant. A maximum-rated generator is usually operated at

110 per cent normal volts and 110 per cent normal current, while higher percentages are used with generators that are designed to carry overloads. The core loss when operating above normal voltage is assumed to heat the armature core to approximately the same temperature as it would be heated when operating under normal voltage and current. The $I^2 R$ in the armature coils when running above normal current is assumed to heat the coils to approximately the same temperature as when operating at normal voltage and current.

On open circuit the voltage taken is assumed to be that which would correspond to a core loss equal to the combined armature losses under load. Hence, in the successful application of this method an approximate knowledge of the core loss curve and total armature losses under load is necessary.

Likewise, the short-circuit current is taken a certain percentage above normal. In some cases this current may cause a heating in the coils which is in excess of the heating on full load. On the other hand, on all classes of machines and particularly on machines using forced ventilation, the amount of the cooling air is proportional to the total losses in the machine. With the absence, therefore, of the core loss in armature, which may form a considerable part of the total losses, the heat conduction to a relatively cool core may be more rapid than would be the case were the machine loaded.

Tables IIA, IIB, IIC and IID give data comparing results obtained from the open- and short-circuit method (continuous) with those from actual load on the same machines.

b. *Intermittent Run.* The generator to be tested is run alternately open and short-circuited. The length of time for a complete cycle usually varies from four to fifteen minutes.

Let X be the per cent of the time during which the machine is short-circuited, I_c the normal armature current and W_c the normal core loss. When the machine is operated open-circuited, the field current is adjusted to give a voltage such that the core loss is equal to $\frac{W c}{1 - X}$, and when operated short-circuited, so that the armature current is $\frac{I_c}{\sqrt{X}}$. If I_0 and I_s are the field currents for the periods of open circuit and short circuit respectively, and I_n the normal field current, the average field current will be

$$\sqrt{I_s^2 X + I_0^2 (1-X)}$$

TABLE II A

*THREE-PHASE, 60 CYCLES, 2500 KV-A., 1800 REV. PER MIN., 2300 VOLTS

Maximum temperature rise, deg. cent	*Full load	Open-circuit 110 per cent volts	Short-circuit 120 per cent amperes
Armature laminations	35	36	—
" ducts	31	28	—
" coils (thermo.)	29	—	35
" " (res.)	34	--	45
Field winding	41	48	—

* Average of runs on two similar machines.

TABLE II B

THREE-PHASE, 33 CYCLES, 5400 KV-A., 1990 REV. PER MIN., 2300 VOLTS

Maximum temperature rise	Full load	Open-circuit 110 per cent volts	Short-circuit 122 per cent amperes
Armature laminations	25	20	—
" ducts	24	16	—
" coils	22	—	34
Field winding (res.)	38	25	—

TABLE II C

†THREE-PHASE, 60 CYCLES, 715 KV-A., 720 REV. PER MIN., 2300 VOLTS

Maximum temperature rise	Full load	Open-circuit 110 per cent volts	Short-circuit 110 per cent amperes
Armature laminations	30	25	—
" ducts	23	24	—
" coils	30	—	30
" " (res.)	28	—	32
Field coils	26	21	—
" " (res.)	38	35	—

† Thermometers were in the same location for all runs.

TABLE II D

THREE-PHASE, 60 CYCLES, 3000 KV-A., 514 REV. PER MIN., 2300 VOLTS

Maximum temperature rise	Full load	Open-circuit 110 per cent volts	Short-circuit 125 per cent amperes
Armature laminations	32	30	—
" ducts	29	24	—
" coils	25	—	24
" " (res.)	34	—	30
Field coils	21	29	—
" " "	21	29	—

the value of X being so chosen that the average field current will be equal to I_n. If this is done, the average losses due to armature current, core loss and field excitation should be about the same as under load. It is not always possible to choose a value for X that will make the average field loss equal to the normal field loss, but the field heating can be approximated.

In some generators the short-circuit core loss is considerable and cannot be neglected. This can be taken care of by slightly modifying the intermittent method.

If W_s be the short-circuit core loss corresponding to armature current $\frac{I_a}{\sqrt{X}}$, used during period of short-circuit, the total loss supplied during interval X will be $W_s X$. This loss should be subtracted from the normal core loss to find the core loss to be supplied during the period of open circuit. This loss will be

$$\frac{W c - W s X}{1 - X} \text{ instead of } \frac{W c}{1 - X}$$

Several tests have been made by this method and the temperatures obtained agree fairly well with load temperatures. In the practical application of this method judgment should be exercised in selecting periods obtained by the foregoing formula so that the armature winding may be not subjected to excessive currents that result in damage to insulation before a dangerous temperature is recorded on a thermometer applied to the external surface.

This test requires a knowledge of both the open- and short-circuit core loss before the run is begun, which sometimes handicaps its use.

Tables IIE, IIF and IIG give data comparing results obtained from the intermittent open- and short-circuit method with those from actual load on the same machines.

3. Open Delta Method

The armature winding of a three-phase machine is connected in delta with one corner open and direct current is introduced at this point. The fields are excited with that current which gives normal volts when the machine is operating under full load. The excitation is calculated from the saturation and impedance curves.

In many machines an alternating cross current may flow in the delta, due to the harmonics. When this current exists it is measured and combined vectorially with the applied direct-current value. The proper value of the current to give armature copper loss corresponding to normal load current (I_a) will be equal to

TABLE II E

THREE-PHASE, 60 CYCLES, 2500 KV-A., 1800 REV. PER MIN., 2300 VOLTS

Maximum temperature rise	Full load	Intermittent run Open-circuit 10 min. 117 per cent volts short-circuit 5 " 173 per cent amperes
Armature laminations	36	32
" ducts	31	29
" coils	29	17
" " (res.)	34	22
Field " "	41	37

TABLE II F

*THREE-PHASE, 60 CYCLES, 715 KV-A., 720 REV. PER MIN., 2300 VOLTS

Maximum temperature rise	Full load	Intermittent run 7.5 min. open-circuit 116 per cent volts. 7.5 " short-circuit 143 per cent amp's.
Armature laminations	30	29
" ducts	23	30
" coils	30	40
" " (res.)	28	40
Field "	26	25
" " (res.)	38	34

* Location of thermometers the same for both tests.

TABLE II G

* THREE-PHASE, 60 CYCLES, 257 KV-A., 720 REV. PER MIN., 2530 VOLTS

Maximum temperature rise	Full load	Intermittent run 4 min. short-circuit 152 per cent amp's. 5.5 " open- " 113 per cent volts
Armature laminations	22	19
" ducts	20	18
" coils	16	15
" " (res.)	31	23
Field "	32	32
" " (res.)	42	44

* Thermometers in same position for both runs.

$I_d = \sqrt{I^2_a - I^2_c}$ where I_d equals the applied direct current and I_c equals the circulating current due to the harmonics.

This method may be used on other than three-phase alternators by properly splitting and connecting the stator windings so as to obtain zero or a low potential across the opening into which the direct current is introduced.

Satisfactory results have been obtained in some cases by this method, but it is unreliable for general testing. This is largely due to the fact that the direct current in the armature forms a

TABLE III A
* THREE-PHASE, 60 CYCLES, 715 KV-A., 720 REV. PER MIN., 2300 VOLTS
SALIENT POLE MACHINE

Maximum temperature rise	Full load	Open delta
Armature laminations	30	29
" ducts	23	25
" coils	30	25
" " (res.)	28	26
Field "	26	21
" " (res.)	38	32

* Location of thermometers the same for both runs.

TABLE III B
THREE-PHASE, 60 CYCLES, 2500 KV-A., 1800 REV. PER MIN., 2300 VOLTS
NON-SALIENT POLE MACHINE

Maximum temperature rise	†Full load	Open delta
Armature laminations	35	51
" ducts	31	53
" coils	29	37
" " (res.)	34	45
Field " "	41	108

† Average of two similar machines.

TABLE III C
THREE-PHASE, 60 CYCLES, 300 KV-A., 1800 REV. PER MIN., 480 VOLTS
NON-SALIENT POLE MACHINE

Maximum temperature rise	Full load	Open delta
Armature laminations	25	31
" ducts	22	27
" coils	21	27
" " (res.)	38	33
Field " "	19	41

magnetic pole out of each group of coils per phase, which results in a pulsating flux and abnormal losses.

Tables IIIA, IIIB and IIIC give data comparing results obtained from the open delta method with those from actual load on the same machines.

4. Phase Displacement Method

Duplicate generators are assembled with their fields coupled together in such a way that a displacement of phases may be obtained by shifting one field with respect to the other or by shifting armature frames. A direct-current motor of sufficient capacity to take care of the losses in both machines is used to drive the generators at normal speed. The required condition of load and power factor must be obtained by trial. The stators are connected together electrically for proper phase rotation so that one machine acts as the motor and the other as the generator.

This method will give results in all cases identical with those obtained under actual load.

TABLE IV
THREE-PHASE, 60 CYCLES, 2500 KV-A., 1800 REV. PER MIN., 2300 VOLTS
TEMPERATURE RISES, DEG. CENT.

	1	2	3	4	5	6
Armature laminations........	35	35	51	36	—	32
" ducts..............	31	29	53	28	—	29
" coils (ther.)........	29	—	37	—	35	17
" " (res.).........	34	27	45	—	45	22
Field winding (res.)..........	41	68	108	48	—	37
" " corrected.......	—	42	—	—	—	—
Room......................	24	19	24	30	31	27

Column 1. Average of two runs on similar machines, 100 per cent power factor, actual load.
Column 2. Zero power factor method, over-excited field.
Column 3. Open delta method.
Column 4. Open-circuit run at 110 per cent normal volts.
Column 5. Short-circuit run at 120 per cent normal amperes.
Column 6. Intermittent run: open-circuit 10 min., 117 per cent volts; short-circuit 5 min., 173 per cent amperes; repeated until temperatures were constant.

The principal objection to this method is that it requires two identical machines, and may be used only when it is possible and practical to couple them together.

Tables IV and V show comparative temperatures obtained on the same machine run in accordance with the several methods described.

The temperatures recorded for the 715-kv-a. machine were obtained during successive runs and the thermometers on the machine were not disturbed during the entire progress of the tests. The rotating parts were also marked so that the thermometers were always applied to the same points when machine was shut down.

TABLE V

THREE-PHASE, 60 CYCLES, 715 KV-A., 720 REV. PER MIN., 2300 VOLTS

TEMPERATURE RISES, DEG. CENT.

	1	2	3	4	5	6	7	8	9
Armature core lam. (max.)	30	32	36	29	38	25	29	35	29
" " " (av.)	29	31	34	28	36	23	28	33	28
Armature core ducts (max.)	23	34	36	28	37	24	30	36	25
" " " (av.)	22	29	32	26	35	23	27	31	23
Armature coils (max.)	30	30	35	26	37	30	40	51	25
" " (av.)	25	27	32	23	36	27	34	42	23
Armature coils res. (max.)	28	37	44	30	46	32	40	47	26
" " " (av.)	28	37	44	30	46	32	40	47	—
Field spiders (max.)	18	22	27	17	28	16	18	24	19
" " (av.)	18	21	27	17	27	15	17	23	19
Collector rings (max.)	18	16	30	19	32	23	20	20	12
" " (av.)	17	15	28	18	31	21	20	19	12
Pole tip lead (max.)	23	27	34	19	31	17	20	29	22
" " " (av.)	22	26	33	19	31	17	20	28	22
Pole tip trail (max.)	23	27	33	20	28	16	20	32	22
" " " (av.)	22	26	32	20	27	16	19	29	22
Bridges (max.)	24	28	57	19	31	22	23	36	20
" (av.)	24	27	51	19	29	20	22	32	20
Squirrel cage (max.)	18	20	20	15	25	12	16	26	15
" " (av.)	18	19	20	14	23	12	16	23	15
Spools (max.)	26	45	59	22	31	21	25	42	21
" (av.)	26	35	54	21	30	21	23	39	21
Spools by resistance*	38	50	69	38	59	35	34	59	32
" " " †	22	37	60	24	32	23	25	36	24
Frame (max.)	17	18	19	16	21	14	16	20	16
" (av.)	17	17	19	16	21	13	16	19	15
Room (max.)	20	23	21	19	21/23	25/22	22	21	18
" (av.)	20	23	21	19	—	25	22	22	—

* Resistance by voltmeter and ammeter.

† Resistance by galvanometer.

Column 1. Machine running as a motor, full load, 100 per cent power factor.

Column 2. Full load, 80 per cent power factor, leading.

Column 3. Zero power factor method with field over-excited to give full load current in armature.

Column 4. Zero power factor method, over-excited 7 min. and under-excited 13 min., except during last hour of run, when periods were changed to 3½ min. over-excited and 6½ min. under-excited.

Column 5. Sum of temperatures from open- and short-circuited runs at normal voltage and normal current.

Column 6. Temperatures from open-circuit run at 110 per cent volts for the iron parts and field, and from short-circuit run at 110 per cent current for armature coils.

Column 7. Intermittent run, 7.5 min. open-circuit at 116 per cent volts and 7.5 min. short-circuit 143 per cent amperes.
Column 8. Intermittent run, open- and short-circuited with periods determined from excitation for 80 per cent power factor condition, 5 min. open-circuit at 140 per cent. volts and 10 min. short-circuit at 130 per cent amperes.
Column 9. Open delta method.

Conclusions

We believe the zero power factor method with intermittent leading and lagging current to be the best substitute for full load test, since the losses have approximately the same distribution as exist in the machine under load. This conclusion is confirmed by the results of the foregoing temperature tests. The zero power factor method with leading current gives practically the same values, with the exception of greater heating on the field. This can be readily corrected for normal field loss.

The open- and short-circuit method has been extensively used, and when proper values of voltage and current are chosen, will give results agreeing fairly well with actual load values. It is primarily recommended as a checking test on machines whose temperatures have previously been determined by the zero power factor or actual load methods, and whenever it is not practical or convenient to use these methods.

II. Temperature Tests which Approximate Actual Load Conditions for Induction Motors

The methods described are as follows:

1. Feeding-back method.
2. Reduced voltage method.
3. Reversed rotation method.

1. Feeding-Back Method

This test is made by belting together two induction motors, in such a manner as to make one run above and the other below synchronous speed. The amount that each differs from synchronous speed is the normal slip for that load. The stators are connected in multiple to an a-c. supply of normal frequency and voltage.

One machine acts as a generator and the other as a motor. Temperatures obtained on the motor under these conditions of load will equal normal operating temperatures.

This method involves difficulties in obtaining proper pulley diameter and is limited by the power that is readily transmitted by belting.

Where the induction motor is direct-connected to a d-c. machine and two sets are available, the feeding-back method may be used in all cases and temperatures will be obtained equivalent to those of full load.

2. Reduced Voltage Method

This test is made by running the motor free at normal volts until temperatures are constant. The motor is then operated at a reduced voltage and sufficient load to give the required current. The kv-a. input is usually $\frac{1}{4}$ to $\frac{1}{3}$ of normal and the power output about 1/16 to $\frac{1}{8}$ of normal. Although the load carried at reduced voltage is small, the slip from synchronous speed exceeds that for the same primary current at normal voltage. Full-load speed is desirable and is obtained by raising the frequency of supply a small percentage.

The temperature rise on the stator at full load may be determined from the above method as follows:

T_f = Temperature rise on stator for full-load normal volts.
T_n = Temperature rise on stator for no-load normal volts.
T_r = Temperature rise on stator for reduced voltage run.
E_n = Normal voltage.
E_r = Reduced voltage.

$$T_f = T_r - \frac{T_n E_r^2}{E_n^2} + T_n$$

The same method of calculation may be followed for any load. The rotor temperature is to be taken as observed on reduced voltage run. As a rule, the calculated temperature rises from this method are somewhat greater than those obtained from actual load.

Tables VI, VII, VIII, IX and X give data comparing temperatures from actual load with temperatures obtained by the reduced voltage method.

TABLE VI
THREE-PHASE, 60 CYCLES, 20 H.P., 1200 REV. PER MIN., 220 VOLTS

Maximum temperature rise in deg. cent.	Full load	Normal amperes 80 volts	Running free at normal volts	Corrected for full load	125 per cent load	125 per cent normal amperes 80 volts	Corrected for 125 per cent load
Stator laminations..	25	20	14	32	33	27	39
" ducts	20	17	8	24	28	23	30
" coils........	20	18	7	23	27	25	30
" " (res.)....	25	—	10	—	30	—	—
Rotor conductors...	18	18	6	18	25	20	20

TABLE VII

* THREE-PHASE, 60 CYCLES, 20 H.P., 1200 REV. PER MIN., 220 VOLTS

Maximum temperature rise	Full load	Normal current 80 volts	Running free at normal volts	Corrected for full load	125 per cent full load	125 per cent current 90 volts	Corrected for 125 per cent load
Stator laminations..	22	19	15	32	29	29	41
" ducts.......	19	16	10	25	29	23	31
" coils........	16?	21	12	31	27	27	37
" " (res.)...	25	26	16	40	35	33	46
Rotor conductors...	15	16	11	16	22	23	23

* Location of thermometers not changed during run.

TABLE VIII

THREE-PHASE, 60 CYCLES, 50 H.P., 900 REV. PER MIN., 440 VOLTS

Maximum temperature rise	Full load	Normal amperes 220 volts	Running free at normal volts	Corrected for full load
Stator laminations..............	24	19	10	26
" ducts.....................	16	12	4	15
" coils.....................	18	14	5	18
" " (res.)................	32	23	22	39
Rotor conductors...............	17	18	6	18

TABLE IX

* THREE-PHASE, 60 CYCLES, 100 H.P., 720 REV. PER MIN., 550 VOLTS

Maximum temperature rise	Full load	Normal current 156 volts	Running free at normal volts	Corrected for full load	125 per cent full load	125 per cent current 238 volts	Corrected for 125 per cent load
Stator laminations..	24	15	12	26	34	24	34
" ducts.......	20	14	9	22	25	22	32
" coils........	23	14	7	20	30	25	31
" " (res.)....	29	16	9	24	31	27	35
Rotor conductors....	16	7	5	7	24	18	18

* Location of thermometers not changed during runs.

TABLE X

* THREE-PHASE, 60 CYCLES, 250 H.P., 600 REV. PER MIN., 440 VOLTS

Maximum temperature rise	Full load	Normal amperes 130 volts	Running free at normal volts	Corrected for full load	125 per cent amperes	165 volts	Corrected for 125 per cent load
Stator laminations..	22	21	10	30	33	32	40
" ducts.......	21	17	7	23	30	30	36
" coils........	21	19	6	24	29	31	36
" " (res.)....	23	18	10	27	33	31	39
Rotor conductors...	20	20	5	20	23	33	33

* Location of thermometers not changed during runs.

3. Reversed Rotation Method

This test is made by driving the rotor at normal full-load speed in a direction opposite to that in which it has a tendency to turn when current is applied to the stator. The impressed volts on stator are adjusted to give the current corresponding to any load.

This method appears to be a practical one and gives temperature rise on the stator in most cases that very closely approaches normal running temperatures. It is difficult, however, to completely explain why the temperatures obtained are in such close agreement with those obtained under actual load.

Tables XI, XII and XIII give data comparing temperatures from actual load with those obtained by this method.

TABLE XI

* THREE-PHASE, 60 CYCLES, 250 H.P., 600 REV. PER MIN., 440 VOLTS

Maximum temperature rise	Full load	Reversed rotation normal amperes	125 per cent load	Reversed rotation 125 per cent amperes
Stator laminations........	22	19	33	31
" ducts.............	21	17	30	27
" coils..............	21	19	29	34
Rotor conductors.........	20	15	23	22

* Location of thermometers same for all runs.

TABLE XII

* THREE-PHASE, 60 CYCLES, 100 H.P., 720 REV. PER MIN., 550 VOLTS

Maximum temperature rise	Full load	Normal amperes reversed rotation
Stator laminations.............	24	23
" ducts..................	20	20
" coils..................	23	25
" " (res.)...............	29	30
Rotor conductors..............	16	18

* Location of thermometers same for all runs.

TABLE XIII

*THREE-PHASE, 60 CYCLES, 20 H.P., 1200 REV. PER MIN., 220 VOLTS

Maximum temperature rise	Full load	Normal amperes reversed rotation	125 per cent load	125 per cent amperes reversed rotation
Stator laminations............	25	18	33	26
" ducts..................	20	18	28	24
" coils..................	20	17	27	23
" " (res.)............	25	18	30	26
Rotor conductors.............	18	17	25	23

* Location of thermometers same for all runs.

From the data presented it will be noted that both the reduced voltage and reversed rotation methods give temperatures closely approximating those obtained under actual load. The distribution of losses in the machine under the reduced voltage method is more nearly normal than in the reversed rotation method. Either method is easily applied and requires the expenditure of only a small amount of power, compared with the rated output of the motor under test.

A paper presented at the Midwinter Convention of the American Institute of Electrical Engineers, New York, February 28, 1913.

NOTES ON METHODS OF MAKING LOAD TESTS ON LARGE INDUCTION MOTORS

BY A. M. DUDLEY

It is desirable, wherever it is possible, to test apparatus fully in all respects before it leaves the premises of the manufacturer. Where this can be done and the results carefully checked by both parties to the contract there is little possibility of the units failing to do in service what was expected of them.

In the case of induction motors such complete tests include a check on the efficiency, power factor, torques, heating, noise, mechanical balance and temperature. With the exception of temperature, it is possible to make observations with the motor at standstill or running under no load which will indicate closely to the trained observer what may be expected of the machine, with reference to the different characteristics enumerated. It is also true to a degree that these same observations give the losses in the various parts of the motor and in this way are a check on the temperature. This check is more evident to the designer, on account of his experience with the amount of loss that his various frames will dissipate, than it is to the man of less experience who represents the ultimate user and who is endeavoring to satisfy himself that the machine in all respects meets the specifications to which the manufacturer is working. For this reason it is desirable to load up the motor to as nearly exactly its normal operating condition as can be reached and make observations of the temperature rise in the various parts.

In the case of motor-generator sets driven by induction motors which come through manufacture in pairs, it is possible by the simple expedient of circulating the power around through all the machines, to secure full-load temperature tests on four machines at an expense merely of making up from an outside circuit an amount of power represented by the losses of the four units. It is also possible, in the case of separate units coming through in pairs, to make a full-load run at a reasonable expense. This may be accomplished in at least two ways. The first depends on the mechanical connection between the two units at the time one is running as a motor and the other as a generator. In practise this is worked out by belting the two machines together, with a pulley on the unit which is to act as a motor, slightly larger than the pulley on the unit which is to act as a generator. When the machines are started up and both are connected to the same source of alternating-current supply there is a tendency for one machine to drive the other slightly above synchronism due to the difference in pulley diameters. If this difference has been chosen or can be adjusted so that it is approximately twice the full-load slip of the units when running normally, the result will be that the combination will automatically divide this difference on either side of synchronous speed and one will run fully loaded as a motor below synchronism and the other fully loaded as an asynchronous generator above synchronism. Since the generator returns to the circuit all the power taken by the motor, with the exception of the full-load losses in both machines, this method is economical. It has the disadvantage, practically, that it is difficult to select or adjust the pulley diameters exactly as they should be.

The same result can be accomplished electrically by having available two sources of alternating-current supply, which can be adjusted so that the frequency of one will be slightly higher than the other. The two units undergoing test are then directly connected by some form of positive coupling and the motor unit is connected to the current supply which is to be higher in frequency. The generator unit is then connected to the source of lower frequency, and since it runs at the same rev. per min. as the motor, it is really running above synchronous speed as referred to the circuit to which it is electrically connected. The two sources of external power supply are then adjusted with just the proper difference in frequency to cause both the tested units to run under full-load conditions, one as a motor and one as an asynchronous generator. As the two sources of external power supply are presumably from a common source still further back, it is possible to balance up the consumed and regenerated energy so that only the full-load losses of the two tested machines need be supplied externally. This method works out very satisfactorily and is frequently employed in making such tests.

Two units are not always available, and in that case, if the machine to be tested is one of large capacity, it may be difficult or impossible to make a full-load test. The reasons for this are obvious. The manufacturer may not have available the necessary mechanical facilities in the way of shafting, bearings, pulleys, belts or gears to line up the motor so it can be loaded, or there may be no suitable machine to serve as a load for the tested motor or to drive the tested motor as a generator, and last, but by no means to be considered negligible, the expense of conducting such a dead load test, from the standpoint of the power consumed alone, would be considerable. For example, a unit of 1000 h.p. would have a full-load input in the neighborhood of 800 kw. and this load for ten hours at one cent per kw-hr. would amount to $80.00.

For these practical reasons it is necessary in such cases to adopt some form of compromise test, which, while it may not give exactly the same results as actual full load, will give a sufficiently close approximation to judge the actual temperatures under operating conditions after installation.

There are a number of different methods of accomplishing this result, of which the following may be mentioned:

1. Operating the unit under test as a motor on normal frequency but at reduced voltage and developing a reduced torque.

2. Driving the tested unit as an a-c. generator by a small auxiliary motor and making compromise tests after the methods followed on a-c. generators. These may consist of over-exciting one member with direct current so as to give high iron losses, or using a lower excitation with the other member short-circuited so as to give high copper losses.

3. Operating the tested unit as a motor without load but at a voltage higher than normal in the effort to increase the iron and no-load copper losses to a sum approximating the full-load copper and iron losses.

4. Operating the tested unit as a motor on a cycle where it will alternately run light for a period at over-voltage and for another period at low voltage and a light load sufficient to cause full-load or somewhat greater current to flow in the windings.

5. Connecting the tested unit to a supply circuit at a greatly reduced voltage and driving it against its normal direction of

rotation by a separate motor. By varying the voltage applied it is possible to cause any desired current to flow in the windings, with proportionate heating.

Considering these methods in order, they will be found to give the following results:

1. In this case it is possible to get full-load copper losses but there is necessarily only a small proportion of the full-load iron loss present. The copper losses may be increased to approximate the iron loss, but the losses so created are distributed differently from those in the normally operated and loaded machine and the resulting temperatures are affected thereby. Such tests ordinarily show the copper temperatures higher than normal and the iron temperatures somewhat lower.

2. These tests are similar in every way to the same tests conducted on a-c. generators. As is shown in Table V, in some cases they give results very close to the tested values under actual load.

3. This method resembles method (1) in the fact that the distribution of losses is different from the normal machine. This test usually shows the iron temperatures too high and the copper temperatures too low. There is a practical limit to the amount the voltage may be increased, due to the limits of the insulation.

4. This test, with a proper selection of the proportion of total time for the various parts of the cycle and the frequency of their alternation, can be made to give results very closely approximating full-load conditions. It is hardly safe, however, to adopt a general cycle as applicable to all machines, on account of the varying proportions of copper and iron losses in machines of different characteristics. Data on this method, where available, indicate very satisfactory results.

5. This method has proved very satisfactory in a large number of instances. It is, however, open to criticism in that it imposes on the rotor core a frequency of about twice the normal primary frequency. This does not occasion any material error from the standpoint of iron loss in the rotor core, since the applied primary voltage and the resulting densities are fairly low. It does, however, occasion considerably increased rotor copper losses due to eddy currents caused by the high secondary frequency. As shown in Tables I and V, this is ordinarily of no consequence on 25-cycle machines. Tables II, III and IV show, however, that it may materially increase the rotor temperatures on 60-cycle machines and through these, the temperatures of the whole

machine. One of the advantages of this method is that it is safe, *i.e.*, it shows temperatures on test which are higher, if anything, than they will be under actual load conditions. It may, therefore, be concluded that, if a machine has been operated in this way and the temperatures are within specified limits, the temperatures under normal operating conditions will be within the same limits. Unfortunately, the converse is not equally true, *i.e*, a machine may show under this test temperatures which are higher than the specified limits and which under actual load conditions may be well inside these same limits. But this is a good fault in any compromise method and should not detract from the excellent results which follow generally from its use.

As the writer's experience has been more largely in connection with method (5), he offers in the tables some typical examples of tests conducted in this way. In every case the columns marked " circulating current " are tests taken in this way, *viz*: a current of normal frequency and greatly reduced voltage is applied to the motor terminals and it is driven against the normal direction of rotation at full-load speed. The applied voltage is then varied until the desired current flows in the windings.

TABLE I

INDUCTION MOTOR—PHASE-WOUND ROTOR

Rating: 1000 h.p., 25 cycles, three-phase, 2200 volts, 12 poles, 245 rev. per min.

Losses.

Primary copper	11,600 watts
Secondary copper	13,800 "
Core loss	7,700 "
Bearing friction and windage	6,400 "
Total	39,500 watts

TEMPERATURE DATA

Kind of load	Actual load driving generator			Circulating current
Length of test in hours	8	2	7½	8½
Volts	2247	2220	2240	360
Per cent normal full -load amperes	139	170	150	139
Rise, in deg. cent., stator core	31	40.5	36.5	29
" stator copper (thermometer)	33.5	47.5	39	38
" " " (resistance)	36.5	48.2	—	34
" rotor copper (thermometer)	24.5	38.5	34	32
" " " (resistance)	33	32.6	—	35
" rotor core	24	34	28	25
Amperes per phase	312	390	341	312
Air temperature, deg. cent	18	18.5	16	21

TABLE II

INDUCTION MOTOR—PHASE-WOUND ROTOR

Rating: 300 h.p., 50 cycles, three-phase, 550 volts, 16 poles, 367 rev. per min.

Losses.

Primary copper	5950 watts
Secondary copper	5050 "
Core loss	4900 "
Bearing friction and windage	3200 "
Total	19,100 watts

TEMPERATURE DATA

Kind of load	Actual	Circulating current
Length of test in hours	3	2
Volts	550	158
Per cent full-load amperes	128.5	128.5
Rise, stator core, deg. cent	25	38.5
" " copper (thermometer)	23.5	36.5
" " " (resistance)	30.1	42
" rotor " (thermometer)	23.5	30.5
" " " (resistance)	25.2	38.5
Amperes per phase	373	372.5
Air temperature, deg. cent	20	30.5

TABLE III

INDUCTION MOTOR—SQUIRREL-CAGE ROTOR

Rating: 300 h.p., 60 cycles, three-phase, 2200 volts, 6 poles, 1160 rev. per min.

Losses.

Primary copper	4125 watts
Secondary copper	7500 "
Core loss	9140 "
Bearing friction and windage	4000 "
Total	24,765 watts

TEMPERATURE DATA

Kind of load	Actual belted	Circulating current
Length of test in hours	11½	2
Volts	2196	326
Per cent of full-load amperes	103	129
Rise stator core deg. cent	40.5	29.5
" " copper (thermometer)	25	28
" " " (resistance)	30.6	34.4
" rotor " (thermometer)	26.5	52.5
Rev. per min	1160	1155
Amperes per phase	71	88.8
Air temperature, deg. cent	23.5	23

TABLE IV

INDUCTION MOTOR—PHASE-WOUND ROTOR

Rating: 800 h.p., 60 cycles, three-phase, 2200 volts, 26 poles, 272 rev. per min.

Losses.

Primary copper	10,500 watts
Secondary copper	10,000 "
Core loss	14,000 "
Bearing friction and windage	7,700 "
Total	42,000 watts

TEMPERATURE DATA

Kind of load	Actual	Circulating current
Length of test in hours	7½	7½
Volts	2260	456
Per cent full-load amperes	126	102
Rise, stator core, deg. cent	42.5	36
" " copper (thermometer)	36.5	37
" " " (resistance)	44.5	41
" rotor " (thermometer)	26.5	50
" " " (resistance)	28	48.6
" " core	24.5	43.5
Amperes per phase	266	214
Air temperature, deg. cent	16.5	23

TABLE V

INDUCTION MOTOR—PHASE-WOUND ROTOR

Rating: 1600 h.p., 25 cycles, three-phase, 6400 volts, 6 poles, 492 rev. per min.

Losses.

Primary copper	17,300 watts
Secondary copper	18,200 "
Core loss	13,200 "
Bearing friction and windage	18,000 "
Total	66,700 watts

TEMPERATURE DATA

Kind of load	Actual		Circulating current	Rotor excited with d-c. and stator short-circuited	
Length of test in hours	5½	5½	6½	7½	8
Volts	6360	6300	950	0	0
Per cent full-load amperes	102	130	104.8	100	125
Rise, stator core, deg. cent	24.5	29.5	18.5	19	28
" " copper (thermometer)	23.5	31.5	25	19	35.5
" " " (resistance)	34.3	48	30	30	49.5
" rotor " (thermometer)	23	28	32.5	24	37.5
" " " (resistance)	28.5	35	34	—	62
" " core	19	20	25	16	26
" collector	22	22	22.5	23.5	31
D-c. volts on secondary				43	58
D-c. amperes in secondary				487	643
Air temperature, deg. cent	21	23	23.5	21.5	21

Table I. This is a fair example of the results of this method. A comparison of columns 1 and 4, run with the same current input, shows remarkably close results for a compromise method. Any slight inaccuracies which may be noted are due to using commercial test results and to a policy on the author's part of avoiding carefully edited laboratory readings.

Table II. In this machine, operated on 50 cycles, can be noted the tendency for the high rotor frequency to heat up the rotor and through it the complete machine. It will be noted that the test was at approximately 28½ per cent overload.

Table III. This test was at 60 cycles, and while an actual load of 103 per cent is compared with a compromise at 129 per cent it can be seen that the rotor on the compromise test ran at a higher temperature than would be the case with the same amount of actual load.

Table IV. This case shows the extreme variation of the compromise method and was on a machine where the depth of the rotor conductors was sufficient to magnify considerably the eddy current loss due to secondary currents at a frequency of approximately 120 cycles.

Table V. This is an interesting comparison and shows the results of compromise tests made by two methods, enumerated as (2) and (5) above, and the performance of the same machine under actual load. The results of all three tests were satisfactory, but the compromise results were, of course, secured with considerably less effort and expense than was true in the case of the actual load tests.

Conclusions

From these considerations are drawn the following conclusions:

1. That the employment of a compromise method of making temperature runs on induction motors of large capacity is in many cases desirable and necessary.

2. That the method outlined above as method (5) gives results which are always " safe " and which are in general a close approximation to the actual temperatures.

3. That it is desirable that the Standardization Rules of the American Institute of Electrical Engineers should recognize the various methods of conducting such compromise tests, together with their reliability and limitations as applied under varying conditions.

A paper presented at the Midwinter Convention of the American Institute of Electrical Engineers, New York, February 28, 1913.

LOAD TESTS ON TRANSFORMERS

BY J. J. K. MADDEN

Load tests on transformers may be conducted in several ways, all of which are intended to approximate as nearly as possible the operating conditions of the transformers, so far as temperature rise is concerned.

A run with actual load might be made by using water rheostats, but as this would be very expensive, some form of motor-generator method is ordinarily used, which will give approximately the same heating.

Fig. 1 shows connections for testing two similar single-phase transformers by the "motor-generator" method. The low-voltage windings are connected in multiple, to which normal voltage is applied. An auxiliary transformer connected in series with the high-voltage windings supplies the impedance losses. The same method may be used for any even number of transformers, and load and excitation may be applied to the same windings or reversed from the order shown in the illustration. The rated voltage of the windings will determine the arrangement.

Fig. 2 shows connections for testing three similar single-phase transformers, or one or more three-phase transformers if delta-connected.

Fig. 3 shows connections for testing two three-phase transformers. The transformers may be connected either delta or Y, but some means of regulation on the loading side must be included so as to be able to balance the load current independently in each phase.

Fig. 4 shows the same connections as Fig. 1, except that the low-voltage winding has several independent circuits. Excitation is applied to one winding only, and the loads are properly distributed by inserting reactance, if necessary, in the circuits so as to balance the loads carried by each winding.

Fig. 5 shows connections for testing two similar polyphase Scott-connected transformers. Excitation and load are applied to the three-phase side.

Fig. 6 shows connections for testing two similar single-phase

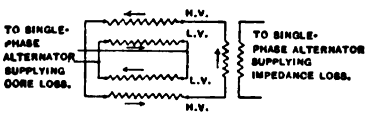


FIG. 1

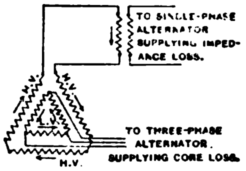


FIG. 2

transformers suitable for two-phase-three-phase operation. Load and excitation are applied to the windings used for the two-phase side, while an extra source of supply of current furnishes the 15 per cent additional current on the three-phase side. This will give the conditions obtained when the transformers are operated Scott-connected. This method is used on transformers which do not have the halves of the two-phase

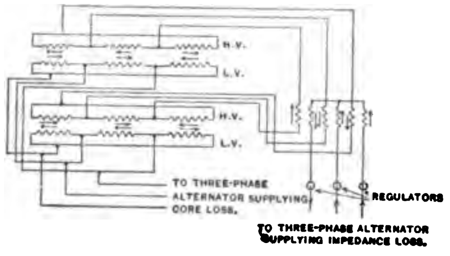


FIG. 3

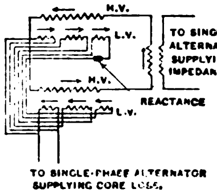


FIG 4

windings connected in multiple. When the halves are connected in multiple both transformers should be connected as mains and should be furnished with load current equal to 115 per cent of their normal single-phase rating, as in Fig. 1.

The "motor-generator" or "opposition" method cannot, of course, be employed when one single-phase transformer is involved, and since the actual loading of such a transformer on water rheostats is not always feasible, it is the idea of this paper to suggest, particularly, methods to be used on such a transformer. These methods must be such that they do not entail a great loss of power and yet approximate as nearly as possible the operating conditions ot the transformer.

These methods for testing single transformers may be classed as follows:—

a. Intermittent runs.

b. Ultimate open-circuit and short-circuit runs.

a. The principle of the intermittent run will be understood from the following considerations: The average temperature rise of oil depends upon the total watts loss in the transformer, and is independent of where the loss takes place. Therefore, if a loss equal to the total loss is intermittently placed in the iron and in the copper by alternating an over-voltage core loss run with an over-current impedance run, the oil should have a perfectly

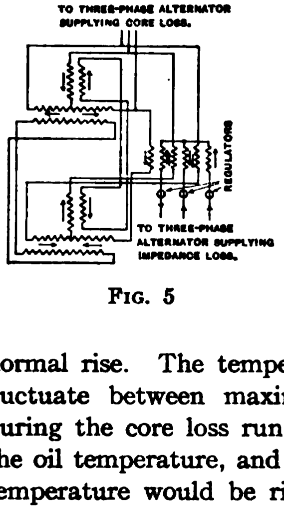


FIG. 5

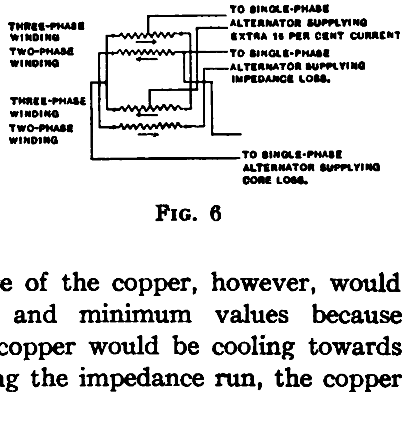


FIG. 6

normal rise. The temperature of the copper, however, would fluctuate between maximum and minimum values because during the core loss run the copper would be cooling towards the oil temperature, and during the impedance run, the copper temperature would be rising.

If, however, the integrated watt-hour loss over the complete cycle of the intermittent run is equal to the normal integrated copper loss for the same time, the mean of the maximum and minimum temperatures attained by the copper above oil would be equal to the normal copper rise above oil. If the time of each cycle is made small enough, the difference between the maximum temperature rise and the normal temperature rise above oil is small enough to neglect, in which case this maximum rise would be taken as the measure of the normal temperature rise of the copper. Actual tests here given show that if the periods are reduced to five minutes, the difference between the two temperatures is less than the errors involved in testing. Moreover, the difference is on the safe side, since it always will tend to show temperature slightly greater than the normal.

It is also evident that if in the last copper run the time interval were cut in half, the maximum rise at the instant of shut-down would be much closer to the normal rise than if the run had been continued for the complete cycle.

Tests were made in which each run was continued until the top oil showed a constant temperature rise of not more than one degree within two or three hours. The run was then stopped at the end of a cycle or half period and the resistance of both windings taken.

Fig. 7 shows the connections for testing one transformer by the intermittent run method. Two oil switches are so arranged that when the one in the core loss circuit is closed the one in the impedance loss circuit is tripped, as is also the short-circuiting switch which is in use during the impedance run. The impedance and short-circuit switches are combined, and interlocked with the core loss switch so as to make it impossible to apply core loss voltage without the other switch first being opened.

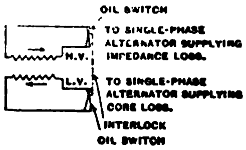


FIG. 7

b. The second method, which consists in taking, separately, ultimate short-circuit and open-circuit runs, is based on the idea that the temperature rises in windings and oil are directly proportional to the heat energy which is being dissipated. For example, if 100 watts total loss causes the oil to rise 10 deg. above air, a total loss of 50 watts should cause the oil to rise 5 deg. above a ir,and for the same reason, doubling the loss in the winding should double also the temperature rise of the winding above the oil into which it dissipates its heat.

Granting this assumption for a moment, suppose a transformer, in which the copper loss equals the core loss under normal load, attains an ultimate temperature rise of 40 deg. oil above air and 5 deg. copper above oil. Under an ultimate core loss run, the watts loss being only half the total, the oil rise should be 20 deg. above air, and the copper, being idle, would be at oil temperature. Similarly, an ultimate impedance run would give an oil rise of 20 deg. above air and a copper rise of 5 deg. above oil. The ultimate rise for either oil or copper for a load run is then obtained by adding together the two rises resulting respectively, from the ultimate short-circuit and open-circuit runs.

Tests were made to prove the correctness of this hypothesis on 25- and 500-kw. transformers, and in both cases the values obtained by adding the temperature of the separate runs, gave values considerably higher than the normal run. In both cases, the sum of the ultimate oil rises obtained from the two is about 5 deg. greater than the oil rise obtained in an opposition run. The discrepancy may be due to the fact that the ability of the air in contact with the walls of the transformer tank to cool the transformer is not directly proportional to the temperature difference between the oil and the air, and also that the radiation from the tank increases more rapidly than the temperature rise.

Whatever may be the explanation, however, the tests given in Tables II and VI show conclusively that this method does not closely approximate the operating conditions of a transformer, but the results are always higher than those obtained under normal operating conditions. This method should be used, therefore, only in cases where it would not be expedient to use another method. These runs shown in Tables II and VI were continued until the top oil showed a constant temperature rise of not more than one degree in two or three hours of the run.

Table I gives data of a 25-kv-a. transformer operated under the following conditions:

1. Motor-generator method with a similar transformer.
2. Dead load on a water-box.
3. Voltage (corresponding to 371 per cent of normal core loss at 25 deg. cent.) for 2½ minutes and current (corresponding to 150 per cent normal impedance loss at 25 deg. cent.) for 7½ minutes.
4. Voltage (corresponding to 198 per cent of normal core loss at 25 deg. cent.) for 5 minutes and current (corresponding to 200 per cent normal impedance loss at 25 deg. cent.) for 5 minutes.
5. Voltage (corresponding to 139 per cent of normal core loss at 25 deg. cent.) for 7½ minutes and current (corresponding to 379 per cent normal impedance loss at 25 deg. cent.) for 2½ minutes.

In the above runs (3) (4) and (5) each cycle had a duration of ten minutes. It will be seen that the cyclic runs give results within 3 per cent of actual operating temperatures.

Table II gives ultimate open-circuit and short-circuit runs on the 25-kv-a. transformer, in which voltage and current corresponding to 25 deg. cent. losses were held as in Table I.

Table III presents the data of Table I and II in a different form, in order to point out the actual losses, core, copper and total, held on these tests.

Tables IV and V give data of a 500-kv-a. transformer, as follows:

1. Intermittent runs in which 199 per cent of normal impedance loss and 196 per cent of normal core loss were held for periods of 5, 10, 15, 20 and 30 minutes. The cycle was then reversed, *i. e.*, core loss first, followed by impedance loss.

TABLE I
Rating H-60-25 1100/2200-110/220

Cycles of run	Motor-generator run	Dead load run	Intermittent runs: 2½ min. core loss 7½ min. imp. loss	Intermittent runs: 5 min. core loss 5 min. imp. loss	Intermittent runs: 7½ min. core loss 2½ min. imp. loss
Length of heat run—hours..	22	17	24	20	24
Initial load applied.					
Core loss..............	145.4	145.4	539	288	302
Imp. loss..............	320.5	320.5	479	641	1212
Rise of high-voltage winding deg. cent..............	41.3	40.5	41.4	39.5	42.1
Rise of low-voltage winding deg. cent...............	43.2	41.2	42.1	40.2	43.5
Oil rise deg. cent...........	29	29	31	27	32

NOTE.—Normal core loss at 25 deg. cent. = 145.4 watts.
Normal impedance loss at 25 deg. cent. = 320.5 watts.
All losses based on cold wattmeter readings.
Heat runs were made by holding voltage and current constant throughout each cycle of intermittent runs; also on motor-generator and dead-load runs.

TABLE II
RATING H-60-25-1100/2200-110/220

	Open-circuit run	Short-circuit run
Length of run—hours...............	10	18
Initial load applied.		
Core loss......................	145.4	——
Imp. loss.....................	——	320.5
High-voltage wind. rise in deg. cent...	8.8	30.8
Low-voltage wind. rise in deg. cent. .	11.2	34.4
Oil rise in deg. cent................	9	24

See Note under Table I regarding losses.

In each case the cycles were equally divided, *i.e.*, 5 minutes impedance loss and 5 minutes core loss, etc. The percentages are based on normal wattmeter readings at ultimate full-load operating temperature.

2. Motor-generator method with a similar transformer.

Table VI gives open-circuit and short-circuit ultimate temperature runs on the 500-kv-a. transformer.

Table VII gives results of intermittent runs of 10, 30, and 60-minute cycles, the periods for each being half this time. These runs are in all respects similar to those of Table V, with the exception that the run was cut off at the middle point of the impedance period. These results are seen to be in very close accord with those secured on the motor-generator run.

TABLE III

RATING H-60-25-1100/2200-110/220

		Per cent normal	Per cent total normal	Per cent time	Per cent total energy normal	Cold normal core loss at 25 deg.cent	Cold normal impedance watts at 25 deg. cent.
Core loss	Intermittent runs 2¾ min.	371	115.5	27.5	102	145.4	320.5
Imp. loss	7¼ "	150	103	72.5	109		
Core loss	5 "	198	61.8	50	99		
Imp. loss	5 "	200	137.5	50	100		
Core loss	7¼ "	139	43.4	72.5	101		
Imp. loss	2¾ "	379	260	27.5	104		
Core loss	Motor-generator	100	31.2	100	100		
Imp. loss	run	100	68.8	100	100		
Core loss	dead load	100	31.2	100	100		
Imp. loss	run	100	68.8	100	100		
Core loss	Open-circuit	100	31.2	100	100		
Imp. loss	run	—	—	—	—	—	—
Core loss	Short-	—	—	—	—	—	—
	circuit	—	—	—	—	—	—
Imp. loss	run	100	68.8	100	100		

All percentages based on 25 deg. cent. wattmeter readings.

Heat runs were made by holding voltage and current constant through each cycle, also for ultimate runs.

Table VIII presents the data of Tables IV to VII inclusive, in a different form in order to point out the actual losses, core, copper and total, used in these tests, in comparison with the normal.

In order to secure more reliable results, these two types of runs were made on two transformers, differing widely both in capacity and in the proportioning of losses. While the 25-kv-a. core and copper losses are proportioned 1 to 2.2 respectively, the 500-kv-a. losses are approximately 1 to 1.1.

It will be noted that these tests were conducted on self-cooled, oil-insulated transformers, but there is no apparent reason why such runs would not also be approximately correct for other types. It will be interesting to have the assumption checked on other types of transformers.

Referring to the intermittent runs, it is seen that in the case of

TABLE IV
RATING H-60-500-11000-2300

Cycles of run	Intermittent runs				
	5 min. imp. loss 5 min. core loss	10 min. imp. loss 10 min. core loss	15 min. imp. loss 15 min. core loss	20 min. imp. loss 20 min. core loss	30 min. imp. loss 30 min. core loss
Average length of runs—hrs.	5	6	6	5	4
Load applied.					
Core loss	6930	6930	6930	6930	6930
Imp. loss	6335	6335	6335	6335	6335
Rise of high-voltage winding in deg. cent.	53.5	53.9	50.9	49.9	48.3
Rise of low-voltage winding in deg. cent.	48.6	48.0	49.5	55.4	52.8
Rise of oil in deg. cent.	50	50	50	50	49

NOTE.—The rises are averages of two or three runs.
Normal core loss at 74 deg. cent. = 3540 watts.
Normal copper loss at 74 deg. cent. = 3175 watts.
All losses based on hot wattmeter readings.
Heat runs were made by holding wattmeter readings constant throughout each cycle.

the 25-kv-a. transformer, a specified voltage and current were held constant, while on the 500-kv-a. unit watts were held constant. Excluding the question of stray losses, the two methods agree in applying the proper loss to core and copper respectively. However, due to the fact that stray losses are affected by temperature at a different rate from the resistance losses, when current is held constant, the proper value of losses will not be present at ultimate operating temperatures. On the other hand, due to the fact that stray losses increase at a less rate than the resistance loss, with increasing current, when watts are held constant, the losses at ultimate operating temperature are not

TABLE V

RATING H-60-500-11000-2300

Cycles of run	Intermittent runs					
	5 min. core loss 5 min. imp. loss	10 min. core loss 10 min. imp. loss	15 min. core loss 15 min. imp. loss	20 min. core loss 20 min. imp. loss	30 min. core loss 30 min. imp. loss	Motor-generator run
Average length of heat run hours	12	6	5	10	6	13
Load applied.						
Core loss	6930	6930	6930	6930	6930	3540
Imp. loss	6335	6335	6335	6335	6335	3175
Rise of high-voltage winding in deg. cent	56.1	57.1	57.5	59.4	62.8	53.6
Rise of low-voltage winding in deg. cent	46.7	52.0	52.2	54.3	58.5	45 9
Rise of oil in deg. cent	50	50	50	50	50	50

NOTE.—See Note, Table IV.

TABLE VI

RATING H-60-500-11000-2300

	Open-circuit run	Short-circuit run
Average length of run, hours	24	12
Load applied.		
Core loss	3540	——
Imp. loss	——	3175
Rise of high-voltage winding in deg. cent	27.8	32.5
Rise of low-voltage winding in deg. cent	28.2	34
Rise of oil in deg. cent	31	25

NOTE.—See Note under Table IV regarding losses.

TABLE VII

RATING H-60-500-11000-2300

	5 min. core loss 5 " imp. "	15 min. core loss 15 " imp. "	30 min. core loss 30 " imp. "
Average length of run—hours	8	8	13
Load applied.			
Core loss	6900	6900	6900
Imp. loss	6340	6340	6340
Rise of high-voltage winding in deg. cent	50.4	52	53.9
Rise of low-voltage winding in deg. cent	44.1	44.8	46.3
Rise of oil in deg. cent	48	48	48

See Note under Table IV

The above heat runs were shut down and temperatures, etc. measured at the middle point of the last impedance period.

TABLE VIII

RATING H-60-500-11000-2300

		Per cent normal	Per cent total normal	Per cent time	Total energy per cent normal	Cold normal core loss at 26.5 deg. cent	Cold normal impedance watts at 25 deg. cent.	Hot normal core loss at 74 deg. cent.	Hot normal impedance watts at 74 deg. cent.
Core loss	Intermittent run	196	103	50	98	3770	2710	3540	3175
Imp. loss		199	94.1	50	99.5				
Core loss	Motor-generator run	100	47.3	100	100				
Imp. loss		100	52.7	100	100				
Core loss	Open-circuit run	100	47.3	100	100				
Imp. loss		—	—	—	—	—			
Core loss	Short-circuit run	—	—	—	—				
Imp. loss		100	52.7	100	100				

NOTE.—All losses based on hot wattmeter readings.
Heat runs were made by holding wattmeter readings constant through each cycle, and also for ultimate runs

distributed exactly as under normal conditions, although the total may be correct. It would appear that the latter method is the better of the two, although no serious errors will probably result from either, except possibly in the case of transformers possessing very large stray losses.

The two sets of runs will also be seen to differ as to the relative duration of core and copper periods. In the case of the 500-kv-a. machine, the complete cycle was divided equally between core and copper, viz., double losses were held for half the cycle on core and copper respectively. On the 25-kv-a. machine, the complete cycle was divided unequally between core and copper, the losses in each case being inversely proportional to the time. Although, from a general standpoint, it would be simpler and more convenient to specify equal core and copper periods, yet cases will often arise where it will be of considerable advantage to increase either the core or copper period, corresponding to either high flux or current densities in the particular design in question. This is due not only to possibility of abnormal local heating, but also to possible errors introduced, for instance, as a high exciting current on the core loss period. In such a case, it is practicable to eliminate the error by reducing the energy loss on copper period by the amount held on copper in core loss period. Similarly, in case of appreciable core loss on copper loss period, reduction in energy loss on core period can be made. The error due to high exciting current can also be eliminated by an increase in frequency.

In the case of the 500-kv-a. unit, various periods for the complete cycle from ten minutes to one hour were tried out, all runs giving fairly good results. Although, from the standpoint of switching, etc., it is of advantage to employ cycles of considerable duration, care must be taken not to jeopardize the transformer by local heating in any part.

It will be seen from the tabulated data that more consistent results were secured by cutting off the load on the last cycle at the middle of the copper period than by cutting off at the end of the cycle. This is, of course, much more correct, although somewhat in error due to the fact that the rate of increase in temperature with a given load diminishes with the time the load is applied. If desired, still greater refinement may be secured by making a change to shorter cycles, say of ten minutes, at the middle point of copper period, when constant cyclic conditions have first been reached.

Recommendations

Since the "motor-generator" method of making heat runs which has been universally used, is satisfactory, this method is recommended, whenever more than one single-phase transformer or when one or more three-phase transformers are available.

Based on the above discussion, and tests presented in the attached tables, the following method is recommended for obtaining as near as possible the normal temperature rise under operating conditions of one single-phase transformer.

By suitable means, raise the transformer windings to estimated operating temperature, after which measure normal core loss and normal impedance loss. Select such a time for the cycle, say thirty minutes, as will avoid excessive local heating. Divide the cycle into two periods, which are inversely proportional to watts loss to be held in the respective periods. Unless considerations such as abnormal exciting current (which can usually be obviated by raising the frequency) prevent, hold total normal loss of machine first on core and then on copper. Alternate the core and copper periods until the temperature of the cooling medium is constant. Cut off run at middle point of copper period, and record temperatures and resistances. Closer refinement may be obtained by reducing the length of the cycle to, say, ten minutes, at the end of the run noted above.

A paper presented at the Midwinter Convention of the American Institute of Electrical Engineers, New York, February 28, 1913.

SOURCES OF ERROR IN TRANSFORMER TESTS

BY W. M. McCONAHEY AND C. FORTESCUE

In order to obtain accurate results in making transformer tests, it is necessary to make all measurements and tests very carefully, and to be able to pick out and eliminate all errors or sources of error. Some errors can be avoided, while others are of such a nature that they must necessarily be included in the measurements and can be corrected later. Only those errors or sources of error that have an appreciable effect upon the results should be eliminated, since to include all would be introducing laboratory methods into commercial testing, thus unreasonably increasing the time and cost of making tests without securing any useful results.

Instruments. Instruments should be selected to give a good scale deflection, as small errors in observation give a greater percentage error for small deflections than for large. They should be calibrated at regular intervals, and if at any time it is suspected that they have been subjected to abnormal usage, their calibration should be checked at once. Care should be taken that they are not used beyond their capacities, as in this way the calibration may be changed, or they may be damaged.

It is best to avoid the use of instrument transformers wherever possible, but when it is necessary to use them, they should be calibrated at the proper frequency, with loads equivalent to the instruments with which they are to be used.

Instruments should be located so as not to be influenced by stray fields. Neglect of this precaution may, in many cases, result in the introduction of serious errors. Careful note should be made of the way in which the instrument is connected in the circuit so that, when necessary, correction can be made for the losses that occur within the instrument itself.

Tests. Complete tests on transformers ordinarily include:

Ratio,
Polarity,
Resistance,
Iron loss and magnetizing current,
Copper loss and impedance,
Heat run,
Over-potential test,
Insulation test.

Each of the above tests will be considered separately.

Ratio. There is little difficulty in measuring the ratio of large power transformers. The number of turns is comparatively small and almost any convenient voltage may be used. Great care, however, must be used in order to get the correct ratio of small high-voltage transformers, and particularly those to be used with instruments. With the latter it is necessary that the ratio be correct within very narrow limits or the accuracy of the instrument readings will be seriously affected.

In measuring the ratio of small transformers with voltmeters in the ordinary way, it is necessary to apply practically normal voltage in order to secure accurate results. If only a small percentage of normal voltage be used, the drop due to the load of the measuring instrument alone will introduce considerable error. The most satisfactory way of measuring the ratio of such transformers is by paralleling them with standards of known ratio that have been specially designed with a large number of taps covering a wide range with very small steps. With a testing set of this kind, the ratio can be determined with a close degree of accuracy.

Polarity. The polarity of single-phase transformers is easily measured and requires no special precautions.

The polarity of three-phase transformers involves the relations between the phase displacement and the direction of the voltages at the terminals of the high-tension and low-tension windings, and the necessary measurements should be made to determine these relations. This can be best done by connecting one of the high-tension to one of the low-tension terminals and impressing a convenient three-phase voltage across either winding and measuring all the combinations of voltages among the six terminals. From these, and a knowledge of the connections of the windings (whether delta or star), a voltage phase diagram showing the polarity can be constructed easily.

Resistance. The utmost care should be used in measuring resistance in all cases where it is used as a basis for determining the temperature rise. In measuring the cold resistance, it is just as important to know the actual temperature of the winding at the time the measurement is made, so as to get a correct basis from which to calculate the temperature at the end of the heat run.

Where the transformer is standing in the air, several thermometers should be placed in close contact with the coils at several points and, before making the measurement, time enough should elapse so that the temperature of the coils will be within a degree or two of that of the surrounding air. In order to secure the best results, the measurements should be made when the air temperature is steady, or at least showing very little fluctuation.

Where the transformer is in oil, it should be allowed to stand until thermometers show a practical agreement between the temperature of the windings and that of the oil.

In making the measurements, the readings should be taken as quickly as is consistent with accuracy, and the current should be small enough to avoid any appreciable heating of the windings. The temperature of the windings, as shown by the thermometers, should be carefully noted at the same time.

Resistances can be measured most satisfactorily with a Wheatstone or a Kelvin bridge, the former being used for the higher, and the latter for the lower resistance. The resistance to be measured is generally known approximately, so that the bridge can be set fairly close to the correct point beforehand, and the time taken in getting the correct setting, when measuring the resistance at the end of the heat run, can be made very short.

Iron Loss and Magnetizing Current. The iron loss is a function of the frequency, the voltage and the voltage wave form. The frequency and voltage can be determined easily, but this is not true of the wave form.

Since the iron loss may be appreciably decreased or increased, according to whether the voltage wave form is peaked or flattened, it is very desirable to have some satisfactory method of getting the proper correction to be applied to the wattmeter reading, so that the corrected result will be the same as would have been secured if the voltage wave had been of the sine form. It is possible to arrive at this by taking an oscillograph curve of the voltage wave when the iron loss is being measured, and analyzing and comparing it with the true sine form. This is a tedious operation, and one not suitable for commercial testing. A very satisfactory way of making the correction easily and directly is by using the iron loss voltmeter. This instrument gives at once the necessary correction without any calculation, and is therefore exceedingly useful for making iron loss measurements.

If the generator used in measuring the iron loss is large enough so that it is only lightly loaded, and if its voltage wave closely approximates the sine form, there will be very little wave distortion, and the correction to the wattmeter reading will be negligible. In order to make sure of this, however, it is best to use the iron loss voltmeter in all cases.

In a transformer having a large magnetizing current, the I^2R loss in the winding during the iron loss measurement may be appreciable. This loss is constant at all loads, and may therefore be properly included in the iron loss.

Due to the voltage drop in the primary winding, the induction in the iron will be slightly decreased in going from no load to full load, thus tending to decrease the iron loss slightly. On the other hand, the path of the leakage flux about the windings lies partly within the iron, and this may tend to increase the iron loss under load. On the whole, the net difference is negligible, and the iron loss may be considered the same at full load as at no load.

Copper Loss and Impedance. In making copper loss and impedance measurements, care should be taken to see that the frequency is correct and that practically no increase takes place in the temperature of the windings during the measurement.

The frequency affects the eddy current loss in the copper, and also the reactance, which varies directly with it.

Since the copper loss varies with the temperature of the windings, correct results can only be secured by maintaining the temperature at a practically known value during the measurement. This is secured by placing thermometers in close contact with the windings and letting them remain there until they show a steady temperature, and then taking the readings as quickly as possible.

Knowing the temperature of the windings at the time the readings are taken, the copper loss for any other temperature can be calculated with very little error, except where the eddy current loss is very large. In the latter case, it is best to take the copper loss at or near the temperature desired.

Distorted wave form of e.m.f. has very little effect upon copper loss or impedance, unless the distortion is very bad.

Heat Run. The heat run is made chiefly to ascertain the temperature rise under given load conditions.

In getting the temperature rise by resistance, great care must be taken in making the measurements, cold and hot, as discussed under "Resistance." Care must also be used in getting the temperature of the cooling medium or water.

For oil-insulated water-cooled transformers, the surrounding air has some effect upon the cooling, but it may be neglected. The temperature of the ingoing water is taken as the basis for calculating the temperature rise. The source of the water supply is generally such that its temperature remains practically constant during the heat run, so that the principal points to be observed are to see that a thermometer is placed in the ingoing water; that its readings are carefully recorded at regular intervals; that the flow of water is kept constant, and that practically an equal amount of water flows through all parallel coils. At the end of the heat run, the flow of water and the power should be shut off at as nearly the same instant as possible.

For air-blast transformers, the temperature of the air in the pit is the basis for calculating the temperature rise, and it should be maintained at a practically steady value during the heat run and particularly near the end. The dampers should be adjusted to give the proper flow of air, which is generally such as will show a temperature rise of 11 or 12 deg. in passing through the transformer. In shutting down at the end of the heat run, the air blast and the power should be shut off at the same instant.

In making a heat run on an oil-insulated self-cooling transformer, its temperature is determined by that of the surrounding air in the room. The room temperature is always a more or less variable quantity and hard to control, and should therefore be given careful attention. The room should be well ventilated, but strong air currents should be avoided. A steadily rising or steadily falling air temperature towards the end of the heat run introduces an error into the determination of the temperature rise that is not easy to eliminate. A satisfactory way to correct for the error is to have an unloaded transformer standing nearby and use the variation in the resistance of its windings as a basis for the correction.

Oil-insulated self-cooling transformers, when on heat run, should be separated by a space approximately equal to the width of the tanks. Thermometers should be placed about the transformers at a height that can be read conveniently, and far enough away not to be influenced by the radiation of the heat. This will generally require a distance of six feet (1.8 m.) or more from the transformers.

It has been the rule, heretofore, to make a correction of one-fourth of one per cent in the temperature rise, as calculated from the resistance, for each degree of variation of air temperature from the standard of 25 deg. Experience, however, seems to show pretty conclusively that this is in error and that, for all practical purposes, no correction should be made.

Insulation Test. Before making this test, all the terminals of the high-tension winding, and also all those of the low-tension winding, should be connected together. Also, while testing between the high-tension and low-tension or ground, the low-tension should be connected to ground. Otherwise, in testing between the high-tension and ground, dangerous stresses may be set up between the low-tension and ground far in excess of any that may occur in service, and a breakdown may result.

In making tests of about 50,000 volts or more, a spark gap should always be used. Because of the voltage rises that take place, the ratio of primary to secondary voltage of the testing transformer cannot be taken as equal to the ratio of turns, so that, without the spark gap, there is no measure of the actual testing voltage. A high resistance, sufficient to limit the flow of current to a small amount, should be placed in series with the gap.

Air bubbles in the oil about the transformer may be a source of serious trouble, and therefore, if there is any indication of their rising to the surface of the oil, the insulation test should not be applied until they cease. The test should be made immediately following the heat run, while the transformer is warm.

Over-Potential Test. This should be the last test applied. With high-voltage transformers, in particular, care should be taken to see that all terminals, connectors, etc., are in proper place. The frequency used should not be higher than is necessary to keep the magnetizing current within reasonable limits.

It is necessary in this test, also, that there be no indication of air bubbles in the oil.

GROUP III PAPERS
(Pages 649 to 708)

METHOD OF TESTING APPARATUS FOR PERFORMANCE

(a) GENERATORS AND INDUCTION MOTORS

Comparison of Methods of Loading Large A-C. and D-C. Generators and Synchronous Converters for Factory Temperature Test, by F. D. Newbury.

Comparison of Methods of Making Load Tests on A-C. Generators and On Induction Motors, by E. F. Collins and W. E. Holcombe.

Notes on Methods of Making Load Tests on Large Induction Motors. by A.M. Dudley.

(b) TRANSFORMERS

Load Tests on Transformers, by J. J. K. Madden.

Sources of Error in Transformer Tests, by W. M. McConahey and C. Fortescue

Discussion on Group III Papers—(Methods of Testing Apparatus for Performance). New York, February 28, 1913.

(a) Generators and Induction Motors

A. E. Averrett: I would say in regard to the results in the Collins and Holcombe paper, that I made some tests at the same time, and the results obtained with the first two motors (two 20-h.p. motors) were somewhat erratic; one of the motors was somewhat unusual, in that the rotor had a completely closed slot. We ran these tests through in a hurry and I believe the results ought to be discounted.

The 50-h.p. 900-rev. tests show remarkably close results, 24 deg. by the actual load, and 26 deg. by the approximation method. The next test, the 100-h.p., shows very close results, 24 deg. by the full load, and 26 deg. by the approximation at normal load, and at 25 per cent overload it shows 34 deg. rise by load and approximation.

The next machine, 250-h.p., was a machine we selected in the test which we were in a hurry to ship, and we did not test it as carefully as it should have been tested. Before we used this method at all, several years ago, Mr. Collins conducted a number of careful tests, running over six or seven machines, and sufficient time was taken to test them out thoroughly. The agreement between the compromise tests, as we called them—that is, the full-voltage, no-current test, and the full-current, low-voltage test—was very close, and we thought that it was as safe as duplicate tests under approximately full-load conditions. They were remarkably close, and as a result we adopted that method for big machines.

This seems to hold on a wide range of machines, machines with a high core loss and low copper loss, or the reverse.

Commenting on Mr. Dudley's paper, the reverse rotation method; on the first test the normal rise was 25 deg. and on the circulating current method, the reverse rotation method, I believe he calls it, it was 38 deg. There is a considerable discrepancy. That machine, probably, is a machine with a small core loss. It is a wound rotor and undoubtedly the eddy losses in the copper at double frequency must have developed to heat the machines up so much.

In the next table the reverse is the case, that is, the core loss is large; I suppose that should be correct; they have given it different currents—the actual load temperature is quite a little higher than the circulating. In the next group the actual is a little higher than the circulating. In the table which follows, the actual is higher than the circulating, and it seems to me, from my own experience, that this reverse rotation method will hold closely in a machine where the losses are largely copper, and the core loss very small, but in a machine where there are no eddies, and where the core losses are high, I do not see how it can hold. It happens in the average run of machines that the copper loss is quite a little higher than the core loss, and undoubtedly the double frequency in the secondary teeth, even at low density, does give enough additional loss to approximate full-load conditions; but I believe it is only applicable to a very narrow range of machines.

R. B. Williamson: A load obtained by means of synchronous motors running idle, and operating the generator at zero power factor, makes about as satisfactory a compromise test as can be obtained.

In it the conditions are worse than under actual load, because the field current is greater, but it shows what the heating will be under the worst possible conditions. This method can be made to approach actual conditions closer by using alternately leading and lagging currents, but this makes the test more complicated.

Where this test cannot be applied, the next best is the open-circuit test at such over-voltage as to make the total losses in the machine approximately equal to what they would be under load. The distribution of the losses is different, but it gives a test of the field heating, and also some idea as to the core heating under actual load. A test of this kind is especially useful for turbo-generators where most of the loss is in the core, and in windage and friction, and where the stator copper losses are comparatively small. A short-circuit test, by itself, does not show much, unless there happens to be local heating due to stray losses of some kind.

R. E. Hellmund: With regard to induction motors, I agree with Mr. Averrett that the method of testing induction motors with reversed rotation can only give results with machines where the core losses are comparatively low. However, the large majority of machines are of that kind, and the test is so convenient, as compared with others, that I think it should be legalized within certain limitations. We have used it a good deal, and although we know its limitations, we find it a cheap and convenient test for many cases. Altogether, in induction motors, any heat test that takes principally into account the copper losses will be satisfactory in most cases, due to the fact that in all but high-speed machines the core losses play a comparatively small part in the heating, except possibly in motors with very high speeds. In the ordinary run, the core losses are a small percentage of the total losses; but not only that, the seat of the core losses is in such close contact with the frame and other large cooling parts that often a doubling of the core losses in medium- and low-speed machines can hardly be found in the heating, while any additional copper losses appear rather rapidly.

B. G. Lamme: I will say something on the general subject of testing. The manufacturers of electrical apparatus make what might be considered four different kinds of tests. In the first place, a test is sometimes made which is primarily for the purpose of obtaining data for design purposes. Such tests may be made on exceptionally good machines upon which the manufacturer wants special data. These tests are of no particular advantage to the purchaser, or to anyone, in fact, but the designer.

A second kind of test is that made by the manufacturer's engineers, to determine whether the machine meets a specified guarantee. A third set of tests is what might be called a witness test, to prove to the customer that the machine meets the guarantee. The second and third kinds of tests should really be covered by one test, but, in many cases, this apparently cannot be done, as the test which indicates to the designer that the machine is all right does not always appeal to the customer as being a satisfactory test. A designing engineer and the customer's engineer look at the machine from two different view-points. The designer judges the test from his experience with similar kinds of machines upon which he has obtained data, and he bases his opinion partly on experience. On the other hand, the witnessing engineer wants full proof that the individual machine being tested meets the guarantees, although this covers a lot of work which has already been carried out by the manufacturers many times on similar machines. What we would like to get is some recommended standard test or tests which would eliminate a lot of this double testing and the disagreements which naturally accompany it.

A fourth class of tests may be called routine tests. For instance, if a great number of duplicate machines are made, only one of these may be tested completely, while, on the remainder, certain tests are made which show only certain characteristic data, which, compared with the more complete test, indicate that the machine is necessarily a practical duplicate in performance.

Leo Schuler: I want to ask whether it is the intention to make this equivalent test a part of the Standardization Rules. I think that would be rather dangerous. There are natural differences between the results obtained by these equivalent tests and by the real load tests. I think, however, it would be a very good thing for you to make an appendix to your Standardization Rules, in which certain methods of artificially loading machines and transformers are recommended, and in which the probable sources of error are indicated. This would facilitate and strengthen the position of the manufacturer if he could show under the authority of the American Institute of Electrical Engineers that the probable error would not be more than such and such an amount.

B. G. Lamme: I want to say that Mr. Schuler's statement represents exactly the attitude of the Revision Committee on this subject. The committee wishes to recommend, if possible, certain tests as the advisable ones, and to state the disadvantages and probable errors of each, so that in any test that is specified and carried out, it will be known just what errors are liable to be found. At the present time, the difficulty lies in the fact that there is a continual disagreement over these errors in testing. If the fact can be brought out conspicuously that certain errors are inherent in our methods, it would eliminate some of our troubles.

E. I. Chute: The simpler the method of testing the more accurate and consistent will be the results obtained. Results that cannot readily be duplicated are oftentimes worse than useless, as they may lead to erroneous conclusions. The intermittent temperature tests proposed, while very pretty in principle, are quite difficult for a tester to conduct in such a manner as to duplicate results. Unless there is some very decided advantage gained by the suggested methods, and this seems contrary to our experience, the zero power factor test with over-excited field when possible, and the circulating current test in other cases, should be recommended.

Paul M. Lincoln: This question of substitute tests or compromise tests, is an important one to the manufacturer, because the tests, practically all of them, are made in his own shops where the facilities for testing are usually limited when it comes to the question of applying real power to the machines. The amount of power which can be so applied is limited, both on account of the fact that the power may not be available, and also on account of the fact that the method of application, such as is used in the final installation, is not usually available at the time of test. I am in perfect accord with the opinions which have been expressed to the effect that the best substitute test is one where the machine operates at zero power factor. That is the best substitute test, and is certainly one which will give as high temperatures as the machine will give at any other power factor load.

That is all very well for such machines as the manufacturer is prepared to load up in this manner, but the modern machine often goes beyond the ability of the manufacturer to supply the apparatus necessary for this test. When we are dealing with machines of 10,000 to 20,000 kv-a., most manufacturers are not prepared to supply loading-back facilities for so large capacity, and therefore it becomes important to have a test which we can substitute in place of the zero power factor test, and it is the effort to find such a test that has led to the paper by Mr. Newbury and also the paper by Messrs. Collins and Holcombe. The most hopeful line of investigation seems to point to some method of alternate open-circuit and short-circuit operation that will give results in heating, equivalent to the actual load test. The results so far reported would indicate that the alternating cycles of short circuit and open circuit will give a test which is sufficiently close to actual results to make it one which may be safely referred to in our Standardization Rules.

F. D. Newbury: I do not like to disagree with my colleague, Mr. Lincoln, but I must take issue with him in his statement that the intermittent short-circuit or open-circuit test is to be placed next to the zero power factor lagging test. In my own experience I have found the direct-current circulating test to be preferable, for machines for which it is suitable, to any other test. The only limitation to the use of the direct-current circulating test is the presence of solid material in the rotor. There is, of course, a field, which is stationary, with respect to the revolving field, so that any solid material in the revolving part will have rather severe eddy currents generated in it. I think that is the explanation of the very high rotor losses shown in some of the tests given by Messrs. Collins and Holcombe. But that is a limitation that is easily foreseen, and the effect can be directly measured by observing the input to the driving motor with the direct current in the armature, and without the direct current in the armature, so it is a perfectly safe test to apply. You know when you can safely apply it, and when you cannot. The objectionable test is the one in which you cannot predict abnormal conditions and cannot, therefore, interpret results when you obtain them. The difficulty with the intermittent open and short circuit test, I think, is one that Mr. Chute brought out, which is the difficulty of exactly duplicating results and the lack of experience, so far, as to the proper relative open-circuit voltage and short-circuit current. In the large generator we tested, the core temperature was about 10 deg. higher by the intermittent test than by other available methods—the circulating or zero power factor methods. That indicates that the theoretical condition of equal losses is too severe a condition for that method of test, or at least, for certain generators. After it has been found that it gives erroneous results in one generator it raises the question as to its reliability in the next generator, and prevents its adoption at least until we have attained as large a number of years of experience with the method as we have with the other methods of testing.

Leo Schuler: We spoke a good deal yesterday about additional losses, or "stray losses" or "load losses," or whatever you might call them. Several very interesting papers have been presented upon this question, but, nevertheless, no simple and easy method has been suggested for calculating these additional losses for every machine, and calculating them so as to convince the consulting engineer. Even if Mr. Lamme is going to guess this additional loss, I do not know whether the consulting engineers will be convinced by that method. If you make an equivalent test of any kind whatever, it will never be possible to take these additional losses into true consideration, because you do not know them, and this will always be a drawback in these equivalent tests.

A. J. Porskievies: In two-phase machines it is not always feasible to get open delta direct-current heat runs because circuits may not be arranged to provide for circulating currents. Also in single-circuit machines it may be difficult to arrange for circulating currents, both from electrical and mechanical standpoints. There are enough two-phase machines in demand to give this consideration some weight.

Carefully made open- and short-circuit heat runs ought to give reliable results provided the equipment does not permit of the circulating method.

F. D. Newbury: The point in regard to two-phase and three-phase generators is taken care of in either the zero power factor test or the direct-current circulating test. Of course, the method of test is more familiar to us in the case of three-phase machines, because all large machines at the present time are three-phase. The only condition necessary for the application of the direct-current circulating test is that you can form a closed circuit, and open this at a point at which the voltage is zero, for the introduction of the direct current. This can be done with a rectangular connection in a two-phase generator as well as with delta connection in the three-phase, so that the direct-current method is just as applicable to two-phase as three-phase.

As to the point brought out by Mr. Schuler, in regard to the stray losses; while we cannot measure these stray losses, some of these methods do take them into consideration. In the zero power factor method all of the stray losses are present, except the difference due to the different flux distortion at zero power factor and operating power factor. Of course, the distortion is less—strictly speaking, there is no distortion at absolute zero power factor. In some generators, notably some low-speed engine type generators, we can obtain higher temperatures on energy load than at zero power factor.

Leo Schuler: I do not consider the zero power factor method an equivalent. I think it is the real method.

F. D. Newbury. I have no comment, then.

Alexander Gray: Regarding the direct-current circulating test; I used to think it was a very good one until I came across a machine recently which on the test floor had a temperature rise of 40 deg. cent., but, when put in operation, got so hot in the center that the machine had to be rewound. It so happened that the design was faulty, because the machine had deep conductors and a core 32 in. (81.28 cm.) long, and the direct-current circulating power test did not disclose the eddy current losses or the hot spots in the machine. It is for such large machines that we want an equivalent test. Moderate size machines can be tested by the zero power factor method, but large waterwheel and turbine units take a large amount of current from the power house, and must be tested by some other method.

B. A. Behrend: A very convenient test in connection with the testing of multipolar alternating-current generators was suggested by me ten years ago. It consists in the division of the field circuit into two circuits of equal number of poles. Hundreds of these tests have been made on machines of all sizes ranging from 50 kw. to 5000 kw., and they have been satisfactory. The method has been termed the "split-field method," and it is fully described in a paper read before the International Electrical Congress at St. Louis in 1904. It is entirely practicable. For instance, you can take a machine like the 40-pole Manhattan generators, and, by tapping the field circuit in one point, and passing different currents through the two field circuits, you can obtain a zero power factor load on that machine. The heating is very nearly the same as under zero power factor. The method eliminates mechanical, vibration, as the armature reaction balances the strength of the magnetic poles. The regulation corresponds to a power factor of zero. The chief objection to all equivalent tests lies in the proper adjustment of the field excitation. If you want to use the direct-current circulating test you have to know what field excitation to use. If you want to use an intermittent test, a core loss test alternating with a short-circuit test, you must also know what short-circuit current and what excitation to use in both tests, and in order to know that, you require a knowledge of the zero power factor regulation, and a method for deducing from this zero power factor the regulation at other power factors under which you desire to make the equivalent test. With all these difficulties before you, I think you will agree with the chairman in his statement that it would be unwise to embody equivalent test rules in the Standardization Rules, because it would open up all doors to discussion and disagreement. I fear that in order to accomplish anything you will have to adopt Mr. Lamme's method, which I consider, personally, an ideal method, viz., the method of guessing, as I can guess a great deal better than most people can test, and so can Mr. Lamme, but we have difficulty in making others believe that we can do it.

R. B. Williamson: I believe that the short-circuit loss curve is the best indication we have of the various stray losses. These stray losses are due to a number of different effects, such as eddy currents in conductors and currents set up in unlaminated parts, particularly of enclosed machines, such as turbo-generators. Mr. Lamme brought up the point of the stray loss also bearing a relation to the core loss. If a machine has a poor core and a high open-circuit core loss it will be reflected in the short-circuit core loss curve. That is, a machine with a bad open-circuit core loss will also show a bad short-circuit core loss. The short-circuit core loss curve thus takes account of nearly all of these items entering into the stray loss, and as shown by Mr. Foster, the total short-circuit core loss checked up very closely with the measured stray loss.

In most open machines the stray loss is small if the machine is properly designed, and in machines where it is sometimes considerable, such as in turbo-generators, it can be easily shown that part of it exists in certain parts of the casing. It seems to me that if such losses are shown by the short-circuit loss curve they must be present on regular load. I would therefore be in favor of taking the whole of the short-circuit core loss in estimating efficiency as being nearer the truth than the one-third part as recommended at present by the Rules.

F. D. Newbury: No method of loading, as such, will in itself bring out the internal heating; that must be secured by better methods of temperature measurement. I have had very deeply impressed on my mind this fact in connection with some large generators which were tested by the zero power factor method. They came through finely, and everybody thought they were good machines, and yet, when some of the coils were taken out, in order to ship the stator in halves, the interiors of some of the coils were found to be very seriously damaged by heat, so much so that the generators had to be rewound; and, to bring out a point mentioned by Mr. Wilson, a comparison of the short-circuit loss before they were rewound and after they were rewound did not indicate any material difference. It was simply a case where the armature conductors were supposed to have been laminated and insulated from each other, but in a few coils the insulation was defective. Certainly that cannot be detected by a method of loading, or in all cases by a method of temperature measurement.

H. M. Hobart: In substitute methods, one wants to get as nearly as possible to the same heating in each principal part of the machine as one gets when the machine is in regular service and it is carrying its rated load. One wants the same number of heat units per hour, or half hour, or ten minutes, developed in each part as under the conditions of rated load. By the intermittent open-circuit and short-circuit method you get exactly that, provided you make each cycle of operations occupy a sufficiently small number of minutes. In very large machines it is generally arranged to have the complete cycle of operations occupy fifteen or twenty minutes. But you can take even greater intervals and taper them off toward the end of the run into very short periods. In any case, I cannot see that there is any flaw whatsoever in this method, except that alluded to by Mr. Schuler and others, that there is a little uncertainty about the flux distortion under the different conditions, and that difficulty is met in all the other methods and sometimes to a much greater extent than in this method.

If in any case there is any startling discrepancy between the results obtained by this intermittent method and by any other method, the suspicion lies on the other method. However, the intermittent method has the disadvantage of requiring the expenditure of mental effort in planning the test in advance and this has usually served to bring it into disfavor. But certainly the time of an engineer is well spent in mapping out the scheme on paper, and ascertaining the appropriate conditions for the test. As an ultimate standard I cannot conceive of anything better than this test, where the power available is limited.

I do not believe that the difficulties of carrying out this test are nearly so great as is at present thought; it is simply lack of experience and chronic aversion to anything new. I believe it only requires time for this scheme of testing to win out on its merits, but it seems to me a pity to wait for several years, for the natural process of evolution to bring this method to the front. When it was first brought to the attention of engineers, there was a great deal said about the loss of time in switching over from one connection to the other. That can be done practically instantaneously, as is now generally admitted. All sorts of other objections were brought forward, but now it seems to me the only one on which engineers fall back, is the fussiness of making these preliminary calculations, and this objection, it seems to me, is magnified unduly. I trust that careful consideration will be given to the merits of the alternate short-circuit and open-circuit tests.

Leo Schuler: I wish to endorse fully the recommendation which Mr. Hobart made with regard to his method. It is certainly a very excellent, and at the same time a very convenient method, and I can say that this method has been applied to a great extent, in the works with which I was connected. As, however, the short-circuit losses are fully taken into account by the Hobart, test the temperature measured in this way will be somewhat higher than that which could be expected in the real load run; this is, of course, all right for the shop test, but you must distinguish between the shop test and the official test, and if you have not much margin on the temperature allowed, then you would naturally hesitate to make this Hobart test as an official test.

Stuart L. Henderson: The intermittent short-circuit and open-circuit test does not work out very well from a practical standpoint on large machines, and it is on this type that the test should have its greatest application. The chief difficulty is to get reliable temperatures. During the time the machine is on short circuit the iron temperatures decrease and when on open circuit the copper temperatures decrease. This necessitates cutting down the time of the cycle towards the end of run to obtain uniform temperatures, and consequently a man does not have time to get around to read all the temperatures before the cycle is changed. This practically means putting a man on each thermometer to obtain satisfactory results.

J. J. K. Madden: The switching arrangement is not as difficult as would at first appear. Tests were made on a 500-kv-a. unit by the use of two interconnected switches, but it was unnecessary to adjust the field current when applied.

Charles P. Steinmetz: In considering the tests by alternate short-circuit and open circuit, we must realize one feature. In all other tests it is always necessary to make a correction for the field excitation, which is higher or lower than that for full load. We alternate over-voltage runs at open circuit, with over-current runs at short circuit. Thus we have two independent variables, and we are enabled to so select the over-voltage, the over-current, and the time period that not only the total losses during each cycle are equal to the full-load losses, but also the total losses in the field are identical with the full-load losses. Consequently you can obtain the correct field heating as well as the correct armature heating. This test seems to me to have considerable merit, and is often the only practical test available.

In regard to the alleged complexity of switching over to change open circuit to short circuit, and vice-versa, that requires two short-circuiting switches, one across the armature and one across the rheostat in series with the field. To operate two switches is not a very complicated matter. You will not have to adjust the field or effect any other adjustments. These adjustments are made before starting the test. We thus see that the switching operations are extremely simple. But whether the test is generally applicable or not, depends entirely on further investigation. There is a possibility that you cannot adjust the two independent variables at the values necessary to obtaining the correct heating by this kind of test. It may be that the excitation required in order to circulate the appropriate armature current on short circuit will give you a higher field heating than, taken in conjunction with the excitation on open circuit, corresponds to the same total heat per cycle of operations with normal load excitation. There are several limitations which have not yet been fully investigated; for a general method, this test has merits which require careful consideration.

F. D. Newbury: There was one point mentioned by Mr. Steinmetz, which was illustrated in the case of a machine we tested. In order to divide the loads between open circuit and short circuit, so that the total losses on test at any given instant would be equal to the total loss on energy load and also that the watt-hours would be equal to the watt-hours in a test under actual operation, we found that the field losses during the test were 30.65 kw. per hour. The same losses at 100 per cent power factor and full load were 19 kw., and under zero power factor, 46.5 kw., that is, during the intermittent loss the field loss was intermediate between what it would have been at 100 per cent power factor and at zero. In the test we made, I added all the short-circuit losses and the I^2 losses, which may account for the higher temperatures we obtained. But that brings up the point that the method requires some experience in order to determine the proper conditions so that the test results will check with actual load. That same disadvantage is present in all tests, except to a very minor degree in the zero power factor test, and in the zero power factor test it is a condition which can be corrected for with full knowledge.

H. M. Hobart: In connection with a lot of machines which I examined, it was impossible to get the field loss right, when the other two losses were right, but the field loss was not nearly so far out as in the zero power factor test. If the field heating is perhaps 10 per cent higher than under normal conditions, it does not seem to me it is vital, you can easily correct it, whereas you cannot make a reasonable correction of what the field temperature would be in actual practise, when it has been tremendously over-excited as in the zero power factor test.

During the short-circuit period the loss can be different from what it is during the open-circuit period, so long as the loss in each part, in the course of a definite period of time, say one hour, is the same as in actual practise. It may be of interest to state that this intermittent open circuit and short circuit method of testing alternators is not new, but was described by Mr. Franklin Punga and myself in the *Electrical World* for April 22, 1905. I have again drawn attention to it in an article entitled "A Method for Testing the Heating of Large Alternators" in the *General Electric Review* for November, 1911. In 1905 I employed the method in testing some large three-phase generators.

Alexander Gray: It seems to be my misfortune always to get erratic results. In testing induction motors, it is very important to notice whether there is a pulley on or not. I remember particularly a small 20-h.p. motor, running at 1800 rev. per min., which was supposed to drive looms by means of four belts and two pulleys. A motor at that speed is, of course, a small dumpy machine and rather long. That machine, tested with a pulley, rose 42 deg.; tested with two pulleys, one on each side of the machine, it rose 28 deg. cent. I could guarantee that the machine if direct-connected so as to operate without pulleys would have a temperature rise of 50 deg. I want to offer the suggestion that motors should be tested, as far as possible, under conditions under which they are going to be run.

W. J. Foster: What occurred to me at first, at the mention of the pulley, is something I have seen in a great many machines, and this is a movement of the air axially, a movement caused by a mechanical connection simply, a thing even more simple than a pulley, and where the rise in temperature in the machine is due to the fact that that small influence overcomes the natural radial movement that would exist and causes a totally different flow of air, an axial movement. Undoubtedly many of you have run across this trouble. Therefore, when Mr. Gray mentioned placing the two pulleys on the machine, it seemed to me that he then had the motor in approximately the same condition it would be without any pulleys whatever. This may not have been the explanation in that particular case, but in certain cases I have known, it has been the cause of high temperatures.

Alexander Gray: I want to confirm Mr. Foster's remarks about alternators. An alternator is generally put on a heat run during the night and temperatures are taken in the morning. It is often found that when the men come in at seven o'clock in the morning and open the door, the method of ventilation of the alternator is completely changed, and the temperatures also change, so that it is always advisable to take temperatures before the men come in. As to the effect of a belt, the effect might be as follows: In a long machine the stator coils stick out beyond the core for a considerable distance and the tendency is for the cooling air to strike the coils and be drawn back into the rotor again, and so become hotter and hotter. The action of the belt creates the same kind of pressure on one side of the pulley and a suction on the other, and tends to draw the hot air out.

S. S. Seyfert: I would like to make a suggestion regarding the loading of alternators. It seems possible to subject a machine to normal current and voltage conditions without incurring the difficulties encountered in the methods discussed.

I was thinking of a method similar to the so-called pump-back test on transformers. Normal voltage conditions may be obtained by running the machine at the proper speed and excitation and normal armature current conditions, by impressing across the phases, properly connected in series, a reduced voltage of *approximately normal frequency*. The armature currents would have no resultant motor or generator action. The prime mover would supply core and friction loss, and the reduced voltage source would supply the armature copper loss. The increased heating developed when direct current is used on the opened delta should not occur.

In case the armature phases could not be opened so as to be properly connected, a bank of transformers, equivalent in capacity to the largest unit tested, would be required.

Edgar Knowlton: *Open Delta Method.* This test has given very erroneous results when the three-phase armature winding had a pitch differing from $\frac{2}{3}$ and the field was of the laminated cylindrical type. In several tests the temperature rise of the rotor winding was about 100 per cent, and of the armature winding about 20 per cent, greater than that obtained under full load conditions. A solid cylindrical field would doubtless cause still greater temperature rises.

C. J. Fechheimer (communicated after adjournment): The zero power factor method of making heat runs on alternating-current generators is in general the most desirable one, provided the necessary equipment is available. Although this method may give slightly pessimistic results in regard to temperature, especially that of the field coils, it should be remembered that it is difficult to predict the power factor of a system. The effect of lagging currents of lightly loaded induction motors upon reducing the power factor is seldom appreciated. Even though generators be sold for 80 per cent power factor, it is well to give the customer the benefit of every doubt and test his machines at zero power factor. If desirable, we can easily determine what the rotor temperature will be, by taking this to vary as the square of the field currents.

When it is impossible to test synchronous machines at zero power factor, compromise heat runs should be made instead.

Among these is the direct-current open delta heat run, which gives results closely approximating those which would obtain in regard to stator temperature when the corresponding load is applied. With this method the losses in the pole shoes with attendant rise in temperature are not nearly so serious a matter as might appear at first thought, as the magnetomotive force required for the air gap is generally so great that the ampere-turns produced by the current in the delta are correspondingly small. It is often possible and desirable to use a multiple-circuit winding or equivalent, and cause direct current to flow so as to have currents in opposite directions in the same slot, provided the familiar two, layer winding is employed. We would call attention to Mr. Sebastian Senstius's paper entitled *Heat Tests on Alternators*, presented to the Institute in 1906*. We believe it adviable for the Standards Committee to consider methods of making heat runs as described by Mr. Senstius as substitutes for the more desirable zero power factor method. It is our opinion that when making such direct-current heat runs, the excitation should correspond to open-circuit voltage equal to the vector sum of the impedance drop and the terminal voltage. The current could be increased to allow for eddy currents if deep conductors are used, as described by Mr. A.B. Field.** This would then give an equivalent of full-load core loss and circulating currents equivalent to full-load copper loss. It is generally advisable to have an open-circuit heat run in addition, from which latter the field heating at any field current can be determined with considerable accuracy, as a fresh supply of air comes into the rotor (which we are assuming to be the field) and hence the temperature of the fields will not be affected by the stator temperature. The latter is undoubtedly affected by the rotor temperature, as a hot rotor causes warm air to be thrown on the stator. For two-phase machines, we could usually employ multi-circuit arrangements as described by Mr. Senstius.

When the above methods are unwieldy, we may resort to the familiar open- and short-circuit heat runs. We do not favor alternate open- and short-circuit runs of short duration. It is frequently difficult, especially on large machines, to which this method should be particularly applicable, to make such changes rapidly, and unless they are made rapidly the method is hardly desirable. Furthermore, the time of application and the magnitude of the voltages and currents are subject to calculations, such calculations being based to some extent on assumptions, and hence we are liable to be misled as to the proper substitute for true full-load conditions.

For a number of years we have endeavored to establish some relation between the sum of the open-circuit and short-circuit temperature rises and the corresponding load temperature

*Trans. A. I. E. E., Vol. XXV, p. 311.

***Eddy Currents in Large Slot-Wound Conductors*, Transactions A. I. E. E., 1905, Vol. XXIV, p. 761.

rise. In order for the Standards Committee to decide what ruling to give when open- and short-circuit heat runs are made, they should have available many tests for comparison. There are, to be sure, many variables that come in, yet we believe it is possible to give an approximate ruling which may be applied in absence of methods for making more accurate tests.

We shall not take space to show why the load temperature rise should be slightly less than the sum of open- and short-circuit temperature rises. This amount to be deducted we have found to be approximately 5 deg. cent. when the sum is 40 deg. and in general is proportional to the sum. The temperature reading to be taken in short-circuit and open-circuit runs, as well as the load heat run, should be the maximum that any of the several thermometers record, whether they be placed on stator coils or stator iron.

In order to determine the internal voltage we add vectorially the impedance drop to the terminal voltage. The reactance can be measured with the rotor removed and normal current circulated at normal frequency in one phase of the winding; for example, if the machine be star-connected three-phase, the current should be circulated between neutral and terminal. This may not be in entire agreement with statements which others have made, but we have found, to substantiate our statement, that such methods when used for the reactance drop in determining the zero power factor curve give extremely close results.

In regard to heat runs on induction motors when the equipment for making the load tests is not available, the "reversed rotation method" frequently gives accurate results, but we believe this is due to a number of errors cancelling each other by chance, and it should generally not be relied upon. It is well known that the frequency of the currents in the rotor circuit when the motor is operated at 200 per cent slip is double the stator frequency and hence the eddy current loss may be considerable. Therefore the results are liable to be misleading. The method, in our opinion, may be used in combination with the no-load heat run, if the depth of the rotor conductor is not greater than 0.55 in. (13.97 mm.) for 25 cycles, nor more than 0.35 in. (8.89 mm.) for 60-cycle motors. Our experience indicates that erroneous results are obtained if these limits are greatly exceeded.

Usually, we prefer the reduced voltage method for determining the temperature rise due to copper loss, the induction motor being operated until the temperature ceases to rise with the impressed frequency raised above normal (*i.e.*, the motor operates with positive slip as an induction motor) and then to have the test repeated with the frequency reduced a corresponding amount below normal (so that the motor operates with a negative slip as an induction generator). In both cases the motor is operated at normal speed in order that normal ventilation may be secured. The reason for operating at two frequencies is to insure an average temperature which would be approximately the same as obtained with normal eddy current loss in the stator. The higher frequency produces too large and the lower frequency too small an eddy current loss in the stator copper. We should then allow for the temperature rise on open circuit, as Messrs. Collins and Holcombe have done, but whose equation we would modify as follows:

$$T_f = \left(T_r - T_n \frac{E_r^2}{E_n^2}\right) + \left(T_n - T_r \frac{I_n^2}{I_r^2}\right) - T_v$$

E_n = Normal voltage.
E_r = Reduced voltage.
I_n = No-load amperes.
I_r = Reduced voltage amperes.
T_f = Temperature rise on stator for full-load normal volts.
T_n = Temperature rise on stator for no-load normal volts.
T_r = Temperature rise at reduced voltage.
T_v = Temperature rise to be deducted.

" T_v " should follow the same general ruling as applies to the alternating-current generators as indicated above.

We believe it would be well to incorporate in the Standardization Rules some means of allowing for a machine feeding back heated air upon itself. For example, if the machine is placed in a testing pit the walls of the pit return to the machine the air which has been expelled, and thus cause a higher temperature rise than would be secured were the entire machine placed above the floor line. We have observed numerous tests which proved beyond doubt that if air once expelled is fed back into the machine before cooling, the temperature rise will be considerably more than if a fresh supply were fed continually.

(b) Transformers

J. M. Weed: I notice in the paper " Sources of Error in Transformer Tests " by Messrs. McConahey and Fortescue, a reference to the iron loss, as follows: " Due to the voltage drop in the primary winding, the induction in the iron will be slightly decreased in going from no-load to full-load, this tending to decrease the iron loss slightly." I infer from this that the authors of this paper are thinking of the name-plate voltage as the no-load voltage, and that the voltage will be lower than the name-plate voltage at the full-load condition. The voltage will be lower at the full-load condition than at the no-load condition, certainly, but if we consider the name-plate voltage as the no-load voltage, this gives a reduced output to our transformers on the basis of a current rating figured on the name-plate voltage.

A similar opportunity for misunderstanding, on this same point, exists in the paper on " Losses in Transformers " by Mr. Lewis, in his recommendation No. 1, which states that the no-load losses should be measured at rated voltage minus $I\,r$, where I equals rated current, and r equals resistance of primary circuit. Possibly Mr. Lewis does not mean name-plate voltage by rated voltage, but something higher, which includes the transformer drop, and will give the name-plate voltage on the secondary terminals at full load. This, however, would make the rated primary voltage dependent upon the power factor of the load, since the transformer drop depends upon the power factor. It would seem preferable to specify that the no-load loss should be measured at name-plate voltage plus $I r$, where r equals resistance of secondary circuit. The exact value of this correction would depend upon which winding is to be used as secondary (where the per cent $I r$ differs for the two windings) and upon the temperature of the transformer when the measurement is made. The correction will ordinarily be small, in any event, and my own recommendation would be to neglect it, unless for special exaggerated cases, measuring the no-load loss at the name-plate voltage.

I should like in connection with these papers (Group 3) to call attention to my discussion of the papers of Group 2, with reference to the opposition method of determining temperature rise.

C. Fortescue: The paragraph referred to by Mr. Weed brings up the question of whether the rated voltage shall be the no-load secondary voltage or the full-load secondary voltage. The paragraph referred to is true whether we consider the rated voltage as no-load or full-load—there is a voltage drop in the primary winding, and the effective induction, that is, the induction that links the secondary circuit, will be slightly decreased. On the other hand, at certain spots in the iron, the leakage flux will cause the induction to be higher, than even in the case of no-load conditions. That is what I refer to in that paragraph.

In connection with Mr. Madden's paper, I want to say that I agree with the points he brings up as improvements over the methods indicated in my paper on temperature measurement. It is preferable to measure the resistance of the copper at some point during the copper or short-circuit period. The middle points are not necessarily the correct points, but if the complete period is reduced to a short length of time, then the error in taking the middle point of the short-circuit period will be negligible.

J. M. Weed: I would like to emphasize the advantages of the use of the idler in determining temperature rises of transformers, which has been fully dealt with in the papers, but I doubt if the full advantages are appreciated by many here. The idler not only supplies a satisfactory base temperature or equivalent room temperature, but also affords a more accurate method of determining the temperature rise of the transformer, which is brought out in the paper by Messrs. Johannesen and Wade. In the formula which represents the calculations which must be made from the test in order to get the temperature rise of the windings of the transformer, nothing appears except the voltmeter readings. In the determination of rise of resistance, by the ordinary voltmeter-ammeter method, it is necessary not only to get correct results from the reading of two meters, which involves the inaccuracies of calibration as well as the inaccuracies of observation, but you must know the exact temperature of the transformer at the time you are measuring this resistance cold in order to get an accurate temperature reading. That involves many chances of error, when you consider that the temperature rise depends upon the difference between two quantities, which are large with respect to this difference, which is a consideration that is not fully appreciated. In the case of the use of the idle transformer, it is not necessary to know the temperature of the transformer when the initial resistance readings are taken, provided the transformer upon which the heat run is made is at the same temperature as the idle transformer; and, again, the same current passes through both transformers, so that it is not necessary to know the exact value of the current. It is only necessary to know the voltmeter readings. At the end of the run, voltmeter readings are obtained with the same current passing through both transformers, again. In this case, in order to calculate the temperature rise, it is necessary to know the temperature of the idle transformer, but we have every opportunity to get this temperature correctly. This method, I believe, reduces the unavoidable errors in determining the temperature rise to a minimum.

There is one open question, however, as to the accuracy obtained with the idle transformer, and that is as to whether it is actually affected by change of room temperature in the same manner as the loaded transformer. In the loaded transformer we have a circulation of oil due to the load itself which causes all parts of the oil to come in contact with the tank within a short period of time, whereas in the idle transformer the circulation is very sluggish. If the room is warmer than the oil, the oil coming in contact with the tank will rise slowly to the top and stand in a layer, which will gradually increase in thickness. Vice versa, if the room is cooler, the oil in contact with the tank will gradually fall and produce a cool layer in the bottom of the tank, which will gradually build up from the bottom. The average temperature of the oil may not be the average between the top oil and the bottom oil, but depends upon the distribution of temperature within the tank from top to bottom.

M. G. Lloyd (communicated after adjournment): While considering the subject of transformers I want to suggest that a definition of the term "ratio of a transformer" should be included in the Standardization Rules. Heretofore this expression has been used in a variety of senses, not only in a casual way but in printed treatises upon the subject. In looking over the literature one finds that many authors fail to define this term and others use it without any exact significance. The principal meanings attributed to the expression are as follows, in the case of a potential transformer:

1. The ratio of the number of turns in the primary winding to the number of turns in the secondary winding.
2. The ratio of the number of secondary to the number of primary turns.
3. The ratio of the terminal voltages.
4. The ratio of terminal voltages under no-load.
5. The ratio of the induced voltages in the two windings. On account of leakage, this is not the same as the ratio of turns.

No word should be used by technical men which has not a definite meaning, and in view of the great divergence in the usage of this term it seems very desirable that a definition should be included in the rules. This definition should be one which will make the term most useful to the engineer, and unless there is some good reason for departing from the most common practise among those who have occasion to use the term most frequently in an exact sense, this practise should be standardized.

Probably the reason for the previous laxity in definiteness of meaning has been the fact that the expression " ratio " has been largely used in a qualitative sense, and naturally the ratio of turns expresses such a value. With the advent of the instrument transformer, occasion arose for the use of exact quantitative values of the ratios of terminal voltages in the potential transformer and of currents in the series transformer. The time has therefore arrived, indeed it has passed, when a quantitative definition should be standardized.

In connection with this definition there are two principal considerations. One is as to whether the ratio of the primary value to the secondary value shall be used or the ratio of the secondary to the primary. General usage as well as general opinion seems to favor using the ratio of the primary quantity to the secondary quantity, whether this be the number of turns or the electrical magnitude. I will consequently not elucidate the arguments on this point.

The second principal question to be decided is as to whether the ratio of turns or the ratio of electrical magnitudes shall be meant when the word " ratio " is used. Owing to internal resistance and to magnetic leakage, the ratio of terminal voltages in a potential transformer is never quite the same as the ratio of turns. In consequence of this it is customary for the manufacturer to slightly alter one of the windings from the number necessary to give the nominal ratio of turns. The ratio of turns is not indicated on the name-plate and is usually unknown to the user. The name-plate should always, and usually does, tell the ratio of the terminal voltages under some definite condition of use, for this is the ratio of interest to the user. A similar condition exists with regard to series transformers. Here again the ratio of turns is not the value which is of interest and importance to the user, and it is not customary for the manufacturer to state on the name-plate this ratio, but rather the ratio of primary to secondary currents under some definite condition of use.

In the case of the constant-current transformer used with primary on a constant-potential circuit, the ratios of potentials and of currents are of little interest and their values need not be known to the user.

Should the ratio represent a quantity which is fixed by the number of conductors or a quantity which varies with the conditions of use? Should it represent a quantity whose value is secreted in the archives of the manufacturer, or a quantity whose value can be determined by a simple measurement? Should it represent a quantity whose exact value is of importance to the user or a quantity which it is only useful to know approximately?

Inquiry among the men who have occasion to make use of exact quantitative values of ratio discloses a universal preference for defining the term " ratio " to mean the ratio of the primary electrical magnitude to the secondary electrical magnitude. The only point which may really seem to be at issue is as to whether this ratio should be defined as a definite constant quantity for a particular transformer, or whether it should represent a magnitude which may be changed under the conditions of use. For instance, in the case of a potential transformer, shall the ratio mean the quotient of terminal voltages under particular conditions of frequency, secondary load, etc., or shall it be regarded as varying when these and other conditions of use are varied? The question is somewhat similar to that involved in making a distinction between the rating of an electrical machine and its capacity. The capacity is a quantity which varies with the conditions of use, such as room temperature, power factor, etc., but the rating may be so defined as to be a definite quantity for a particular machine, independent of any temporary conditions under which it may be used. To me it seems preferable to regard ratio as a variable quantity, and I therefore suggest the following definitions:

" The ratio of a potential transformer is the ratio of the effective primary terminal voltage to the effective secondary terminal voltage."

" The ratio of a current transformer is the ratio of the effective primary current to the effective secondary current."

It is to be noted that with the above definition the regulation of a potential transformer is the change in ratio between full load and no load, expressed as a fraction of the ratio at no load.

A paper presented at the Midwinter Convention of the American Institute of Electrical Engineers, New York, February 28, 1913.

RATING OF OIL CIRCUIT BREAKERS WITH REFERENCE TO RUPTURING CAPACITY

BY GEORGE A. BURNHAM

There are several ways of rating the rupturing capacity of oil circuit breakers in use at the present time, and when comparing this class of apparatus of different manufacturers considerable explanation and detail are involved. For example, one company will rate the circuit breaker in "ultimate rupturing capacity;" another will specify that the circuit breaker is suitable for use on a circuit of certain characteristics or base the rupturing capacity on the aggregate full-load capacity of all synchronous apparatus.

The rating of oil circuit breakers with reference to rupturing capacity is an important matter and deserves the careful consideration of the Standards Committee.

The purchaser or user of this class of apparatus is interested in how much energy or kilovolt-amperes a particular circuit breaker will safely interrupt. The answer at the best is only an estimate based on familiarity with design, tests and actual service. I believe that most engineers and designers will agree that the proper selection of a circuit breaker with reference to rupturing capacity depends as much on the characteristics of generator, transmission line and translating devices as on the design of the circuit breaker itself. The designer has fixed the characteristic of the circuit breaker, but, on the other hand, has no control over the characteristic of the distribution system, and the characteristics are vastly different in transmission systems of the same kilovolt-ampere capacity.

Rating circuit breakers with reference to the aggregate full-load rating of all synchronous apparatus alone is not sufficient to guide in the selecting of the proper circuit breaker, as the location of the switching equipment, the interposed lines and apparatus are of equal importance.

It appears to the writer that the most definite and clear way of rating oil circuit breakers is to give the maximum "instantaneous " rupturing capacity, meaning by " instantaneous " the elimination of time-limit relays in tripping. This gives the engineer something definite, and one can then judge whether or not the circuit breaker is suited for the particular requirements; and the engineer of the distribution system or central station is in a better position to judge, as a general rule, than the designer, unless many details involving the characteristics of the system are first considered.

If rupturing capacity were rated as " maximum instantaneous " this would eliminate all ratings in reference to non-automatic, cell-mounted, pipe-frame mounting, time-limit tripping, limitations as to reactance, etc., and be confined entirely to the switch itself, which after all is the important factor.

It is fairly well settled that, other things being equal, the rupturing capacity of an oil circuit breaker depends on the head of oil over the break at the starting of the arc, the amount of space above the oil for gas expansion, the shape and strength of the oil tank and its fastenings, and, to some extent, the length and rapidity of contact movement.

An automatic circuit breaker with its tripping features removed becomes a non-automatic circuit breaker, but has its rupturing capacity been altered? Does the application of the time-limit relay or cell construction actually increase the rupturing capacity, or does the introduction of reactance affect the circuit breaker itself? I think most engineers will agree that these factors do not actually affect the rupturing capacity of the switch but change only the character or value of the short-circuit current and at the instant of break tend to limit the kiiovolt-amperes of the circuit to that of the circuit breaker or switch controlling it.

In view of these conditions, I would suggest for the consideration of the Standards Committee that all oil circuit-breaking devices be rated with reference to rupturing capacity on their " instantaneous action."

A paper presented at the Midwinter Convention of the American Institute of Electrical Engineers, New York, February 28, 1913.

THE SPHERE SPARK GAP

BY S. W. FARNSWORTH AND C. L. FORTESCUE

For many years the spark gap method of measuring high voltages has been universally used. No indicating meter satisfactory for commercial uses has yet been developed which will measure the maximum voltage as will the spark gap. Numerous investigators have made calibration curves for the spark gap using needle point electrodes under widely varying conditions. The results obtained under like conditions have checked fairly well over a range of voltage up to about 100,000. Above this voltage it has been very hard to duplicate conditions near enough for different investigators to obtain results which are in agreement.

Since voltages of 100,000 and above have come into use, requiring test voltages of 200,000 and above, the need for some more reliable means of measuring the voltage has been strongly felt. While investigating the dielectric strength of air the authors had occasion to use a sphere gap and were so favorably impressed by the consistent results obtained that the idea soon presented itself of using such a gap to replace the needle point gap.

Those who are daily handling high voltages know well the inconsistency of the needle point gap and others should be convinced of it by the great number of different empirical equations derived by different investigators showing relation between distance of separation and breakdown voltage. Besides giving inconsistent results the needle point spark gap is cumbersome and requires a great deal of space. Section 245 of the A. I. E. E. Standardization Rules specifies the following:

" The spark points should consist of new sewing needles supported axially at the ends of linear conductors which are at least twice the length of the gap. There should be no extraneous body near the gap within a radius of twice its length." For 300,000 volts, the highest voltage given in the A. I. E. E. table, the sparking distance between needle points is 77.4 cm. (30.50 in.). If constructed according to the rule given above, the structure will have the dimensions given in Fig. 1. It is seen that a space 6.19 meters (20.33 ft.) long by 2.9 meters (11.83 ft.) wide and high is required. Three-hundred thousand volts is by no means the highest voltage manufacturers are being called upon to measure, and with increased voltage the problem becomes more difficult to solve satisfactorily. Compare the space required for the needle point gap with the space of 1.22 meters by 1.52 meters by 2.42 meters high required for a 37.5-cm. sphere gap having one end grounded and having a range of 412,500 volts, effective value. Fig. 2 gives the dimensions of such a gap as constructed and used.

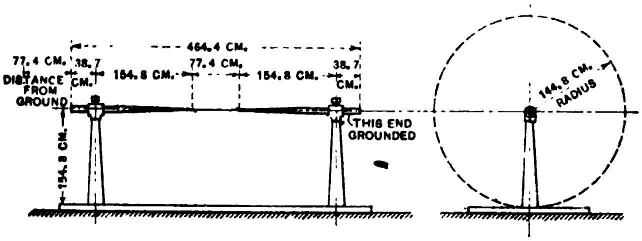


FIG. 1

A sphere gap becomes inconsistent as soon as corona forms preceding break-down voltage. This only happens when the spheres are separated a distance greater than their diameter. When separated less than their diameter a progressive breakdown follows immediately upon the break-down of the film of air at the surface of the sphere.* Consequently a pair of spheres should only be used for measuring voltages up to that necessary to break down the air between them when separated a distance approximately equal to their diameter. With one end grounded, 25-cm., 37.5-cm. and 50-cm. spheres have ranges from 50,000 up to approximately 275,000, 412,500, and 550,000 volts, effective values, respectively. Other sizes have ranges in proportion.

*Alexander Russell and others have given mathematical proof as to why this is so. See "The Dielectric Strength of Air." *The Philosophical Magazine and Journal of Science*, Volume 11, Sixth Series, page 237.

With middle point grounded, 25-cm., 37.5-cm. and 50-cm. spheres have ranges up to approximately 330,000, 440,000 and 650,000 volts, respectively. Other sizes have ranges in proportion.

Fig. 2 gives the dimensions for the 25-cm., 37.5-cm., and 50-cm. gaps. Being constructed vertically, they use a very small floor space as compared with equivalent horizontal needle point gaps. The top sphere is stationary but slightly adjustable in height so as to just make contact with the lower sphere when it is set for zero separation. The lower sphere is mounted on a piece of brass tubing which carries a threaded bushing on its lower end. This bushing works on a carefully threaded rod having a pitch of two per centimeter. The bushing being graduated to fiftieths on its circumference, separation may be measured to the nearest 1/100 cm.

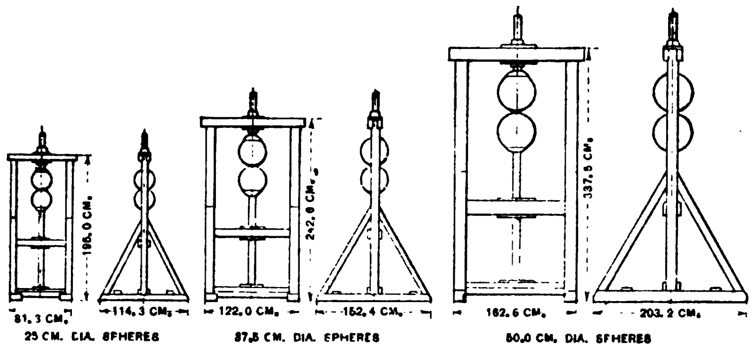


FIG. 2

directly. Thus, there is a micrometer adjustment provided. Being made of large parts, the whole arrangement is mechanically strong and the spheres are kept in constant alignment. Being mounted on large wheels the spark gap sets are very portable and may also be picked up by a crane without risk of damage.

The first pair of spheres 25.4 cm. (10 in.) diameter were made up early in 1910 and immediately became so popular on the testing floor because of their consistency and convenience that the use of the needle point gap was entirely abandoned. These original spheres are still in use and show no sign of surface deterioration due to the arc. Various resistances have been used in series with the gap to limit the current upon break-down. A value of one ohm per volt for the maximum voltage for which the set is to be used has been found to give entire satisfaction and the resistance may be of any convenient form. The proximity of neighboring bodies has been found to have little effect on the break-down voltage of a given gap. The effects of atmospheric pressure and temperature are well known, the break-down voltage varying directly as the pressure and inversely as the absolute temperature. Humidity has a negligible effect.

The sphere gap thus offers a means of measuring high voltages which has the following advantages over the needle point gap:

More consistent, the break-down voltage varying directly a the pressure and inversely as the absolute temperature and being affected only to a negligible degree by humidity, proximity to neighboring bodies, frequency, etc.

More convenient, because the terminals do not have to be renewed, and a micrometer adjustment provides ready means of setting accurately for any separation without adjusting for zero.

Requires much less floor space.

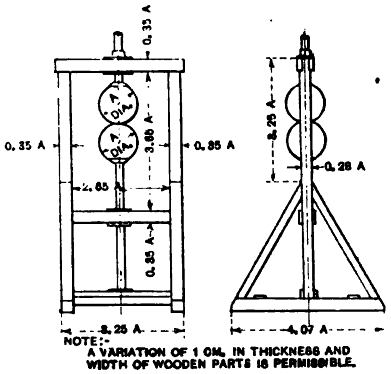


Fig. 3

Is more portable because of its better mechanical construction and smaller size.

In view of the above advantages which have been found to exist during nearly three years' use in commercial testing, the authors feel that the sphere spark gap may be well considered as a standard to replace the needle gap standard.

It is suggested that the following features be incorporated in the Standardization Rules covering the measurement of high voltages:

1. Either one end or the middle point of the high-tension winding should be grounded. In case the middle point is grounded,

the high-tension voltage from one terminal to ground may be measured by the spark gap, and total voltage taken as twice this value.

2. The spark gap shall consist of two accurately machined spheres supported vertically by a wooden frame-work having its dimensions proportional to the diameter of the sphere used, as shown in Fig. 3.

3. The 25-cm., 37.5-cm. and 50-cm. spheres shall be standard sizes covering voltage ranges of 50,000 to 275,000, 50,000 to 412,500 and 50,000 to 550,000 volts, effective values, respectively. The lower sphere shall always be grounded.

For voltages below 50,000 a smaller size of sphere may be used. The authors are not at present in a position to recommend a definite size. It is suggested that the calibration of the standard sizes should be done under the direction of the Standards Committee.

A paper presented at the Midwinter Convention of the American Institute of Electrical Engineers, New York, February 28, 1913.

CALIBRATION OF THE SPHERE GAP VOLTMETER

L. W. CHUBB AND C. FORTESCUE

The breakdown strength of air between spherical terminals has been found to be rather constant, and the sphere gap has been suggested as a standard instrument to be used in the measurement of high voltage. Results obtained with different apparatus by different experiments, however, have not been in perfect accord, and the ultimate dielectric strength of air indicated by calculating the maximum intensity from these tests, has shown some calibrations of the gaps to disagree with results obtained by other methods.

If spheres of sufficient size are used, and if the separation and breakdown voltage are such that there is no corona at the surface of the spheres, it is fair to assume that the complete breakdown of the air gap will occur at the voltage at which the stress due to the intensity at the surface of the spheres corresponds to the ultimate strength of the air. It also seems as though the rupture of a given sphere gap should be independent of frequency of time of voltage application, and that it should depend only upon the maximum value of the voltage impressed, provided that there is no ionization before breakdown which in effect alters the shape of the terminals and changes the dielectric gap.

The purpose of this paper is to present the calibration curves for the three sizes of sphere gaps which have been suggested as standards.*

To be of value in measuring voltage, the sphere gap must be furnished with a calibration curve showing the relation between the breakdown voltage and length of gap. The results of any

*See paper by Farnsworth and Fortescue, p. 733 of this volume.

calibration can be no better than the method used in the test, and unless such calibration is accurate the sphere gap voltmeter is not a desirable standard.

Different methods used to calibrate spark gaps at high voltage show great variations in the relation between voltage and separation of spheres. Some also show a great variation with frequency. It can be shown that such differences are due to conditions of test rather than to any real variation of the sphere gaps themselves.

The most usual method of measuring the high-tension voltage is to measure the primary potential and multiply by the ratio of the transformer. Voltages obtained by this method are generally very much in error, due to the distributed capacity in the high-tension winding of the transformer, harmonic distortions of the applied voltage wave, and the capacity of the terminal bushings and the apparatus to which the high-voltage winding is connected. The effective, or r.m.s., low-tension voltage is usually indicated so that there is no measure of the maximum unless a pure sine wave of voltage is applied, there are no appreciable distortions due to the harmonic components of the exciting currents, and the capacity regulation can be corrected.

The use of a second high-voltage transformer to step down the voltage for measurement with a voltmeter, is an improvement over the straight ratio method, but requires corrections in most cases, and another expensive transformer.

Another method of measuring the high voltage which has been used by the authors is to connect an electrostatic voltmeter of low electrostatic capacity in parallel with one or several sections (at the ground side) of a condenser type terminal. This method corrects the reactive errors of the ratio method but is also an effective reading and gives no indication of wave shape and maximum value.

The calibration of the sphere gaps was thought to be dependent only on the maximum voltage and it was the aim of the authors to obtain the calibrations in terms of maximum of the voltage of the high-tension winding, and then reduce the results to effective values, assuming sine wave shape of voltage.

A very satisfactory method of measuring the maximum of the voltage wave was to rectify the capacity current taken by an air condenser and measure the average value of the rectified current with a d'Arsonval galvanometer. The details of this method will probably be of interest.

Fig. 1 shows diagrammatically the apparatus and circuits necessary to calibrate the gap in terms of the maximum voltage.

The air condenser is shown in Fig. 2. It was constructed of wood carefully turned to dimensions and coated with tin-foil and lead sheeting. The central and high-voltage member was 60 cm. (23.6 in.) in diameter and 458 cm. (15 ft.) long. The outside or ground member with the flared ends had a total length of 240 cm. and internal diameter of 162.8 cm. (5.34 ft.) It was divided into three sections; the middle or working part was 47.7 cm. (18.8 in.) long and the end sections or guard rings were each of equal length in the cylindrical part and were flared with toroidal surfaces having a radius of 47.7 cm. (18.8 in.). The capacity of the central section was figured and found to be $2.657 + 10^{-11}$ farad.

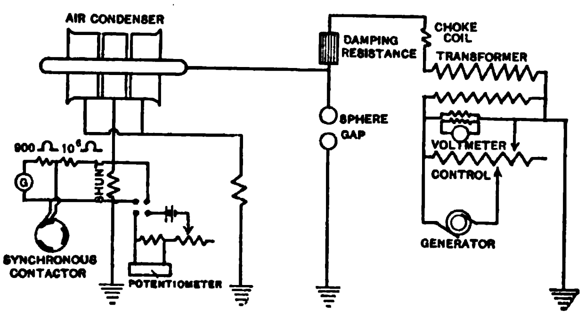


FIG. 1

The central section of the outside member was connected to ground through a non-inductive resistance which served as a shunt to measure its charging current. The guard ring sections were connected together and grounded through a resistance of such a value that the time constants of the center and ends would be approximately the same, and there would be no cause for leakage between the three sections. Across the resistance between ground and the central section, was connected the galvanometer circuit consisting of a megohm of series resistance and a d'Arsonval galvanometer shunted with a synchronous contactor. The contactor was driven by a six-pole synchronous motor and arranged with three equal brass segments which short-circuited two brushes during every alternate half cycle. The brushes were connected to a long lever so that they could be readily shifted in phase. A maximum deflection of the galvanometer in this case indicates commutation at the zero points of the current wave, and this maximum deflection was proportional to the average charging current of the condenser section and also proportional to the maximum voltage impressed upon the condenser.

If Q is the quantity required to charge the condenser to any maximum potential V, the passage of $2Q$ is required to change the potential from $+V$ to $-V$ for the symmetrical periodic charge. While the potential changes from $-V$ to $+V$, the reversal of current is suppressed in the instrument by the shunting effect of the synchronous contactor. The steady deflection of the instrument is therefore caused by a unidirectional pulsating current. The average value of the condenser current(disregarding sign) is equal to the quantity flowing, in coulombs per second.

$$I = 4\,Q f = 4\,C\,V f \tag{1}$$

where C = Capacity of condenser.
f = Frequency.
V = The maximum voltage.
and I = The average value of the condenser current.

The steady current deflection (d) of the galvanometer is

$$d = K\,I \tag{2}$$

where K is the instrument constant in divisions per ampere. From (1) and (2)

$$V = \frac{d}{4\,C\,K f}$$

In all cases the value of K was obtained by applying the battery voltage to the galvanometer circuit while the contactor was running.

In order to make sure that the contactor was making good contact at all times, frequent check tests were made with the contactor running, and still to make sure that currents for equal deflections were in the ratio of 2 to 1. The galvanometer was critically damped so as to obtain quicker readings, and to eliminate errors due to overshooting and swinging which would occur with voltage variations and phase shifts. With the instrument under-damped it was difficult to distinguish between voltage variations and false setting of the brushes. When over-damped the proper position of the brushes for the maximum reading could readily be found, when the voltage was steady, but the response to quick changes in voltage was not sufficient, and it

FIG. 2. [CHUBB AND FORTESCUE]

T—Transformer; C T—Condenser terminals; C C—Choke coil; R—Resistance; S S—Sphere gap; A C—Air condenser; G—Galvanometer.

was impossible to tell whether the breakdown of the gap was due to a surge of voltage or not. Throughout the work, all wiring, instruments, switches, resistances in the circuit, and the contactor were shielded with grounded coverings of tin-foil or wire screen in order to remove static troubles. Serious errors were introduced when any part of circuit was left unshielded, but the strong static field had no bad effects when the shields were properly grounded. There was an appreciable capacity between the high-tension terminal and the ground side of the condenser, and in order to prevent errors due to this additional condenser current, through the measuring shunt, it was necessary to place a grounded network of wires between the condenser and the high-potential circuits. Before each test a high voltage was applied, with the condenser disconnected and short-circuited, to find out if there were any static or electromagnetic troubles which would give a deflection of the galvanometer. If no such troubles were present the high-tension lead was connected to the condenser and tests made as follows:

The gap was opened two or three cm. beyond the breaking point. Voltage was then applied to the condenser and gap. After the maximum deflections of the galvanometer had been carefully observed, the sphere gap was very slowly closed until breakdown occurred. The maximum value of the voltage was then worked out from the galvanometer readings and a direct-current calibration made after each test. The relation between this maximum voltage and the separation of the spheres was then plotted.

In the later tests the contactor was driven with a 30-h.p. induction motor instead of a synchronous motor. By making this change both positive and negative maxima could be observed, and no shifting of brushes was necessary, as the adjustment for maximum deflection was obtained each time the rotor of the motor had slipped a pole pitch. At 60 cycles, the motor slipped less than one revolution (6 poles) per minute, so that with the critically damped galvanometer, accurate observations of the two maxima could readily be made. At lower frequency the per cent slip was of course greater, but the frequency of galvanometer maxima was about the same.

Tests were made at frequencies ranging from 25 to 60 cycles with two high-voltage transformers, each excited from two different sources of power.

The authors hoped to calibrate the 37½-cm. and 50-cm. spheres to 500 kv. (effective) on a third and larger transformer, but lack of time and a great volume of commercial testing delayed these tests.

It was assumed that a good test on the 25-cm. spheres up to the voltage corresponding to a diameter separation could be made on the 300-kv. transformer and the calibrations of the larger gaps obtained by extrapolation and the theory of proportional fields.

If the calibration of the 25-cm. spheres showed a constant surface intensity throughout, and if the larger set agreed within the same range of voltage, such an extrapolation could not be

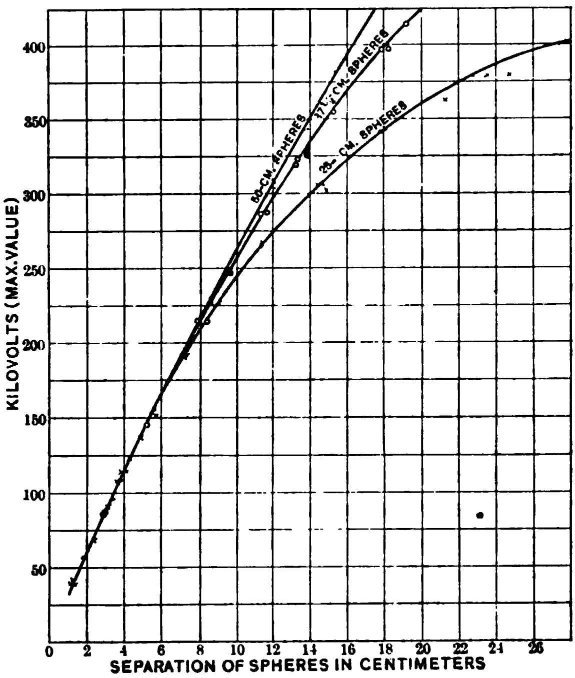


Fig. 3

questioned. The results, however, show increasing intensity with an increase in separation, and show the air between the larger spheres to be apparently weaker than between the small spheres for the same ratio of separation.

The increase of surface intensity with increase in separation has been observed by several experimenters, but not satisfactorily explained. The relative weakness of the air gaps between the large spheres is probably due to the effect of neighboring bodies.

Extraneous objects at constant distances from the gaps will of course weaken the gap between the larger spheres more than that between the smaller spheres. Quantitative tests of the effect of extraneous objects will be made later.

The results of the tests show frequency and wave shape to have no appreciable effect upon the calibration.

TABLE I

25-CM. SPHERES

d_1	d_2	d	E ratio kv.	f cycles per sec.	Gap cm.	$K \times 10^{-3}$	V kv.	Remarks Test No. 36
32	37	69	24.8	25	1.31	658	39.6	
32.5	36.5	69	24.8	"	1.19	"	39.6	
76.5	82	158.5	58.5	"	3.10	"	90.9	
76.5	82.5	159	58.5	"	3.10	"	90.9	
96.6	102.5	199.1	75	"	3.86	"	114	
97	102.3	199.3	75	"	4.07	"	114.3	
98.5	102.2	200.7	75	"	4.06	"	115.1	
131.5	135.5	267	100	"	5.52	"	152.8	
131.5	137	268.5	100	"	5.56	"	154	
118.5	122	240.5	90	"	4.92	"	137.8	Damping not good
106	108.5	214.2	80	"	4.38	"	122.6	brush tightened
92.5	94.5	187	70	"	3.79	"	107.2	
92.5	94	186.5	70	"	3.76	"	106.8	
60.5	60	120.5	47.3	"	2.34	"	69	
49.2	49.6	98.8	34.6	"	1.82	"	57	
								Test No. 38
42	45.5	87.5	100	25	5.71	218	151	
42.5	45.5	87.5	100	"	5.68	"	151	
53.5	57.5	111	125	"	7.29	"	191	
54	57.5	111.5	125	"	7.32	"	192	
63.5	68.5	132	150	"	9.02	"	227	
63.3	68.8	132.1	150	"	9.08	"	227	
74	79.6	153.6	176	"	11.37	"	265	
75	80	155	175	"	11.36	"	268	
85	92.5	177.5	200	"	11.17	"	306	
85	92.5	177.5	202	"	14.67	"	306	
85.5	92.5	178	200	"	14.43	"	307	
95.3	103.5	198.8	225	"	17.80	"	343	
96.2	103.6	199.8	225	"	18.02	"	345	
106.3	114	220.3	251	"	23.56	"	381	
106.2	114	220.2	251	"	24.73	"	381	Surge of voltage
106.2	113.6	219.8	249	"	23.55	"	380	
112.5	121.5	234	264	"	27.85	"	404	
102	110.5	212.5	240	25.2	21.28	"	363	
112	121	233	260	25	27.58	"	402	
105.5	115	220.5	250	"	22.79	"	381	
84.5	93	177.5	200	"	14.88	"	303	
62.3	70.1	132.4	150	25.2	9.20	"	225	
41.5	49	90.5	100	"	5.56	"	154	
41.2	49.5	90.7	100	"	5.66	"	155	

TABLE II

37½-CM. SPHERES

d_1	d_2	d	E ratio kv.	f cycles per sec.	Gap cm.	$K \times 10^{-8}$	V kv.	Remarks
								Test No. 40
25	25.2	50.4	50	25	3.04	214	87.5	
24.6	25.3	49.9	50	"	2.98	"	86.4	
41.4	41.7	83.1	100	"	5.26	"	144.1	
41.5	42.5	84	100	"	5.24	"	145.6	
61	62	123	150	"	7.96	"	215.5	
60.3	62.4	122.7	150	"	8.44	"	214.8	
70.8	73	143.8	176	"	9.61	"	249.2	
70.2	72.5	142.7	175	"	9.70	"	247.5	
81.6	83.8	165.4	201	"	11.38	"	287	
81.8	84.3	166.1	202	"	11.69	"	288	Quick rise of voltage
81.2	84	165.2	201	"	11.99	"	286.6	
91.8	95	186.8	225	"	13.35	"	323.7	
91.6	94	185.6	225	"	13.27	"	321.7	
101.5	104	205.5	250	"	15.40	"	356.3	
101	104.3	205.3	250	"	15.29	"	356	
112.5	116.5	229	275	"	18.12	"	398	
112.5	116.5	229	274	"	17.88	"	398	
118	121	239	286	"	19.18	"	415	
41.2	42.5	83.7	100	"	5.27	"	145.2	

TABLE III

50-CM. SPHERES

d_1	d_2	d	E ratio kv.	f cycles per sec.	Gap cm.	$K \times 10^{-8}$	V kv.	Remarks
								Test No. 45
28.1	26.3	54.4	61	24.75	3.45	214	96.5	
28.1	26.2	54.3	60	25	3.38	"	95	
45.3	43.6	88.9	100	25	5.68	"	155	
45.3	43.7	89	100	25	5.68	"	156	
66	64.1	130.1	150	25.25	8.66	"	226	
65.8	65.2	131	150	24.75	8.66	"	231	
88.6	88	176.6	200	25	12.23	"	308	
88.8	88.1	176.9	201	25	11.99	"	309	Frequency unsteady
88.7	88	176.7	201	25	12.17	"	308	
99.4	99	198.4	225	25.25	13.88	"	344	
99.7	99	198.7	225	25.25	13.80	"	344	
109.4	109	218.4	249	25.25	15.43	"	378	
108.8	109.2	218	249	25	15.32	"	381	Guard ring discharges through shunt
87	89.2	176.2	202	25.25	12.20	"	303	

Fig. 3 shows the calibration curves obtained for each of the three sets of spheres and Tables I, II and III show the results for

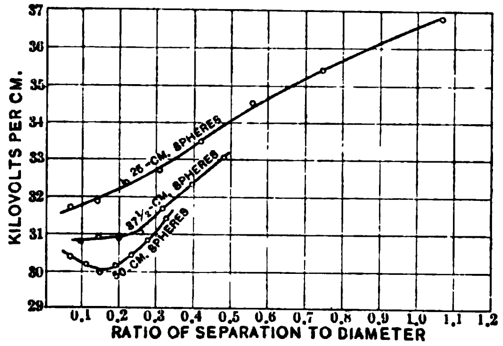


Fig. 4

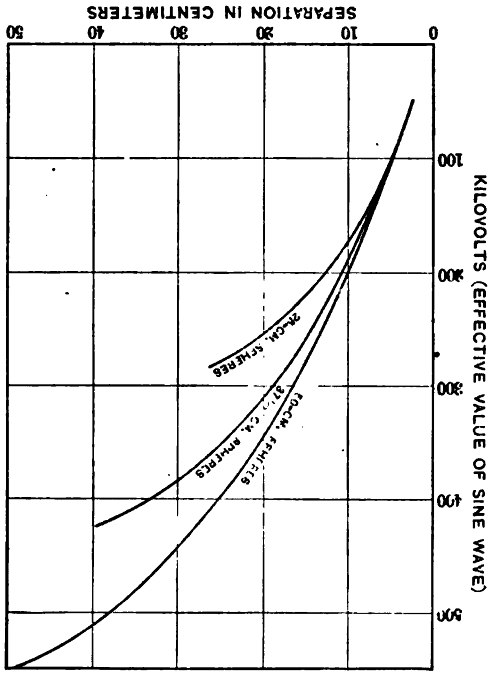


Fig. 5

a 25-cycle test of each. The curves have been drawn weighing the points of many tests. To avoid confusion, only the points of the tests shown in the tables are plotted.

Fig. 4 shows the relation between surface density and the ratio of separation for the three curves of Fig. 3.

Fig. 5 shows the calibration for the three gaps extended and expressed in terms of effective values of a sine wave voltage. These curves have been derived from the curve for the 25-cm. sphere.

The authors regret that limitation of time makes it necessary to show extrapolated curves in place of direct calibration for the high-voltage results. However, the curves will serve as a basis for comparison until the direct calibration can be completed.

A paper presented at the Midwinter Convention of the American Institute of Electrical Engineers, New York, February 28, 1913.

POTENTIAL WAVES OF ALTERNATING-CURRENT GENERATORS

BY W. J. FOSTER

It is not the purpose of this paper to discuss pro and con the advisability of building generators that develop "a sine wave under all conditions of load," as sometimes stipulated in specifications. Suffice it to say that designers and users of alternating-current generators have been content up to the present time with simply a rough approximation to a sine wave.

The purpose of this paper may be said to be three-fold; first, to show some potential waves that have more or less close relation to the evolution of a-c. generators; second, to show how load and other conditions affect the no-load or open-circuit wave; third, to exhibit waves of several generators that have supplied commercial systems, large and small, for many years.

The exhibit, for the most part, pertains to generators that have open slots in the armature and form-wound coils.

In connection with definite pole generators of high periodicity and high voltage, and at the same time of small capacity, or more exactly, small capacity per pole, it is of great importance that poles shall be so shaped that there shall be a sinusoidal distribution of flux in the air gap. The three waves, Curves 1, 2 and 3, showing the potential as affected by shape of pole, were taken many years ago during a study of the problem. The machine was a 60-cycle, 150-kw., 6600-volt, three-phase, belt-driven generator which happened to be available for the purpose. The only changes made were on the pole faces. It is probable that a better wave could have been obtained by an additional trial.

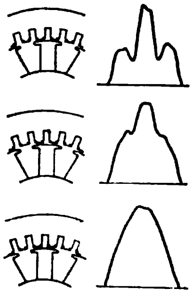
Curves 4 and 5 show what was accomplished on another alternator by a slight change in the curvature of the pole face, *viz.*, a reduction from 8¼ in. (21 cm.) to 7¾ in. (19.6 cm.), of the radius to which poles were turned.

Assuming a pole of proper shape, the fundamental of the potential wave becomes a sine. The problem is then to reduce the harmonics to a minimum. Curves 6, 7 and 8 give typical curves of one slot, two slots and three slots, respectively, per phase per pole for star-connected, three-phase, high-voltage generators. These plainly show harmonics that are caused by the slots in armature. The harmonics due directly to the slots are never lower than of the fifth order in commercial generators, since there are three slots per pole in a one slot per pole per phase, three-phase machine. It is the third harmonic, more especially, that needs to be reckoned with in laying out armature windings. If the wave across each phase itself were a perfect sine with no harmonics, the wave from terminal to neutral in star-connected would be identical with that across terminals, and also with the delta-connected. Few commercial machines approximate this ideal condition.

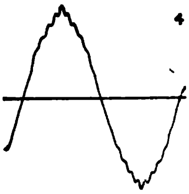
Curve 9 is an example of a poor wave, due largely to the circulating third harmonic current in the closed delta. Curve 10 is taken across the delta when opened at one corner. The wave would be poor on this generator if star-connected. Curve 11 may be taken as representative of a large number of delta-connected commercial generators which have quite good waves when star-connected. Curve 12 shows the possibilities in a delta-connected armature. This belongs to a five slot per phase per pole generator with large air gap and well shaped pole.

Sometimes in star-connected machines the wave across one phase or between terminal and neutral is broad-topped as compared with that across two phases or between terminals (see Curves 13 and 14), in other cases the converse is true.

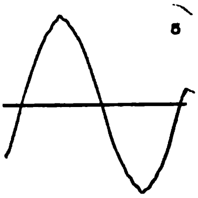
It is evident that the harmonics and, consequently, the resultant or completed wave, will be modified by varying the pitch of the armature winding or by staggering the poles, or by an irregular spacing of the poles. Equally good results have been obtained by making the number of slots in the armature not an exact multiple of the product of poles by phases—a construction much preferable to diagonalling the poles, when considered from the standpoint of mechanical design. A vernier effect is thus established between the armature and the field. This design might be designated the "hunting tooth." Curve 16



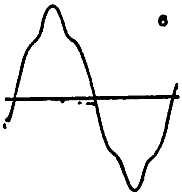
CURVES 1, 2 AND 3.—WAVE FORMS AT NO-LOAD AS AFFECTED BY SHAPE OF POLE.



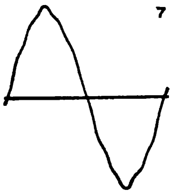
CURVE 4.—POTENTIAL WAVE—NO LOAD.
18 kv-a., 25 cycles, 750 rev. per. min., three-phase, 110 volts, pole face 8½ in. radius.



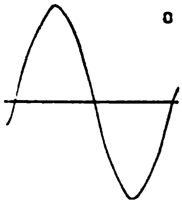
CURVE 5.—POTENTIAL WAVE—NO LOAD.
18 kv-a., 25 cycles, 750 rev. per. min., three-phase, 110 volts, pole face 7¾ in. radius.



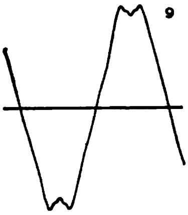
CURVE 6.—POTENTIAL WAVE—NO LOAD.
Three-phase star-connected—one slot per phase per pole, 3000 kv-a., 60 cycles, 6600 volts.



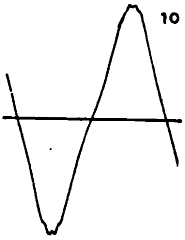
CURVE 7.—POTENTIAL WAVE—NO LOAD.
Three-phase, star connected, two slots per phase per pole, 2700 kv-a., 35 cycles, 13,200 volts.



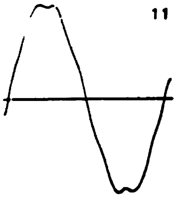
CURVE 8.—POTENTIAL WAVE—NO LOAD.
Three-phase star connected, three slots per phase per pole, 2700 kv-a., 60 cycles, 6600 volts.



CURVE 9.— POTENTIAL WAVE—NO LOAD, ACROSS TWO TERMINALS OF A DELTA CONNECTION.
4200 kv-a., 60 cycles, 720 rev. per. min., three-phase, 2300 volts.



CURVE 10.—POTENTIAL WAVE—NO LOAD, ACROSS SAME TERMINALS AS IN FIG. 9 BUT WITH OPEN DELTA.
4200 kv-a., 60 cycles, 720 rev. per. min., three-phase, 2300 volts.



CURVE 11.—POTENTIAL WAVE—NO LOAD —DELTA CONNECTED
900 kv-a., 25 cycles, 150 rev. per. min., three-phase, 400 volts.

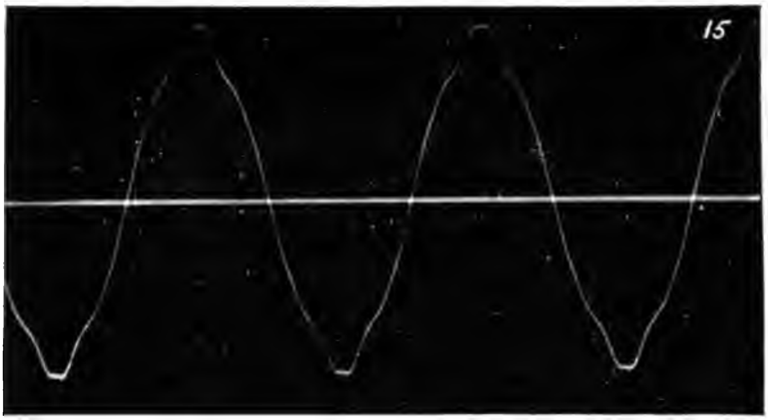
compared with 15 shows what may be accomplished by this scheme. There is no other difference between the two generators to affect the wave except the slot ratio. Curve 17 is another illustration of the good results that have been obtained in this way.

It has been the general practise to connect armature windings indiscriminately star or delta, except in certain designs where the third harmonic is too pronounced. Curves 18 and 19 superimposed, show at a glance the differences that existed in connection with one of the earlier steam-turbine generators that was standard for several years. It is sometimes advisable, when testing a delta-connected armature of a new design, to open a corner of the delta and insert an ammeter to measure the circulating current, or take an e.m.f. wave across the open corner. Curves 20 and 21 are oscillograph records of investigations made on a cylindrical rotor with distributed field winding. Incidentally, these records show the influence of the third harmonic current, when flowing in the closed delta, on the potential wave of the fundamental itself.

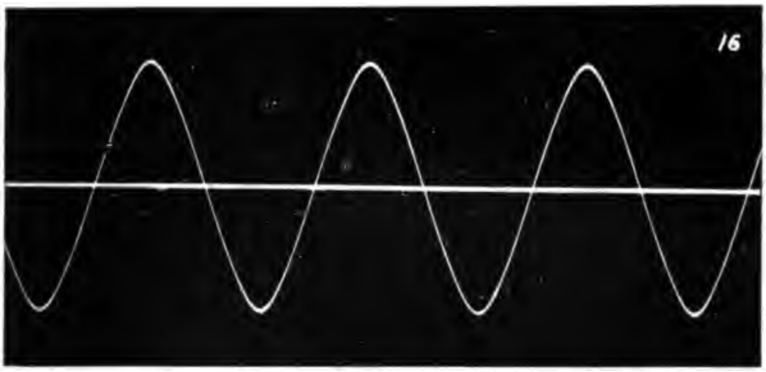
The star-connected generators are not immune from third harmonic trouble in case the neutrals of generators differing in potential wave are grounded without resistance, or with very little resistance. Curves 22 and 23, superimposed for easier comparison, are of 7000- and 11,000-kv-a. generators, respectively, in connection with which the current to ground was so large as to overload the armature windings and make parallel operation impossible with both neutrals direct to ground.

Potential waves are often modified, both under load and no-load conditions, by circulating currents in the pole faces or in amortisseur windings on the field. Curve 24 shows a wave taken on a 13,200-volt synchronous motor with broad open slots, rather small air gap and squirrel-cage winding on the field where the spacing of the bars in the poles was too close to that of the slots in the armature. Curve 25 is the wave with the squirrel-cage winding removed. Curve 26 is the wave with squirrel-cage winding in place and magnetic wedges in the armature slots. Unfortunately this wave had to be taken at about three-fourths voltage, on account of the heat generated in the wedges at voltages approaching the normal. Curves 27 and 28 are examples of good waves of synchronous motors with squirrel-cage windings.

Potential waves taken anywhere that differences of potential

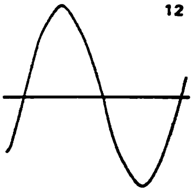


CURVE 15.—POTENTIAL WAVE, NO LOAD. [FOSTER]
150 kv-a., 60-cycles, three-phase generator, one slot per phase per pole.



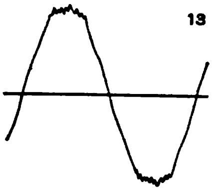
CURVE 16.—POTENTIAL WAVE NO LOAD [FOSTER]
150 kv-a., 60-cycle, three-phase generator, 1½ slots per phase per pole.

CURVE 33.—POTENTIAL WAVE ON EXPLORING COIL IN FIELD. [FOSTER]
800-kv-a., 25-cycle, 3300-volt generator carrying single-phase load.



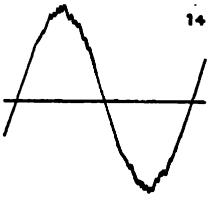
12

CURVE 12.—POTENTIAL WAVE—NO LOAD.
Delta connected, 6000 kv-a., 50 cycles, 250 rev. per. min., three-phase, 2300 volts, long distance transmission in system of 30,000 kw.



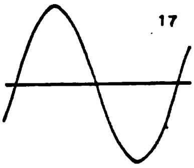
13

CURVE 13.—POTENTIAL WAVE—NO LOAD. ACROSS ONE PHASE.
2400 kv-a., 60 cycles, 360 rev. per. min., three-phase, 11,000 volts. (Amortisseur windings).



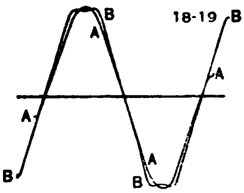
14

CURVE 14.—POTENTIAL WAVE—NO LOAD. ACROSS TWO PHASES.
2400 kv-a., 60 cycles, 360 rev. per. min., three-phase, 11,000 volts. (Amortisseur windings.)



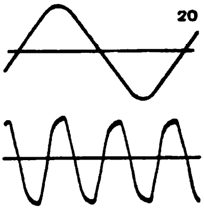
17

CURVE 17.—POTENTIAL WAVE—NO LOAD.
8400 kv-a., 50 cycles, three-phase, 11,000 volts, 3½ slots per phase per pole.



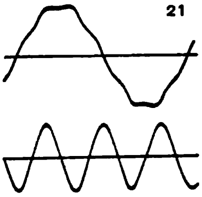
18-19

CURVES 18 AND 19.—POTENTIAL WAVES—NO LOAD
No. 18—Curve A, star connected. No. 19—Curve B, delta connected 2100 kv-a., 60-cycle, three-phase generator.



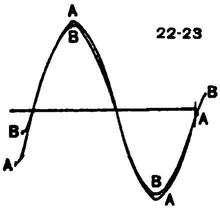
20

CURVE 20 — OSCILLOGRAMS—NO LOAD, OPEN DELTA.
Upper curve, potential across one phase. Lower curve, potential across open delta. 1250 kv-a., 60 cycles, 4000 volts.



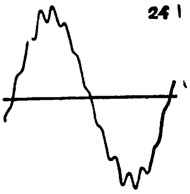
21

CURVE 21. — OSCILLOGRAMS—NO LOAD, CLOSED DELTA.
Upper curve, potential across one phase. Lower curve, potential across closed delta. 1250 kv-a., 60 cycles, 4000 volts.



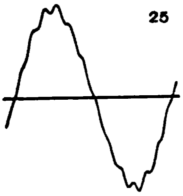
22-23

CURVES 22 AND 23.—POTENTIAL WAVES NO LOAD
Two generators with neutrals grounded, three-phase, 25 cycles, 9000 volts. A, 7000 kv-a., B, 11,000 kv-a.



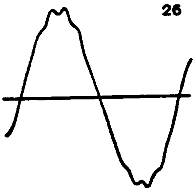
24

CURVE 24.—POTENTIAL WAVE—NO LOAD.
700 kv-a., 60 cycles, synchronous motor, squirrel cage windings, three-phase, 13,200 volts.



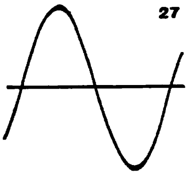
25

CURVE 25.—POTENTIAL WAVE, NO LOAD.
700 kv-a., 60 cycles, synchronous motor, squirrel cage removed, three-phase, 13,200 volts.



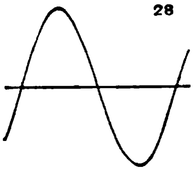
26

CURVE 26.—POTENTIAL WAVE, NO LOAD, AT 9500 VOLTS.
700 kv-a., 60 cycles, synchronous motor, squirrel cage winding three-phase, 13,200 volts, magnetic wedges.



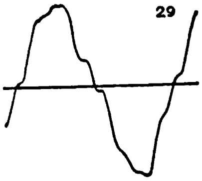
27

CURVE 27.—POTENTIAL WAVE—NO LOAD
90 kv-a., 60 cycles, synchronous motor, with squirrel cage winding, three-phase, 220 volts.



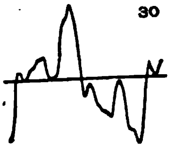
28

CURVE 28.—POTENTIAL WAVE — NO LOAD.
450 kv-a., 60 cycles, synchronous motor, three-phase, 480 volts, squirrel cage winding.



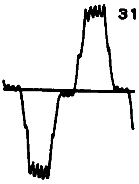
29

CURVE 29.—POTENTIAL WAVE BETWEEN SHAFT AND BEARING.
4500 kv-a., 25 cycles, 2200 volt, two-phase, generator.



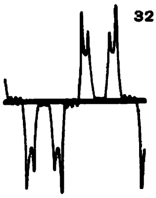
30

CURVE 30.—CURRENT WAVE BETWEEN SHAFT AND BEARING.
9375 kv-a., 60 cycle, three-phase, 5000 volts.



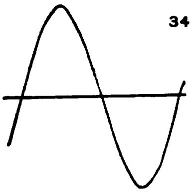
31

CURVE 31—POTENTIAL WAVE ON EXPLORING COIL, SPANNING 12 TEETH — FULL POLE PITCH.
1400 kv-a., 50 cycles, three-phase, 10,000 volts.



32

CURVE 32.—POTENTIAL WAVE ON EXPLORING COIL, SPANNING 1 TOOTH OF 12 SLOTS PER POLE
1400 kv-a., 50 cycles, three-phase, 10,000 volts.



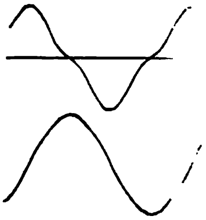
34

CURVE 34.—POTENTIAL WAVE — FULL LOAD UNITY POWER FACTOR
18 kv-a., 25 cycles, 750 rev. per min., three-phase, 110 volts—pole face 7½ in. radius

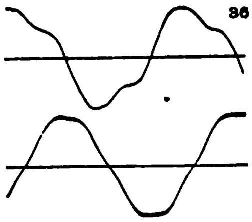
are found to exist are often useful, as are those on special coils made for the purpose. Curve 29 shows potential between shaft and bearing, where currents in the bearings were causing injury. Curve 30 shows current wave on a different machine. Curve 31 is the potential across a special exploring coil located in slots an exact pole pitch apart, or spanning 12 teeth. Curve 32 is the potential between two adjacent slots as determined by a special coil wound around one tooth in the same 12 slots per pole armature. Curve 33 is potential on a special coil located in the innermost slots of a cylindrical rotor (field) of a steam-turbine generator when carrying the single-phase load, and shows the double-frequency e.m.f. induced in the field when single-phase armature reaction is not compensated.

The general effect of load on a generator is to broaden slightly the top of the wave, bending it to the left and making it somewhat unsymmetrical. An inductive load tends to smooth out the harmonics, a condenser load to emphasize them. Curve 34, full-load, unity power factor, should be compared with Curve 5, the no-load wave. Curves 35 and 36 show two curves each, one pair at no load, the other at full load. Curves 37-48, inclusive, show the distribution of the flux in the magnetic field in an 860-kv-a., 60-cycle synchronous motor under different conditions, as generator or motor, at 50 per cent and 100 per cent load, unity power factor, 0.8 lagging and 0.8 leading, the potential waves in all cases being taken on a special coil. These waves are badly saw-toothed or affected by harmonics. This is due to the fact that they represent the potential of a special coil located in two slots only (see Curve 31). The 100 per cent unity power factor load waves are shown on the same oscillograms as the no-load (Curves 37 and 38). This is simply for convenience of comparison. The spacing between the two curves has no relation to the angular displacement produced by load. Curves 49 and 50 are the potential waves at the terminals of the armature winding itself, of the same machine, for the no-load and 100 per cent load, unity power factor conditions. Curves 51 and 52 show the damping effect on harmonics of a unity power factor load in the case of an induction motor tested as generator, where the definite-wound secondary was used for the direct-current excitation.

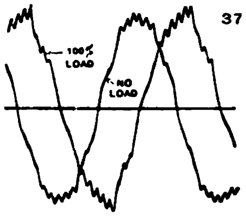
The worst distortion of potential wave is caused by leading current or condenser single-phase load, as a rule. Curves 53 and 54 show the effect of such a load, consisting of underground cable under high-potential test. On the other hand, a poor



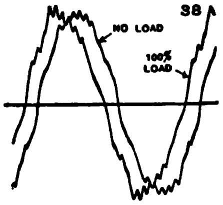
Curve 35.—Potential Waves—No Load.
Upper curve — across one phase. Lower curve —across two phases. 2100 kv-a., 60 cycles, 1800 rev. per. min. three-phase, 2300 volts.



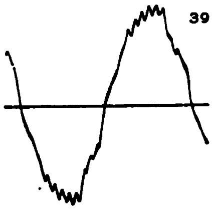
Curve 36.—Potential Waves, — 100 per cent Load — Unity Power Factor.
Upper curve, across one phase. Lower curve, across two phases. 2100 kv-a., 60 cycles, 1800 rev. per. min., three phase, 2300 volts.



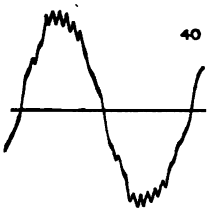
Curve 37.—Potential Waves on Exploring Coil—No-Load, and 100 per cent Load—Unity Power Factor.
860 kv-a., 60 cycles, three-phase, 2300 volts.



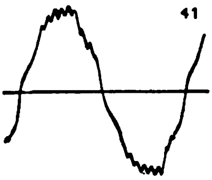
Curve 38.—Potential Waves on Exploring Coil—No Load and 100 per cent Load Unity Power Factor.
860 kv-a., 60 cycles, three-phase, 2300 volts, as motor.



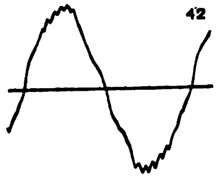
Curve 39.—Potential Wave on Exploring Coil—50 per cent Load —Unity Power Factor.
860 kv-a., 60 cycles, three-phase, 2300 volts, as generator.



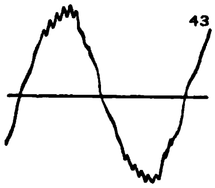
Curve 40.—Potential Wave on Exploring Coil—50 per cent Load —Unity Power Factor.
860 kv-a., 60 cycles, three-phase, 2300 volts, as motor.



Curve 41.—Potential Wave on Exploring Coil—50 per cent Load — 0.8 Power Factor Lagging.
860 kv-a., 60 cycles, three-phase, 2300 volts, as generator.



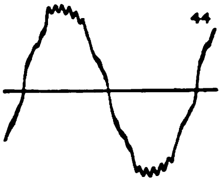
Curve 42.—Potential Wave on Exploring Coil—50 per cent Load — 0.8 Power Factor Lagging.
860 kv-a., 60 cycles, three-phase, 2300 volts, as motor.



Curve 43.—Potential Wave on Exploring Coil—50 per cent Load — 0.8 Power Factor Leading.
860 kv-a., 60 cycles, three-phase, 2300 volts, as generator.

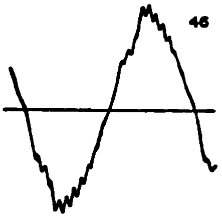
wave at no load may show a pretty good wave with single-phase condenser load. Curves 55 and 56 illustrate this. The necessity for providing compensating windings in the field to obtain good waves with single-phase leading current is shown in Curves 57-59, inclusive. The compensating windings were not connected in for Curve 58. Curves 60, 61 and 62 are interesting as showing to what extent the single-phase double-frequency flux may be eliminated by a squirrel-cage winding serving as compensating coils.

There is considerable deviation from sine wave in many of the synchronous machines in commercial service. Some of them in operation for fifteen or twenty years, have waves as ragged as Curve 55, which was taken on a standard belt-driven, 60-cycle, three-phase generator. Curves 63 and 64 show the no-load potential of two standard 60-cycle, three-phase, 11,000-volt generators of slightly different ratings and speed. Curves 65 and 66 show the waves of two generators, widely different in certain features of design, which are operating in a system of over 100,000 kw. Curve 67 is the wave of an older type of generator operating for several years in parallel with other generators of quite different potential waves in a system of approximately 20,000 kw. The poles of this generator were originally chamfered in a manner similar to that shown in Curve 2. The machine was very noisy when brought to test. The chamfer on poles was changed, with a decided improvement in the matter of noise. Many high-potential synchronous motors with waves showing pronounced harmonics have been operating for years. Curve 68 was taken on a motor where several are in service on a 25-cycle long-distance transmission of approximately 30,000 kw. Approximately 25,000 kw. of motors with waves as in Curve 69 are operating on a 60-cycle system, a long-distance transmission of 35,000 kw. Curves 70 and 71 show waves of star-connected generators that operated in parallel for several years without grounded neutral. Curves 72 and 73 show rather unusual examples of star- and delta-connected generators. Several machines of each type in different places have been in service for nearly ten years. Curve 74 is the wave of a 4800-kv-a. generator in a system of about 25,000 kw. Curve 75 belongs to a plant of 24,000 kw. feeding, through step-up transformers, a 60,000-volt transmission. Curve 76 is the wave of a generator operating with others of different design on a system of approximately 10,000 kw. Curve 77 was taken on a generator operating on a

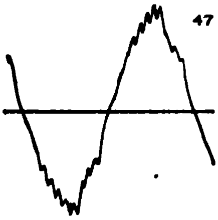


CURVE 44.—POTENTIAL WAVE ON EXPLORING COIL—50 PER CENT LOAD—0.8 POWER FACTOR LEADING.
860 kv-a., 60 cycles, three-phase, 2300 volts, as motor.

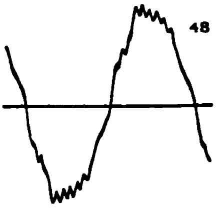
CURVE 45.—POTENTIAL WAVE ON EXPLORING COIL — 100 PER CENT LOAD — 0.8 POWER FACTOR LAGGING.
860 kv-a., 60 cycles, three-phase, 2300 volts, as generator.



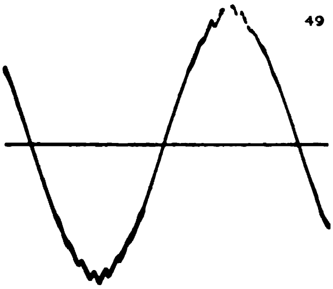
CURVE 46.—POTENTIAL WAVE ON EXPLORING COIL — 100 PER CENT LOAD—0.8 POWER FACTOR LAGGING.
860 kv-a., 60 cycles, three-phase, 2300 volts, as motor.



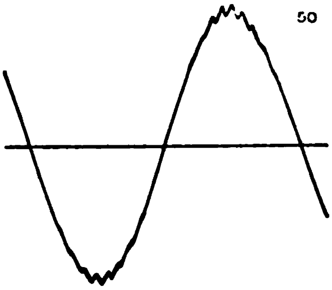
CURVE 47.—POTENTIAL WAVE ON EXPLORING COIL — 100 PER CENT LOAD — 0.8 POWER FACTOR LEADING.
860 kv-a., 60 cycles, three-phase, 2300 volts, as generator.



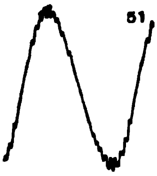
CURVE 48.—POTENTIAL WAVE ON EXPLORING COIL — 100 PER CENT LOAD — 0.8 POWER FACTOR LEADING.
860 kv-a., 60 cycles, three-phase, 2300 volts, as motor.



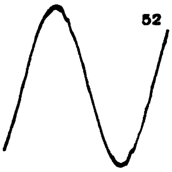
CURVE 49.—POTENTIAL WAVE — NO LOAD.
860 kv-a., 60 cycles, three-phase, 2300 volts.



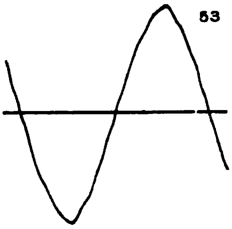
CURVE 50.—POTENTIAL WAVE—100 PER CENT LOAD — UNITY POWER FACTOR.
Motor-generator] combination of a pair of machines. 860 kv-a., 60 cycles, three-phase, 2300 volts.



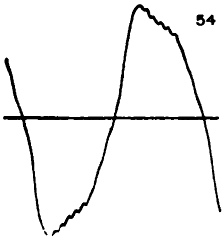
CURVE 51.—POTENTIAL WAVE—NO LOAD.
9 kv-a., 60 cycle, three phase, induction motor, as generator, with d-c. excitation from all three phases of secondary.



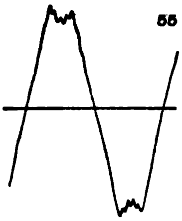
CURVE 52.—POTENTIAL WAVE — 100 PER CENT LOAD.
9 kv-a., 60 cycle, three-phase, induction motor, as generator, with d-c. excitation from all three phases of secondary.



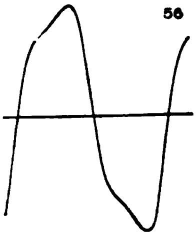
CURVE 53.—POTENTIAL WAVE — NO LOAD.
90 kv-a., 25 cycle, 500 volt, three-phase generator.



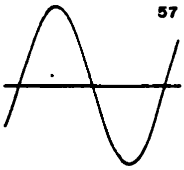
CURVE 54.—POTENTIAL WAVE, SINGLE PHASE, CONDENSER LOAD.
90 kv-a., 25 cycle, 500 volt, three-phase generator.



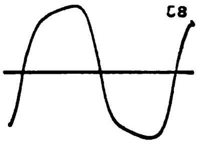
CURVE 55.—POTENTIAL WAVE — NO LOAD.
36 kv-a., 60 cycle, 1150 volt, three-phase generator.



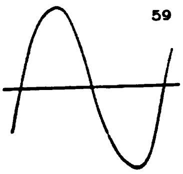
CURVE 56.—POTENTIAL WAVE — SINGLE PHASE—CONDENSER LOAD.
36-kv-a., 60-cycle, 1150-volt, three-phase, generator.



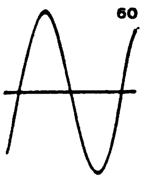
CURVE 57.—POTENTIAL WAVE—NO LOAD.
50 kv-a., 60 cycle, cylindrical rotor, single-phase alternator.



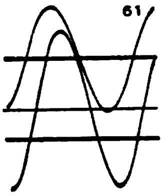
CURVE 58.—POTENTIAL WAVE — CONDENSER LOAD.
50 kv-a., 60 cycle, cylindrical rotor, single-phase alternator—compensating coils in field inactive



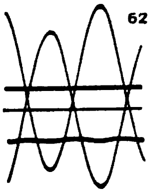
CURVE 59.—POTENTIAL WAVE — CONDENSER LOAD.
50 kv-a., 60 cycle, cylindrical rotor, single phase alternator, compensating coils in field active.



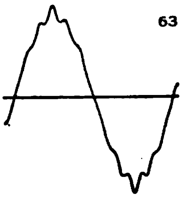
CURVE 60.—POTENTIAL WAVE — NO LOAD.
625 kv-a., 80 cycle, 2100 volt, single-phase generator.



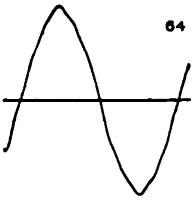
CURVE 61.—OSCILLOGRAMS, UNITY POWER FACTOR, THREE-PHASE LOAD.
Upper curve, armature current. Middle curve, armature voltage. Lower curve, field current. (Upper curve, separate exposure) 625 kv-a., 80 cycle, 2100 volt, single-phase generator.



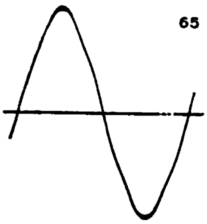
CURVE 62. — OSCILLOGRAMS, 100 PER CENT LOAD, UNITY POWER FACTOR.
Upper curve, armature current. Middle curve, armature voltage. Lower curve, field current.



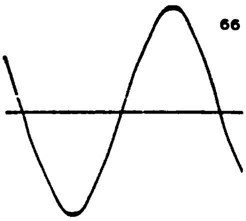
CURVE 63.—POTENTIAL WAVE — NO LOAD.
600 kv-a., 60 cycle, 600 rev. per. min., three-phase, 11,000 volts.



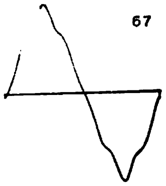
CURVE 64.—POTENTIAL WAVE — NO LOAD.
650 kv-a., 60 cycle, 514 rev. per. min., three-phase, 11,000 volts.



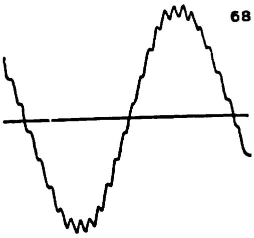
CURVE 65.—POTENTIAL WAVE—NO LOAD.
4500 kv-a., 25 cycle, 250 rev. per. min., two-phase, 2200 volts, external field generator.



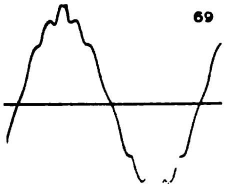
CURVE 66.—POTENTIAL WAVE—NO LOAD.
4500 kv-a., 25 cycle, 250 rev. per min., two-phase, 2200 volt, internal field generator.



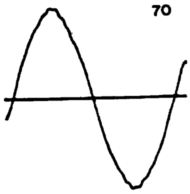
CURVE 67.—POTENTIAL WAVE — NO LOAD.
2400 kv-a., 60 cycle, 106 rev. per min., two-phase, 6000 volts.



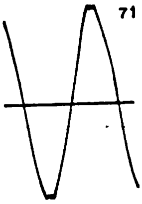
CURVE 68.—POTENTIAL WAVE — NO LOAD.
2100 kw., 25 cycle, three-phase, 13,200-volt synchronous motor.



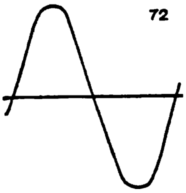
CURVE 69.—POTENTIAL WAVE—NO LOAD.
2100 kw., 60 cycle, three-phase, 11,000-volt synchronous motor.



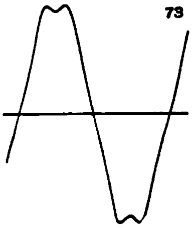
CURVE 70.—POTENTIAL WAVE — NO LOAD.
7000 kv-a., 60 cycle, three-phase, 6900-volt generator.



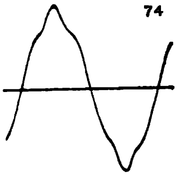
CURVE 71.—POTENTIAL WAVE — NO LOAD.
7000 kv-a., 60 cycle, three-phase, 6900-volt generator.



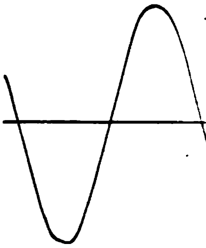
CURVE 72.—POTENTIAL WAVE — NO LOAD.
2800 kv-a., 25 cycle, three-phase, 13,200-volt generator, star connected.



CURVE 73.—POTENTIAL WAVE — NO LOAD.
4200 kv-a., 60 cycle, three-phase, 2300 volt generator; delta connected.

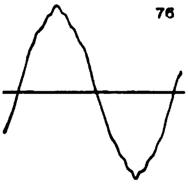


CURVE 74.—POTENTIAL WAVE — NO LOAD.
4800 kv-a., 60 cycle, three-phase, 13,200-volt, generator.



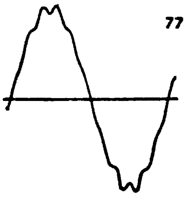
75

Curve 75.—Potential Wave — No Load. 3000 kv-a., 60 cycle, three-phase, 2300 volt generator.



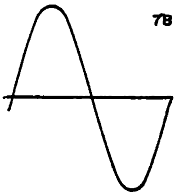
76

Curve 76.—Potential Wave — No Load. 2400 kv-a., 25 cycle, three-phase, . 5000 volt generator.



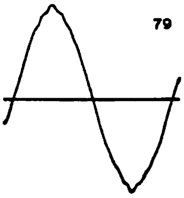
77

Curve 77.—Potential Wave —No Load. 4200 kv-a., 60 cycle, three-phase, 11,000 volts.



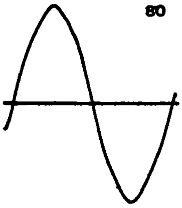
78

Curve 78.—Potential Wave — No Load. 3600 kv-a., 40 cycles, three-phase, 4400 volt generator.



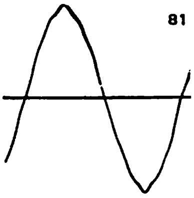
79

Curve 79.—Potential Wave — No Load. 12,000 kv-a., 60 cycle, three-phase, 11,000 volt generator.



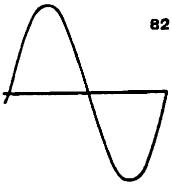
80

Curve 80.—Potential Wave — No Load. 3000 kv-a., 60 cycles, 360 rev. per min., three-phase, 6600-volt generator.



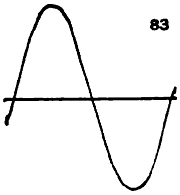
81

Curve 81.—Potential Wave — No Load. 3000 kv-a., 60 cycle, 164 rev. per min., three-phase, 6900 volts.

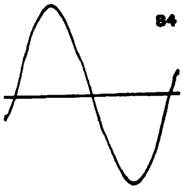


82

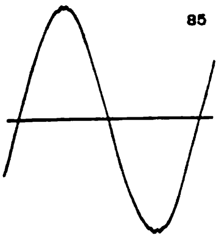
Curve 82.—Potential Wave — No Load. 3000 kv-a., 60 cycle, 133 rev. per min., three-phase, 2300 volts.


83

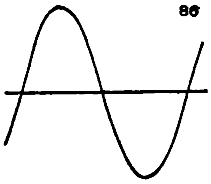
CURVE 83.—POTENTIAL WAVE — NO LOAD. 3600 kv-a., 60 cycles, three-phase, 4000 volts.


84

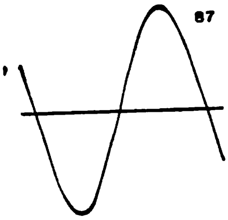
CURVE 84.—POTENTIAL WAVE — NO LOAD. 9000 kv-a., 25 cycle, 375 rev. per min., three-phase, 6600-volt generator.


85

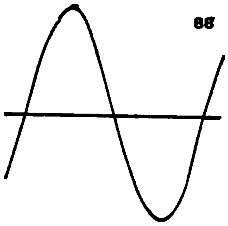
CURVE 85.—POTENTIAL WAVE — NO LOAD. 9000 kv-a., 25 cycle, 750 rev. per min., three-phase, 13,200 volts.


86

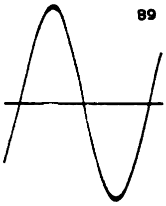
CURVE 86.—POTENTIAL WAVE — NO LOAD. 5555 kv-a., 25 cycle, 1500 rev. per min., three-phase, 13,200 volts.


87

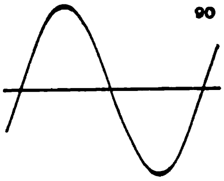
CURVE 87.—POTENTIAL WAVE — NO LOAD. 14,000 kv-a., 60 cycle, 720 rev. per min., three-phase, 4600 volts.


88

CURVE 88.—POTENTIAL WAVE — NO LOAD. 2500 kv-a., 25 cycle, 1500 rev. per min., three-phase, 2300 volts.


89

CURVE 89.—POTENTIAL WAVE — NO LOAD. 9375 kv-a., 60 cycle, 1800 rev. per min., three-phase, 5000 volts.


90

CURVE 90.—POTENTIAL WAVE — NO LOAD. 9375 kv-a., 30 cycle, 1800 rev. per min., three-phase, 7200 volts.

system extending over a vast area with several generating plants. Curve 78 is the wave of a generator in one plant of a 30,000-volt transmission of 15,000 kw. Curve 79 is the wave of a generator in a 50,000-kw. plant stepping up to 100,000 volts. Curves 80, 81 and 82 show waves of 3000-kv-a. 60-cycle generators of three different speeds. Curve 83 shows the wave of a generator of the same design, in most of its parts, as that of Curve 81. Curve 84 is the wave of generators in a large hydroelectric plant.

In connection with cylindrical rotor generators with uniform air gap the distributed field winding serves the same purpose as the shaping of pole in shading the flux in air gap. This class of machines has the advantage of a greater distribution of the armature winding. Curves 85-90, inclusive, are examples of steam-turbine generators of comparatively recent designs. Curve 85 is on a generator at 750 rev. per min. and is not quite so good as Curve 86 on a 1500-rev. per min. generator of the same potential, 13,200 volts, and same periodicity, 25 cycles. Although the latter is of smaller capacity, it has the advantage, in the matter of potential wave, of a greater distribution of windings in both armature and field.

The ratings used in this paper are on the single or continuous rating basis. Consequently, the ratings on the name-plates of the generators do not in all cases agree with those here used.

A paper presented at the Midwinter Convention of the American Institute of Electrical Engineers, New York, February 28, 1913.

WAVE FORM DISTORTIONS AND THEIR EFFECTS ON ELECTRICAL APPARATUS

BY P. M. LINCOLN

While the general question of wave shapes has given rise to comparatively little discussion among operating engineers, it has received considerable attention from those interested purely in the theoretical side of electric engineering. It has been in the past a question that has attracted the theoretical man and student rather than the practical operating man. The reason for this is not far to seek. It lies in the fact that the average operating engineer has but a faint conception of what the shapes of his e.m.f. and current waves are really like. This lack of knowledge is due to two reasons: First, to the fact that there is no ready and inexpensive method of determining wave shapes. Determination of wave shapes requires the use either of an oscillograph or some point-by-point-contact method, either of which is rather expensive, and difficult of operation, as compared to an ammeter, voltmeter, wattmeter, power factor meter, etc., that are every-day tools of the operating engineer. Second, and more important, this lack of knowledge of wave shapes on the part of the operating engineer is due to the fact that he rarely has had reason to suspect that any of his troubles can be traced to wave shape, and whatever is free from trouble-breeding has but little direct interest to him. The only interest, therefore, that attracts the average operating engineer to the study of wave shapes is purely a scientific one, and as a rule this is not a sufficient incentive to cause him to spend the time and money that such a study would entail. The fundamental truth in the old saying "what we don't know, doesn't hurt us" is undoubtedly at the bottom of his indifference in this matter.

This indifference is more or less justified, since there are but few cases on record where distortion of wave forms has actually given rise to difficulties. It is, however, quite possible that wave shape distortion may cause serious trouble in electric circuits, and it is the object of this paper to discuss the question of wave form in some of the aspects that the operating engineer might be expected to meet in the discharge of his regular duties.

The sine form of wave has always been recognized as the standard. The reason for recognizing the sine rather than some other shape of wave as the standard, is sufficiently obvious and so well known as to need no discussion in this paper. Unfortunately, however, not all wave shapes are of the sine form. As is well known, any electrical wave, no matter how much distorted, may be viewed as being made up of a pure sine wave of fundamental frequency upon which are superposed other pure sine waves of a higher frequency, usually called harmonics. That is, any shape of wave may be taken as being made up of a multiplicity of pure sine shapes.

The origin of all the wave shapes which occur in any alternating-current system lies in the shape of the e.m.f. wave of the generators. The shape as determined by the generator may be modified by other synchronous apparatus, as will be indicated later in this paper.

The Standardization Rules of the Institute state that the generator e.m.f. waves shall not depart from the sine shape by more than 10 per cent. The present form of the rule is not entirely satisfactory, since it does not penalize the higher frequency harmonics as much as they deserve. A 10 per cent deviation on the part of one of the higher harmonics is admittedly more dangerous than the same deviation on the part of a lower harmonic, but the existing rule does not recognize this. A modification of the existing rule so as to obtain such a recognition is desirable.

Current waves, as a rule, deviate from the sine form much more than e.m.f. waves. There are various kinds of circuits through which the e.m.f. of a generator may cause a current to circulate. These may be classified as, first, pure resistance; second, pure inductance; third, pure capacity, and fourth, any combination of these three, either in shunt or in series. Arc lighting and rectifier circuits are not considered.

The influence of these various kinds of circuits on the current wave that is caused by a given form of e.m.f. wave will be as follows:

First, a pure resistance will have no effect whatever upon the current wave; that is, the shape of the current wave will be exactly the same as the e.m.f. wave which causes it.

Second, a pure inductance will tend to dampen out the higher harmonics in the current wave. If we have a pure inductance, therefore, we may expect that the current wave will be less distorted than the e.m.f. wave which causes it. The dampening out of the higher harmonics is in proportion to the value of the harmonics. For instance, the third harmonic in the e.m.f. wave will be reduced to one-third value in the resulting current wave, the fifth e.m.f. harmonic, to one-fifth in the current wave, the seventh, to one-seventh, etc. This result follows immediately from the consideration that the impedance offered by a pure inductance increases directly as the frequency.

Third, the effect of a capacity is exactly opposite to that of an inductance. It tends to exaggerate the harmonics in a current wave resulting from a given e.m.f. wave. Also, the amount of the exaggeration is in exact proportion to the order of the harmonic. The third harmonic appears in the current wave as three times its value in the e.m.f. wave; the fifth harmonic, as five times, and the seventh harmonic, seven times, etc. For this reason the current waves that are taken by any condenser, such as, for instance, an empty transmission line, are as a rule very much distorted.

Fourth, a complex circuit consisting of any possible combination of the above simple circuits may have its current wave either smoothed out or exaggerated, depending upon the predominance of the particular kind of impedance contained. The unloaded transmission line is about the only kind of pure capacity that is met with in practical work. All other circuits are made up of resistance and inductance only. Further, in most operating circuits, inductive reactance predominates over resistance, so that there is a tendency to smooth out the current waves. This applies, of course, only to circuits with pure resistance and inductance, which is not the case when synchronous apparatus is in use, nor when saturation of iron circuits exists in the inductance, as, for instance, in transformer magnetizing currents.

The most important case, so far as distortion of wave forms is concerned, is in a circuit which contains synchronous apparatus, such as synchronous motors or converters, and such a circuit is not included in the foregoing classification. Circuits containing pure resistance, inductance or capacity, or complex circuits containing any combination of these, have no inherent wave form of their own; they receive whatever wave form is applied to them and their action in modifying the generator wave form, if any, is an indirect one. All synchronous apparatus, however, has a distinctive wave form of its own which may be very different from the generator wave form, and usually is at least slightly different. Not only is the shape of the current wave affected by the presence of synchronous apparatus, but also the e.m.f. waves of both the generator and the synchronous apparatus. The final wave form is the resultant of the action of all the synchronous apparatus on a given system.

Perhaps the best way of studying this matter is to assume a concrete case and study that case in detail. Let us assume, therefore, that we have a generating system that has a pure sine

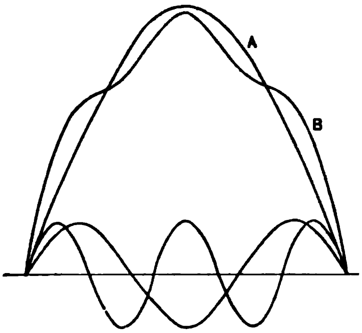


FIG. 1

wave as indicated in curve A, Fig. 1. Let us assume, further, that we have operating on this system a synchronous motor that has a distorted wave form as shown in curve B, Fig. 1. In this case the distortion of wave B is made up by superposing a third harmonic of 10 per cent and a fifth harmonic of 10 per cent on a fundamental sine wave of 99 per cent. When harmonics are superposed on a fundamental the effective value of the resultant wave is the square root of the sum of the squares of the effective values of the various component sine waves. Thus the effective value of curve *B* is

$$\sqrt{(99)^2 + (10)^2 + (10)^2} = 100$$

The effective value of curve *B* is therefore exactly the same as *A*. Let us assume further that the synchronous motor with wave shape B has zero losses and that its field adjustment is such that its voltage before being synchronized is exactly the same as the supply circuit. If it were not for the difference in wave forms, these assumptions would result in no current flow in the motor, since it would oppose an e.m.f. exactly equal and opposite to the supply e.m.f. at every instant. However, with differing wave forms shown in Fig. 1, this is far from the case. Inspection will show at once that there are instantaneous e.m.fs. in the motor wave that are entirely unopposed by the e.m.f. of the generator wave. From our assumptions as to the amounts of wave form differences it follows that the value of the unopposed fundamental frequency is one per cent, and of the third and fifth harmonics 10 per cent each. If there is no deformation of either the generator or motor waves, the only thing that will oppose the flow of current due to these unopposed e.m.fs. will be the impedance of the circuit composed of generator armature, motor armature, leads, cables, etc. The inductive reactance of such a circuit is so large as compared with its resistance that the flow of current is governed by inductive reactance. Currents will therefore flow due to these e.m.fs., and the value of this current is directly in proportion to the amount of this differential e.m.f. and inversely proportional to its frequency. Suppose we assume, further, that normal voltage at fundamental frequency will cause fifteen times full-load current to flow in the motor armature and that the motor is small compared to the system upon which it operates. The differential e.m.fs. between curves A and B will then give rise to a fundamental frequency current of 15 per cent, a third harmonic current of 50 per cent and a fifth harmonic current of 30 per cent, or a total resultant current of over 60 per cent of full-load. Although the assumptions made would result in no current flow if the wave shapes were the same in motor and generator, the assumed difference in wave shapes gives rise to a circulating current of 60 per cent of full-load. Fig. 2 indicates the theoretical shape of the current wave resulting from these assumed conditions. It will be seen that the current wave is made more distorted than either of the voltage waves of which it is the resultant.

A source of considerable error in our theoretical calculations is the assumption of a constant fixed inductance in the armature circuits of both motor and generator. This assumption is not strictly true, since the inductance will vary to a considerable extent, depending on the angular position of the field.

The reluctance of the magnetic path for the flux set up by the armature current varies with the position of the field; sometimes the minimum air gap between pole face and armature is included in the magnetic path and sometimes the much larger gap of the interpolar space is included. This cyclic variation of reluctance may give rise to a still further distortion of the circulating currents.

In the foregoing it is assumed that the inherent wave forms of both motor and generator remain unaffected by the currents that circulate in their armatures. This is far from being an admissible assumption. In the modern synchronous machines, full load current in the armature has some 50 per cent to 100 per cent of the magnetomotive force of normal field, that is, the field that gives normal voltage at no load. It is at once apparent, therefore, that the instantaneous value of the current that circu-

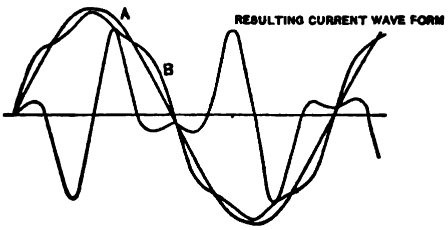


FIG. 2

lates in the armature of a synchronous machine has a very considerable influence over the magnetic flux at that instant and thereby over its wave form. For instance, look at the condition that is shown in Fig. 2. Here the effective value of the current is, as we have seen, 60 per cent of full-load and hence it has a value in setting up magnetic fluxes in the magnetic circuit of the synchronous motor equal to 30 per cent to 60 per cent of the normal field. The actual field strength at any instant is of course the resultant of all the magnetizing forces acting at that instant, and hence the variation in field strength from instant to instant will be very considerable. The direction of this field form variation will of course be such as to oppose the flow of armature current which produces it.

Therefore the inductive reactance of the armature circuit is not the only influence that limits the flow of current due to differences in e.m.f. wave forms. The relative values of these two influences will depend to a large extent upon the construction of the field of motor or generator. As we have seen, superposed harmonics in the armature circuit will tend to cause rapid fluctuations in the magnetic fluxes of our motor. If the construction of the field is such that there is little opposition to these fluctuations, they will extend throughout the entire magnetic circuit. If, however, as is usually the case, the field structure is provided with " dampers," there will be currents set up in these dampers tending to oppose the fluctuation on the magnetic circuit. The net result of the harmonic in the armature current and the current that circulates in the dampers will be a " stationary wave " of magnetomotive force that will tend to superpose, on the normal field form, a harmonic flux of the same order of frequency as that in the armature current. This harmonic in the field flux produces a harmonic in the resulting e.m.f. wave, as

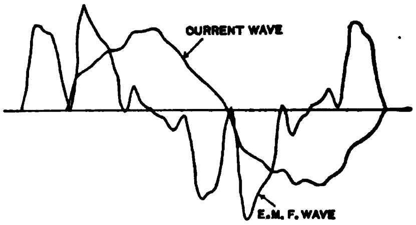


FIG. 3

has been ably shown by Professor C. A. Adams in various papers that he has presented before the Institute.

A striking example of how an e.m.f. wave may be distorted by the effect of armature current recently came to the writer's attention. A 9000-kw. three-phase transformer was under test. The magnetizing current required amounted to some 500 or 600 kv-a. and the iron loss to some 60 or 70 kv. The test was being run from a 750-kv-a. generator. The behavior of the transformer, particularly in regard to noise, led to an investigation of the wave forms. Fig. 3 shows both the e.m.f. and current waves that were taken by the transformer. Fig. 4 shows the open-circuit e.m.f. wave form of the generator. It seems almost unbelievable that this amount of armature current could cause a distortion in the e.m.f. wave from that shown in Fig. 4 to that shown in Fig. 3, but this is an actual determination of wave forms, not a theoretical one.

This 750-kv-a. generator has nine slots per pole and its air gap is only about 40 per cent of the width of one slot. This condition gives rise to quite a noticeable tooth ripple. A careful analysis, however, shows that this wave contains comparatively small harmonics. The third and fifth are approximately two per cent each, and no other harmonic reaches a value of one per cent. The 17th, 19th and 21st vary from 0.6 to 0.95 per cent and apparently make up the ripple.

Subsequent tests on this transformer were carried out on a 5000-kv-a. generator with an open-circuit e.m.f. wave form as shown in Fig. 5. Both e.m.f. and current wave when exciting the 9000-kv-a. transformer are shown in Fig. 6. In this case the generator is so large that the exciting current for the 9000-kv-a. transformer causes practically no distortion.

In this connection, it may be well to call attention to a fundamental difference in the action of an a-c. generator

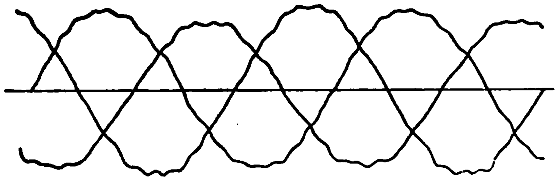


FIG. 4

as compared with a synchronous converter. This fundamental difference must necessarily result in circulating currents of higher harmonic frequencies. An a-c. generator, as we have seen, has a pronounced armature reaction. This armature reaction may be resolved into two components, one of which is demagnetizing to the degree that the generator carries wattless or inductive load, and the other cross-magnetizing to the degree that the generator carries true or real load. The demagnetizing component causes some field distortion and therefore wave form distortion, on account of the fact that the field form caused by the magnetizing action of the armature windings is of a different shape from that caused by the field windings. The cross-magnetizing effect, however, causes a much more marked distortion of the field form, since it causes a marked decrease in magnetic flux at one pole tip and a marked increase at the other. It is the cross-magnetizing effect, therefore, that causes the most pronounced field form distortion and, therefore, wave distortion. This cross-magnetizing effect is entirely absent in synchronous converters, and therefore their wave forms do not become modified due to true load in the same manner as a-c. generators. As the true load on an a-c. generator varies, its wave form may be distorted. There is no equivalent distortion of the wave forms of the synchronous converters with changes in their loads. It follows, therefore, that there must be some

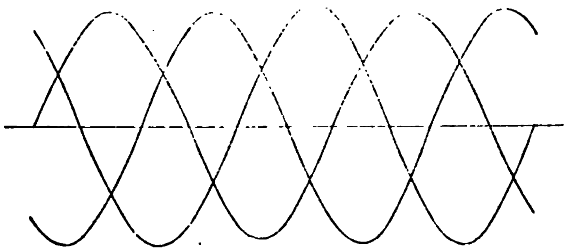


FIG. 5

circulating currents of higher harmonics in order to compensate for this modification in generator wave form.

It is the writer's opinion that the distortions of current waves, particularly in the currents that circulate between synchronous apparatus, are of greater magnitude than ordinarily supposed. It is evident that any distortion in the current wave is reflected to some extent in the e.m.f. wave. There is no particular harm

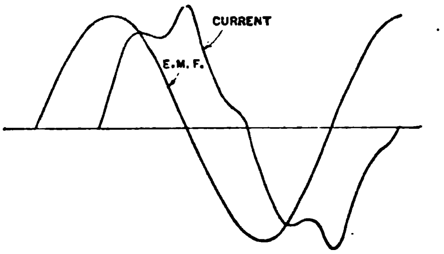


FIG. 6

resulting from these distortions except that the losses are somewhat increased.

The effect of superposed higher harmonics in current and voltage waves so far as the standard types of meters are concerned, deserves some discussion. The standard types of voltmeters, ammeters and wattmeters are not appreciably affected by wave form. This holds true no matter whether the actuating principle is magnetic attraction or heating. Each type integrates over the wave so as to obtain an indication depending on the r.m.s. or effective value.

Frequency meters of the vibrating reed type are unaffected, since they respond to the fundamental frequency only. The type that balances the pull of a current through a resistance against that of a current through an inductance is subject to a slight error, since the current due to the higher harmonics is partially suppressed by the inductance. The greatest error, however, may be expected in the power factor meter. The standard power factor meter is simply a device that measures the angle of lag or lead between a voltage and a current. A moment's consideration will show that this angle may be zero and still the power factor of the circuit be far from unity. Suppose, for instance, we consider the condition shown in Fig. 2. Here we have at no-load a circulating current of 60 per cent of normal full load. Even if we had full load on the motor and adjusted the field so that a power factor meter showed 100 per cent, we would still have the 60 per cent current (or at least that part of it caused by the third and fifth harmonics—by far the larger part), shown in Fig. 2, superposed on the current that represents true energy. One-hundred per cent power factor is that condition where the kilovolt-amperes and the kilowatts are equal—have a ratio of unity—and it is manifest that this condition can never occur unless the e.m.f. and current waves not only have no angular displacement but also are of identical shapes. A power factor meter recognizes the angular displacement but does not recognize differences in wave shape. This is a matter that should be borne in mind when determining power factor with a power factor meter.

In conclusion, we may summarize as follows:

First, actual knowledge of the extent of wave form distortions existing on the average a-c. system is largely lacking. Second, with the exception of empty high-voltage transmission lines, the usual circuit that does not contain synchronous apparatus has a tendency to smooth out the harmonic in a current wave that is caused by a given e.m.f. wave.

Third, when two synchronous machines of different wave forms are operated on the same circuit, harmonics in the current waves are bound to appear and these currents have a marked tendency toward modifying the e.m.f. waves of all the synchronous apparatus through which they circulate.

Fourth, the indication of power factor meters cannot be taken at face value when harmonic currents flow.

A paper presented at the Midwinter Convention of the American Institute of Electrical Engineers, New York, February 28, 1913.

A PROPOSED WAVE SHAPE STANDARD

BY CASSIUS M. DAVIS

The present wave shape standard states that "a maximum deviation of the wave from sinusoidal shape not exceeding 10 per cent is permissible," where the deviation has to be determined by measurements from an oscillogram or a wave form determined from a wave meter.

The objections to this standard may be summarized thus:

1. The use of an oscillograph or a wave meter is required; often neither is available.

2. It is necessary to measure a large number of ordinates on the wave form, from which the equivalent sine wave is calculated.

3. The position of minimum difference between the distorted wave and its sine equivalent must be determined either by repeated trial calculations, or by plotting the equivalent sine wave on a separate sheet which can be applied to the distorted wave and the minimum difference measured. As a rule, in practical work, little attention is paid to the position of minimum difference.

4. The results obtained discriminate in favor of the higher harmonics. For instance, consider a voltage wave that consists of a fundamental and 17th harmonic of 10 per cent amplitude. The equivalent sine wave is almost identical with the fundamental and the deviation is approximately 10 per cent. If this wave be impressed upon a circuit containing dielectric capacity, as in the case of a transmission line, the capacity current does not consist of the fundamental and 10 per cent 17th harmonic, but has the fundamental and 170 per cent 17th harmonic. Again, if a voltage wave consists of a fundamental and 3rd harmonic of 10 per cent, while the equivalent sine wave is still nearly

identical with the fundamental and the deviation is about 10 per cent, yet its capacity current into a condenser consists of 100 per cent fundamental and a 30 per cent 3rd harmonic. In the first instance the effective value of the current is 197 per cent, based on the fundamental of current, while in the second case the effective value is about 105 per cent on the same basis, and in both cases the deviation of the voltage wave is the same. If these same voltage waves be impressed upon an inductive circuit the opposite effect is produced and the currents have a larger effective value when lower harmonics are present. Now, however, the effective values tend to approach 100 per cent and the distortion does not become so important.

To overcome the above objections, a revision of the rule governing permissible wave shape distortion is suggested which shall take into account the various harmonics of a wave in proportion to the maximum effect which they respectively can produce.

The objection noted under (4), relative to the increase of capacity current taken by a condenser, serves as the basis for one way of determining wave shape distortion. The capacity current of a condenser is proportional to the frequency and the applied e.m.f.; thus when a distorted voltage wave, consisting of a fundamental and its harmonics, is impressed upon a condenser, the effective current is greater than for a sine wave of the same effective value, and furthermore, it is greater in a definite proportion the greater the amplitude and the frequency of the harmonics. In other words, the apparent reactance of a condenser is less on a distorted wave than it is on a sine wave. Thus, we may measure wave shape distortion by the ratio of the reactance of a given condenser on a sine wave to the reactance of the same condenser on the distorted wave, and the permissible wave shape distortion may be defined by assigning a definite value to the ratio. This method of determining the distortion takes into account all harmonics in exact proportion to their amplitudes and frequencies, and gives a measure of the maximum effect which a distorted wave can have on a circuit.

Since the harmonics which are liable to occur in electrical apparatus are of smaller amplitude the higher their order, it is possible to say that their probable amplitude is expressed as a/n per cent of the fundamental, where n is the order of the harmonic and a is a constant. If we assign a value to the distortion ratio mentioned above, and assume there are a given number of harmonics present, a has a definite value. It is only necessary then to determine the distortion ratio which is permissible in commercial electrical apparatus to fix a fair value for a. This has been done in a preliminary way and the average of tests on 22 commercial alternators gives a distortion ratio of 1.135, which, assuming there are five odd harmonics present besides the fundamental, gives a a value from 0.242 to 0.246.

TABLE I.
Values of a for the first m consecutive odd harmonics.

δ	m						
	1	2	3	4	5	6	7
1.05	0.342	0.236	0.191	0.163	0.146	0.133	0.123
1.10	0.493	0.340	0.274	0.236	0.210	0.191	0.176
1.15	0.615	0.423	0.341	0.293	0.261	0.237	0.219
1.20	0.724	0.497	0.400	0.343	0.305	0.278	0.255

The constant a has a series of values depending upon which five harmonics are present. The accompanying Table I shows the values which a may have when there are the first m odd harmonics present, and Table II gives the values of a when there are the last m odd harmonics present, assuming the 25th as the highest probable harmonic present in commercial waves.

TABLE II.
Values of a for the last m consecutive odd harmonics, assuming the 25th to be the highest probable harmonic in commercial generating apparatus.

δ	m						
	1	2	3	4	5	6	7
1.05	0.320	0.227	0.185	0.160	0.143	0.131	0.121
1.10	0.459	0.324	0.265	0.229	0.205	0.187	0.174
1.15	0.568	0.402	0.328	0.284	0.254	0.232	0.216
1.20	0.664	0.470	0.383	0.332	0.297	0.271	0.251

NOTE: Attention is called to the fact that for a given number of harmonics the values of a lie within relatively narrow limits, as may be noticed by comparing Tables I and II

Where the distortion ratio is to be determined on alternators, and other apparatus subject to load, it should be measured at no load. It is recognized that any load may change the wave shape; but since its character may vary widely, no definite conditions can be specified under which a load distortion ratio might be taken. With a load of synchronous machines, no specification of the generator wave shape can be made, as it depends more or less on the wave shape of the synchronous motor or converter, and in such a case the wave shapes of the generator as well as the synchronous motor or converter would have to be considered; the wave shape of the circuit being a combination of the two.

On non-inductive load the wave shape is generally rather immaterial; on inductive load it is more sinusoidal, due to the suppression of higher harmonics by the reactance, hence an exaggeration of wave shape distortion over that observed at no-load can be expected only from a load with leading current,

TABLE III.
Amplitudes of harmonics for various values of a. Values are in per cent.

Harmonic	a			
	0.20	0.25	0.30	0.35
3rd	6.67	8.33	10.00	11.67
5th	4.00	5.00	6.00	7.00
7th	2.86	3.57	4.29	5.00
9th	2.22	2.78	3.34	3.89
11th	1.82	2.27	2.73	3.18
13th	1.54	1.92	2.31	2.69
15th	1.33	1.67	2.00	2.34
17th	1 18	1.47	1.76	2.06
19th	1.05	1.32	1.58	1.84
21st	0.95	1.19	1.43	1.67
23rd	0.87	1.09	1.30	1.52
25th	0.80	1.00	1.20	1.40

as a transmission line or cable system. As the proposed method of wave shape specification by the current flowing in a condenser exaggerates the distortion of the no-load wave in the same manner as a leading current load does, the proposed method specifies the no-load wave shape in such a manner as to give weight to the probable distortion which may be expected with a leading current load, and thus appears to make wave shape tests under load unnecessary. It is difficult to load large machines non-inductively, impractical to load them inductively and almost impossible to give them a condenser load. Therefore the proposed method of wave shape specification, which in the no-load wave gives the maximum distortion probable under any kind of load, appears especially suitable.

The apparatus necessary to measure voltage wave shape distortion is very simple. It may consist of a portable set made up of a condenser, a reactive coil and a transformer with various taps, and connected as indicated in Fig. 1. The voltmeter and ammeter readings determine the condenser reactance. The reading is first taken when the double-pole double-throw switch is in position 1, and the reactive coil is in series with the condenser. This gives the sine wave reading, since the reactive coil damps out the harmonics and gives practically a sine wave of impressed e. m. f. on the condenser. Then the reactive coil is cut out by throwing the switch to position 2, and the condenser reactance measured with the distorted wave impressed directly upon the condenser. The ratio of the first reactance to the second gives the wave shape distortion ratio. The transformer is necessary

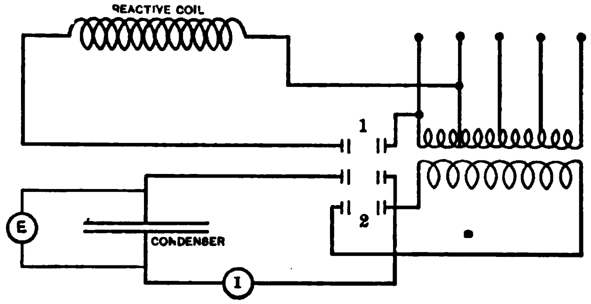


FIG. 1

here in order to give approximately the same voltage across the condenser for either position of the switch.

By a convenient arrangement of switches and transformer taps, a set may be made which could be readily applied to a circuit of any frequency and voltage. The current taken by the set should be less than 5 per cent of the full-load current of the apparatus under test in order not to distort further the wave which is being measured. The reactive coil should be large enough to damp the lowest harmonic, the third, to at least 2.2 per cent of its fundamental, for if it is above this value the distortion ratio would become greater than 1.002, which would introduce an appreciable error.

Of course this method of determining wave shape distortion does not show which harmonics are present, and where this information is necessary the wave form must be found by the oscillograph or the wave-meter, and analyzed.

The accompanying Figs. 2 to 4 show a few of the more distorted waves which have been measured recently by both the present A.I.E.E. method and that proposed in this paper.

In each case the wave has been carefully analyzed and the theoretical and measured distortion ratios compared. Fig. 5 shows a wave which has an exceptionally large distortion ratio.

Using the above as a basis the following are suggested as

TABLE IV

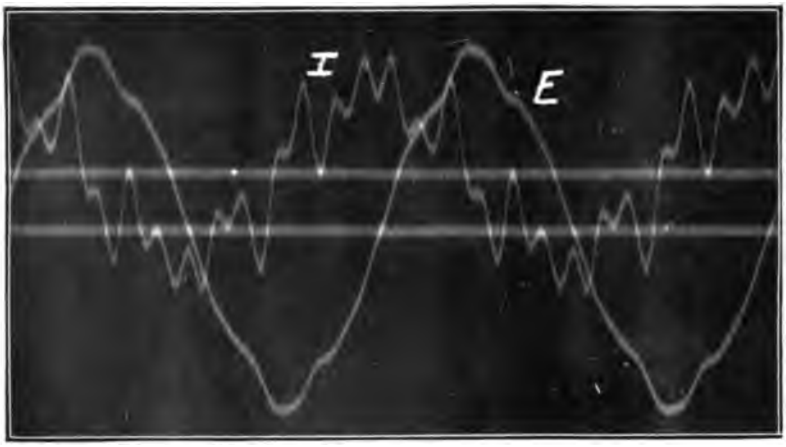
List of alternators upon which the distortion ratio has been measured. Values taken at full voltage and no load. All these machines have a frequency of from 40 to 62½ cycles, and the output is given on a single rating basis.

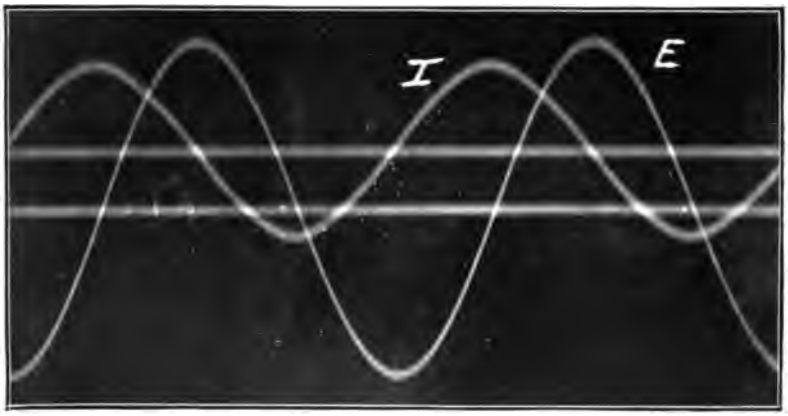
No. of phases	Kv-a. output	No. of poles	Voltage	Ratio
3	1500	24	2300	1.031
3	900	36	2300	1.014
3	500	48	2300	1.135
3	720	60	600	1.010
3	1600	14	2300	1.047
3	1200	48	600	1.017
3	1440	48	2300	1.068
3	700	10	2200	1.030
2	1440	24	2300	1.017
3	390	48	2200	1.111
2	375	12	2300	1.017
3	1350	14	11000	2.360
3	375	28	2300	1.002
3	4500	12	6600	1.090
3	240	18	600	1.036
3	288	10	2300	1.018
3	450	48	600	1.034
3	300	14	2300	1.022
3	675	32	600	1.001
3	300	60	2300	1.673
3	1200	24	2300	1.238
3	1140	30	2200	1.006

substitutes for the present A.I.E.E. Standardization Rules on Wave Shape Distortion:

The wave shape distortion ratio at no-load should not exceed 1.15, except when otherwise specified.

The distortion ratio is determined by impressing the wave form on a suitable condenser, measuring the reactance offered by such condenser to the flow of current and comparing this reactance to the true reactance of the same condenser as measured with a sine wave of current. The latter may be determined by measuring the condenser reactance on any wave shape with an inductive reactance in series, having less than 10 per cent





[C. M. DAVIS]

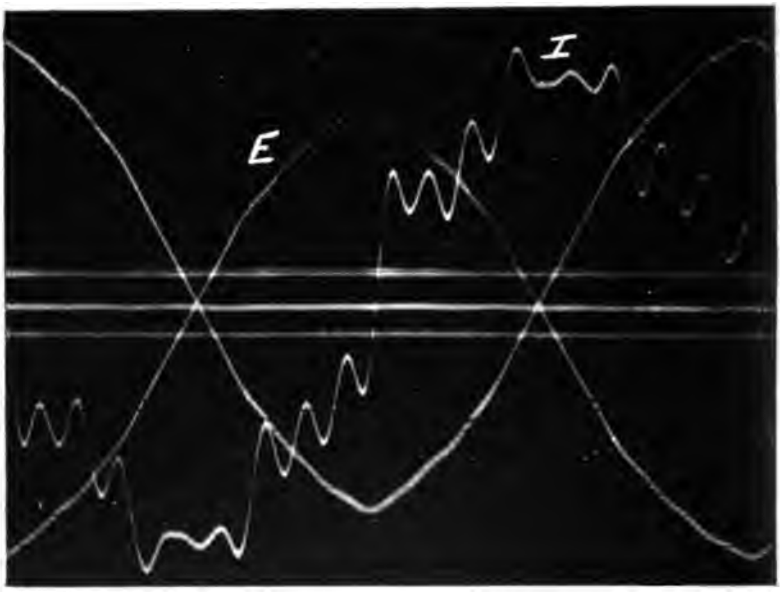
FIG. 2.—WAVE SHAPE OF THREE-PHASE, 500 KV-A., 48-POLE, 2300-VOLT GENERATOR.

Distortion ratio = 1.135. Deviation (by present A. I. E. E. rule) 8.7 per cent.

$$E = 3248 \cos(\theta - 87°) + 172 \cos(5\theta - 87°) + 80 \cos(7\theta - 67°) + 34 \cos(11\theta + 70°) + 39 \cos(13\theta - 43°)$$

$$I = 1.32 \cos(\theta - 83°) + 0.41 \cos(5\theta - 65°) + 0.27 \cos(7\theta - 62°) + 0.11 \cos(11\theta - 27°) + 0.36 \cos(13\theta + 30°)$$

The lower curves show how the same generator voltage wave is smoothed out to nearly sinusoidal shape by the reactive coil in series.



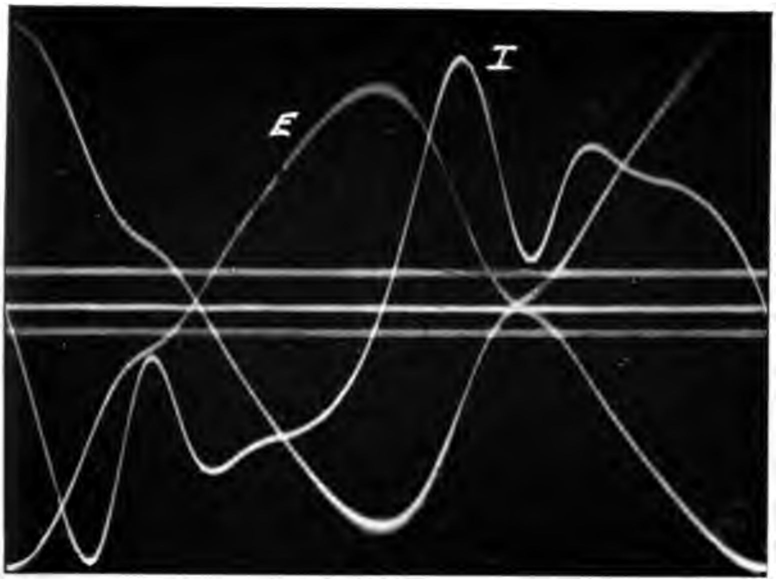
[C. M. DAVIS]

FIG. 3.—WAVE SHAPE OF THREE-PHASE, 45-KV-A., SIX-POLE, 120-VOLT GENERATOR.

Distortion ratio = 1.020. Deviation 5 per cent

$E = 166.7 \cos(\theta - 90°) + 2.95 \cos(5 \quad + 84°) + 2.04 \cos(7\theta + 85°) + 0.80 \cos(11\theta - 67°) + 0.66 \cos(17\theta + 77°) + 0.49 \cos(19\theta - 69°)$

$I = 5.43 \cos(\theta + 87°) + 0.55 \cos(5\theta + 77°) + 0.50 \cos(7\theta + 80°) + 0.37 \cos(11\theta - 68°) + 0.57 \cos(17\theta - 68°) + 0.33 \cos(19\theta - 79°)$



[C. M. DAVIS]

FIG. 4.—WAVE SHAPE OF THREE-PHASE, 10-KV-A., SIX-POLE, 120-VOLT GENERATOR.

Distortion ratio 1.140 Deviation 15.7 per cent

$E = 166 \cos(\theta - 88°) + 22 \cos(3\theta + 87°) + 8.3 \cos(5\theta - 4°) + 2 \cos(7\theta + 31°) + 1.5 \cos(9\theta - 22°)$

$I = 5.41 \cos(\theta + 82°) + 2.04 \cos(3\theta + 85°) + 1.45 \cos(5\theta - 8°) + 0.86 \cos(7\theta - 67°) + 0.23 \cos(9\theta + 55°)$

[C. M. DAVIS]

FIG. 5.—WAVE SHAPE ILLUSTRATING A VERY BAD DISTORTION RATIO

Distortion ratio = 1.92

power factor and consuming a voltage three or four times that at the condenser terminals. The distortion ratio is the fraction

$$\frac{\text{true condenser-reactance}}{\text{condenser reactance on distorted wave}}$$

Measurements of wave shape distortion on apparatus should be made at full voltage and no-load.

APPENDIX

Derivation of Distortion Ratio

Let x_0 = true reactance of condenser
n = order of the harmonic
E_n and I_n = max. values of voltage and current, respectively, of the nth harmonic
E and I = effective values of the distorted wave.
x_c = apparent reactance of the condenser on the distorted wave.
δ = distortion ratio
a = amplitude constant of the harmonics
m = the number of harmonics present above the fundamental.

Then:

$$I_n = \frac{n E_n}{x_0}$$

$$E = \sqrt{\tfrac{1}{2} \Sigma E_n^2}$$

$$I = \sqrt{\tfrac{1}{2} \Sigma I_n^2}$$

$$x_c = \frac{E}{I}$$

and

$$\delta = \frac{x_0}{x_c}$$

If

$$E_n = \frac{a}{n} E_1$$

$$I_n = \frac{n E_n}{x_0} = \frac{a E_1}{x_0}$$

$$I = \sqrt{\tfrac{1}{2} \left(I_1^2 + m \frac{a^2 E_1^2}{x_0^2} \right)}$$

$$= \frac{E_1}{x_0} \sqrt{\tfrac{1}{2} (1 + m a^2)}$$

$$E = \sqrt{\tfrac{1}{2}\left(E_1^2 + \frac{a^2}{n^2} E_1^2 + \ldots \text{to } m \text{ harmonics}\right)}$$

$$= E_1 \sqrt{\tfrac{1}{2}\left\{1 + a^2 \left(\Sigma \frac{1}{n^2}\right)\right\}}$$

$$x_c = \frac{E}{I} = x_0 \sqrt{\frac{1 + a^2 \left(\Sigma \frac{1}{n^2}\right)}{1 + m\, a^2}}$$

and

$$\delta = \sqrt{\frac{1 + m a^2}{1 + a^2 \left(\Sigma \frac{1}{n^2}\right)}}$$

from which

$$a = \sqrt{\frac{\delta^2 - 1}{m - \delta^2 \left(\Sigma \frac{1}{n^2}\right)}}$$

A paper presented at the Midwinter Convention of the American Institute of Electrical Engineers, New York, February 28, 1913.

THE EXPERIMENTAL DETERMINATION OF THE REGULATION OF ALTERNATORS

BY A. B. FIELD

The section of our Standardization Rules* which deals with alternator regulation has remained in force practically unchanged for the past ten years. When this was originally drawn up, it was generally customary to specify the regulation for non-inductive loads, and for this condition, the suggestions contained in the rules are satisfactory; the rules, however, are not explicitly restricted to non-inductive loads, and when at a later date the regulation for lower power factors began to be more frequently the subject of specification, the same methods of computation from test results were naturally followed. It has been somewhat of a superstition for many years, although now rapidly dying out, to put an undue importance upon the regulation of a-c. generating apparatus, upon the equality of regulation of machines which are to run in parallel, and so on. It is now well recognized that greater adaptability of the machine to occasional operation at over-voltage, excess current, and lower power factors, is of more value than a comparatively close regulation, the combination of the two properties being seldom commercially feasible. Other considerations also have arisen which call for higher regulation rather than low. As an illustration of the earlier attitude may be cited the fact that the A. I. E. E. rules for the determination of the regulation have been accepted readily by the same purchasers who specify closely a given regulation and are inclined to base acceptance or rejection of apparatus upon the supposed regulating qualities within a narrow margin, although the *real*

*Section 209 in the 1907 and 1911 editions and Section 71 in the 1902 edition.

regulation on, say, an 80 per cent power factor load, may vary considerably from the figure anticipated by one who applies the methods of Section 209, and the variation is in the direction of higher regulation.

To review the situation very briefly, let us consider the regulation at zero power factor, a characteristic more easily checked experimentally, than the regulation at any other power factor. According to Section 209, we obtain the zero power factor saturation curve from the no-load saturation curve by shifting this over, parallel to itself, a given distance horizontally, the vector addition of excitations being the same as arithmetical addition when the power factor is zero (see Fig. 1). This is equivalent to representing the effect of the load as a simple demagnetizing action on the field magnets, while we know that, to put it at its

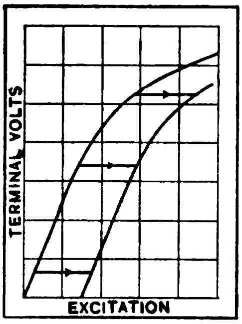


FIG. 1

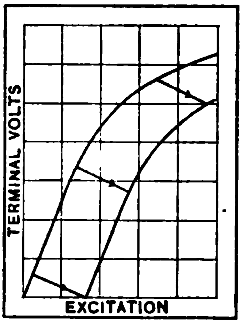


FIG. 2

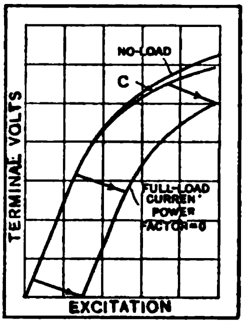


FIG. 3

simplest, we have the combination of a demagnetization and a reactive voltage drop, which would suggest moving the no-load saturation curve over parallel to itself, not horizontally, but in an inclined direction, the inclination being determined by the ratio of reactive drop to the demagnetizing action (see Fig. 2). It should be noted that these two give the same results only for a no-load curve which has no saturation bend.

As a matter of fact, this method again will give an approximately correct result only when the percentage pole leakage is small, or when the load considered produces a demagnetizing effect upon the armature which is small compared with the ampere-turns of the air gap and teeth.

To obtain a closer approximation to the load saturation curve for zero power factor, deduced from the no-load and short-circuit curves, we must lay out an auxiliary no-load saturation curve with increased pole leakage (curve C in Fig. 3) and move this curve parallel to itself horizontally by an amount corresponding to the armature demagnetizing ampere-turns, and vertically downwards by an amount corresponding to the armature reactive voltage drop. The amount of pole leakage for curve C is determined approximately by considering (A) the armature demagnetizing ampere-turns, and (B) the magnetizing ampere-turns for the air gap plus armature laminations. The ratio

$\frac{A + B}{B}$ is the factor by which the no-load pole leakage is to be multiplied in order to obtain that for curve C. With this procedure the experimentally determined zero power factor saturation can generally be checked fairly closely.

As a matter of fact, the *general shape* of the load saturation curve, within the range that we are usually concerned with, closely resembles that of the no-load saturation curve; that is to say, if the curve be transferred to a piece of tracing paper and laid on top of the no-load curve, a position can be found in which they nearly fit one another. This indicates that by using a *fictitious* armature reaction and reactive voltage drop for the method of Fig. 2, a compromise inclination can be found for the direction in which to move over the no-load curve, and obtain the zero power factor curve. If the curves have been experimentally determined, the inclination can be found readily, by moving the load curve horizontally to the left till it passes through the origin, thus matching the no-load curve over the initial straight part, and then moving it up in the direction of the initial slope until the higher points also agree. This process is followed in Fig. 4, where, taking a fairly high point P on the load curve, we set out $P\,Q$ to the left, equal to $O\,R$, and then draw $Q\,S$ parallel to the initial part of the saturation curve. It will be found that the distances between the two curves measured parallel to $S\,P$ are nearly constant, provided we do not push the method to points too high up on the curves. The distance $O\,R$ is obtained from the direct short-circuit curve for the load current in question, and a few high points on the load curve enable us to construct the curve fairly well.

It should be noticed that the actual drop in voltage on inductive load, with a given saturation and a short-circuit curve, depends upon the inclination of $S\,P$, and therefore is not determinate from the saturation and short-circuit curves alone.

Further information is required, preferably the experimentally determined saturation curve for full-load current at a power factor nearly zero. To illustrate this graphically, there are given in Fig. 5 the no-load and short-circuit curves of two generators which, when plotted to suitable scales, practically coincide. However, the pole leakage of one machine is a more prominent feature than in the case of the second machine, and we have a correspondingly lower load saturation curve. Both machines are of modern type and construction and have the same number of poles, and the curves are plotted from test figures slightly adjusted to bring them to a comparable load basis. It will be seen

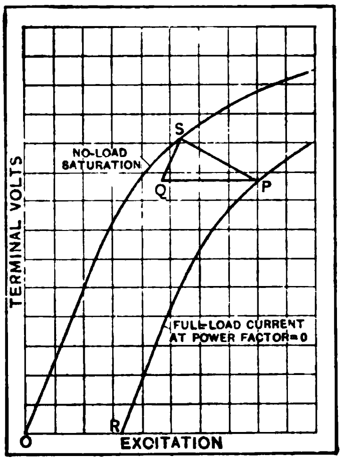


Fig. 4

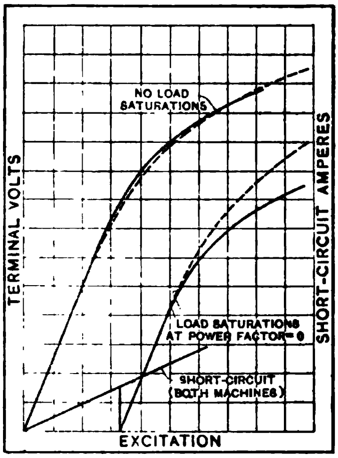


Fig. 5

that, in spite of the coincidence of the no-load and short-circuit curves, the zero power factor curves do not coincide, except on the straight parts at low voltages.

Recommendations

This memorandum upon regulation has been written merely to introduce a discussion upon the subject. In order to have a definite basis to start from, the author suggests that this Institute make the following recommendations:

1. That the regulation of alternators for unity power factor be specified only in those cases in which the actual operation of the plant may be expected to be at a power factor very near unity, and then only when conditions make the regulation a feature of importance.

2. That for alternators which are to operate at power factors of 85 per cent or below, it should be recognized that the actual value of the regulation (which is necessarily high) is of minor importance, and that when it is desired to specify regulation for power factors below 80 per cent, it is preferable to do so for power factor zero, since this can be directly checked experimentally on the manufacturers' test floor in many cases, while the regulation at higher power factors cannot be so checked, except for very small machines. The operating regulation will always be lower than the zero power factor regulation for a given ampere load.

3. That when it is desired to determine experimentally the regulation at a given load, and at power factors between unity and zero, for machines which cannot be actually loaded at the desired power factor, it is preferable to do so from the experimentally determined saturation curves for no-load, and for the given ampere load at zero power factor.

4. That to determine the load saturation curve for a given power factor from these two curves, it may be assumed, for uncompensated alternators, that at any excitation value the drop in voltage between no-load and zero power factor load is equivalent to a reactive drop, which may be treated vectorially for the power factor in question. The armature ohmic voltage drop may similarly be treated vectorially, but generally has a negligible effect upon the regulation at power factors below unity.

5. That for the experimental determination of the load saturation curve at zero power factor, any power factor below 20 per cent may be assumed to be equivalent to power factor zero.

Conclusion

In conclusion, the author wishes to direct attention to a paper* on this subject presented ten years ago, before the Institute, in connection with the revision of the clauses on regulation in the Standardization Rules. That paper drew attention to the importance of the zero power factor curve, and showed the relation between this and the no-load saturation as outlined above. The revision of Section 209 can advantageously be based upon the conclusions submitted there.

***The Experimental Basis for the Theory of the Regulation of Alternators*, by B. A. Behrend, Transactions A. I. E. E., 1903, XXI, p. 497.

A paper presented at the Midwinter Convention of the American Institute of Electrical Engineers, New York, February 28, 1913.

REGULATION OF DEFINITE POLE ALTERNATORS

BY SOREN H. MORTENSEN

The question of determining regulation of synchronous generators from shop tests has been treated by various authors in the past.* In most cases, it is hardly practicable to determine regulation under actual load conditions. As an alternative, the American Institute of Electrical Engineers has recommended a method of obtaining regulation from no-load saturation, short-circuit characteristics, and armature resistance. However, this method, as has been proved, gives in most cases incorrect results.

The object of this paper is to supply data collected from tests on various machines showing that, with the same tests as needed for the A. I. E. E. method, and very little more work, approximately correct regulation results can be obtained by what may be called the "triangle method." In it, regulation is derived by means of the Kapp diagram from a full-load, zero power factor saturation curve, which in turn has been derived from a no-load saturation and short-circuit curve with the aid of Potier's triangle.

The tables given further on show results obtained by the triangle and the A. I. E. E. methods, as compared with regulation derived from actual full load, zero power factor saturation curve.

To determine Potier's triangle for a machine, when its no-load saturation and short-circuit curves are known, it is only necessary to calculate the magnetomotive force of direct armature reaction, that is, the back ampere-turns per pole. Armature

*B. A. Behrend A. I. E. E. TRANSACTIONS Vol. XXI —1903, p. 497
Hobart and Punga " " " XXIII—1904, p. 291
B. T. McCormick " " " XXIII—1904, p. 330

transverse reaction is practically zero when a machine is run on short circuit. The armature reactance can be found directly from the no-load saturation and short-circuit curves as shown in Fig. 1 ($I\,x$ component).

The generally accepted equation for magnetomotive force of direct armature reaction under purely inductive load, zero power factor, may be expressed as follows:

$$A\,T = 0.9 \times K_s \times K_m \times K_d \times \sin\phi \times N \times I \times T \quad (1)$$

$A\,T$ = Reaction (back ampere-turns) expressed in ampere-turns.

K_s = Factor for fractional stator coil pitch same as for e.m.f calculations.

K_d = Distribution factor taking into account number of slots per pole per phase, same as used for e.m.f calculations.

*K_m = Coefficient of direct reaction.

Sin ϕ = Factor taking into account that displacement between e.m.f. and short-circuit current is not quite 90 deg. due to short-circuit stray losses.

N = Number of phases.

I = Armature current per phase.

T = Number slots per pole per phase

$$\times \frac{\text{No. conductors per slot}}{2 \times \text{No. circuits of arm. wdg.}}$$

For practical purposes, we may substitute mean values for several of the factors found in equation (1) without committing any appreciable error, as variation of these factors is small, as shown below.

The value of sin ϕ was determined for a number of machines from short-circuit stray loss curve, and it was found to vary between 0.97 and 0.99.

K_m is a function of the ratio $\frac{\text{pole arc}}{\text{pole pitch}}$ which, for machines of modern design, generally will fall between 54 per cent and 75 per cent. For these values K_m would vary between 0.885 and 0.78 if the pole face is shaped concentric with armature bore. As the pole face is frequently chamfered, these values have to be modified. From tests on numerous machines of different design,

*For derivation of K_m see Kapp's " Dynamo Machines "—1904.

it has been found that a mean value of 0.79 for K_m gives consistent results. As the variation in the value of the distribution factor K_d for three- and two-phase machines is rather small, a value of $K_d = 0.96$ for three-phase machines, and $K_d = 0.92$ mean for two-phase machines, may be taken.

Single-phase machines are often built on two- or three-phase armature punchings, by leaving certain slots empty. K_d thus varies over a much wider range than is the case for two-phase and three-phase machines, and hence no mean value has been

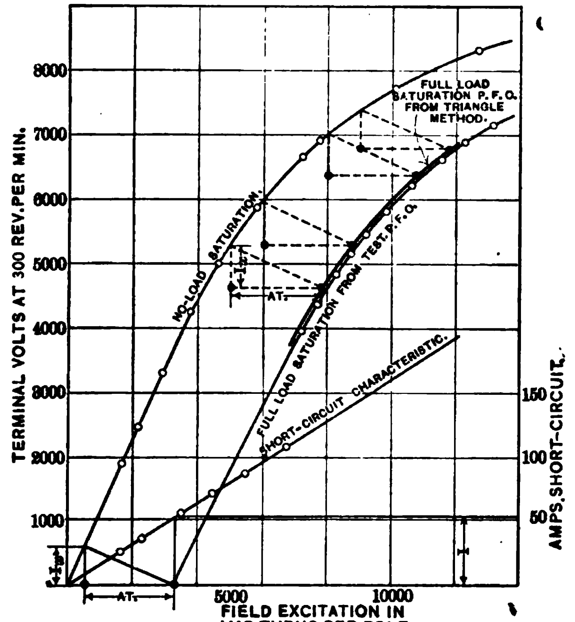


FIG. 1

assumed. By choosing average values as indicated above, we thus obtain the following simple equation for back ampere-turns:

$A\,T = 2\,I\,T$ for three-phase machines (2)

$A\,T = 1.3\,I\,T$ for two-phase machines (3)

$A\,T = 0.72\,K_d\,I\,T$ for single-phase machines (4)

Using the above equations for determining Potier's triangle, the results, shown in tables, were worked up from tests. The full-load saturation, zero power factor curve was determined by sliding triangle along the no-load saturation curve as shown in Fig. 1.

Regulation for power factors given in Tables A, B and C was then derived by means of a Kapp diagram.

For machines with high pole densities and a large field leakage, the triangle method will give more favorable regulation results than will actual tests, since in deriving the load saturation curve from no-load saturation and short-circuit curve, no consideration is taken of increased field leakage due to load conditions.*

Figs. 2 and 3 show curves from machines with high magnetic densities. In Fig. 2, saturation is in armature iron, and here

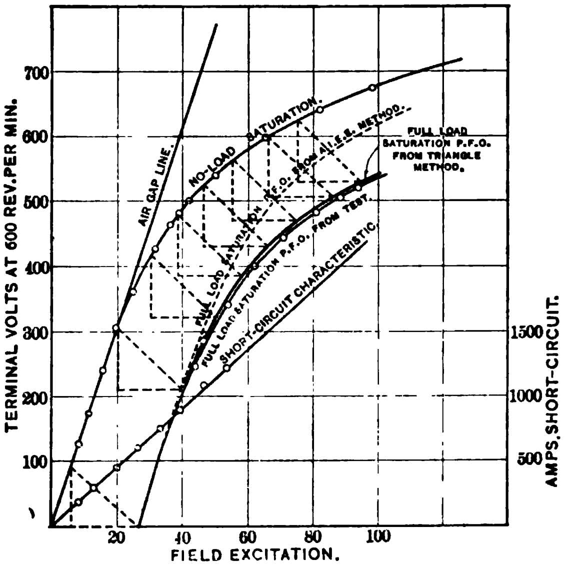


FIG. 2

curves from triangle method and test check each other closely. Fig. 3 is from a machine with an unusually large pole density

*Corrections for load field leakage may be made if, before applying triangle method, the no-load saturation curve is replotted for a field leakage of σ' instead of σ if

σ' = field leakage flux for full-load zero power factor saturation curve.

σ = field leakage flux for no-load saturation curve.

And further

K = constant depending on pole pitch and shape

$A\,T$ = armature back ampere-turns from equation (1)*

Then σ = $K \times$ ampere-turns for (air gap + armature iron)

and σ' = $K \times$ amp.-turns for (air gap + arm. iron + $A\,T$).

TABLE A—FOR THREE-PHASE MACHINES

Rating			No. slots per pole per phase	Pole arc pole pitch	Regulation					
					By A. I. E. E. method		From full load sat. curve		By triangle method	
Kw.	Volts	Speed r.p.m.			P. F. 1	P. F.0.8	P.F. 1	P. F.0.8	P. F. 1	P. F. 0. 8
200	2300	1800	4	0.54	11.	23.	11.2	25.	11.2	25.2
600	6600	300	3	0.62	4.3	15.	6.5	17.5	6.2	17.1
2800	2300	720	3	0.685	9.	21.8	10.	24.	9.7	23.6
600	2300	150	1	0.60	7.1	17.4	8.5	24.2	8.3	23.4
250	480	120	2	0.72	8.3	18.6	11.4	27.	10.3	25.5
525	2200	500	4	0.54	16.4	31.	18.2	33.6	17.6	33.
200	2300	200	1	0.61	5.1	19.	8.3	25.5	8.2	24.6
200	2300	150	2	0.72	11.1	25.	13.5	31.2	12.5	30.
500	11000	100	1	0.66	9.2	21.8	12.8	28.2	12.2	27.8
1000	2300	82	1	0.68	6.4	17.2	7.9	23.5	7.6	23.
1750	6600	100	2	0.71	6.	18.2	11.	22.	9.9	21.1
1000	2400	180	3	0.66	9.1	21.	10.6	26.2	10.2	25.2
225	2200	900	2	0.55	10.	22.3	11.8	24.2	11.7	24.1
625	2300	300	2	0.702	3.7	11.5	4.3	16.6	4.4	16.7

TABLE B—FOR TWO-PHASE MACHINES

Rating			No. slots per pole per phase	Pole arc pole pitch	Regulation					
					By A. I. E. E. method		From full load sat. curve		By triangle method	
Kw.	Volts	Speed r.p.m.			P. F. 1	P. F.0.85	P.F. 1	P.F. 0.85	P. F. 1	P F. 0.85
500	2300	120	2	0.597	4.5	15.6	8.5	19.5	8.	19.5
250	1100	120	2	0.57	5.	17.	5.7	20.5	5.9	20.6
420	125	240	2	0.64	16	33.	20	38.	20	38.
125	440	277	3	0.68	8.5	21.5	9.1	24.8	8.7	23.8
1000	2200	500	6	0.56	5.	14.8	5.4	16.5	6.	17.3
700	2300	120	2	0.598	6½	13.9	8.7	20.4	8.6	20.3

TABLE C—FOR SINGLE-PHASE MACHINES

Rating			No. slots per pole per phase	Pole arc pole pitch	Regulation					
					By A. I. E. E. method		From full load sat. curve		By triangle method	
Kw.	Volts	Speed r.p.m.			P. F. 1	P. F.0.85	P.F. 1	P.F. 0.85	P. F. 1	P.F. 0.85
			empty							
75	440	250	6—2	0.66	2.7	12.2	5.4	17.9	5.9	18.2
90	2300	900	6—2	0.55	8.8	20.	11.	28.	10.6	27.3
260	2300	150	3—1	0.60	3.	10.4	6.	18.	6.	17.9
160	2200	200	3—1	0.60	11.7	24.	16.	30.	15.	28.9
65	2200	277	3—1	0.62	9.1	20.2	14.	31.5	14.	31.

and a large field leakage. Here, field leakage due to ampere-turns compensating armature reaction causes the load saturation curve to lean over more than indicated by the triangle method. The latter, however, is more accurate than the A. I. E. E. method.

In general, it may be said that results show that the triangle method gives regulation considerably nearer the actual than does the A. I. E. E. method. Moreover, the triangle method is so simple as to be easily applied. Data for use in equations (2), (3) and (4) can be taken directly from a winding specification. No-load saturation, short-circuit curve, and armature resistance are

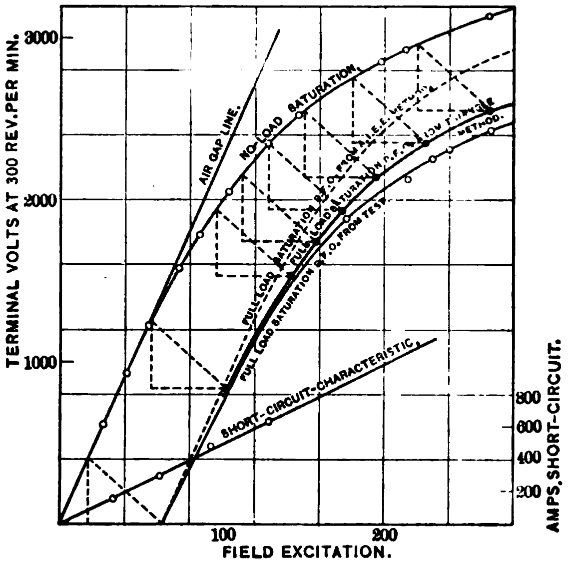


FIG. 3

always available, and the graphical operation involved to get regulation at any power factor is as simple as the present A. I. E.E method. The Kapp diagram can be simplified if the influence of ohmic resistance is neglected. This can be done in most cases without impairing the accuracy of the results.

Thus, it would seem that the triangle method has proved itself sufficiently accurate and also simple enough to be practicable; it might therefore be considered as a fitting substitute for the present A. I. E. E. method.

A paper presented at the Midwinter Convention of the American Institute of Electrical Engineers, New York, February 28, 1913.

GENERATOR AND PRIME MOVER CAPACITIES

BY DAVID B. RUSHMORE AND ERIC A. LOF

The chief distinctive characteristic of electrical apparatus is that its capacity is limited by thermal considerations. Electrical generators are of necessity driven by prime movers, and the combination must be treated as a single unit when considering the questions of ratings and capacities. Such prime movers as steam engines, steam turbines, gas engines and waterwheels have various limitations regarding their output which must be considered in connection with that of the electric generators they are driving. The proper adjustment of ratings for generators and prime movers with these different limitations as determined by the conditions of service, is the object of this paper. In direct-current generators the limitation imposed by commutation has almost entirely been removed by the use of interpoles. With alternators as now constructed, the limitation of output due to regulation of the machine is but seldom found; therefore the question of heating may be considered as the important factor for consideration. Under certain conditions of violently fluctuating loads or intermittent service, the questions of commutation and regulation may, however, become of considerable importance.

The capacity of an alternator is limited not only by the actual energy load which the machine is carrying, but also by the power factor of such load. The additional heating at low power factors and constant power is due to the wattless component of the armature current and also to the additional field current necessary to counteract it. In many cases it is possible to predetermine approximately the power factor under which the generators will operate, and the special design to meet this particular condition is largely desirable. The curves in Fig. 1 show the different conditions of load and power factor which will produce the limiting conditions of heating in an inductor alternator. The synchronous impedance here is considered to consist almost entirely of reactance.

The importance of the proper adaptation of capacities of generators and prime movers is emphasized by the fact that there are in operation in many stations in this country units in which the output is unnecessarily limited by an improper rating

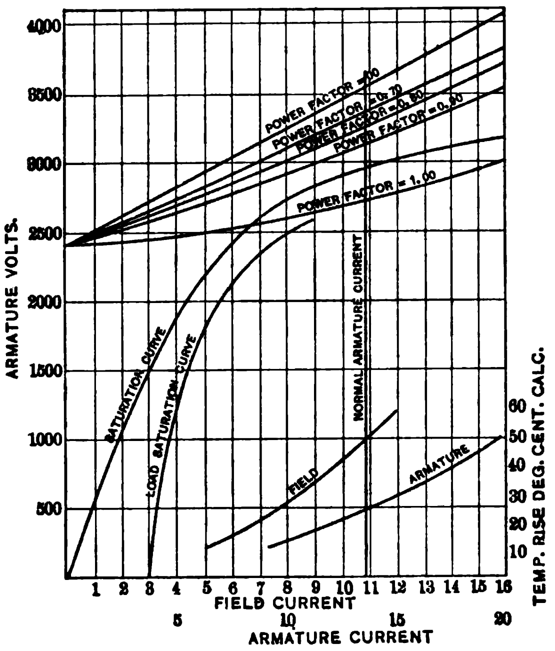


Fig. 1—Characteristic Curves of Induction Alternator.

or design of either the generator or prime mover. The generator may be designed for unity power factor while the condition of operation may be the power factor 0.8, in which case only a part of the prime mover capacity can be utilized. In not a few instances in hydroelectric stations, the waterwheel is either too small or too large for the generator and a like result ensues. In some hydroelectric plants where the generators are installed to handle peak loads, this maladjustment of ratings is a necessity, but with most of the larger systems it is unnecessary to run machines except at or near their maximum ratings. In the past, every effort was made to adjust the ratings of generators to the load curves and we had 25, 50 and 100 per cent overload guarantees for certain apparatus. Experience and better load conditions have changed this to a maximum rating which has become possible largely through the growth in the size of stations and systems.

The speeds of engine-driven units, both gas and steam, have become nearly standardized, and turbine-driven sets are rapidly approaching the same condition. With waterwheel units, however, the situation is such that the capacity and speed of the generator is often determined largely by the hydraulic conditions and the characteristics of the waterwheel which is to be used.

With steam engines the point of maximum efficiency is rather marked, as shown in the curve of Fig. 2, and the ratings are

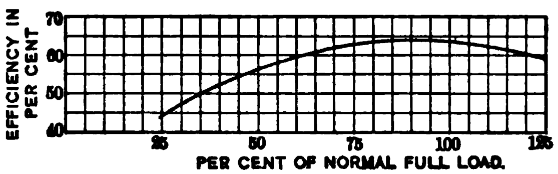


Fig. 2—Performance Curve of 8000-h.p. Compound Condensing Steam Engine.

usually such that the engine is working under its most economical load at the rating of the electrical generator. With gas engines, however, the efficiency increases with the load, beyond the capacity of the engine, as shown in Fig. 3, and for this reason the rating of the engine is generally made as nearly as possible to its maximum capacity, leaving a small margin for regulating purposes. With the steam turbine, Fig. 4, the efficiency curve is usually so flat that it is a question of desirable overload capacity which limits the rating of the turbine.

In the waterwheel unit, the efficiency usually falls off rapidly above and below the maximum point, so that the rating of the generator should correspond to the point of maximum efficiency of the waterwheel. Steam engines and steam turbines are designed to operate with certain variations both in pressure of the steam and conditions of vacuum. Gas engines must accommodate themselves to variation in the quality of the gas.

With the waterwheels, however, by far the large majority of installations are subject to a change in head which varies over a wide percentage. In many of the low-head installations the back water may bring about a change in head which is beyond the capacity of one wheel to accommodate, and in some plants additional wheels must be mounted on the same shaft and cut into service at times of low head. One particular instance of this kind is the plant of the Chattanooga and Tennessee River Power Company, where the head may vary from 20 to 42 ft. (6.1 to 12.8 m.). In most of the large developments this change in head is the limiting feature in the design of the waterwheel as related to the generator capacity, for in all electrical work it is necessary that the speed of the generator be kept constant.

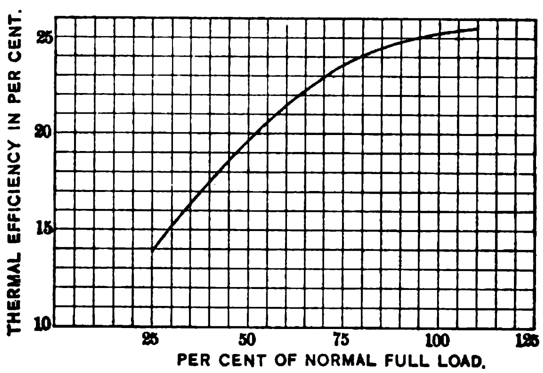


FIG. 3—PERFORMANCE CURVE OF 2000-H.P. GAS ENGINE

Waterwheel runners of different designs are compared on the basis of their specific speeds. The specific speed of a waterwheel is the number of revolutions per minute at the point of maximum efficiency that a similar wheel will give when it delivers one horse power under one meter head. By comparison of the specific speeds we can judge of the characteristics of waterwheel runners without respect to their actual speed, power or head. A high specific speed means a high actual speed, and a low specific speed means a low actual speed in revolutions per minute. For this reason waterwheels with low specific speeds are generally used with high heads in order to make the speed of the generator within the range of good electrical design.

Waterwheels with high specific speeds have very deep runner vanes, and these are liable to erosion under high heads; also, the efficiency curves of runners of high specific speed are more pointed than with the low specific speed type, and this allows a narrower margin for operation under the best conditions. This is clearly shown in curves in Fig. 5.

The maximum full-load capacity of a turbine is that point beyond which the output decreases with an increase in gate opening. The margin between the point of maximum efficiency and of maximum capacity depends upon the specific speed of the runner, and is smaller the higher the specific speed. This is illustrated in Fig. 5, which shows that as the specific speed is increased, the point at which maximum efficiency occurs approaches nearer to the power delivered at full gate opening. The specific speed may thus be increased to such an extent that the point of maximum efficiency and maximum output coincide. With low heads and high specific speeds it is therefore desirable

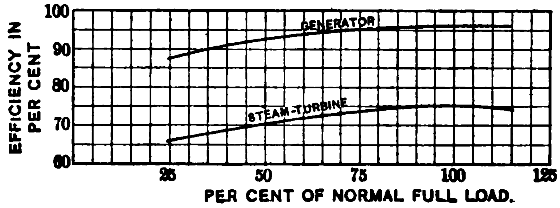


FIG. 4—PERFORMANCE CURVES OF 5000-KV. STEAM TURBO-GENERATOR SET

to operate wheels near their point of maximum output, and to obtain the best results, the generator should be designed giving consideration to this point.

Referring again to the curves in Fig. 5, it will be noted that the full-load capacity occurs at about 6 per cent above normal or rated full load in all three cases. This is in accordance with the general practise, the margin being allowed for governing. It is also noted that for curves *B* and *C* the efficiency is falling off very rapidly at 6 per cent overload, and that should the gate be opened still further the output would reduce instead of increase. If, with low specific speed wheels, as represented by curve *A*, the gates were still further opened, the power would continue to increase to some extent.

The point of maximum efficiency for wheels represented by curve *A* occurs at about 90 per cent of normal full load, in the case of *B* at 93.5 per cent, while in the case of *C* the maximum

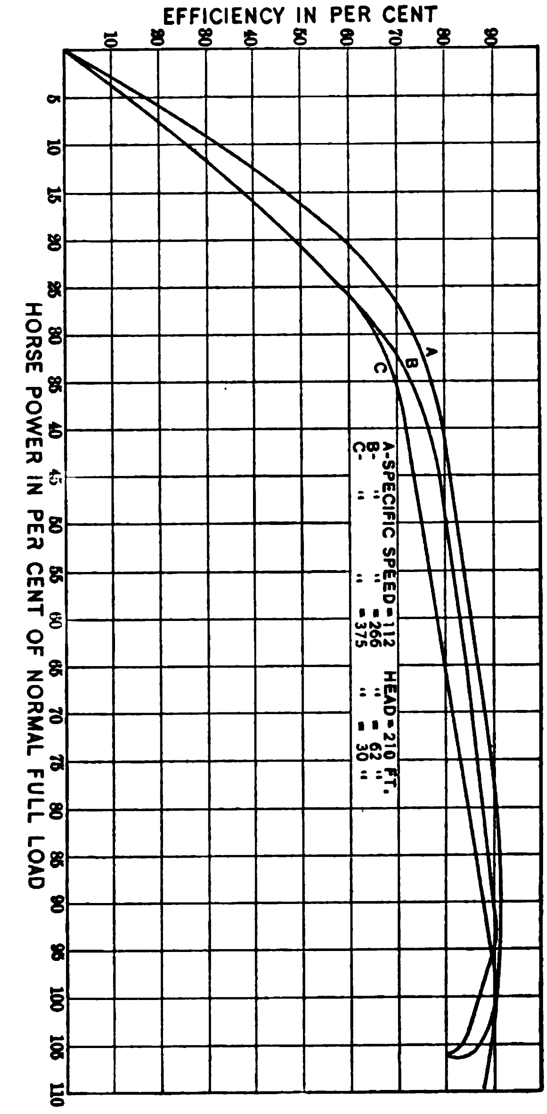


FIG. 5—PERFORMANCE CURVES OF SEVERAL TURBINES FOR VARIOUS HEADS AND SPECIFIC SPEEDS AS SHOWN

efficiency occurs just at the point of normal or rated full load. Thus, as stated before, the power at which the maximum efficiency occurs approaches nearer to full load as the specific speed increases.

With high-head wheels, as represented by curve *A*, the efficiency remains very high over a very large range in power, while for low-head wheels, curve *C*, the efficiency falls off rapidly as the power is reduced below the normal full load. For this reason it is desirable to run low-head wheels under practically full load conditions. With high-head wheels this is not so important, as the efficiency is still high at partial loads. With wheels as represented by curve *C*, it is also necessary to allow some margin above the normal full load for governing, as it is desirable to operate the turbine at its point of maximum efficiency. With high-head wheels, curve *A*, such a margin need not be allowed.

The curves plotted in Fig. 5 represent operating conditions under constant head. This, however, is not always realized, especially in low-head plants where floods and dry seasons sometimes cause quite a variation in the head, and this has, as previously mentioned, quite a bearing on the selection of the waterwheel, and should therefore be given careful consideration.

If the speed of the unit could be allowed to vary at all times as the square root of the ratio of the heads, the shape of the performance curve for any head other than normal would be the same as that secured at normal head, but the output would vary as the 3/2 power of the ratio of the heads. In the case of wheels driving alternating-current generators a speed variation is not permissible and the speed must be kept constant, irrespective of any variation in head which may occur.

In Fig. 6 is plotted a set of curves illustrating the effect of a varying head. A 10,000-h.p. turbine is assumed to operate normally under a 32-ft. (9.75-m.) head, the speed to be constant for a range of heads from 26 to 38 ft. (7.92 to 11.6 m.). As the head goes up to 38 ft. (11.6 m.) the shape of the curve approaches more closely curve *B* in Fig. 5, while when the head falls to 26 ft. (7.92 m.), the speed being constant, it approaches more closely to curve *C*. In other words, when operating under a 38-ft. (11.6-m.) head, the speed is lower than the best speed for the runner under that head, while when operating under the 26-ft. (7.92-m.) head, the speed of the wheel is higher than the best speed. Under 38 ft. (11.6 m.) head the point of maximum

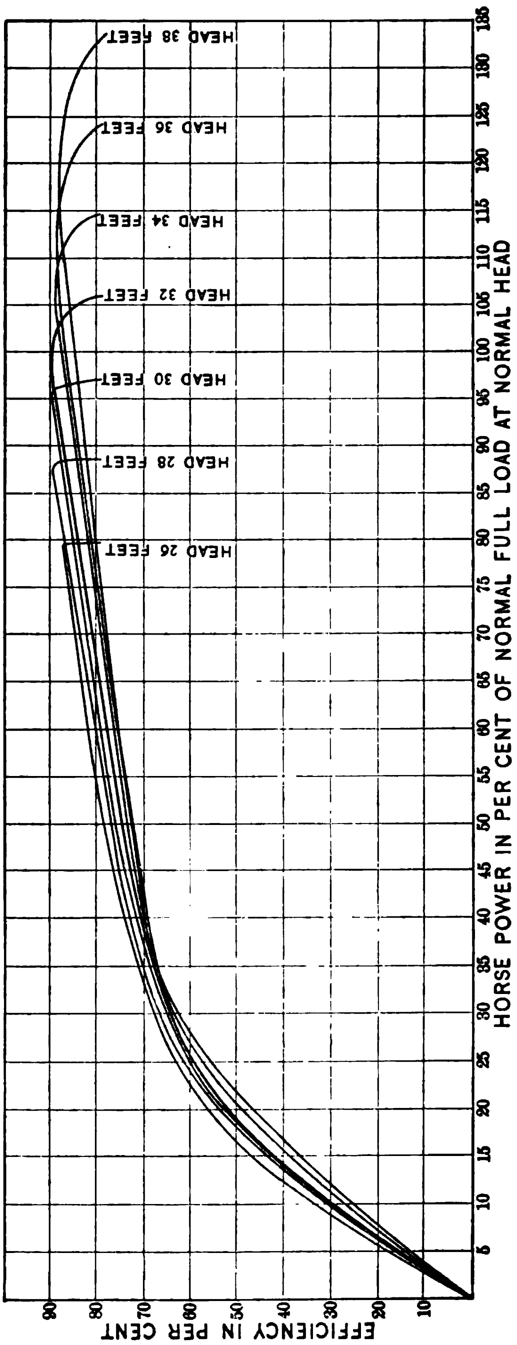


Fig. 6—10,000-h.p. Turbine Curves Showing Efficiency and Power for Constant Speed and a Normal Head of 32 ft. For Various Heads as Shown

efficiency is, furthermore, considerably below the normal full load at that head, while under 26 ft. (7.92 m.) head the power at which maximum efficiency occurs is the actual full load, illustrating the points discussed above in reference to the relation of the power at which maximum efficiency occurs and the normal full-load power for various specific speeds.

Let us assume that a selection of a wheel is to be made for an installation, and that performance curves are desired, showing the expected efficiency for various loads and speeds. Curves *A*, *B* and *C* in Fig. 5 may each represent a possible curve, dependent upon the revolutions selected for the turbine in question, the revolutions being directly proportional to the specific speeds, and they will illustrate the manner in which the efficiencies at partial gate openings will fall off in any one

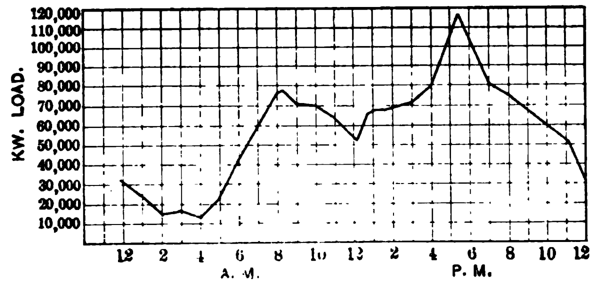


Fig. 7—Typical Load Curve of Large Central Station—December Load

case, depending upon the actual revolutions per minute selected for the design of the wheel. They will also give an idea as to the margin between the normal full load and the power at which the point of maximum efficiency will occur. In the selection of a speed for any installation, therefore, aside from the cost of the generators, the question of the wheel efficiencies at partial gate openings has a considerable bearing. Where a unit is likely to operate under a very wide range in power, it would be advisable to select a wheel represented by curve *A*, giving a high efficiency for a considerable range in power.

All power systems have a more or less varying load, and this has a very important bearing on the selection of the proper generating equipment and on the economy of the plant. The load will vary considerably, not only during the 24 hours of the day, but also for different periods during the year. The load curve also differs materially for different kinds of load, such as for central stations, industrial establishments, street railways, etc.; a typical load curve of a central station is represented in Fig. 7.

The size and capacity of the units in a hydroelectric station is determined, in many cases, quite largely by the hydraulic conditions. In operating, the number should, wherever possible, never be less than four, unless the station forms part of a larger system. It is better to operate machines as near full load as possible, and to start new units instead of utilizing overload capacities. Where sudden overloads of considerable magnitude come on the system for very short periods it is, of course, necessary to have a wheel capacity sufficient to care for them.

The foregoing discussion indicates that waterwheel characteristics will vary with different installations. For the sake of standardization it is desirable to give the generators a maximum rating at a certain temperature. The proper relation of the capacities of generator and waterwheel should be considered for each installation.

GROUP IV PAPERS

(Pages 731 to 804)

Miscellaneous Subjects Relating to Rating

DISCUSSION ON GROUP IV PAPERS—[(*a*) OIL SWITCHES, (*b*) SPARK GAP, (*c*) WAVE FORM, (*d*) REGULATION]. NEW YORK, FEBRUARY 28, 1913.

Paul M. Lincoln: The paper of Mr. Burnham's on oil circuit breakers opens up an interesting subject. The question of the method of rating an oil circuit breaker is an important one, and is one upon which the Standards Committee, I believe, should take some action. I am prepared to accept most of the suggestions made in Mr. Burnham's paper. It is necessary to rate circuit breakers in various ways; one way to rate them is in regard to their current-carrying capacity, and they must have such a rating. They must also have a voltage rating to indicate the maximum voltage of the circuit upon which they may be used.

These two ratings, however, do not fix the ability of a given breaker to *protect*, and it is this ability to *protect* that is of foremost interest to the operating man. It seems to me that the best method of giving such a rating is the one which is suggested in this paper, namely, the kilovolt-ampere capacity which a circuit breaker will be guaranteed to interrupt. Now, the kv-a. capacity which the breaker will interrupt successfully will depend almost entirely upon what is back of the breaker. Of course, it stands to reason that the breaker which has a small power plant back of it will not be called upon to interrupt as much as one which has a large power plant back of it. Moreover, the amount of power which the breaker is called upon to interrupt will depend not only on the size of the power plant back of it, but also on the character of the generators, and particularly on the question whether or not there are current-limiting devices placed in those generators or in other portions of the circuit, so as to limit the amount of power which the breaker is called upon to interrupt. In these days when there is such a tendency to use current-limiting devices either in generating circuits or feeder circuits, or between the sections of busbars, the method of rating breakers suggested in the paper becomes, in my opinion, the logical one. It is not logical to rate a breaker with regard to the amount of synchronous apparatus that is back of it, because a given breaker may be protected by reactance in series with it, so that the amount of power which it is called upon to interrupt is not a function of the total plant back of it, but an amount limited by the current-limiting devices. It seems to me logical therefore to rate breakers for the kv-a. capacity which the breaker will be guaranteed to interrupt.

M. G. Lloyd: I ask if there is any limitation on what happens to the breaker when it ruptures the circuit?

Paul M. Lincoln: Of course, good practise must place a limitation on that, but just what that good practise is, has not been definitely determined. It is the practise of a number of operating companies at present to overhaul breaker contacts, after they have been called upon to interrupt short circuits, and the practise in the past has indicated that such an inspection is essential to the continuity of service. Such inspections, of course, are necessary only when the breaker is called upon to act at somewhere near its ultimate breaking capacity. In ordinary conditions, a breaker may not show the slightest signs of inconvenience or distress, but when the breaker is used at such capacities as tend to push it to the limit, there may be some throwing of oil or burning of contacts, which it is wise to investigate, before the breaker is put back into service.

M. G. Lloyd: I do not think that quite answers the point as regards the rating in the rules. How much overhauling would be permissible for deciding that the breaker had been overrated?

Paul M. Lincoln: I do not know that the rules could go so far as to make any definite determination of that point. I am not prepared, at least, to suggest any reading of the rule which covers that point.

F. D. Newbury: I do not think it is a matter for the rules to be explicit upon, and I think that, as long as the damage to the breaker has not been greater than can be remedied by replacement of the contact the breaker has not been overrated.

A Member: The author has called attention to the complexity of the problem of rating circuit breakers, and the difficulty of expressing that rating is something that can be readily understood. Because of this difficulty many of the manufacturers have paid no attention whatever to the rating of the switches they buy. That may sound like a confession, if it was taken for what it is worth. Our company pays very little attention to the guarantee of the manufacturer, depending on a knowledge gained of the switches by testing and experience. The reason for that is this—the rating at present, as the author has indicated, is unsatisfactory. The amount of energy that is back of the switch is the determining factor as to what will happen to the switch when it is called upon to do extreme work, that is, opening a short circuit. It is put on a device and an automatic arrangement is put on the switch, so that it will open the short circuit when called upon. The extreme condition that it is called upon to meet is a short circuit directly back of the switch. What will happen to the switch will depend entirely on the available energy back of the switch. Just what volume of current the switch will open cannot be determined, and you cannot tell whether it will come up to specifications, as we have not a micrometer in the short circuit when it opens. We do not know what it is called upon to do and we do not know whether it meets specifications or not. It seems to me that a far better rating would be the kv-a. capacity that would be ruptured, not the currents passing through the switch, but the kv-a. back of the switch, under which conditions the switch may open sufficiently to protect the circuit—switches would be rated in certain values—instead of certain ampere values of continuous capacity, they would be rated to protect a circuit having back of it certain available instantaneous energy. Suppose they are rated for 100,000 kv-a., or any capacity that the manufacturer sees fit; that will be more good to the operating engineer than a kv-a. rating he knows he can never find out whether the switch is meeting or not.

Paul M. Lincoln: The man who installs a plant, who fixes the size of the choke coils, etc., is the man who can determine how much current a given switch will be called on to interrupt as a maximum. The manufacturer cannot determine that. If the manufacturer will say that such a switch will protect 10,000 amperes on a 100,000-volt line, and if you go beyond that you are taking chances, that is as much as the manufacturer can do. The man who installs it and applies the limiting devices is the man who can determine the maximum the switch will be called upon to rupture under the worst conditions. It seems to me that when a manufacturer has said that a given switch is good for rupturing so many kv-a. on such and such a voltage line, he has gone as far as he can. The amount of instantaneous kv-a. which the switch is called upon to rupture is not only a function of the current-limiting devices in series with it, but also a function of the time-limit which is placed on the breaker. It is well known that if you allow a breaker to stay in for several seconds after the short circuit has come, so that the instantaneous rush of current is over, and the generators have settled down to somewhere near their normal condition, the stress on the breaker will be less severe than if it is called upon to rupture instantly.

A Member: Apparently I did not make myself clear, because my position is exactly the same as Mr. Lincoln's. We both agree absolutely. One point of my suggestion is this—that if it were stated in terms of the available energy back of it, the same thing as stating the capacity, you call the attention of the engineer to the fact that he must figure it from that side, and not figure on what is going to happen on the other side of the switch, the apparatus it protects. At the present time the rating leads the mind of the average engineer to the amount of energy that is behind the switch rather than the energy that can be pumped into it.

Ford W. Harris (by letter): The suggestion to rate breakers by the maximum current they can safely open is not a new one, being one of those more or less obvious thoughts that occur to a considerable number of people at different times. It has to my certain knowledge been several times suggested to at least one manufacturer and on every such occasion has been finally rejected as undesirable.

In the first place it involves complications that are not at first sight evident. For example, the influence of phase relation of the voltage and current at the instant of rupture is very marked in limiting the value of this current. If the voltage wave at the instant the current reverses is near its maximum this current is much more likely to reverse than if the voltage wave is at a low value. It would probably not be sufficient to give a single maximum value, but in addition it would be necessary to state exactly how this value would be modified by the characteristics of the circuit as to inductance and capacity.

Then this safe maximum current would represent the current that would produce a failure of the breaker, divided by a factor of safety. We have, however, no accurate measure of the failure point. At a certain current the breaker will start to throw oil, and as the severity of the short circuit is increased it will throw oil, burn contacts, and produce other mechanical distortions in a greater degree. At just what point shall we apply our factor of safety? If this rating is to be a definite one, some agreement on this point will be necessary.

In the same way it would be necessary to come to some agreement as to what the factors of safety should be and how they should be modified by enclosure which prevents the breakers enclosed from being regarded as so much of a life and fire hazard.

Even if the rating were adopted I am not at all sure that it would be of any considerable engineering value, due to the difficulty of determining in advance just what current to expect at certain points and in general the impossibility of waiting until tests can be made before specifying the breakers. While there are in this country certain men who can figure what this current will be and while there exist data that could make this calculation feasible to the average engineer, these data have been obtained at a very considerable expense and it is very doubtful if those who have them would care to make them public.

Considered commercially, I cannot see where this rating would be of any very considerable value. The average purchaser would rather have a blanket guarantee, that the breakers he is purchasing will take care of conditions in the applications that he can define to the manufacturer, than to have a partial guarantee that the breakers will open a certain current. Indeed, it is very doubtful if the purchaser would be willing to dispense with the broader guarantee even if the current were defined.

In other words, it seems to me that this matter of circuit breaker ratings is a much more complicated matter than has evidently been assumed, and that the rating proposed is not likely to be of any very considerable value either to the purchaser or to the manufacturer.

Chester Lichtenberg (by letter): The rating of an oil circuit breaker, unlike that of most other electrical apparatus, must be given in terms both of the normal and abnormal circuit conditions under which it is intended to operate. Its complete rating requires, therefore, an enumeration of the following properties:

1. Continuous current-carrying capacity.

2. Maximum circuit pressure capacity.

3. Maximum energy-dissipating capacity.

The continuous current-carrying capacity of an oil circuit breaker is the maximum current at any given frequency which its parts will carry continuously without exceeding a specified temperature rise. This will depend primarily on the design of the device and the materials used in constructing it, and also upon the temperature and configuration of the leads connected to its terminals and the quality of this connection. The latter points are very important, and in making tests of oil circuit breakers, great care must be exercised to have the temperature of the leads not in excess of that of the oil circuit breaker terminals. In general, the maximum temperature of any part of an oil circuit breaker should not exceed 35 deg. cent. above an average room temperature of 25 deg. cent., but in no case should the maximum temperature of the oil exceed 75 deg. cent.

The maximum circuit pressure capacity, commonly known as rated voltage of an oil circuit breaker, is the maximum equivalent pressure of the circuit to which it may be safely connected. This rating depends on the design of the device, the pressure rises which may occur on the circuit in which it is connected, and the desired factor of safety. On most circuits operating at 45,000 volts and below, it is admissible to give the pressure rating of the oil circuit breaker in terms of the circuit pressure. Above this point, however, and in some special cases below it, it has been found advisable to follow the practise adopted by insulator manufacturers, and give the pressure rating of the oil circuit breaker in terms of the maximum pressure it will withstand for a short interval of time such as 30 or 60 seconds, and the pressure under which it can operate continuously. The ratio between these two ratings varies from 1.5 to 10, depending on the circuit conditions and the degree of safety specified.

The maximum energy-dissipating capacity, generally known as the rupturing capacity, of an oil circuit breaker, is the maximum amount of energy which the device can dissipate when interrupting a circuit of given voltage and frequency. This factor of the rating is by far the most difficult to determine and fix, as it depends on a large number of independent variables of design and circuit conditions. It can only be determined experimentally with considerable difficulty and within wide limits which require exact definition.

It is, therefore, suggested that the Standards Committee consider a method of rating oil circuit breakers which will include the following:

1. A current rating based on temperature rise.

2. A pressure rating based on ordinary circuit pressure rises together with a reasonable factor of safety.

3. An energy-dissipating rating based on the maximum current which the device can safely interrupt on a circuit of given pressure and frequency at the least favorable power factor, without showing any external signs of distress.

It is also suggested that:

1. The maximum temperature rise on any part of an oil circuit breaker should be limited to 35 deg. cent. above an average room temperature of 25 deg. cent., but in no case should any maximum temperature exceed 75 deg. cent.

2. Oil circuit breakers for use on circuits between 2500 and 45,000 volts shall be able to withstand a high-pressure test between live parts and ground of three times rated pressure for 30 seconds.

3. Oil circuit breakers for use on circuits exceeding 45,000 volts shall be able to withstand a high-pressure test between live parts and ground of 2½ times rated pressure for 30 seconds.

4. The safe rupturing capacity of an oil circuit breaker shall be the maximum equivalent current which the device can interrupt at rated pressure and frequency at the least favorable power factor without showing signs of distress, and shall be given in amperes at rated pressure and frequency.

5. The maximum rupturing capacity of an oil circuit breaker shall be the maximum equivalent current in amperes which the device can interrupt without being destroyed and shall be given in amperes at rated pressure and frequency.

F. W. Peek, Jr.: In the discussion of the interesting paper of Messrs. Chubb and Fortescue on their development of the sphere gap voltmeter it may be of interest to state our experience, and add data which we have obtained in this work.

The needle gap has long been a useful means of approximating high voltages; with the present extra high voltages, however, we have about outgrown it. Although it is possible to measure high voltages with a fair degree of accuracy with the needle gap, too much skill is required, and too many variables must be considered, especially at extra high voltages. The voltmeter coil offers a reliable means of high-voltage measurement, but a gap method is often desirable because the gap measures the maximum point of the wave and this is what determines the breakdown of insulation. With a gap method it is thus not necessary to take oscillograms, except to know that the wave fairly approximates the sine: that is, is a good commercial wave. The sphere gap used within the limits described below seems the best solution of the practical problem. It is free from the eccentricities of the needle gap, requires less skill in manipulation, the space factor is small and, furthermore, the curve can be readily calculated within small percentage error. There is one variable that must affect all gap measurements—air density. Over the ordinary range of temperature and variation of barometer ***at or near sea level*** correction may be made by multiplying by δ, where

$$\delta = \frac{3.92\, b}{273 + t}$$

For high altitudes, where the range of δ is large, the correction

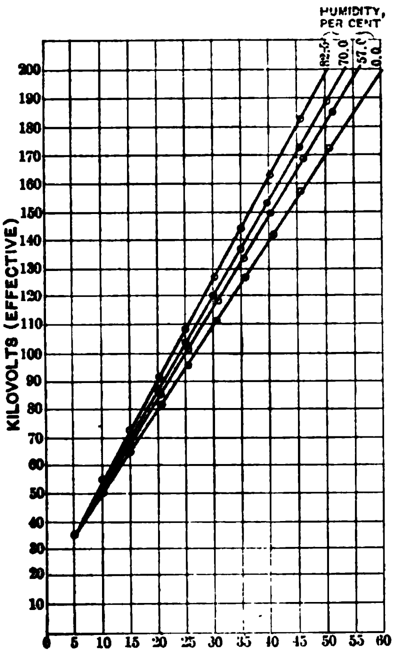


FIG. 1—NEEDLE-GAP CURVES FOR DIFFERENT RELATIVE HUMIDITIES.

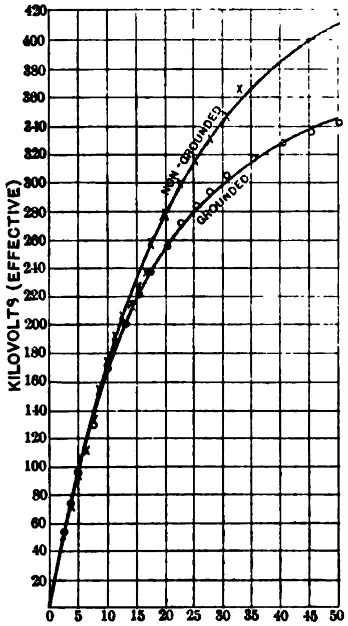


FIG. 3—SPARK-OVER CURVE, 25-CM. (DIAMETER) SPHERES.

Drawn curve, calculated; points, measured values.

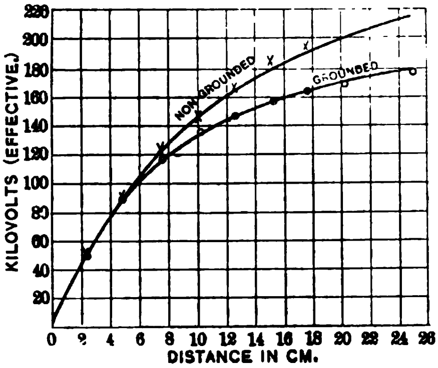


FIG. 2—SPARK-OVER CURVES, 12.5-CM. (DIAMETER) SPHERES.

Drawn curves, calculated; points, measured values.

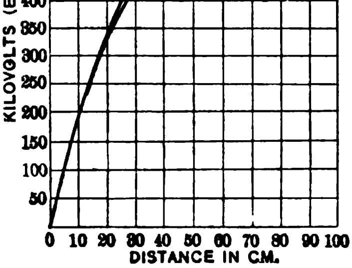


FIG. 4—SPARK-OVER CURVES (CALCULATED) 50-CM. (DIAMETER) SPHERES.

is slightly different and will be given later. The following curves are for 25 deg. cent. and 76 cm. barometer.

The Needle Gap. The needle gap is generally unreliable, due to the broken-down air which surrounds the gap long before the spark passes, and to the large space factor which makes it necessary to remove surrounding objects to a great distance for consistent results. The broken-down air causes discrepancies by heating the gap, and there is also a very great variation with varying humidity. The effect of humidity is shown in Fig. 1, where it can be seen that a higher voltage is required to spark over a given gap when the humidity is high than when it is low. The curve thus varies from day to day as much as 20 per cent.

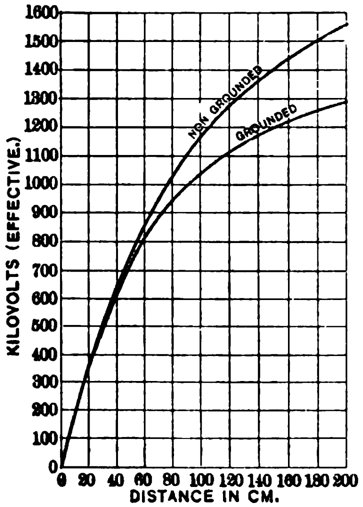


FIG. 5—SPARK-OVER CURVE (CALCULATED) 100-CM. (DIAMETER) SPHERES.

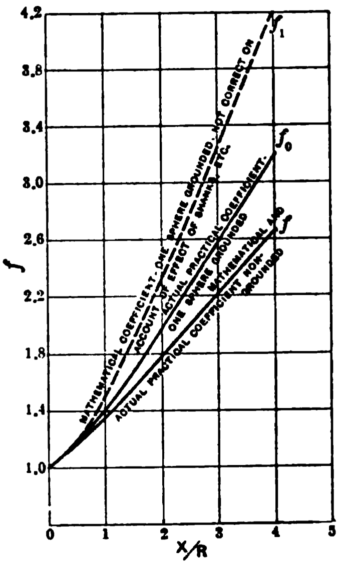


FIG. 6

It is probable that the corona streamers in humid air cause a fog, as it were, agglomerating the water particles, and these, in effect, increase the size of the electrode. There is also considerable variation with the sharpness of the needle, and probable variation due to local resonance set up by the streamers. Needles must be changed after each spark-over.

The Sphere Gap. The voltage required to spark over a given gap between spheres increases with the diameter of the sphere. If a sphere is chosen so that the spacing for the required voltage is never greater than two times the sphere radius, the first evidence of stress is complete spark-over, corona can never form, and all of the undesirable effects and variables due to the broken-down air are eliminated. Humidity has no measurable effect The space factor is small—for instance, at 200 kilovolts, the gap between needle points is from 50 to 60 cm., for 25-cm. diameter spheres it is only 13 cm. (It is desirable to have the spheres or needles at least twice the gap distance from surrounding objects.) It is not necessary to polish the spheres after each spark-over. Several thousand measurements may be made without repolishing. Discrepancies in sphere-gap tests made years ago, 1895, were probably not due to condition of sphere surface, but to changes in wave-shape and difficulty in measuring voltage. The curves may be accurately calculated. With 12.5-, 25-, 50- and 100-cm. spheres, a range of voltage from 20,000 to 1,500,000 may be covered. It must be noted that the curves are different when one

TABLE I—SPHERE GAP SPARK-OVER VOLTAGES*

12.5-CM. SPHERES

Spacing		Kilovolts effective	
Cm.	In.	Non-Grounded	Grounded
0.25	0.098	6.5	6.5
0.50	0.197	12	12
1	0.394	22	22
1.5	0.591	31.5	31.5
2	0.787	41	41
3	1.181	59	59
4	1.575	76	75
5	1.969	91	89
6	2.362	105	102
7	2.756	118	112
8	3.150	130	120
9	3.543	141	128
10	3.937	151	135
12	4.72	167	147
15	5.91	188	160
17.5	6.88	201	168
20	7.87	213	174

*At 25 deg. cent. and 76 cm. barometer. Effective sine wave voltage.

sphere is grounded and when both spheres are insulated. Fig. 2 gives grounded and non-grounded curves for the 12.5-cm. sphere, Fig. 3 gives curves for the 25-cm. sphere, Fig. 4 gives curves for the 50-cm. sphere, and Fig. 5 gives curves for the 100-cm. sphere. In all of these the drawn curve is calculated, while the crosses mark the measured values. The calculated curves were drawn long before measurements were made on the larger spheres, from laws derived from a series of tests on spheres ranging from 0.32 to 5.0 cm. in diameter. No measurements have been made on the 100-cm. sphere, but the calculated curve, Fig. 5, should be correct within a small percentage. Measured values are given in Tables I, II and III. Practical range for different diameters is given in Table IV.

TABLE II—SPHERE GAP SPARK-OVER VOLTAGES*
25-CM SPHERES

Spacing		Kilovolts effective	
Cm.	In.	Non-Grounded	Grounded
0.5	0.197	11	11
1	0.394	22	22
1.5	0.591	32	32
2	0.787	42	42
2.5	0.983	52	52
3	1.181	61	61
4	1.575	78	78
5	1.969	96	94
6	2.362	112	110
7.5	2.953	135	132
10	3.937	171	166
12.5	4.92	203	196
15	5.91	230	220
17.5	6.88	255	238
20	7.87	278	254
22.5	8.85	297	268
25	9.83	314	280
30	11.81	339	300
40	15.75	385	325

TABLE III—SPHERE GAP SPARK-OVER VOLTAGES*
50-CM. SPHERES

Spacing		Kilovolts effective
Cm.	In.	Grounded value
2	0.787	40
4	1.575	76
6	2.362	112
8	3.150	145
10	3.937	185
12	4.72	220
14	5.50	250
16	6.28	275
18	7.07	300
20	7.87	320
22	8.65	345

*At 25 deg. cent. and 76 cm. barometer.
Effective sine wave voltage.

TABLE IV

Diameter cm.	Grounded Effective kv.† range	Non-grounded Effective kv.† range
12.5	50–170	50–200
25	50–320	50–375
50	50–600	50–725
1000	50–1200	50–1400

† Sea level—spacing not exceeding 3R.

A curve on the 12.5-cm. sphere was made up to 25,000 volts at 1000 cycles and coincided with the 60-cycle curve. At 50,000 cycles similar curves were made on spheres and needles. The sphere gap curve for this frequency was somewhat lower than the 60-cycle curve, while the needle gap curve was very much lower. If a needle gap is set so as to just spark over when a steep wave-front or high-frequency voltage of constant value is applied, and a sphere gap is similarly set, and these two gaps are then placed in parallel, and the same impulse voltage applied, apparent discrepancy results. Spark-over will take place across one gap, and not the other, even when the spacing on the non-sparking gap is decreased. This will be noticed in all cases where electrodes of different shape are employed in multiple. The reason, apparently, is that energy is necessary to start rupture in the dielectric, the amount of energy varying with the shape of the electrode. This introduces a very small time element, which differs for different gaps. The effect, however, is rarely

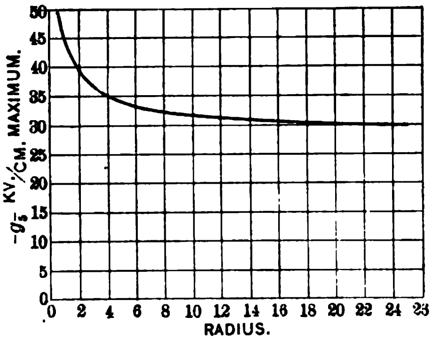


FIG. 7

noticed in commercial voltage waves, as any variation in the wave shape, in commercial waves, is not sufficiently abrupt, or is slow compared to the time lag. Naturally, as the time lag is very short, it can not be measured by the oscillograph or any other instrument with mechanically moving parts. It has been studied by comparing spark distances of different electrodes in parallel. The effect is important in lightning arrester gaps, where the protecting gap should have a smaller time lag than the protected apparatus—that is, the protecting gap must discharge before the apparatus breaks down.

In making arc-over tests, as, for instance, on insulators, the effective transformer ratio should first be calibrated by sphere gaps. The arc-over test should then be made with sphere gaps out of the circuit and voltage determined by the calibrated ratio curve. Care should be taken that the same generator and the same method of voltage control be used in the arc-over test as in the calibration test.

Method of Measurement. Up to 200,000 volts, measurement was made by a voltmeter coil giving a great degree of accuracy. Check was made on this by step-down transformer, by ratio and by corona starting point, results from which were all in agreement. Above 200,000 volts, step-down transformer and ratio were used. The voltage ratio was very close to turn ratio. A good wave, very nearly sinusoidal, was used, and oscillograms were taken for correction in low side of transformer, in voltmeter coil and in step-down transformer. The waves on high and low sides were practically the same.

Water tube resistances were used in series with the gap, limiting the arc current to from 0.25 to 1.00 ampere. The potentiometer method[1] of voltage control was used.

Calculation of Curves. The voltage gradient on the air is greatest at the sphere surface. This stress or gradient derived mathematically is expressed[2]

$$g = \frac{e}{x} f \qquad \text{(kv/cm.)}$$

where e is the applied voltage, and x is the distance between sphere surfaces. e/x is the average gradient, and f is a function of $\frac{x}{R}$, where R is the radius of the sphere in cm. The f is different in the two cases when one sphere is grounded, and when both spheres are insulated. The values of f for the two cases are given in Fig. 6. We have found experimentally that g_s, the surface gradient at spark-over, as in the case of g_v for corona on wires (see Fig. 7), increases with decreasing radius. It may be expressed[3]

$$g_s = g_0 \left(1 + \frac{a}{\sqrt{R}}\right).$$

g_s for a given size of sphere is constant for the practical range of spacing used in measuring, that is, when x is not less than about $0.5\sqrt{R}$, not greater than $3R$. When x is less than $0.5\sqrt{R}$, g_s increases very rapidly, because the spacing is then less than the "rupturing energy distance."[4] At very small spacings, gradients as high as 200 kv. per cm. are required for rupture. The "rupturing energy" has been calculated for wires.[4]

The increased value of g_s when x is large, seems to be only apparent and due to the shank, surrounding objects, etc., better distributing the flux or lessening the flux density. When

1. See *The Law of Corona and the Dielectric Strength of Air*, by F. W. Peek, Jr., TRANSACTIONS A. I. E. E., 1911, XXX, p. 1889.

2. Mathematical values of f have been derived by Russel, *Philosophical Magazine*, vol. XI, 1906.

3. The constants are approximately $g_0 = 27.2 \qquad a = 0.54$.

4. See *The Law of Corona and Dielectric Strength of Air-II*, TRANSACTIONS A. I. E. E., 1912, Vol. XXXI, p. 1051.

both spheres are insulated this effect is small and is inappreciable for large spheres, where the mathematical f may be used. When one sphere is grounded, however, this apparent increase of gradient is very great if the mathematical f_1, which does not take account of surrounding objects, is used. The f values given in Fig. 6 are for the *non-grounded case.* They are the *mathematical values* and, within the limits prescribed, *give a practically constant graded* g_s. The dotted line, Fig. 6, is the mathematical curve for the grounded case. This does not hold, due to the effect of shanks, etc., which it does not consider, and the actual curve is f_0. This was derived experimentally, assuming g_s constant. For a given value of $\frac{x}{R}$, f_0 is constant, independent of the size of the sphere (from tests on spheres from 0.32 to 25 cm.). Where x is greater than 2 R (practically 3 R) the expressions do not hold, because corona then forms before spark-over.

We have then

$$g = \frac{e}{x} f \text{ (mathematical)} \quad \text{(kv./cm.)}$$

$$g_s = g_0\left(1 + \frac{a}{\sqrt{R}}\right) \quad \text{(experimental) (kv./cm.)}$$

Therefore

$$e_s = g_0 \left(1 + \frac{a}{\sqrt{R}}\right)\frac{X}{f} \quad \text{(kilovolts max.)}$$

or

$$e_s = g_s \frac{X}{f}$$

As an example of its use—

What is the spark-over voltage for 25-cm. spheres (one grounded) 20 cm. apart?

$$R = \frac{25}{2} = 12.5$$

$$x = 20$$

$$\frac{x}{R} = \frac{20}{12.5} = 1.60$$

$$g_s = g_0 \left(1 + \frac{a}{\sqrt{R}}\right) = 31.2 \text{ kv./ cm. max.}$$

(See Fig. 7)

$$f_0 = 1.74 \text{ (from Fig. 6).}$$

$$e = g_s \frac{x}{f_0} = 362 \text{ kv. (max.)}$$

$$e = \frac{362}{1.41} = 256 \text{ kv. (effective)}$$

For small spheres the range of the constant part of the g_s curve is very small, as the effect of shanks extends over a greater range. Hence, in practise, the above expression is especially applicable to spheres 10 cm. in diameter and above.

In order that the sphere gap may be used at various altitudes, and corrections made, curves must be taken at various air densities. The full correction will be made as in the case of parallel wires:

$$g_s = \delta g_0 \left(1 + \frac{a}{\phi(\delta)\sqrt{R}}\right)$$

This is being investigated. The constants of these equations will be given when the data are more fully worked up.

C. E. Skinner: Those of us who have had to deal with the old needle gap in the years gone by know all the trials and tribulations through which we have had to go in trying to get accurate measurements of voltages higher than 50,000. In listening to the discussion of the various papers that have been offered here in the last three days, we have all noted the differences and the difficulties which have come up, and the problems which have been put up to the Sub-committee on Revision of Standards. Here is a case where it would appear that the work done could be accepted without change, and probably with little or no criticism. The use of the sphere gap for something like two years in a practical way has shown its adaptability and its convenience, its accuracy for all kinds of conditions, and the calibration, by different observers and by different methods, is very close. I think the Institute and the Standards Committee are to be congratulated on one set of papers, which ought not to require much further testing or question.

I might incidentally mention that one of the greatest difficulties encountered in connection with the sphere gap is the manufacture of the spheres themselves, as the manufacture of accurate spheres of the sizes which are required for these gaps is no easy matter.

J. A. Sandford, Jr.: There is one decided advantage, I think, in the use of the sphere gap as compared with the needle gap, which was not mentioned either by the authors of the paper or by Mr. Peek, to any one who has a large number of measurements to make by the use of the spark gap. I refer to being able to take measurements with the voltage on the test piece, and moving the gap from a wide separation up to the point where the spark jumps. If I am not mistaken, the sphere gap will give accurate readings under those conditions, and on this point I would like to have Mr. Farnsworth and Mr. Fortescue corroborate what I say. I think we will at once realize what this means. It simply means we need take only one reading in each case to establish what the voltage is at that particular instant; this in contrast to the large number of readings necessary to determine the test voltage when using the method described by Mr. Peek in the A.I.E.E. TRANSACTIONS for 1912, page 908. Of course, naturally, as in all other work, it requires several readings in order to get a fair average, but with the needle point spark gap I think it is quite well known that if the voltage is applied to the test piece with a wide separation of the points, however carefully as you may bring the points together, it will be found that the readings will vary greatly, and at least there will be some difficulty in getting accurate results. I feel that this one advantage alone has probably saved me at least 75 or 80 per cent of the time I have to use in tests of this kind.

Comfort A. Adams: May I ask those who have used this method what percentage of accuracy it is reasonable to expect in the use of the sphere gaps by men such as are ordinarily called upon to make routine tests of this sort in manufacturing establishments?

L. W. Chubb: In answer to Prof. Adams's question in regard to the accuracy reasonable to expect with the sphere gap, I will say that we believe that variations depend entirely upon the steadiness of the circuit, transformer, and switching apparatus. We believe that the true accuracy of the gap is within a very small part of one per cent. If a spark passes, the potential difference between spheres has reached a very definite voltage, and after the spark has passed, any further rise of potential between the spheres is practically prevented.

On a steady circuit below 100 kv-a., if the gap is opened beyond the breakdown and slowly closed, the results have been found to check within less than 0.05 cm. At higher voltage, when little jumps of corona in the high-tension circuit are apt to produce surging, or on unsteady circuits, repeated determinations may vary considerably, but such variation is not chargeable to the sphere gap.

If you will refer to the curves in the paper you can see with what precision the points fall upon the (envelope) curve drawn.

S. W. Farnsworth: In answer to Mr. Sanford's, question, we can say that the method of measuring a given voltage by moving the spheres together until breakdown occurs is a reliable one.

The paper which we presented, purposely avoids entering into a theoretical discussion on the breakdown voltage between two equal spheres, for this has been well covered by others. We do not feel that we are the first who have considered using spheres for a spark gap, but, so far as we know, we are the first in this country who have used large spheres for the high voltages which we are daily using, and the results which we have obtained have been so satisfactory that we feel the manufacturers will be benefited by having spheres adopted as a standard in place of the present needle-gap standard. It may be well to quote from an article by Mr. J. Lustgarten on "High-Tension Porcelain Line Insulators," which appeared in the July, 1912, number of the *Journal of the Institution of Electrical Engineers:*

" With regard to the spark gap, there is a tendency to measure voltage by the needle-point spark-gap standardized by the American Institute of Electrical Engineers. Those who have worked with the gap specified, know that it is difficult to check the American values and even to repeat their own results on successive days. One reason for this lies in the effects on the brush discharge of humidity, pressure and temperature, position of the needles with respect to the supports and neighboring objects, and the local conditions of the circuit. The brush discharge in the case of needle-points always precedes the spark (excepting at very small distances). A screening by metallic disks at the back of the needles will not prevent humidity, pressure and temperature destroying the standard gap. The author uses spheres, the diameters being chosen so that no brush discharge, or rather, no glow, will be observed at the sparking voltage. Thus all sparking voltages are below the uncertain kink stage in spark-distance curves, the kink being due to the formation of the brush discharge. The effect of humidity is eliminated. The effect of temperature can be corrected, the spark potential varying inversely as the absolute temperature. Variations in atmospheric pressure affect the spark potential less before the brush stage than after. Weicker gives the correction for spark potentials for a 10-mm. variation in pressure from 735 mm., as 1.36 per cent. Up to 70 kv. (r.m.s. values) 2-cm. diameter spheres are suitable, to 125 kv. 5 cm., and to 200 kv. 10 cm."

This article gives the opinion which an English experimenter holds of our present needle-gap and the proposed sphere gap.

Messrs. Chubb and Fortescue in their calibration of the proposed gaps, have not dealt with the effects of humidity, temperature and pressure, and while it may be advisable to investigate with respect to these, it hardly seems necessary in view of the great quantity of evidence already available.

The article by Weicker which was referred to in the quotation above, is the most complete investigation of the general subject of sparking voltages that has come to our attention. It is to be found in the " Mitteilungen ueber Forschungsarbeiten auf dem Gebiete des Ingenieurwesens," Berlin, 1911, No. 100, pp. 1-48.

His investigations bear out the results obtained by others, and it can be stated that, for the sphere spark-gap used over a separation not greater than the diameter of the spheres, the influence of the factors of humidity, temperature, pressure, frequency and electrode capacity on the sparking voltage is as follows:

Humidity—No effect.

Temperature—The sparking voltage is inversely proportional to the absolute temperature.

Pressure—The sparking voltage is directly proportional to barometric pressure.

Frequency—Within the range of commercial frequencies, namely, 20 to 75 cycles, frequency has no effect on the sparking voltage.

Electrode capacity—So far as we know, Weicker is the only one who has investigated the effect of electrode capacity, particularly, and he states that it has no influence on the sparking voltage.

Comfort A. Adams: May I ask again in regard to the frequency? It is stated that it was between 60 and 70 cycles. Was the investigation carried beyond this range, and is there any difference between a flat-topped e.m.f. wave and a very peaked e.m.f. wave?

L. W. Chubb: The frequency range for our work was from 25 to 70 cycles. The results were independent of frequency as far as we could judge. The results expressed in terms of maximum voltage were also found to be independent of wave-shape through a rather wide range of voltage distortion. I believe that it can be shown both experimentally and theoretically that the break is dependent upon only the maximum potential between spheres, and independent of frequency even as high as one million cycles, provided the spheres are working below the corona point.

Oscillograph tests were made with some very peaked waves at 100 kv. and directly on the high-voltage circuit. The records showed that the break came at the peak of the wave, as closely as could be measured.

Voltage across the gap was recorded on the film. No change could be found in the cycles preceding the break. The voltage dropped quickly to zero when the break came, and by comparison with the previous cycle it was evident that this drop started at the maximum point. Such is not the case when the needle gap or other electrodes are used above the corona point and in series with resistance. There is quite a disturbance due to the streamers before the break.

I agree with Mr. Peek that it requires a certain amount of time for the spark to take place, but I believe that with the spheres it will take place if the voltage reaches the critical point and drops at a rate corresponding to the peak of a million-cycle wave. Certainly the unstable point has been reached and the electrostatic charge of the spheres can flow through the spark just as quickly as it can flow back into the circuit. Such would not be the case with the progressive discharge of the needle gap, as the electrostatic charge would be expended in ionizing the air near the electrodes.

Percy H. Thomas: It seems to me with this sphere gap we have made quite a distinct advance in the measurement of alternating-current voltages. I want to suggest two thoughts; the first is this: In view of the relatively high capacity of the gap itself, on account of the spherical form of terminal, compared to the needle-point terminals, a certain material charging current will flow to the terminals. If series resistance is used, as is usually the case with the gap, we cannot rely on the gap to take account of disturbances of all frequencies, because the resistance will cause a drop on high frequency on account of the charging current of the spheres. For waves of 60 cycles, or 120 or 133 cycles, undoubtedly resistances can be introduced so small as not at all to interfere with the accuracy of the method, but with frequencies of 10,000 cycles or 100,000 cycles, I should say it would be necessary to be very careful to see that no resistance in series with the gap terminals was vitiating the results.

One result of this condition is to cut out from the gap the effect of any accidental oscillations of very high frequency that may be superimposed on the alternating voltage, and if in testing a transformer, some little spark from the terminal somewhere sets up an oscillation, with spark-gap needle points, that oscillation will make the needle-points break down. With the sphere gap, using a large series resistance, I should expect the gap would not show these little superimposed high-frequency oscillations.

The second thought I wish to offer is that the sphere gaps can not always be safely used, except without series resistances, for determining the maximum voltage on high-frequency experiments, and even without series resistance, it could be used only where its capacity is small in regard to the capacity of the apparatus which is being tested.

I want to take this opportunity to ask Mr. Peek some questions, with regard to the actual time required for the breakdown of an air-gap. Suppose we apply instantly, a voltage to an air-gap four times as great as the voltage necessary to break it down, I would like to know if Mr. Peek can tell us how long it will take for the first flow of current to occur across the gap, assuming there is no inductance in the system other than the discharge gap.

I would like also to have some statement of the experimental evidence on which the conclusion is based that there is a time-lag in the breakdown of an air gap.

This is a very important and a very interesting matter, and it is a thing which keeps coming up—this matter of the lag of the spark gap at breakdown, and it is put forward as the explanation of a great many of our high-frequency phenomena. Personally I am open-minded on the subject. I have not been convinced, by any of the experiments I have so far seen or heard reported, of the existence of this lag as a material factor, and yet I am not sure that there is not such a thing. Are we not justified in concluding that, if there is a lag in the breakdown of an air-gap, it is only material for extremely high frequencies, less than a millionth of a second? Take, for example, the famous surges of Hertz, with Maxwellian electromagnetic waves. Hertz explored with a small circle of wire having a small air-gap in it, and his discharge apparatus, if I am not mistaken, was small and of very high frequency. He had sparks across a small air-gap. If his air-gap must necessarily have considerable time to break down, it could not break down on frequencies the alternations of which are less than the time of breakdown, because the mechanical force on the electron will alter in direction with the alteration of the applied e.m.f. The air-gap was able to break down on the frequencies Hertz used, which were very, very high, and if this logic is sound, the range of spark lag must be much less than a million cycles per second.

Paul M. Lincoln: The gaps which Hertz used in his experiments were extremely minute. I do not remember just what they were, but they were of the order of a few thousandths of an inch at the most, as I recall. As I see it, this breakdown of the air is a progressive action; particles of the air next to the terminals become ionized and they, by collision, ionize other particles. That means, if I am correct, that the time of breakdown of the air-gap is a function of its length, so that the air-gap which Hertz used would break down many, many times quicker than the air-gaps described, for instance, in the paper read by Mr. Thomas in December.

C. Fortescue: In the first place, the condition of breakdown through the insulator is entirely different from the condition of breakdown between two large spheres that are separated a distance less than their diameter. We might compare the operation of the small sphere or an insulator and the operation of the two large spheres which are separated less than their diameter, to the operation of an ordinary trigger of a gun and the operation of a hair trigger; the sphere gap being represented by the hair trigger. The very instant that the intensity at the surface of the sphere reaches a certain point the breakdown occurs. No energy is required to complete the rupture outside of that already stored in the electric field between the spheres. The distance the spark has to travel is a minimum, and the action is like that of a hair trigger.

Mr. Thomas makes mention of the lagging of the spark. It may be safely said that the sphere gap is, in that respect, infinitely superior to any other method of measuring the maximum voltage that has yet been suggested. In the case of the needle spark-gap, in order to produce breakdown, all the air surrounding the points has to be ionized. This means that there is an energy component in the e.m.f. between the points and the edge of the corona which not only produces a lag of the actual e.m.f. at the point of breakdown but also causes a change in the wave form of the e.m.f. between the edges of the corona from that between the points. Thus in the needle spark-gap there is no doubt quite an appreciable lag for very high frequencies, but with the sphere spark-gaps as we have recommended them, I think that the lag, if such does exist, is extremely small.

As far as the small spheres are concerned, they produce just about the same action as needle-gaps. Wherever there is corona at breakdown there is bound to be a lag, because there must be enough energy in the oscillation to produce the corona before breakdown can take place, and where the energy has to be stored in the field there must necessarily be a time lag of the e.m.f. at the points of rupture. This is entirely eliminated where large spheres are used because then we have a condition of breakdown without corona.

M. W. Franklin: I do not happen to call to mind now any definite researches bearing on the matter of the time required for the formation of a spark, and therefore I can only speak from a sort of integrated activity in reading and following such papers for the last ten or fifteen years. As I remember it, it is a definite, experimental fact that there is a time element in the formation of a spark, even in the case of a spark between two spheres, and that time must, I think, be reckoned in millionths of a second at the utmost.

Furthermore, in regard to the suggestion that Mr. Thomas made, it does not at all follow that it requires even a millionth of a second for a spark to break down, that that spark gap cannot break down at ten million cycles. If you only consider that there may be a dozen cycles of e.m.f., each one causing a to and fro surging of the two electrons which happen to lie in the body, and that to and fro surging creates more electrons, until there is a cumulative effect, you can see that such a thing is possible from a half dozen cycles of enormously high frequency, and I do not think it possible to argue, because the spark gap breaks down at 10,000,000 cycles, therefore the time lag must be less than a ten-millionth of a second. I am quite firmly of this opinion, not from a theoretical point of view, but merely as a result of having read nearly everything that has been published on the subject of discharge through gases during the last fifteen years, and I am sure Mr. Peek and those who have been working on the subject are justified in thinking of a time-lag as existing. I am quite sure, also, that that time must be extremely small, in the neighborhood, no doubt, of millionths of a second, but I am also quite sure that the criterion suggested by Mr. Thomas would not be a proper criterion, even for forming an estimate of the extent of that time lag.

Charles P. Steinmetz: A few years ago Mr. Hayden and myself made some rather extensive investigations on the disruptive strength of air between spheres and needles, using impulses. The results of these investigations show a time lag, which was startlingly large, and measured not by microseconds, but by milliseconds, under the conditions of the experiments. Our conclusions, however, were that, (at least under our test conditions), it was not so much a time lag, as an energy lag; that the breakdown is not a question of time but a question of energy and that the time lag may vary with the rate of energy supplied, and be variable at the disruptive point.

That puts an entirely different phase on the question of high frequency. You may have millions of cycles and still no appreciable time lag, because as each successive half wave subsides, the next half wave continues. So it is quite likely that the phenomenon which we call the time lag, and which has been more or less elusively indicated in very numerous tests and observations, is merely the result of the obvious fact that it requires energy to break down air, and that energy must be supplied, and that the breakdown, therefore, must be compared to the amount of time necessary to bring that energy to the point of disruption.

That leads us, however, to some interesting conclusions. At the spark-gap between needle-points, the corona or brush discharge which appears is very extensive and affects an enormous volume of air. The amount of energy which is absorbed by the brush discharge is enormously large, very many times greater than the energy of electrostatic charge of large spheres, and experience seems to show that the time lag in discharging at very high voltages between needle points is very many times greater, and that the energy of the current flowing into the needle points before the discharge, is also very much greater, than the energy or current absorbed by large spheres, and that also explains the superiority of spheres, provided they are used at such voltages as do not occasion corona. Under these conditions no energy is absorbed in the gradual breaking through of the air. On the contrary, energy is statically stored in the spheres, and is available to puncture the air.

As regards our recent discovery that spheres are really better than needle-points, you must realize that that applies only within a certain range of voltage. Probably for low voltages, up to 20,000, the needle-gap will remain the standard, because the sphere gap at these low voltages is so small as to be inaccurate, owing to the fact that any arcing at the surface of that sphere, which is negligible in its effect at distances corresponding to 100,000 volts, is fatal and entirely changes the disruptive voltage when you come to 2000 or 3000 volts.

That explains why in those early days where the range up to 20,000 volts covered practically the entire important field of high voltages, when you carefully studied the relative advantages of the needle-gap and sphere gap in the early 90's, the general consensus of opinion was that the needle-gap was the only one which could be considered, because it was definite and was of a length which is measurable, ranging from 5 mm. to 20 mm. But that very advantage becomes a fatal disadvantage when you are dealing with a half million volts or more, and the needle-gap becomes many feet in length. You then have to build a specially large structure to accommodate the spark-gap, and you have to dissipate so much energy in the corona, before you get a discharge, that it requires very large apparatus, very large time lag, and a smaller energy oscillation which is not observable at all. It is merely a question of the relative value of voltages, whether the sphere-gap or needle-gap is preferable. At lower voltages we do not recommend changing from the needle-gap to the sphere-gap, but when you come to voltages of such magnitude that the sphere gap is a measurable length, then the sphere gap is the more reliable and the more workable method.

C. Fortescue: I think that Dr. Steinmetz has summed up the attitude of those who have recommended the spark-gap very thoroughly. There are one or two points in this spark-gap paper of ours that I want to call attention to. We say: "The effects of atmospheric pressure and humidity have also had only a negligible effect on the break-down voltage." I want to correct that statement and say that what we meant was this: that during the time we made the tests the change in temperature and atmospheric pressure was too small to produce an appreciable effect.

I would like to say a few words in regard to Mr. Layman's experiment some years ago in which he found those discrepancies. I think the discrepancies were due to the fact that the spheres were so good; in other words, oscillations which take place in any commercial circuit will break down the sphere-gap, where they might not break down the needle-gap, and I think that is probably the cause of his discrepancies. We found when we had a circuit that was kept very steady, and carefully observed, that all the points were consistent throughout, but we happened to work part of the time on a circuit on which some cranes were operating, and every time a crane started or stopped it produced a surge which would break down the spark-gap, and that was noted time and again, and pointed plainly to the fact that the slow surge set up by the starting current of the crane caused an oscillation which, at the peak of the wave, produced a superimposed ripple which broke down the spark-gap.

C. E. Skinner: I want to add that the very fact that the sphere gap does break down, due to these surges, is a distinct advantage in its use for measuring voltages where we are dealing with insulation, because it is these same surges which break down the insulation.

Percy H. Thomas: I think that Dr. Steinmetz has put this thing in pretty nearly its true light. It does require energy to cause a discharge through air, but it seems to me reasonable if we can apply the requisite amount of energy in a very short time, no matter how short, we can get the discharge in that time. Taking that point of view, we have only to consider how much energy has to be put in and how quickly it can be supplied. As Dr. Steinmetz pointed out, the electrostatic charge on the sphere-gap does supply stored energy very close to the breakdown point, and is thus able to maintain a difference of potential rigidly at that point.

The amount of energy which it takes to start a discharge through the air, will I think, be found to be very small. The discharge is started by the velocity produced in the electrons, and it does not take much time to start the cumulative liberation of more electrons. Furthermore, the time that it takes to produce such motion must be extremely minute, since these very light bodies have to move only a very short distance.

L. W. Chubb: Dr. Steinmetz has mentioned his experiments made to find out whether a single impulse of voltage would break the same air gap as a continuously applied voltage. The authors conclude that the break is a function of time, but I believe that the maximum peak of the impulse voltage was the true variable. I would like to ask Dr. Steinmetz whether this impulse voltage could not have been very much in error due to the flux lag, eddy currents in the core, and leakage reactance, in such a transient test.

Charles P. Steinmetz: I think I can explain that this phenomenon could not have been present, because whatever magnetic effect eddy currents in the iron can exert, would be exerted on the primary and secondary of the transformer simultaneously. The counter e.m.f. appears instantly in the direct-current primary supply by the closing of the switch, and therefore it must have appeared instantly in the case of the secondary. The only source of error which might exist would be the distributed capacity of the secondary winding, and that can be calculated, but in these particular transformers we measured the distributed capacity, at least the magnitude of it, and so knew in which condition of test that effect was negligible; that is to say, negligible within the errors of test, a matter of 10 per cent, more or less.

Since that time we have repeated some of our tests and have taken an oscillogram of these waves, that is, these single impulses, and we employed so much more energy that you can, in the oscillograms, see the effect of the initial rapid rising and tapering of the impulses. We checked up that phenomenon, and while there is some error, the error is of very small magnitude indeed.

L. W. Chubb: I would also like to ask Dr. Steinmetz whether the sparking distances and size of terminal were such that they worked above the corona point?

Charles P. Steinmetz: Most of them were above the corona point. Some of them, those with spheres, were below the corona point. At that time, which was before the investigation of corona made with Mr. Peek, we did not specially register that, but my impression is that in previous cases we did observe marked corona, and previous to that we arranged the curve of sparking distances between the spheres, so it appears that at the circumferential condition, with very sharp and marked break in the characteristic of the disruptive strength curve, which is observable, that in the case of the sphere gap at that point the corona begins. I think corona has a material time-lag resulting from its energy-lag, but that phenomenon requires still further investigation. We have started some investigations trying to study this form of corona, but have not proceeded far enough yet to arrive at any satisfactory results.

J. B. Whitehead (by letter): There will be little dissent from the opinion of the authors that the needle-point air gap as at present described by the A.I.E.E. Standardization Rules is an inconsistent and unsatisfactory standard for voltage measurement. In support, however, of that much abused apparatus it may be pointed out that the standardization rules have apparently not taken proper account of the best information and study of the needle gap as a means of measurement of voltage. The instrument has been studied somewhat extensively by W. Weicker and his results published in the *Electrotech. Zeitschrift* in 1911. Without in the least advocating the needle gap as a standard, it may be pointed out that it has now been shown that the needle gap gives widely varying results below 60,000 volts, but that above that figure under uniform external conditions the results are very constant, provided the angles of the points are chosen between 20 deg. and 100 deg. Sewing needles, therefore, if used in the spark gap introduce a source of error which it would be fairly easy to remove by stipulating a wider angle for the point.

The use of the sphere gap was suggested by Alexander Russell in 1907 as a satisfactory arrangement for testing dielectric strength. In an earlier paper, referred to by the present authors, he attempted to reduce the formulas of Kirchhoff for evaluating the electric intensity between the spheres. In this way he aimed to present ready methods for calculating the voltage at which a given sphere gap would break down.

He also worked out a number of cases from the simpler though still somewhat unwieldy expressions and presented them in the form of a table for reference. Subsequently Russell's discussion and results were attacked by de Kowalski and Rappel in an article in the *Philosophical Magazine* for 1909, in which they presented a number of careful measurements with alternating voltages. They used spheres up to 30 cm. diameter but did not carry the width of gap above 2 cm. Russell has presented two other papers dealing with the sphere gap to the Physical Society of London, which have been presented in the Proceedings of that Society for 1911. The result of the discussion so far is that there is considerable doubt as to whether the electrical intensity within the sphere gap may be accurately calculated and the fact that measurements of different observers show a rather wide discrepancy.

The authors have not in my opinion strengthened the case for the sphere gap. They have made an important contribution to the experimental knowledge of this instrument, but their paper hardly presents a sufficiency of data to warrant the claim that the sphere gap should be used as a standard of measurement. I wish to express my interest and admiration for the ingenious method they have adopted for deriving the maximum value of voltage.

In offering my few criticisms I trust that the authors will realize that they are due only to my conviction that the sphere gap presents almost, if not quite, the same limitations as the spark gap. First, while admitting possible influences of pressure, temperature and moisture, the authors present no data showing the magnitude of the influence or the absence of influence of any one of them. Second, the influence of proximity of extraneous objects is granted by the authors and suggestion made of various screens and future measurements to study this influence. Third, little if any statement is made of the degree of accuracy with which the observations may be repeated. In fact in this connection the single values as given in the tables show discrepancies in many cases of an order of magnitude of from 1.5 to 2.5 per cent; *e.g.* with the 25-cm. spheres for the gaps of 4 cm. and 11 cm., for the 37.5-cm. spheres the gap at 5 cm., and with the 50-cm. spheres the gaps at 8 cm. and 12 cm. have been selected without any close scrutiny of these tables. Although the range in which the authors' observations coincides with those of de Kowalski and Rappel is very narrow, there is a considerable discrepancy in the values obtained.; *e.g.* for gaps of 1.35 and 1.23 cm. and 30-cm. spheres the results of the latter experimenters show 37.5 and 34.6 kilovolts respectively. The present authors also fail to interpret the symbols at the tops of their tables. It would be interesting also to know by what method they arrive at the figures of electric intensity as given in Fig. 4.

The principal objection to both the needle gap and the sphere gap in my opinion lies in the fact that they do not take advantage directly of natural constants. It has been amply shown now that the electric strength of air depends markedly on the distribution of electric intensity in relation to the volume of air. For this reason it has heretofore proved impossible to present a certain method for calculating length of a needle or sphere gap to break down at a definite value of voltage. On the other hand, the use of the appearance of corona on the interior of two concentric cylinders obeys a law upon which close agreement now obtains among many observers. The influences of pressure, temperature, moisture have also been studied with resulting good agreement. It is therefore possible to write down at once the dimensions of a concentric-cylinder measuring apparatus which under given conditions of temperature and pressure will develop breakdown at the surface of the inner conductor at a given voltage. The objections to this method are the difficulties of observing the point at which corona starts, and the necessity of changing the inside cylinder for different voltages. The first of these objections is not a serious one and it is my hope soon to present to the Institute a paper describing the adaptation of the above principle as a means of measuring voltage. For over two months daily observations have shown a consistency under widely varying atmospheric conditions to within less than 1 per cent.

F. M. Farmer and E. D. Doyle (by letter): This proposal to measure the deviation of the wave form of an alternator from a pure sine wave by means of condenser reactance appears to be so simple and practical that one wonders why it has not been suggested before. The present Institute standard is indeed unsatisfactory, as it not only does not penalize the harmonics in proportion to their undesirability but is cumbersome and expensive to apply.

The reason for using a large inductive reactance to determine the standard value is not apparent. It would obviously seem

TABLE I

(a) Measurements on a badly distorted wave *without* voltage transformers.

Test No.	Capacity microfarads	E.m.f. volts	Current milliamperes	Distortion ratio
	Ammeter—Weston 7.5 volt dynamometer type voltmeter No. 3201. Resistance 55 ohms, inductance 19 millihenries.			
1	1.959_5	107.2	8.13	1.02_6
2	1.007	122.9	8.0	1.71_5
3	1.007	114.8	9.04	2.07_3
4	1.007	129.4	11.47	2.33_8
	Ammeter—Weston 7.5 volt dynamometer type voltmeter No. 6776. Resistance 93 ohms, inductance 19 millihenries.			
1	1.007	107.0	41.4	1.01_8
2	0.603_3	122.1	47.4	1.70_4
3	0.603_3	114.5	53.9	2.07_0
4	0.603_3	129.4	69.1	2.34_4
	Ammeter—Weston soft iron type milliammeter No. 435, 74 milliamperes. Resistance 101 ohms, inductance 292 millihenries.			
1	1.007	106.7	42.8	1.05_6
2	0.454_1	122.1	40.7	1.94_8
3	0.454_1	144.5	65.1	3.32
4	0.454_1	128.7	74.6	3.38_2
	(b) Measurements on a badly distorted wave with voltage transformers.			
	Voltage stepped up and stepped down with two 6600-110 volt, 200-watt voltage transformers. Ammeter, Weston voltmeter No. 6776.			
1	0.603_3	105.2	24.3	1.01_4
2	0.603_3	120.6	46.9	1.70_6
3	0.603_3	112.3	53.2	2.08_3
4	0.603_3	126.1	67.4	2.34_8
	Voltage stepped up and stepped down with two 2200-110 volt, 50-watt voltage transformers. Ammeter, Weston voltmeter No. 6776.			
1	1.007	109.0	42.1	1.01_7
2	0.603_3	123.9	48.1	1.70_7
3	0.603_3	117.1	55.8	2.09_5
4	0.603_3	130.4	70.1	2.36_3

Test No. 1, fundamental only, (60 cycles).
Test No. 2, fundamental with 53.5 per cent third harmonic.
Test No. 3. fundamental with 37.5 per cent fifth harmonic.
Test No. 4, fundamental with 53.5 per cent third harmonic and 37.7 per cent fifth harmonic.

more simple to use a condenser of known capacity, in which case the sine wave reactance is of course simply $X_s = \frac{1}{C\omega}$. The distortion ratio would be obtained by the simple measurement of the condenser reactance on the distorted wave.

As the convenience of the application of any new standard is of great importance, it occurred to the writers that some figures taken in actual measurements would be of value, since Mr.

Davis has not given any figures indicating the magnitude of the quantities with which he had to deal in obtaining the values that he gives in his paper. Furthermore, two questions arise in the application of this method which made it desirable to make some tests. First, is it practicable, in order to avoid the use of large condensers, to use ammeters of small range without introducing too much resistance and inductance in series with the condenser? Second, can the distortion of high-voltage machines be measured by using small capacity (voltage) transformers?

TABLE II

(a) Measurements on a moderately distorted wave without voltage transformers. Current measured with Weston voltmeter No. 6776.

Test No.	Capacity microfarads	E.m.f. volts	Current milliamperes	Distortion ratios	
				By calculation*	By measurement
1	1.007	119.5	47.5	1.04_2	1.04_6
2	1.007	118.7	51.4	1.11_8	1.13_4
3	1.007	120.2	53.5	1.15_2	1.17_2

(b) Measurements on a moderately distorted wave with voltage transformers. Current measured with Weston voltmeter No. 6776.

Test No.	Capacity microfarads	E.m.f. volts	Current milliamperes	Distortion ratios	
				By calculation*	By measurement
Voltage stepped up and down with two 2200-110-volt, 50-watt, voltage transformers.					
1	1.007	116.4	46.4	1.04_2	1.04_4
2	1.007	116.4	50.3	1.11_8	1.13_6
3	1.007	116.8	52.1	1.15_2	1.17_2

Test No. 1, fundamental with 10.2 per cent third harmonic.
Test No. 2, fundamental with 10.2 per cent fifth harmonic.
Test No. 3, fundamental with 10.2 per cent third harmonic and 10.2 per cent fifth harmonic.

NOTE: " Calculated " values of distortion ratio obtained as follows:
For fundamental wave $R = 1.00$ by definition.
For distorted wave,

$$R = \sqrt{\frac{E_1^2 + (3E_3)^2 + (5E_5)^2}{E_1^2 + E_3^2 + E_5^2}}$$

where E_1, E_3, E_5, are mean effective voltages of fundamental, third harmonic and fifth harmonic respectively.

* Sine wave assumed. Oscillograph tests of the charging current of a condenser showed that the fundamental is not a perfect sine.

Tables I and II show results of tests made at the Electrical Testing Laboratories with various distorted, waves with and without voltage transformers. A high-grade subdivided mica condenser was used and the current was measured with different types of ammeters.

These results lead to the following conclusions:

1. As is to be expected, milliammeters of the soft iron vane type have too much inductance. The inductive reactance becomes appreciable, so that the voltage across the condenser is no longer

equal to the generator voltage. Therefore, in order to use an ammeter of this type it should not be less than 250 milliamperes in range and suitable indications on such an instrument would require about 10 or 12 microfarads capacity at 25 cycles.

2. Voltage transformers of as small as 50 watts capacity can be used for stepping down the voltage without introducing an objectionable error.

3. All that appears to be necessary to obtain the distortion ratio of any machine as defined in the proposed definition is a standardized subdivided mica condenser of one or two microfarads capacity, a 100-milliampere ammeter with low inductance such as a dynamometer or hot wire instrument, a voltmeter of any standard type, a standard voltage transformer and a speed counter. The condenser should obviously be a high-grade one in which the phase angle is within a very few minutes of 90 deg.

Charles P. Steinmetz: The purpose of Mr. Davis's paper is to recommend the establishment of a wave standard which shall be based on the distortion ratio; that is, on the ratio of the current taken by a condenser with the distorted wave and the current which the same condenser would take with a sine wave. Mr. Davis's paper also suggests a method of obtaining the distortion ratio, where the alternator is not accessible. This condition exists when you are considering a commercial circuit, as, for instance, in my laboratory. Under such circumstances it is not always practicable to determine the exact frequency. You can use the same wave with the same frequency, smoothing out the higher harmonics by high inductance. The natural way, where you have a generator available, would be to measure capacity with a condenser and have the capacity exactly known, and measure the current input at measured voltage, and also measure the frequency.

Naturally, what we are interested in is the voltage wave as it exists during all conditions of operation, not only at full load, but at no-load. Thus it would be very nice to standardize and specify that the voltage should be taken at no-load as well as at full load, and, more particularly, at condenser load, where the distortion is probably greatest. The only trouble is that we have already spent much time in the discussion of "equivalent" loads and it will be agreed that the subject presents grave difficulties. You see the importance of specifying a wave test at full non-inductive load and at full condenser load, where we are discussing means of getting equivalent load tests and equivalent heat tests. Even though we may know the condition which we should desire, we cannot load the generator, because we do not have the power available. Zero power factor load naturally does not mean anything here, because while it may be leading current it is not a condenser load, and does not exaggerate the harmonics. Thus the only practical solution seems to consist in assuming that the different harmonics which appear under load, would probably be there at no-load, and would show the distortion ratio of the no-load wave. It might be exaggerated at full load, especially condenser load, but we merely make allowance for that by specifying a low enough distortion ratio.

The fact is that the only additional harmonics which should appear under load are probably those resulting from the field distortion. Field distortion is the effect of non-inductive load. We do not much care for the harmonics, because a non-inductive load is equivalent to a resistance load, where the current follows the energy wave, and whatever distortion the voltage wave may have it would not have any serious effect. The case where harmonics are apt to be objectionable is mainly where the load is such as to exaggerate them, condenser load, and in that case the armature reaction is not distortional, but magnetizing, and therefore it is not practical to introduce additional harmonics. We thus see that we can get from the no-load test some good indication of the wave shape which we will need in practise. This appears to be the only test which is practically feasible.

B. G. Lamme: Mr. Foster's paper shows a great number of wave forms of different generators. Apparently one object of the paper is to show what variations seem to be permissible in good practise. It is a fact that many alternators working today without any trouble whatever, have what appear to be very bad wave forms. It is only in special cases that wave forms give any particular trouble, and sometimes the cause of the trouble does not really lie in the machine itself, but the bad wave form in connection with external conditions may result in disturbances in the system.

It is an old, well-known fact that any symmetrical wave form can be split up into a fundamental and harmonics of the odd order. A wave form obtained by means of the oscillograph may show us by analysis what harmonic is sufficiently large to cause disturbance, but it does not show us how to eliminate the harmonic. There are some suggestions in Mr. Foster's paper as to how this can be done by distributing the windings differently, or by differently shaping the poles. However, shaping the poles by blindly cutting off what one thinks should cure the trouble, is a dangerous proceeding, as this might result in exaggerating the very harmonic that it is desired to eliminate. In order to shape the poles to obtain the desired result, it is necessary to predetermine the diagram representing the field flux distribution, that is, the field form; and from the study of this and its relations to the e.m.f. wave, one can determine pretty definitely just what change is necessary to eliminate any particular harmonic.

I note that Mr. Foster mentions a hunting tooth to eliminate harmonics. I wish to call attention, however, to the fact that with a polyphase machine, one should be careful in using a hunting tooth, or an exact symmetry of phases will not be obtained. In three-phase machines, three hunting teeth should be used in order to obtain symmetry. One hunting tooth will not give the desired result.

A. E. Kennelly: In regard to Mr. Davis's paper, the plan presented therein seems to be a great advance over that offered in the existing Institute rule for determining wave form. The existing rule calls for determining the wave shape, then determining the corresponding equivalent wave shape, superimposing the two and measuring the greatest difference. That is a tedious operation which involves a considerable amount of personal equation. This proposed plan seems much more definite, more easily accomplished, and much more simple. It is, however, as has been before suggested by Dr. Steinmetz, a little indefinite in that it calls for the measurement of reactance in a condenser, and that suggests measuring the length of a bar, by measuring the volume of the cubical bulk whose side was the length of this bar. Would not it cover all the purposes Mr. Davis has in mind, if we were to define simply the distortion ratio of a voltage wave, as the ratio of the current produced by that voltage in a condenser to the current supplied in the same condenser from the sinusoidal e.m.f. of the same root-mean-square value? That definition would call for a standard condenser of specified dimensions, perhaps a standard frequency meter, a standard voltmeter, and a standard ammeter. By these instruments the measurements could be made without involving the definition of reactance in certain condensers.

M. G. Lloyd: Most of us are agreed that the present rule defining the limit of tolerance to departure from the sine form of wave is unsatisfactory, and Mr. Davis has given a possible substitute for it. All three papers point out that the present rule does not sufficiently penalize the higher harmonics under certain practical cases, such as the charging current on a transmission line. There are other cases, also, where the higher harmonics have greater importance than the rule gives to them, as, for instance, in eddy-current effects where they are large enough to make ripples in the flux wave. In other cases, however, it is not the higher harmonics which are most objectionable. In the case of hysteresis loss the lower harmonics are more detrimental than the higher harmonics, if compared on the basis of their equivalent value, as given by Mr. Davis's rule, that is, assuming them in inverse proportion to the order of the harmonic. It must be obvious then that in some cases this distortion-factor is a desirable criterion, while in other cases some other property of the wave is of greater importance, such as a form-factor; and in still other cases, perhaps an amplitude-factor or crest-factor.

While I am hardly ready to indorse the suggestion of Mr. Davis, it does seem better than the present rule. If the distortion-factor is to be made the measure of sinuosity, I should like to make the suggestion that the limit of tolerance suggested by Mr. Davis be cut down. I think he is entirely too liberal in defining what we shall accept as sufficiently close to a sine wave, or what may be called a conventional sine wave. To illustrate this I have computed the values in the accompanying table, applying this limit of tolerance to the single case of hysteresis loss, such as would occur in the core of a transformer, since this is one of the practical cases to be considered in weighing the effect of departure of the wave from the sine shape.

In this table the percentage variation in hysteresis loss is given for the case of a single harmonic (a frequency three or five times that of the fundamental) for the two extreme cases where the phase angle is such as to produce the greatest increase and the greatest decrease in the wattage. The last column in the table gives the range in hysteresis values possible for the case of the third or fifth harmonic and the particular values of distortion ratio indicated.

CHANGE IN HYSTERESIS FOR THIRD AND FIFTH HARMONIC HAVING EXTREME VALUES OF PHASE ANGLES—FOR GIVEN DISTORTION RATIO

Distortion ratio	*a*	Order of harmonic	*a/n*	Increase in per cent	Decrease in per cent	Range in value possible, per cent
1.05	0.342	3	0.114	5.0	6.7	11.7
		5	0.068	1.6	2.6	4.2
1.10	0.493	3	0.1 4	6.5	10.8	17.3
		5	0.099	2.2	4.0	6.2
1.15	0.615	3	0.205	7.5	13.5	21.0
		5	0.123	2.7	5.3	8.0

It is seen from this table that if we allow a distortion ratio of 1.15 it is possible, with the third harmonic alone present, to have a hysteresis loss differing 13.5 per cent from the value with a sine wave, and in the case of the fifth harmonic the difference may be 5.3 per cent. The possible range in value is 21 per cent for the third harmonic, and eight per cent for the fifth harmonic. Even with a distortion ratio of only 1.10, the range for the third harmonic is 17.3 and for the fifth harmonic, 6.2.

The table illustrates that in the case of hysteresis it is the lower harmonics that are most objectionable, and it also illustrates, in my opinion, that the permissible allowance suggested by Mr. Davis is too great to come under the definition of a sine wave. If we should make a distortion ratio of 1.05 the limit of tolerance in a sine wave, it will be seen that the range in the value of hysteresis possible with a third harmonic is 11.7 per cent. It consequently seems desirable to me to limit the conventional sine wave to this value.

In adopting such a limit it is not a question of choosing a figure which will bring the best generators or other apparatus within the limit, for it is not necessary that a good machine be considered as giving an approximately sinusoidal wave. To cut down the limit merely means that instead of being able to say that a certain generator gives a sine wave within the limits of the Standardization Rules we should have to say that it exceeds the limit by — per cent. The only reason I see given by Mr. Davis for such a wide latitude for distortion ratio is the fact that the average for 22 commercial alternators was 1.135. In my opinion the object in defining a sine wave should not be to make the average commercial alternator meet the definition.

While on three-phase circuits the third harmonic does not appear and the range in hysteresis value with the fifth harmonic would not be greater than eight per cent with a distortion ratio of 1.15, we should bear in mind that our definitions are not to be confined in use to the most common commercial conditions, but must be applicable to any case that may arise.

L. W. Chubb: Dr. Lloyd has covered about what I was going to say in regard to the low harmonics.

I think that it is a mistake to set a new standard of wave shape which penalizes the higher harmonics, in order to reduce transmission troubles, and disregards the low harmonic distortions which affect iron losses and in some cases prevent satisfactory parallel operation.

The paper starts with three objections to the present specification, which are not valid if the tester knows how to take wave-shapes and check them up according to the present specifications.

In answer to the first I would say that any maker of machines under a wave shape specification should have an oscillograph available. The oscillogram of the voltage wave can profitably be used for record, and to check the design, as well as to show that it meets the specification.

I object to the second objection because it is not necessary to measure any ordinates to obtain the equivalent sine wave. This I will show later.

I object to the third objection because no trial calculations are necessary to determine the position of minimum maximum deviation, nor is it necessary to plot any curves except to draw a circle of a certain diameter with ordinary compasses.

Suppose the wave shape in question is a polar curve. Its area which is proportional to the root-mean-square value can readily be measured with a planimeter and the equivalent sine represented by a circle of the same area. This circle, drawn on a piece of thin or transparent paper, should be placed over the polar curve of the wave in question and a needle should be driven through a point on the circumference of the circle placed over the pole of the other curve. The small piece of paper is then to be moved around the needle as a pivot until the minimum maximum deviation between the two curves measured on a radius vector is obtained. This difference, expressed in per cent of the diameter of the circle, is the final result.

The polar curve can be obtained by a special mechanical tracing table if the original record has been taken in rectangular co-ordinates. But it is better to take the picture in polar form at once by revolving a celluloid disk film at synchronous speed in front of the slit of the oscillograph. The latter method, of course, eliminates the tracing operation, which is apt to introduce error.

The new wave shape standard proposed by Mr. Davis and the test method to be used give the designing engineer no idea of the resulting wave. All he will know is whether his machine meets a certain ratio. If his machine fails to show the specified ratio he will want to know why, and what can be done to reduce the harmonic distortions. If an oscillogram of the wave is taken, all of the nêcessary information can readily be obtained and a permanent record of the wave can be kept on file for future reference.

Mr. Lamme forgot to mention one source of quite serious harmonic distortion—that which is caused by the total flux pulsation in the machine.

If the number of teeth is divisible by the number of poles, there is a pulsation of the air gap reluctance under each pole, and as these pulsations will all be in phase, the total flux in the machine will pulsate, and cause decided ripples in the voltage wave. If the teeth per pole is an even number, this ripple will be composed of the odd harmonics next above and below this even component. In other cases it may be of tooth frequency or composed of the two odd components on each side of double tooth frequency.

Except for these tooth pulsations the wave shapes of machines can be quite accurately obtained from the designer's plot of the field form.

We find that the most satisfactory way to examine the wave shape of a generator is to take the oscillogram and find out not only how much distortion there is, but where the distortion is and what the cause is.

Oscillograms can profitably be taken and analyzed in all cases in which there is any question regarding the wave shape.

Good wave-shape design is a matter of evolution. Improvement comes from the study of the actual waves of the machines.

Accurate harmonic analysis can be made by taking the oscillogram on a disk film run at synchronous speed, printing it on a zinc disk, etching the line into the disk and placing this etched record on the table of a mechanical harmonic analyzer, and turning a crank. We have recently made up such apparatus and can analyze a wave in a few minutes, directly from the oscillogram and without any manual tracing.

The present specification does not sufficiently limit the high harmonics. The proposed standard will more than correct this fault, but allows too much latitude for the low-frequency components and takes no account of the phase relation of these low components.

This method of analysis is more applicable to the examination of wave shapes on finished machines, but can be applied in design.

before the machine is built, if the designer wishes to go to the trouble of analyzing his field form and adding this result trigonometrically according to his chording and winding. This latter method has not been found practical, but has some advantages over the method of predicting the wave shape by adding the field forms at a finite number of ordinates.

Comfort A. Adams: In regard to the pulsations of flux which have been mentioned, there are two varieties due to tooth variations of reluctance in the magnetic circuit. One affects the conductors in between the poles and puts kinks into the steep part of the wave. This does not affect the conductors under the center of the pole, or produce kinks in the peak of the wave. The other is a variation of flux distribution rather than a pulsation of flux in the whole magnetic circuit. It is sometimes called the flux swing, and affects the conductors under the center of the pole. It thus puts kinks into the peak of the wave. These two sets of kinks are of the same frequency and may both occur in the same machine, but they are not generally in the same phase, consequently it may appear, in counting the kinks in a wave, that you have an even number of kinks, but in analyzing these you will find that they actually produce odd harmonics in the theoretical sense of the term. The reason for this is that each set of kinks constitutes a tapering even harmonic which when analyzed gives two odd harmonics, one of the next higher order and one of the next lower order.

However, as Mr. Lamme has said, in the vast majority of turbo-alternators, as well as in many other machines, the air-gap is so long in proportion to the slot opening, that these pulsations are not appreciable.

Also, if it is desired to eliminate this effect, it is a simple matter to do so, by having a fractional number of teeth per pole. It has been shown in Mr. Foster's paper that such a change actually eliminates the harmonics in question.

The reason for this is very simple, namely, that in this case the several conductor belts of a given phase are not located in exactly the same position with respect to the poles under which they happen to be at the instant under consideration. It can be easily shown that a fractional number of slots per pole per phase is equivalent to a much larger number of slots per pole per phase as far as the wave shape is concerned, *e.g.*, 1¼ slots per pole per phase is practically equivalent to 5 slots per pole per phase, 4¼ to 17 and so on. It is thus a simple matter to wipe out all tooth kinks, even when the gap is short and the available number of slots small.

Mr. Chubb has pointed out a method of computing the harmonics of the e.m.f. wave from the analysis of the flux distribution curve. Much time may be saved in this method by making use of carefully computed tables which will be found in a paper* which I read before the Institute on the subject of *Electromotive Force Wave-Shape in Alternators*, nearly four years ago.

*A. I. E. E. Trans., XXVII II, p. 1053.

It would seem therefore that there is little excuse for building alternators which do not conform more closely to the sine wave than suggested by Mr. Davis. On the other hand, it should be remembered that the object of a rule of this sort is, simply to set an upper limit for general purposes, and not to write specifications for the consulting engineer. If the case requires a closer conformity to a sine wave, it can be so stated in the specifications. Even some other form of test, such as an oscillogram, may be specified, where more information is desired. The important consideration in connection with the Standardization Rules, is to have a simple definition of wave form deviation which can be applied by a simple test, and which will be as nearly as possible a measure of the undesirability of the deviation.

In the case of multi-circuit windings, it is important that the circuits connected in parallel should be of the same phase and wave shape; but this is not always possible with a single hunting tooth, as Mr. Lamme has pointed out. It is possible in many cases, however, with a fractional number of slots per pole per phase.

It is entirely possible so to design an alternator that its no-load wave shape will approach the sinusoidal as closely as it can be measured by any method; and so that it will substantially maintain that wave shape under all conditions of load, single-phase or polyphase, barring the effect of an excessive current distortion produced externally to the alternator, which absorbs an e.m.f., which, subtracted from a sinusoidal generated e.m.f. wave, leaves at the terminals a slight distortion.

Paul M. Lincoln: I want to call the attention of the Committee to a piece of constructive criticism in Mr. Davis's paper. Here is a proposed method of judging wave shape, which I think is a distinct advance on what we had before. It is a method of measuring wave shapes, which takes cognizance of the higher harmonics and penalizes them in the order of those harmonics. That is a feature in the determination of wave shape which we have not had heretofore, and which is a very valuable thing to have.

Some critics have expressed the fear that this allowable latitude given by the proposed factor 1.15 is too large. I am not prepared to express a definite opinion on the exact size of that factor; it may be that further investigation will make it desirable to make some slight modification in its exact value. In this connection we might cite the table appearing in this paper, in which 22 machines are listed. Of these 22 machines three have a form factor above the suggested limit, 1.15. If this list of machines is typical of the ordinary run of machines that come along, I think the rejection of 15 per cent of them indicates we have a fairly close limit.

Another fear has been expressed, namely, that the latitude allowed in the lower harmonics is so great as to make a variation in form factor which may give rise to considerable variations in the iron losses of transformers. The one which has been stated particularly, is the variation in the third harmonic. So far as the third harmonics go, I think we can forget them, because there are practically no third harmonics. In dealing with three-phase circuits we do not get third harmonics in the circuit from one terminal to another. They cannot appear in those circuits, and since practically everything is three-phase, we are reasonably safe in neglecting the effects of third harmonics. Therefore, the fifth harmonic will be the first one which comes in to affect the form factor, and the fifth harmonic, entering to the maximum extent it can under this rule, will not have a very large effect on the iron losses of transformers.

Charles P. Steinmetz: The determination of the wave shape by an oscillogram, the resolution and separation of the higher harmonics, the study of their origin and their elimination, for giving the designer data by which to design machines of good wave shape, is one thing. But to provide a specification to determine by some simple test whether an actual machine will operate satisfactorily in commercial service, and therefore is acceptable, is quite another thing.

The paper deals with the latter consideration. It does not deal with the study of wave shape for the purpose of obtaining the sort of information required by designing engineers. That we have to consider. With regard to the discrimination against higher harmonics, as far as I can remember at the present time, the only condition under which harmonics of the wave are harmful in an electric circuit, is in the case of transmission lines and underground cable systems, that is, in their relation to the capacity of the circuits, and there the harmonic is approximately proportioned to the frequency. Consequently the proposed new standard provides an appropriate criterion. It is not correct to say that the lower harmonics are harmful regarding hysteresis losses. It is not the harmonics there, but the crest value of the voltage wave, which is of significance. That, however, is not determined by the wave shape specification to within 10 per cent for the sine shape alluded to. That may mean a peaked wave or a flat-topped wave, and between the two the difference in core loss is about 36 per cent. So you see that is another question, which is not dealt with in the wave shape specification.

In regard to latitude, the old rules specify that the deviation shall not exceed 10 per cent from the sine shape. It appears to me that to obtain an equivalent with the new specification, we would have to take at random a large number of machines, see how large a percentage of them fall outside the former specification, and ascertain on the new basis the limitations which would result in approximately the same percentage of machines falling outside of the specification. That would give you the equivalent. Then we may consider whether we are ready now to draw the lines closer, make more rigid specifications, and either say (in the terms of the old rule) that the machines must be within 5 per cent of sine wave, or, (in the terms of the new rule), that the distortion factor must not exceed 1.10. That, however, is entirely aside from the question of the suitable specification for determining the distortion factor.

Charles F. Scott: It seems to me, repeating what others have said, that the means herein given of having a simple practical determination of the variations from sine wave, is an admirable one. However, there may be a case in which machines which may fall within the limitations of the paper may have differences of importance. This test does not indicate whether a single harmonic may cause the limit to be nearly reached, or whether it may be a combination in minor degree of several harmonics. The question then may arise whether the objectionable result of variation from the sine wave may not come from the preeminence of some particular harmonic: For example, one thing in which harmonics may be objectionable is in the induction caused in other circuits. Suppose induction is caused in a telephone circuit. If the harmonic be near the fundamental, say the fifth, it may be too low to cause a disturbance. If it be a high harmonic, several times that frequency, such as the eleventh or thirteenth or higher, then if this permissible limit is nearly reached by the predominance of one high harmonic, a disturbance may result which might not have occurred if the harmonics had distributed throughout the whole range. I bring this point up to illustrate that while the specification is good as far as it goes, there may be cases where it does not cover everything.

L. T. Robinson: Some have urged objections against this method, because it apparently is more complicated than it would be to take the calibrated condenser. I think the proposed method is all right; it makes use of the voltmeter and ammeter, things we are familiar with, and which we know have a certain degree of permanence, as against a condenser which may be permanent to a certain extent, but I do not believe is in the same class as the instruments. Therefore, it is not a complication, but a matter of simplicity.

M. G. Lloyd: I agree with Dr. Steinmetz that it is not a question of whether the departure from the sine wave is harmful or not. In the case of core loss it may be either harmful or beneficial, depending upon the phase of the harmonic. It is a question of how close the wave must be to a true sine wave. If you specify for a transformer a certain core loss, are you satisfied to come within 13.5 per cent or even five per cent of the value you would have with the sine wave?

Taylor Reed: The measurement of wave records is assumed, or authorized by implication, in the Standardization Rules, where a deviation from sine wave of 10 per cent is designated as ordinarily permissible (79, 80, 5e–j). Method and apparatus for determining the deviation, as defined and limited, properly come in for consideration. In measurement of wave records at the present time practise is probably very diverse, and it is desirable that the measures should be adequate and that those who use them should have a proper understanding of their degree of reliability and accuracy.

The condenser charging current referred to prominently in Mr. Davis' paper is a useful aid in detection and analysis which has received too little attention. Its effect in making prominent the harmonics, as well illustrated in the figures of the paper (Figs. 1 and 3), appears particularly well in case only one harmonic is present in appreciable amplitude. It is also useful as a detector of very small deviations or imperfections in exceedingly good waves where the departure from the sine wave is so small as to be within or near the limit of accuracy of direct wave measurement.

Mr. Davis describes in his paper an interesting combination of condenser and reactance coil by which the reactance of the condenser for generator wave and for sine wave are made directly comparable. The proposed rule based upon its use, tests the generator wave without specific knowledge of the form of the wave: it differs radically from the present rules, notably in penalizing the higher harmonics. The paper does not describe means of transfer to the proposed rule from the present, or correlation between them, as, for instance, comparison under the proposed rule of a generator of new design with a previous generator for which only the wave record might be available, if made by computing the harmonics and magnifying them in proportion to their order, would probably not be reliable or closely made.

The proposed rule, and the apparatus it describes, provides a criterion for waves which is good in many respects. It should be placed under varied and extended test under all possible conditions that have to be met, and its adoption should await general acquaintance on the part of those who would have to apply it, and adequate demonstration of practicability.

The caution Mr. Davis expresses about restricting the wave testing combination to a small part of the rated generator load, which might properly be incorporated in the proposed rule, is very pertinent in view of the excessive sensitiveness of some generators to wave distortion even under small fractional loads. In fact, the load waves at various power factors are of such practical importance that the Standardization Rules seem distinctly incomplete in declaring a generator acceptable on a fair no-load wave, however distorted its load waves may be.

Mr. Davis's apprehension of lack of means of obtaining wave forms would have seemed more justified some years ago when systems were smaller and more isolated, andwhen oscillographs were less plenty. In fact, while the amount of material obtained by oscillograph now being presented is considerable, it is impossible for one acquainted with the number of oscillographs in operation and with the character of those who have them in charge not to believe that engineering demands requiring its application are being adequately looked after: and indeed that, further, a large volume of material has been secured, the presentation of which would contribute to the advancement of the art, and to its beauty as well.

Cassius M. Davis: It might be interesting to know the magnitudes of the quantities used in making the twenty-two tests listed in the paper. The condenser had a reactance, on 60 cycles, of approximately 750 ohms. Inserted in series with this was the inductive reactance to give the sine wave, which had about 3000 ohms. The current measured on the ammeter was in the neighborhood of one ampere, and the voltage across the condenser was approximately 700 volts. The question has been raised as to the use of the inductive reactance in series—that we might as well calibrate the condenser in the first place. This reactance is used because the idea of the test is to make it as simple and as rapid as possible, to be purely a commercial test; and if further details are required, an oscillogram should be taken.

Dr. Kennelly suggested the ratio of currents rather than the ratio of reactances of the condenser. This would be possible if the voltage were held constant, but it is frequently difficult to do that, especially if we are using the reactance in series, therefore we would have to read the voltage and current anyhow, which gives the reactance at once.

Mr. Reed brought up the question of correlation between a test which has been made on an old machine under the old rules and a test which may be made on a new machine under the proposed rules. I have calculated a large number of oscillograms, and by taking the ordinates close enough together, or, as has been suggested, by using an analyzer, the oscillogram can be analyzed with sufficient accuracy, to derive the distortion ratio from it; then the wave shapes of the two machines can be compared.

Comfort A. Adams: While such criticisms as we have heard have their place, it should be remembered that the method proposed is a distinct advance over the old one, both in the significance of the definition of distortion and in the simplicity of the method of measuring it; also that the critics have not yet suggested a better one. Unless we find something still better, this should certainly find place in the Standardization Rules.

F. D. Newbury: I wish to recommend the method of calculation of regulation, advocated by Mr. Field, because it is based as nearly as any method can be on test data. The present rule is not adapted to modern machines or modern conditions. Any method that separates the reactance voltage from the armature demagnetization is a step in the right direction. I believe it is still better if separation by calculation is made unnecessary by the test data, as it is in the method recommended by Mr. Field.

S. S. Seyfert: In Mr. Mortensen's paper, the use of the Kapp or the Potier diagram is recommended for the predetermination of alternator regulation. During 1908 and 1909, Mr. F. T. Leilich and the writer made a study of this subject. The effects of armature reaction and armature impedance were specially investigated. After passing from one to another of a large number of methods of predetermination, a comparatively simple one was arrived at, which gave results checking very closely with those obtained from the direct tests.

The chief faults of the old methods are that they either assume that armature demagnetization may be reduced to an equivalent reactance or that the field current on short circuit is a measure of the true armature reactance. The so-called synchronous reactance obtained from the short circuit test is a fictitious quantity. The reading of the voltmeter across the armature after the short circuit is broken is far in excess of the induced voltage while the current is flowing. When normally loaded, the current of the machine encounters no reactance as large as this. The major portion of the field excitation is directly annulled by the armature demagnetization which is at its maximum value in this case. It seems better, therefore, to compute all voltage drops by using values from the static impedance tests, which are easily made.

In using static impedance values it is necessary, if accuracy is required, to consider the saturation of the machine, and to remember that, for power factors near unity, the minimum value is more correct, while, for power factors near zero, the maximum should be used. The following figures show the comparative values of the *synchronous* and true reactances of the machine on which tests were made.

Apparent synchronous reactance
(I_a = 30 amp. I_f = 1.2 amp.) 6.30 ohms
Real or static reactance (maximum) . . . . 2.84 ohms
Real or static reactance (minimum) . . . . 1.92 ohms

In the discussion, Mr. Leilich has outlined the tests required and the essentials of the method. It should be noted that, for any condition of running, the induced armature voltage is found from the terminal voltage by what is virtually the old e.m.f. method, excepting that the *static reactance* is used for getting the armature drop. The armature demagnetization is found and reduced to an equivalent field current by what seems to be a rational method.

The accuracy of the method has been checked for all degrees of saturation of the machine and for power factors other than unity. There was no marked failure of the method for abnormal running conditions.

Comfort A. Adams: It should be noted that regulation tests as made with ordinary instruments and by ordinary observers, are more liable to error than a really careful calculation. I have devoted a great deal of time to this subject, have made very careful measurements, and many calculations by all of these methods, and I have rather come to feel that I would trust my calculations as closely as observations made by ordinary commercial methods and instruments.

Alexander Gray: My experience has been that the method suggested by Mr. Field is by far the best method. I also think we should carry out his suggestions and guarantee regulation on zero power factor, then we have something which we can test. If we desire a simple method by which to check regulation, then let us use the pessimistic method proposed by Mr. Behrend, and forget about the optimistic method altogether. The majority of alternators have a full-load saturation curve on zero power factor very close to the pessimistic curve. I have seen a few machines of which the full-load zero power factor saturation curve was below the pessimistic curve, due to the enormous increase of field leakage which we have under these conditions.

Comfort A. Adams: The curve calculations I referred to took account of increased field leakage.

Frank T. Leilich: Messrs. Mortensen and Field in their papers on alternator regulation have called attention to a part of the standardization rules that has long needed revision. The late Professor Henry Rowland once remarked, when told that practise did not agree with theory, "So much the worse for the practise." This is the case with the present Standardization Rules regarding the predetermination of regulation; the practise is wrong from a theoretical standpoint and as a consequence the practise gives very poor results.

Mr. Mortensen calls attention to the fact that regulation has been ably handled in a number of papers presented before the Institute and methods proposed for its predetermination which give results in good accord with actual test. Professor Arnold in his celebrated works on design has also given the subject excellent treatment.

The methods which give the best results have in general been open to the criticism that they are rather long and laborious. To get a clear understanding of the principles underlying a rational method of calculating regulation it may be well to outline the factors which cause the voltage of an alternator to fall, when a load is put on the machine. These are:

A. Armature resistance.
B. Armature reactance.
C. Armature demagnetizing action.
D. Increased leakage.

The effect of resistance in causing voltage drop is usually quite small, and, as pointed out by Mr. Field, can in most cases be neglected.

Armature reactance, on the contrary, has a pronounced effect on the voltage regulation and must be considered. Messrs. Hobart and Punga in their paper before the Institute (TRANS. Vol. 23—1904, p. 291) give a method of estimating reactance. Professor Arnold also derives formulas for this factor. In fact, reactance may be calculated from the design data of the machine with a very fair degree of accuracy. When the machine is on the test floor, why not determine the reactance by direct measurement? This can readily be done by the application of a voltage of the same frequency as the machine is designed for, and measuring the voltage drop across the armature. As is well known, the reactance will vary with the position of the armature bands with respect to the poles and also with the saturation of the iron in the neighborhood of these bands. However, full-load conditions are the ones usually considered and these may be reproduced with sufficient accuracy for reactance measurements by fully exciting the field and having the current in the armature at full-load value.

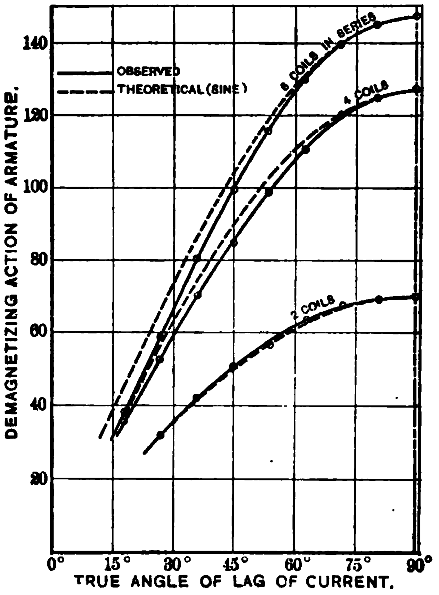


Fig. 8—Armature Demagnetizing Action

Under this condition the machine corresponds to a synchronous motor that has fallen out of step and come to a standstill, the armature current being hunted, however, to normal value. Under the above conditions the armature may be jacked around through 180 electrical degrees and the maximum and minimum voltage drops observed. According to experiments, the average reactance may be figured with sufficient accuracy for practical purposes by using the average of the maximum and minimum drops for calculating the impedance, and, from the known value of armature resistance, figuring the reactance.

The sine formula as given by Mr. Mortensen for calculating the armature demagnetizing action will give remarkably close results.

This bucking action of the armature is the principal factor acting to cause a fall in voltage of a generator and it is essential that it be determined as accurately as possible. The curves in Fig. 8 are the results of some experiments performed by Professor Seyfert and the writer, at Lehigh University, to see how close the demagnetizing action as calculated by the formula agrees with actual results. The machine tested was a salient pole revolving field generator so arranged that the number of the coils per phase could be changed at will. The full-line curves are the test results and the dotted curves are sine curves. The curves show that the width of the band of conductors has a slight influence on the demagnetizing action, but on the whole the agreement with the sine law is very close.

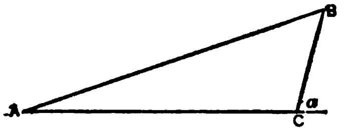


FIG. 9

A B—Ampere-turns for full-load current at O power factor.
C B—Ampere-turns for full-load impedance drop.
A C—Armature demagnetizing ampere-turns at 0 power factor.
a—An angle of which the cosine is equal to the ratio of the armature resistance to impedance.

Mr. Field has given a method of correcting for the increased leakage under load, so that now all quantities affecting regulation may be considered mathematically.

Demagnetizing action as determined from short-circuit conditions is not a true measure of its effect under normal conditions of operation. Referring to Fig. 10, it is clear that a reduction in field strength on the straight part of the saturation curve produces a greater fall in voltage than the same reduction beyond the knee of the curve. However, an assumption that the effects are the same will tend to compensate for the effects of increased leakage, which may then be neglected for most practical calculations.

In conclusion, the following modification of the methods proposed for determining regulation is suggested. With the machine on the test floor, make the following tests:

1. Armature resistance.
2. Armature impedance, from which reactance may easily be calculated.
3. No-load saturation curve.
4. Field ampere-turns for full-load current at zero power factor.

Having the above, the armature demagnetizing ampere-turns for full-load current at zero power factor may be calculated graphically as shown in Fig. 9. Lay off, Fig. 10, the line E_t equal in length to the terminal voltage of the machine at an angle with OX such that the cosine of this angle is equal to the power factor. Draw AB the RI drop, if this is to be considered, parallel to OX. From B draw BC perpendicular to OX and of a length representing the reactance drop. Then OC is the internal voltage of the machine and θ' the total angle of lag.

From the figure it is evident that *OD* represents the ampere-turns that would be required if demagnetizing action were not present. Step off *OE* on *OC*, equal to the demagnetizing ampere-turns of the armature at full-load current, zero power factor, to the same scale as the field ampere-turns. Then *OF* represents the armature demagnetization under the given conditions. From *D* step off *DG* equal to *OF*, then *OG* represents the total ampere-turns re-

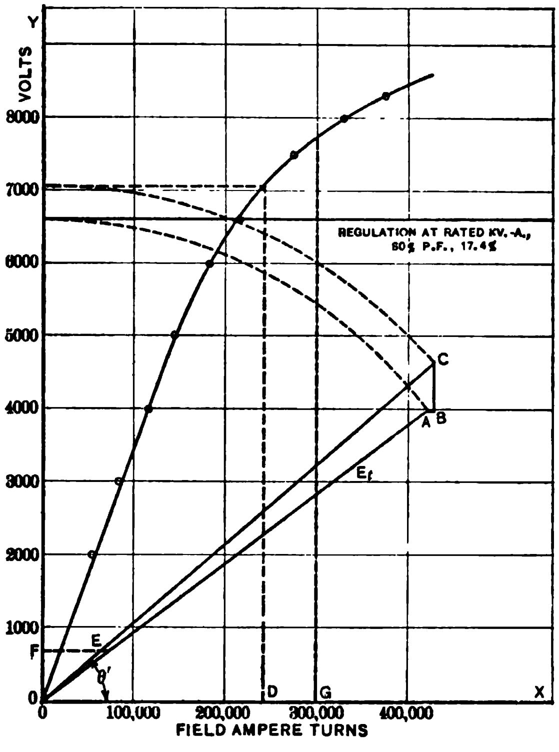


FIG. 10—3600-KV-A. GENERATOR, 6600 VOLTS, THREE-PHASE, 60 CYCLES, 225 REV. PER MIN.

quired for normal load under the given conditions. The intersection with the no-load saturation curve of a vertical line drawn through *G* gives the value to which the voltage will rise if the load is removed.

This method has been found to give results close to actual test. Inasmuch as all of the quantities admit of predetermination, the calculations may be made from design as well as test data.

W. L. Waters: The Potier triangle method of determining regulation, which is described by Mr. Mortensen, and especially the empirical method of applying it outlined by Mr. Field, is undoubtedly the most satisfactory method used by designers to-day. But I think it will be well to refer all measurements to a complete zero power factor load saturation curve determined by direct test, rather than to rely too much upon theoretical or empirical deductions. Mr. Field's triangle method requires one zero power factor load reading, and it will require very little more time and expense, and be much more satisfactory, to complete the zero power factor load curve. With this and Mr. Field's recommendation No. 4, we have a reliable basis for checking any regulation guarantee for any power factor from 100 per cent to 0 per cent, and for any current load not greater than the highest value for which the zero power factor curve was determined.

George Smith: The writer can testify to the accuracy as applied to widely different types of alternators of the method proposed in Mr. Mortensen's paper for the calculation of alternator regulation. He would call attention, however, to a method set forth by Professor C. A. Adams in the *Harvard Engineering Journal* in 1902, which is based on the same fundamental principles and is quite as accurate as the method under discussion. Prof. Adams' method appeals to the writer as eliminating all graphical construction. With the armature reaction known, and with the no-load saturation curve, the short-circuit characteristic and armature resistance taken from test, the regulation for any load at any power factor may be calculated by a few simple formulas.

The armature reactance (x) is determined from the short-circuit test. The field m.m.f. at short circuit is used to overcome the armature reaction and the impedance drop. As the resistance is generally small compared with the reactance, the reactance and the impedance may be assumed to be of the same value. Then the reactance drop, IX, may be read directly from the no-load saturation curve at the field strength.

$$F_x = F_s - A$$

where F_s = field ampere-turns giving the current I at short circuit,

and A = the armature reaction at the current I.

With the reactance, X, thus determined, and the resistance, R, taken from test, the virtual generated voltage, E_v, for any load or power factor, may be calculated from

$$E_v = \sqrt{(E \cos \theta + I R)^2 + (E \sin \theta + I X)^2}$$

where E = normal rated voltage
and $\cos \theta$ = power factor of load.

The field strength, F_v, corresponding to E_v is read from the no-load saturation. Then the total field strength, F, required to give the voltage E at the load I is figured from the formula

$$F = \sqrt{\left(F_v + A\ \frac{E \sin\theta + IX}{E_v}\right)^2 + \left(A\ \frac{E\cos\theta + IR}{E_v}\right)^2}$$

The no-load voltage, E_0, corresponding to the field strength F, is then read from the saturation curve and the regulation determined therefrom.

In the application of the above formulas it is to be remembered that the values of voltage, current, resistance and reactance are per phase of the armature windings.

C. J. Fechheimer (by letter): The methods which the authors propose for the determination of the zero power factor curve, from which the curves at other power factors may be determined with sufficient accuracy for any commercial purposes by means of Kapp's diagram, are in general those which are the results of experience and have been found to agree very closely with experimental results.

We wish only to suggest a method for enabling the purchaser or his representative to determine what the inherent regulation of his machine is, without resorting to the test and calculated no-load saturation curve with full-load leakage advocated by Mr. Field, nor to the indeterminate proportions of the triangle of Mr. Mortensen.

We have found from considerable experimentation that the reactance component of the triangle used by both authors can be determined by direct measurement with the rotor removed, the current being circulated at normal frequency in one phase. The voltage then measured will be very close to the reactance drop in the machine with the same current. In order to allow for the armature reaction we can determine this from the short-circuit curve, after allowing for the reactance drop, and determining how many ampere-turns remain. These ampere-turns, however, should be increased in the case of a machine in which the magnetic circuit contains iron which is to some extent saturated, by increasing these ampere-turns by an amount equal to the leakage factor.

In general this factor is 1.1 to 1.3, and sufficiently accurate results could in general be obtained by assuming it to be 1.2 for the Institute rule. This in general gives results which are but very little different from those obtained by actual test at zero power factor.

A. E. Kennelly: I will call on Mr. Lamme to make a summation of the conclusions arrived at thus far in this conference, in his opinion.

B. G. Lamme: It is difficult to state what conclusions can be reached, as a result of the discussions, because we have not gotten all the evidence together. However, I can make a few definite statements.

When we first took up this question of the revision of the rules, the two sub-committees got together and decided upon certain lines upon which to work, and then later the Sub-committee on Revision took up many of the points which have been discussed in the papers presented at this meeting. One thing, evident all the way through, was that a great many desired rules could not be defined accurately. We could not measure temperature accurately. We could not specify how to make equivalent load tests, and we even could not say how to measure the losses accurately. Apparently, nothing could be done accurately, and yet there has been a general impression that what we have been doing in the past was all right.

So many changes were suggested that it was decided that rather than go ahead and make changes ourselves, we would put the whole matter before the public, present the evidence, and acknowledge that we could not do anything accurately and then find what others thought. It was suggested time and again that we would bring a tempest about our ears, and we have done so to some extent, but we decided that it was better to bring out the facts, and then, if we could not draw any conclusions from an open discussion, we would go ahead as best we could.

From the discussion it appears to be accepted that we cannot obtain accurate results, but in some cases, it seems to me, an exaggerated impression of the inaccuracies has been obtained. Take, for instance, the question of load losses. From the discussion, one might assume that these are very large in many cases. I think one of the reasons why it was thought that the load loss in alternators is excessive is because, in many cases, it was referred to as a percentage of the armature copper losses on short circuit, and this percentage looked rather large. But it must be remembered that the armature copper loss is sometimes relatively small, so that an extra loss of 100 per cent on short circuit may be a very small item in the total losses. It may sound large as a percentage, but the total amount may not be very noticeable.

Take a large turbo-generator, for instance; the copper losses in the armature may be only 20 per cent as great as the iron loss and may be also only 20 per cent as great in the friction and windage, or in effect the copper loss may be only 10 per cent of the other losses, or less than 10 per cent of the total losses. In such cases, it makes but little difference if the armature copper loss is increased 50 per cent, or 100 per cent, since it represents but a small part of the total. Upon analysis, it will be seen that, in general, most of the extra or stray losses are quite small in proportion to the total losses in the machines.

It is the same way with many other things. We must not get an exaggerated idea of the value of the inaccuracies or discrepancies. In most cases, it is possible to get a fair indication of them. That is what we have been trying to do, in bringing them out in this convention. The fact that we cannot make accurate measurements in many cases, is, from one point of view, rather discouraging. On the other hand, we might say that it is very encouraging to have these facts so fully recognized, for if we cannot make accurate determinations, it is better for everybody to know it, which has not been the case heretofore. It is better to know that we cannot make accurate determinations than to think we can and be wrong about it.

We must consider that while the facts brought out show how indefinite is our understanding of some of the laws with which we are working, yet our admission of ignorance does not really make the application of these laws any more inaccurate than heretofore, and does not make our errors any larger than they were, and it was thought by the Standards Committee that, in bringing these facts forward, we would be conferring a benefit on the industry by showing just what the true situation is.

Heretofore, there has been a general air of confidence concerning many of these things. Until comparatively recently, many of the now questionable points were considered as entirely satisfactory. Electrical designs are now better than ever before, but we now recognize many of the real difficulties, and do not hesitate to tell about them.

Leo Schuler: I wish to congratulate the American Institute of Electrical Engineers and especially the Standards Committee, on this very successful convention, and I must confess that I have learned a great deal, not only from the papers and discussions presented, but also from the general manner in which this Convention was organized and its transactions carried on. I was quite surprised to see how many of your engineers took an active interest in the Standardization Rules, and I shall bring this as a brilliant example before the eyes of my German colleagues.

I wish to thank you, gentlemen, for having not only allowed me to attend your convention, but to take active part in the discussion. I feel sure that the report I am going to make to the German Standards Committee will help us a good deal in the successful completion of our work, and I also hope that the remarks I have occasionally made here will somewhat contribute to make the American and the German Standardization Rules, if not equal, at least comparable.

If you will consider that the world's market of electrical machinery is practically controlled by the American and the German firms, you will understand the importance of such an agreement in the Standardization Rules. I thank you, gentlemen, for the very kind reception you have given me.

A paper presented at the San Francisco Section of the American Institute of Electrical Engineers, February 28, 1913, and at the Los Angeles Section, April 22, 1913.

OPERATION OF TRANSMISSION LINES

BY LEE HAGOOD

This paper will deal with controlling voltage and power factor in transmission lines by means of synchronous machines. The first part will relate to moderate voltage systems having small charging currents but large inductive loads, and the second part to systems having high-voltage transmission lines in which the charging current is considerable. The underlying principles are substantially the same in both cases.

SYSTEMS OPERATING AT MODERATE VOLTAGES

Where the transmission line is short and the voltage is 60,000 volts or below, the charging current is so small that it may be neglected so far as voltage or power factor is concerned.

Fig. 1 represents a transmission line and Fig. 2 a vector diagram illustrating the relation between generator voltage and receiver voltage for an inductive load.

It is assumed that the current, voltage and power factor are measured* at the load end of the line. The actual current, I, lags behind the receiver voltage by the angle θ whose cosine is the power factor; it causes a drop through the resistance, R, in phase with it, *i.e.*, IR can be drawn parallel to I; and causes a drop through the reactance 90 deg. out of phase with I, the current lagging with respect to IX, *i.e.*, IX may be drawn at right angles to I in the direction indicated. The vector E_G represents the required voltage at the generator end of the line.

*In complex quantities $E_2 = E_1 + I_0 Z$ since $Y = 0$, (see Dr. Steinmetz's formulas, page 868 of this paper) where $I_0 = i_1 + j i_2$ and $Z = r - jx$. Hence $E_2 - E_1 = i_1 r + i_2 x + j\,(i_2 r - i_1 x)$. The imaginary quantity affects the result so slightly that it may be neglected, especially when the power factor is lagging.

By an inspection of Fig. 2, the following is evident:

$$V = E_G - E_R = AD = AB + BC + CD$$
$$AB = IR\cos\theta = I_e R$$
$$BC = IX\sin\theta = I_w X$$
$$CD = E_G - E_G\cos x = E_G(1 - \cos\alpha)$$

Then $V = I_e R + I_w X$ (approximately)

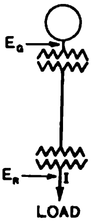


FIG. 1

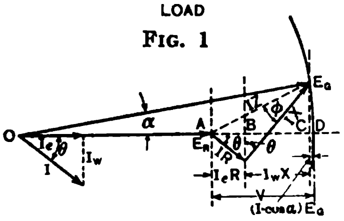


FIG. 2—VECTOR DIAGRAM OF A TRANSMISSION LINE SHOWING RELATION BETWEEN GENERATOR AND RECEIVER VOLTAGE.

E_G = Generator voltage.
E_R = Receiver volts.
V = Voltage drop.
I = Receiver current.
I_e = Energy component of I.
I_w = Wattless component of I.
R = Resistance between E_G and E_R.
X = Reactance between E_G and E_R.
θ = Angle whose cosine is the power factor.
ϕ = Angle whose tangent is R/X.
α = Angle between E_G and E_R.

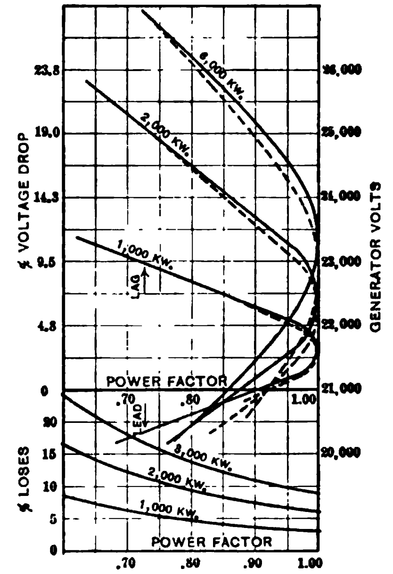


FIG. 3—RELATION OF GENERATOR VOLTAGE TO RECEIVER VOLTAGE FOR DIFFERENT LOADS AND POWER FACTORS FOR A CONSTANT RECEIVING VOLTAGE OF 21,000 VOLTS.

The dotted curves are calculated, using the approximate formula.* For the constants of the line and transformers refer to Table I and Fig. 7.

* The error in using the approximate formula decreases with lagging power factors, with a decrease in ratio of X to R and with a decrease in kilowatts. For a transmission line itself under normal conditions this formula is quite accurate. See Table I, page 867.

The quantity $E_G(1 - \cos\alpha)$ is so small that it may be neglected. In dropping this quantity the maximum error is about two per cent for the usual conditions that arise in practise, which precision is below the requirements of the present problem. This gives us a very simple and convenient formula for voltage drop, viz. $V = I_e R \pm I_w X$; the plus sign should be used if the current is lagging with respect to the receiver volts, and the negative sign if leading.

Fig. 3 illustrates the effect of power factor on voltage drop in transmitting power. It is assumed that the generator voltage is varied in such a manner as to maintain constant voltage at the receiving end of the line. For simplifying the problem equivalent high-tension voltages and resistance are used. These curves represent a condition where the ratio of resistance to reactance is 0.42, this ratio being tangent ϕ. (See Fig. 2.) Had this ratio been greater, the curves would have been steeper. The dotted curve shows the calculations using the approximate formula, while the full line is accurately calculated.

It is thus seen that when the reactance in a transmission circuit is of any magnitude, the effect of the wattless current on

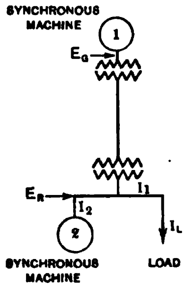


FIG. 4

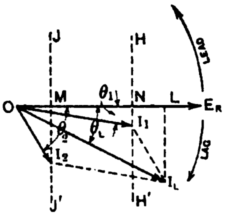


FIG. 5—CURRENT AND POWER FACTOR RELATION OF TWO ALTERNATORS IN PARALLEL.

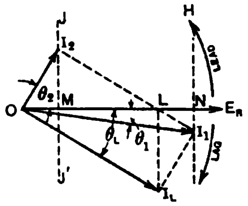


FIG. 6—CURRENT AND POWER FACTOR RELATIONS FOR AN ALTERNATOR IN PARALLEL WITH A SYNCHRONOUS MOTOR

voltage drop is considerable. With a synchronous machine on the receiver end of the line, we can control this wattless current and therefore regulate the voltage difference. For this method of control, considerable reactance in the circuits involved is a desirable quality.

PHASE OR POWER FACTOR CONTROL

Fig. 4 is a one-line diagram of a simple transmission system and Fig. 5 is a vector diagram of the phase relations, assuming both machines No. 1 and No. 2 to be synchronous generators, while Fig. 6 illustrates the phase relations assuming machine No. 1 to be a generator and machine No. 2 to be a motor.

Referring to Fig. 5, OL to scale is the kilowatts supplied the load and is equal to ON plus OM, which are the outputs of generators No. 1 and No. 2 respectively. The division of load between these generators, as with all synchronous generators in parallel, is entirely a question of prime movers; proper division of load may be obtained automatically by means of governors, or non-automatically by hand control of the throttle or water gate, depending upon whether the prime mover in question is a steam engine or waterwheel. In a similar manner the wattless current, or wattless kv-a. of the load, is the algebraic sum of that supplied by the generators, and its division between them is entirely a matter of relative field excitation; suitable division of the load's wattless kv-a. may be obtained automatically by means of voltage regulators, or non-automatically, by hand control of their field rheostats. If the excitation of generator No. 2 is increased, I_2 will increase, but the terminal of this vector necessarily* moves in the locus JJ' because the energy component is unaffected, since division of load can only be changed by adjusting the relative inputs of the prime movers; to satisfy the new position of I_2, I_1 must move to a position where I_L is the vectoral sum of I_1 and I_2; thus by changing the excitation of generator No. 2, we can change at will the power factor or phase of the transmission line.

In Fig. 6, it is assumed that synchronous machine No. 1 is a generator and No. 2 a motor. $O\,N$ represents the generator output and this equals $O\,M$ plus $O\,L$, the kilowatts required by the motor and load respectively. In this case again the wattless current, or wattless kv-a. of the load is equal to the algebraic sum of the wattless kv-a's. supplied by the synchronous machines. By varying the excitation of the motor, the power factor or phase of the transmission line may be varied at will.

Whether machine No. 2 is a motor or generator, raising its excitation raises the voltage of its busbars, and vice versa. When, for example, we raise the excitation of machine No. 2, we raise the flux and therefore the internal generated e.m.f. and this produces a change in the idle wattless current between the machines, the reaction of which in the armature of No. 1 is equivalent to an increase in field excitation on No. 1. The reaction caused by any tendency towards a new voltage condition on a system, therefore, is a mutual one; the machine

*This statement neglects the fact that any changes in voltage will tend to change the load on the system, since the power consumption of lamps, etc., depends upon the square of the applied voltage. Any change in load on a system simply tends to change the frequency, which in turn actuates the governors on the prime movers, dividing the load according to their individual speed characteristics.

whose excitation is raised suffers a change in its power factor so that an armature reaction occurs, tending to demagnetize its fields, while the other synchronous machines react by changing their power factors, tending to magnetize their fields; the system will thus acquire a new voltage, exciting current being furnished by the machine on which we attempted to raise the voltage to the other synchronous machines in the system. This mutual reaction between synchronous machines occurs whenever the voltage balance between them is disturbed. The magnitude of these phenomena depends mainly upon the number and sizes of the synchronous machines on the system, and the resistance and reactance in the circuits involved.

If we should take any single machine and control its excitation automatically with a voltage regulator, holding the voltage constant, the power factor of this machine, as well as those in parallel with it, would vary through certain limits, tending to preserve the voltage at this point in spite of variations in load tending to destroy it.

Automatic Voltage Regulation

Referring to Fig. 7, we could apply a voltage regulator to either machine No. 1 or machine No. 2 or to both. To make the analysis of this problem simple, it will be assumed that voltage regulators are applied to both machines, and that machine No. 2 is a synchronous condenser. A synchronous condenser is a specially designed synchronous motor for operating without energy load from minimum current to full kv-a. As the maximum losses are quite small, in the magnitude of four per cent of the kv-a. rating of the synchronous condenser, their consideration for the present will be neglected.

Since the voltage drop is constant, the wattless current, I_w, which must be maintained in the transmission line at the receiver end for different loads, can be obtained from the following equation:

$$I_w = \frac{V - I_e R}{X}$$

and the energy component of the current (for a three-phase circuit) is

$$I_e = \frac{\text{kilowatts}}{E_R \times 1.73}$$

The actual current at the receiving end of the line is therefore

$$I = \sqrt{I_e^2 + I_w^2}$$

and the actual power factor at the receiving end of the line is

$$\cos \theta = I_e / I$$

All of these quantities have been plotted in Fig. 8 for a given condition of voltage drop and resistance and reactance.

To obtain the synchronous condenser current, which is substantially wattless, we first determine the wattless current of the load, viz:

$$I_{WL} = I_e \tan \theta_L$$

and the synchronous condenser current will be

$$I_S = I_{WL} \mp I_W$$

The minus sign should be used if I_W is lagging and the plus sign if it is leading. Fig. 9 gives two sets of synchronous condenser curves, one assuming the load to have a constant power factor of 0.8 and the other of 0.6.

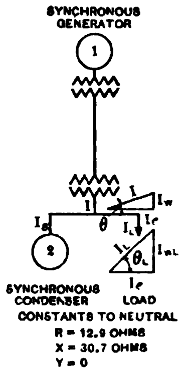


FIG. 7

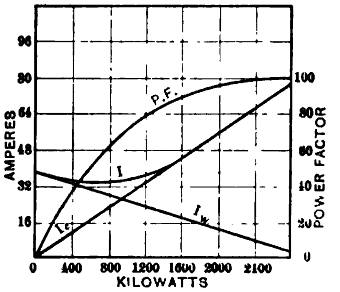


FIG. 8—ILLUSTRATES RELATION OF CURRENT, ITS ENERGY AND WATTLESS COMPONENT, TO THE KILOWATTS FOR CONSTANT VOLTAGE DROP

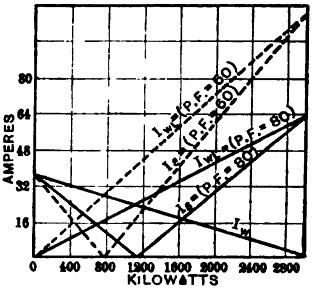
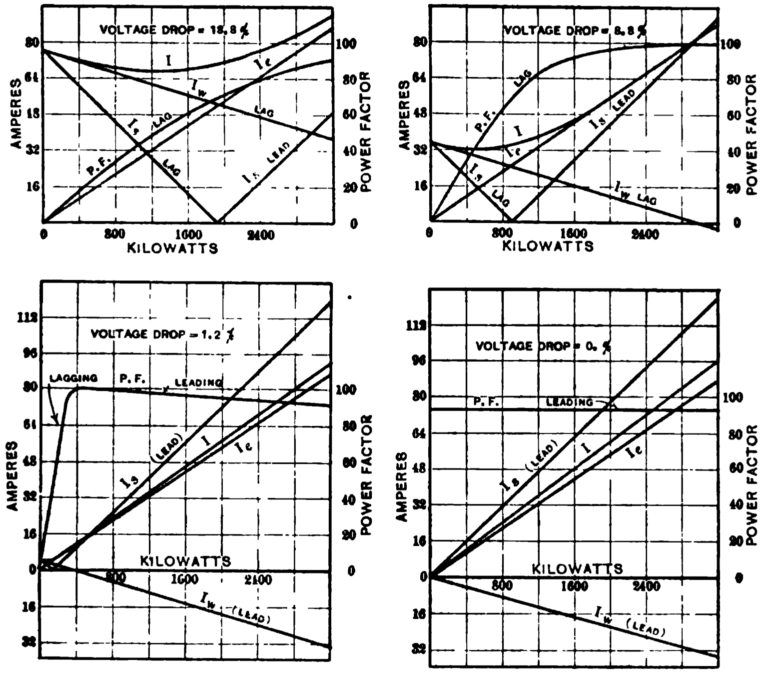


FIG. 9—ILLUSTRATES THE WATTLESS CURRENT FROM A SYNCHRONOUS MACHINE TO MAINTAIN CONSTANT VOLTAGE DROP. TWO CONDITIONS OF LOAD ARE ASSUMED; ONE AT 0.6 AND THE OTHER AT 0.8 POWER FACTOR.

Figs. 10, 11, 12 and 13 illustrate the effect of maintaining different voltage differences between the generating and receiving ends of a certain transmission line. The values of voltage drops are taken at 18.8, 8.8, 1.2 and 0 per cent. It may be noted that the power factor appears to approach some constant value for all loads as the voltage difference becomes small. This condition occurs when the line I_e intersects I_w at the origin, which is the condition of zero voltage difference, if the capacity current is negligible. The value of the power factor under this condition approaches unity as the ratio of R to X increases, this ratio being the tangent of angle ϕ. These data apply to the transmission line illustrated in Fig. 7. It is a three-phase, 22 000-



FIGS. 10, 11, 12 AND 13—SHOW THE RELATIONS FOR VOLTAGE DROPS* OF 18.8, 8.8, 1.2 AND 0 PER CENT, RESPECTIVELY, FOR THE SYSTEM IN FIG. 7.

The equivalent high-tension resistance and reactance to neutral are 12.9 and 30.7 ohms, respectively. The equivalent high-tension receiving voltage is 21,000 volts. The synchronous condenser current is based on a load of 0.7 power factor.

*Calculated by the approximate method, the accuracy of which is within the limits of practical requirements. For accurate calculations the method illustrated in Figs. 18, 19 and 20 may be used.

volt, 60-cycle line, 12.7 miles (20.37 km.) long, consisting of three No. 2 copper conductors spaced 30 in. (76.2 cm.). The resistance and reactance given are phase to phase values, and include the transformers.

Evidently, if the voltage established at the receiving and generating end of a transmission line is such that the voltage drop is small, the voltage at the receiver end being controlled by a synchronous machine with a voltage regulator, there is no necessity for a voltage regulator at the generating station. It becomes practical, therefore, to operate a system* by controlling the voltage at the distribution centers by means of synchronous condensers with voltage regulators, without using voltage regulators on any of the generators.

Controlling the voltage at the receiving ends of transmission lines by governing the wattless current so as to operate the lines with small voltage drops, not only brings about highly satisfactory service on account of the excellent voltage regulation, but makes the operation of the system very flexible. Since the power factor in the transmission line and its transformers and generators is above 0.9 except at light loads, the waterwheels or steam units, as the case may be, can operate at all times at efficient points of their load curves without any limit being encountered due to transformers, generator and transmission line capacity, such as might occur if the power factor were low. Since the copper losses vary inversely as the square of the power factor for a given load, considerable net saving may be accomplished by correcting the power factor, although losses occur in the synchronous machines effecting the correction. Another advantage that comes from operating a system in this manner is that the small voltage drops avoid to some extent the necessity for transformer taps. But most important of all seems to be that considerable reactance may be used in the generators, transformers and transmission lines, and this makes the problem of switching a simple one, since the destructive effects of short circuits may be largely eliminated.

HIGH-VOLTAGE TRANSMISSION SYSTEMS

In the transmission of power over a long high-voltage line, say for voltages of 60,000 volts and higher, the exciting or wattless currents which must be provided are not only those for the induction motors, transformers, etc., but whatever is required by the transmission line itself. A transmission line has both inductance and capacity, both of which require exciting current. The leading current required by the capacity is of very much greater magnitude than the lagging current required by the in-

*For a description of a system so operated see the author's article in the *General Electric Review* for December, 1912, entitled "Operation of Synchronous Machines in Parallel."

ductance, hence the exciting current to charge a transmission line is always leading, that is, with reference to the generators. To charge a line by means of a synchronous motor requires lagging current. On some of the 110,000-volt lines, as much as 10,000 kv-a. is required to charge a single line under normal voltage conditions. Even considerably more than this is necessary for 140,000-volt lines.

It may appear that a happy solution of the voltage and power factor problem arises if the exciting currents caused by an induction motor load just offset the charging current of the transmission line. The difficulty arises in such a case that the number of induction motors in operation is under the control of customers and is therefore a variable quantity, whereas the charging current is fixed by the voltage and constants of the line; furthermore, other loads such as railway, lighting, etc., have their variations: hence, to maintain suitable voltage at the receiving end of a transmission line requires in general that the generating voltage be varied through large limits. In many stations the characteristic load is such that for different periods of a day the necessary generator voltage can be foretold; if the lines are short the operators can often adjust successfully the field currents to meet the demands at the receiver end, or the field current may be controlled automatically. But this method of control may be totally inadequate for long high-voltage lines.

Voltage Regulation Limitations

In the design of a transmission system, the voltage regulation must be within such limits on all parts of the system that satisfactory service is secured and, at the same time, all the transformers obtain proper exciting voltages and the lightning arresters be exposed only to safe dynamic voltages.

Service for lighting loads is very exacting, since a 2 per cent variation in voltage causes a change of approximately 8 per cent in candle power. Service for power loads is not so exacting; nevertheless, it is of considerable importance, because on reduced voltage the starting torque and maximum horse power of induction motors fall off as the square of the voltage. Since the power consumed by any load falls off approximately as the square of the voltage, it is of great importance to power companies that the voltage be maintained as high as is consistent with satisfactory service.

Transformers should have proper exciting voltage, because in general, a departure from normal rated voltage reduces the capacity for a given heating rise. By exciting voltage is meant the voltage applied on the side from whence the power comes. Reducing the voltage by a given per cent reduces the kv-a. capacity substantially by the same per cent, since the ampere capacity depends on the size of conductors. Increasing the voltage above normal decreases the output, because the exciting current is increased and also the core losses. Just how much increased losses occur for a given over-voltage depends upon characteristics in design, but it is safe practise, in general, never to exceed say 5 per cent of the rated voltage.

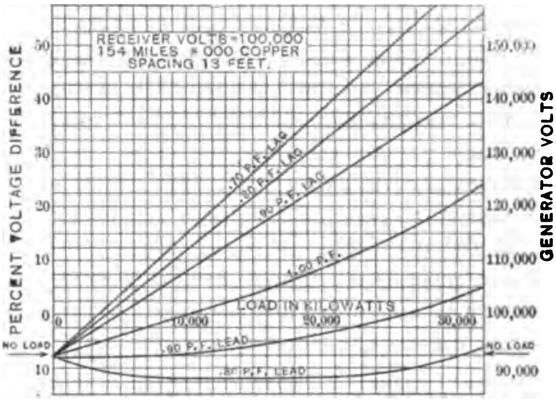


FIG. 14—SHOWS THE GENERATOR VOLTAGES TO MAINTAIN A RECEIVER VOLTAGE OF 100,000 VOLTS FOR DIFFERENT POWER FACTORS AND KILOWATTS. THE TRANSFORMER CONSTANTS ARE INCLUDED.

To prevent lightning arresters from being endangered by over-voltage, they should not be exposed to a voltage regulation exceeding 15 or 20 per cent. Lightning arresters are designed to protect against transient voltages, and their characteristics are such that they offer protection only around the normal voltage rating. Should a lightning arrester be called upon to relieve a transient voltage, when the dynamic or steady voltage of the system was 15 or 20 per cent above that at which the lightning arrester was charged, it would be exposed to serious damage, on account of the large flow of current occurring. Hence, it is not considered safe practise to expose a lightning arrester equipment to a voltage regulation exceeding 15 or 20 per cent.

Effect of Power Factor at Receiver on Voltage Drop

Figs. 14 and 15 illustrate the effect of voltage drop for different loads and power factors, while Fig. 16 shows the system to which the data apply. In Fig. 14 the ordinates represent the kilowatts supplied the receiver and the abscissas the voltages at the generators. Equivalent high-tension values of voltage are used, the per cent difference being between the generator and receiver low-tension busbars, since the transformer reactance has been included in the calculation. For example, if 20,000 kw. at 0.8 power factor were required by the receiver at 100,000 volts, the generator voltage would have to be 132,000 volts, or 32 per cent above the receiver volts, and at no-load, to maintain the same receiver volts, the generator volts would have to be 93,000 volts, or 7 per cent below the receiver volts, causing a total range at the generating station of 39 per cent. This is, of course, an operating condition impracticable to meet. Fig. 15 gives a set of curves where the generator voltage is plotted against power factor. For example, suppose 20,000 kw. were to be received at 100,000 volts, the curve for this load shows the different voltages that would be required at the generating station for different power factors at the receiver.

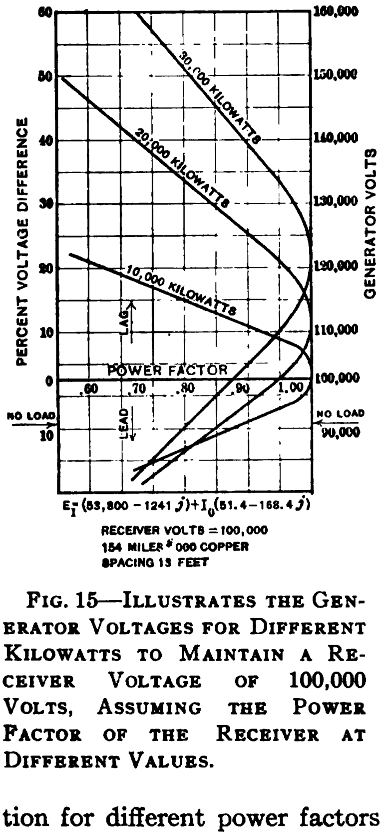


Fig. 15—Illustrates the Generator Voltages for Different Kilowatts to Maintain a Receiver Voltage of 100,000 Volts, Assuming the Power Factor of the Receiver at Different Values.

For a given set of conditions on a long transmission line, the power factor varies at different points along the line, due to the effect of the distributed capacity. The only points which need be considered are the generator and receiver ends of the line, since the power factors here determine the kv-a. capacities of the generators and transformers. As will be seen below, the power factors at these points for a given transmission line depend upon the voltage difference.

Constant Voltage Drop

As with the lines previously considered, having negligible charging current, as soon as the voltage is fixed at two points, fixed conditions of power factor are established from no load to full load, independently of the power factor of the load.

Fig. 16 is a one-line diagram of a system. The transmission line is 154 miles long, the conductors being No. 000 copper, spaced 10 feet in a vertical plane, which is equivalent to 13 ft. (3.96 m.) equilaterally. The constants to neutral are, resistance 53.9 ohms, reactance 171 ohms and shunted admittance 0.0008 mhos. The transformer resistance and reactance are included

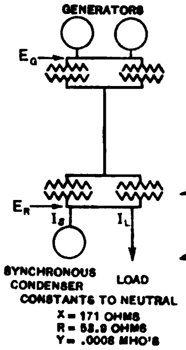


Fig. 16—(The Characteristics of this Line are given in Col. 2, Table I.)

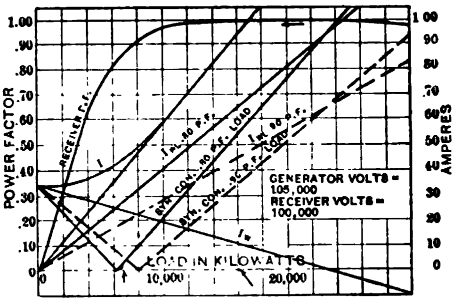


Fig. 17—Shows the Approximate Relations for a Constant Voltage Drop of 5 per cent for the Line in Fig. 16.

It will be assumed that this line is to be operated at five per cent voltage drop, constant voltage being maintained by voltage regulators on the generators and synchronous condenser.

As it will simplify the problem, first the approximate relations will be established and then the same general results obtained accurately.

The following approximate formula applies for voltage drop, as follows:

$$V = I_e R + I_w X - I_c X/2$$

This formula is a modification of that already given to include the charging current I_c. Since the capacity is distributed

TABLE I

Length of line in miles	12.74		153.4		153.4		127		275		275	
Size of conductor	No. 2 Copper		3/0 Copper		3/0 Copper		5/0 Aluminum		605,000 cir. mil.		605,000 cir. mil.	
Equilateral spacing, feet	2.5		13		13		13		14		14	
Line constants to neutral	$Z = 10.5-8.8j$ $Y = 0$		$Z = 50-127j$ $Y = -0.0008j$		$Z = 50-127j$ $Y = -0.0008j$		$Z = 41.4-100j$ $Y = -0.006930j$		$Z = 41.4-171j$ $Y = -0.00132j$		$Z = 41.4-171j$ $Y = -0.00132j$	
Transformer constants to neutral	$Z = 2.5-21.8j$ $Y = 0$		$Z = 3.9-44j$ $Y = 0$		$Z = 0$ $Y = 0$		$Z = 0$ $Y = 0$		$Z = 0$ $Y = 0$		$Z = 0$ $Y = 0$	
Load in kw	2,000		20,000		20,000		20,000		20,000		50,000	
Voltage at receiver	21,000		100,000		100,000		100,000		140,000		140,000	
Power factor at receiver	1.00	0.80	1.00	0.80	1.00	0.80	1.00	0.80	1.00	0.85	1.00	0.85
Generator voltage (accurate)	22,400	24,500	109,100	132,000	108,000	124,500	106,800	120,000	132,000	146,000	140,000	182,000
Generator voltage (approximately)	22,200	24,400	103,900	129,500	105,000	123,800	105,200	119,600	130,000	146,500	139,000	176,500
Per cent error	0.8	0.4	4.8	1.9	2.8	0.6	1.5	0.3	1.5	0.3	0.7	3.0

The above table gives the characteristics of the various transmission lines referred to in the text. As a matter of interest a comparison is shown to illustrate the accuracy of the approximate formula on page 866. The error is quite small except in column 2, where the constants of the transformer were included.

along the line, we can assume that the voltage drop is approximately $I_c X/2$. The actual wattless current in the receiver end of the line is I_w. For most conditions this formula will be accurate within a few per cent. Table I gives an idea as to the limitations of its use.

Fig. 17 shows a set of curves similar to those in Figs. 8 and 9. For simplicity it is assumed that the synchronous condenser losses are negligible. The current required by the synchronous condenser will depend upon I_w, the amount of wattless current which must be maintained in the receiver end of the line for a given voltage difference, and upon the load's wattless current I_{wL}. For the sake of comparison two conditions of load are assumed, one at 0.8 power factor and another at 0.9 power factor. The actual current at the receiver end of the line is, of course, independent of the load's power factor, but depends upon the voltage difference maintained and the kilowatts delivered.

Results obtained in the above manner are accurate enough for a preliminary examination of a problem, but for important calculations greater accuracy is necessary. Dr. Steinmetz has made possible a very simple solution of the problem by the following formulas*, viz:

$$E_1 = E_0 (1 + ZY/2) + I_0 Z (1 + ZY/6).$$
$$I_1 = I_0 (1 + ZY/2) + E_0 Y (1 + ZY/6).$$

These equations involve complex quantities; E_1 and I_1, and E_0 and I_0 represent the voltage and current at the generator and receiver ends respectively, while Z and Y are the impedance and shunted admittance between the points in question.

Thus, by knowing the constants of a line and assuming the voltage, kilowatts and power factor at the receiving end, the voltage, kilowatts and power factor of the generating end can be determined as well as the efficiency of transmission. Fortunately it works out here, as with those lines where the charging current is negligible, that for a given voltage difference, the relation of wattless current, or wattless kv-a. in the receiver end of the line has a straight line relation with the kilowatts.

Fig. 20 is a set of curves similar to those in Fig. 17. The

*For a solution of a problem with these formulas see article by F. W. Peek in the June 1913 number of the *General Electric Review*, entitled "Practical Calculations of Long-Distance Transmission Line Characteristics."

voltage difference and line constants are the same. Since the line "Receiver Wattless kv-a." in Fig. 20 has a straight line relation with the kilowatts, only two points are necessary for its location. It is convenient to locate one point by the amount of wattless lagging kv-a. in the receiving end of the line which will give the specified voltage drop at zero load. Fig. 18 is a curve from which the information may be obtained, it being 8500 wattless lagging kv-a. for a voltage drop of 5 per cent. Another convenient point is where the kilowatts transmitted is at unity power factor, that is where the "Receiver Wattless kv-a." is zero, gives the same voltage drop. Fig. 19 is the curve from which this information may be obtained, it being 13,000 kw. at unity power factor for a 5 per cent voltage drop. Thus the line "Receiver Wattless kv-a." can be established and from it the receiver kv-a. and power factor as well as the synchronous condenser kv-a., for different values of kilowatts. To determine the kilowatts and power factor at the generator end involves the use of the complex equations. It will be found that the voltages calculated will check up with the assumption that for a given voltage difference the wattless kv-a. in the receiver end of a line has a straight line relation with the kilowatts.

Fig. 21 gives the values of voltage at which the generators must operate, if no synchronous condensers are used, and a voltage of 100,000 volts is to be maintained for a load of 0.8 power factor. The efficiency of transmission and the generator power factor is also given. It would not be very satisfactory to operate the generators at voltages higher than about 15 per cent above the receiver, since this would involve a total range in voltage at the generator station of about 23 per cent. A maximum load of about 12,000 kw. at 0.8 power factor could be carried with a 15 per cent drop. As may be seen from Fig. 20, 30,000 kw. could be delivered over the same line with better efficiency and better power factor with a constant voltage drop from no-load to full load of 5 per cent.

Operating a system at small voltage drops in transmission would offer the advantage that all the transformers could be standardized for one voltage and the troublesome question of transformer taps could be avoided. The wattless corrective kv-a. to control the power factor of a transmission line and hence its voltage, need not all be under automatic control, but just so much as is necessary to control any tendency towards voltage changes. In the case cited, all the power was taken off the end of the line, and the wattless corrective kv-a. was applied at that point. As a matter of fact the load could be applied at several points along the line, and synchronous machines used at these points to control the voltage. Some of the synchronous machines could be in the secondary distribution at very remote points from the main transmission line; in fact, the further away the better, since the power factor correction would improve the condition in the circuit in question as well as in the main transmission line. It is thus possible to operate an entire system with

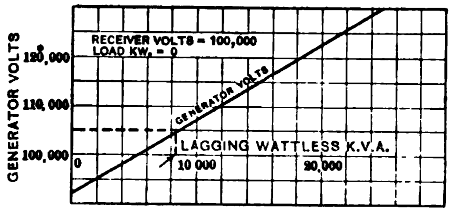


Fig. 18—Generator Volts to Maintain Receiver Voltage of 100,000 for Zero Kilowatts and Different Amounts of Wattless Kv-a. at the Receiver

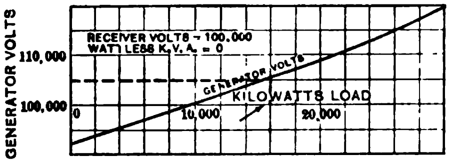


Fig. 19—Generator Volts to Maintain Receiver Voltage of 100,000 for Zero Wattless Kv-a. and Different Amounts of Kilowatts at the Receiver: that is, the Power Factor at the Receiver End is Assumed at 1.00 for all Loads.

voltage drops of little consequence; in fact, the voltage of any system can be controlled entirely by means of synchronous condensers with voltage regulators located at the principal centers of distribution, without using voltage regulators on the generators.

In Fig. 20, the voltage drop was taken at 5 per cent. By making the difference greater, a smaller amount of wattless corrective kv-a. would be required. However, the voltage difference should not exceed a certain amount depending upon the constants of the transmission line, otherwise too much lagging current would be required at light loads.

Generator Voltage Varied with Load to Supply Constant Receiver Voltage

To meet the requirements of high efficiency and favorable use of the available kv-a. of the generator and transformer capacities, the voltage drop can be increased as the load comes on. This may be done in such a manner as to make maximum use

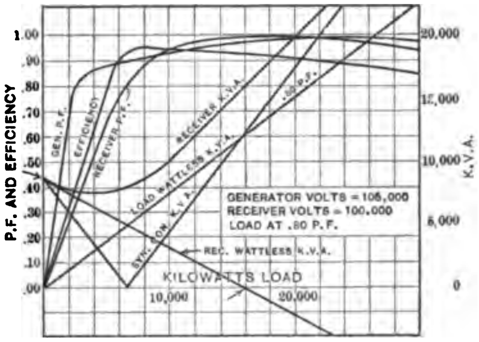


Fig. 20—Shows the Accurate Relations for a Constant Voltage Drop of 5 Per Cent for the Line in Fig. 16

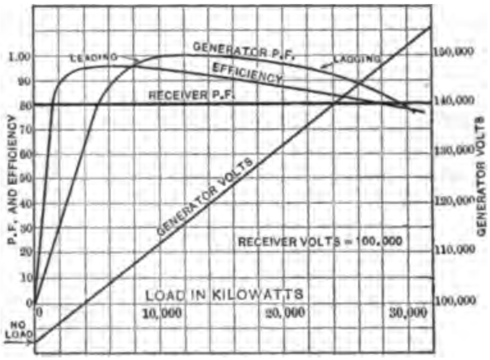


Fig. 21—Shows the Relations if the Voltage Drop is Not Controlled for the Line in Fig. 16

of a given number of synchronous condensers with voltage regulators.

A concrete example of design will be given to illustrate the practical value of this application: Assume a transmission system consisting of two lines 127 miles (204.4 km.) long, each having three steel reinforced aluminum conductors spaced 10 ft.

(3 m.) in a vertical plane, and two synchronous condensers normally rated at 5000 kv-a. located at the receiver end of the line. It will be assumed that 42,000 kw. is the maximum output of the generating station and this is to be applied normally over two

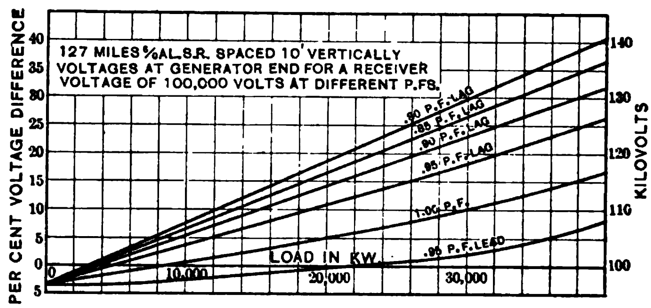


FIG. 22—TRANSMISSION LINE REGULATION. CONSTANTS OF TRANSFORMERS NOT INCLUDED. SIZE OF CONDUCTOR 6/0 ALUMINUM

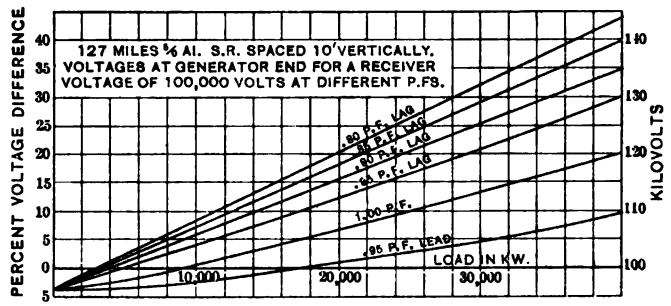


FIG. 23—SAME AS FIG. 22 EXCEPT THE CONDUCTOR IS 5/0 ALUMINUM

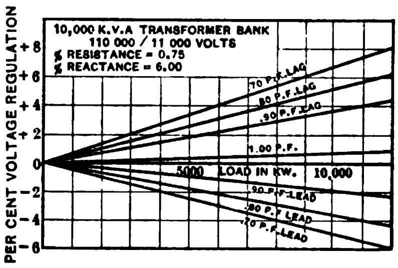


FIG. 24—VOLTAGE REGULATION OF A 10,000-KV-A. TRANSFORMER

lines, but in case of trouble, one line must carry the entire output. Since the nature of the use of the synchronous condensers involves their operation on rather poor load factors, the 25 per cent overload rating will be used in the calculation. The actual energy consumed by each synchronous condenser will be about 200 kw. at its full kv-a. load. It will be assumed also that four 10,000-kv-a. banks of transformers are located at both the generating and receiving ends of the line.

It will simplify the problem, as well as lead to greater accuracy,

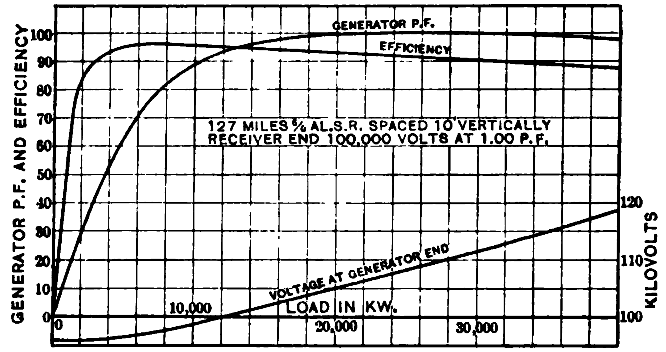


FIG. 25—RELATIONS AT GENERATOR END OF LINE FOR AN ASSUMED RECEIVER VOLTAGE OF 100,000 AND POWER FACTOR OF 1.00. TRANSFORMER CONSTANTS INCLUDED AT GENERATOR END ONLY. SIZE OF CONDUCTOR 6/0 ALUMINUM.

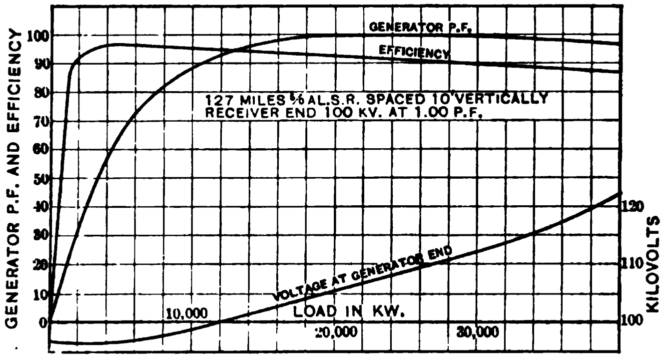


FIG. 26—SAME AS FIG. 25 EXCEPT THE CONDUCTOR IS 5/0 ALUMINUM

if the transmission line is considered separately from the transformers. A slight error would occur if the transformers and line were treated as one, because the capacity of the line is distributed.

What little capacity occurs in the transformer itself may be neglected. Hence the calculations below are given for the line and transformer separately. Since the spacing of conductors is considerable for the higher voltages, the reactance introduced thereby is great. Increasing the size of conductors therefore will help but slightly in the matter of voltage regulation. Figs. 22 and 23 afford a comparison between 6/0 and 5/0 steel-reinforced aluminum conductors. Fig. 24 is a set of curves to illustrate the regulation at different power factors for any one of the transformer banks.

Since the power factor is to be kept near unity at the receiving end by means of the synchronous condensers, not sufficient advantage would be gained by using the larger conductor to justify the extra expense. A comparison of the two sizes of conductors for unity power factor in the receiving end is afforded in Figs. 25 and 26.

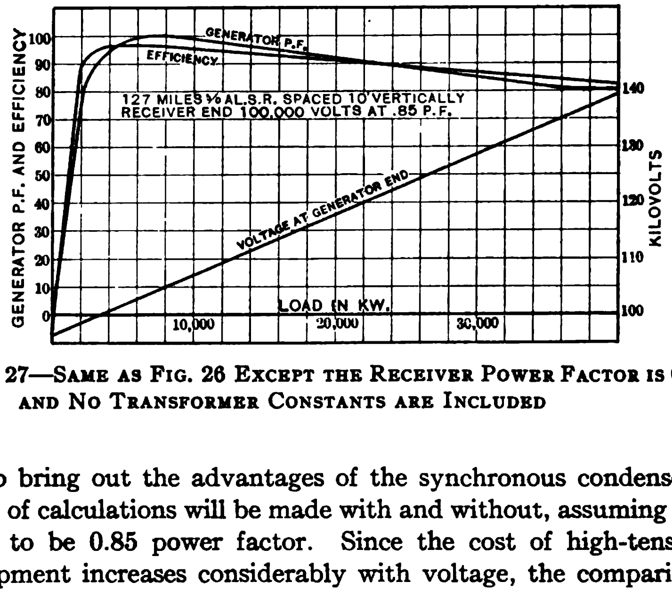


Fig. 27—Same as Fig. 26 Except the Receiver Power Factor is 0.85 and No Transformer Constants are Included

To bring out the advantages of the synchronous condensers, a set of calculations will be made with and without, assuming the load to be 0.85 power factor. Since the cost of high-tension equipment increases considerably with voltage, the comparison will be made on the basis of approximately the same generator voltage. To simplify the calculation, the effect of the transformers will be neglected, as this will not injure the accuracy of the comparison to any marked extent.

Without Synchronous Condensers

Refer to Fig. 27 and to Table II. With both lines in parallel a load of 38,600 kw. at 0.85 power factor and 100,000 volts can be carried at an efficiency of 92 per cent, which would require at the generating end 42,000 kw. at 0.93 power factor (45,000 kv-a.) and a voltage of 117,000 volts; the control of voltage at the generating end of the line would have to be from 96,000 to 117,000 volts, a range of 21 per cent, if a constant voltage of 100,000 volts be maintained at the receiving end of the line.

With only one line, a load of 35,600 kw. at 0.85 power factor and 100,000 volts could be carried at an efficiency of 85 per cent, which would require at the generating end of the line 42,000 kw. at 0.83 power factor (50,600 kv-a.) at a voltage of 135,000 volts. The control of voltage at the generating station would have to

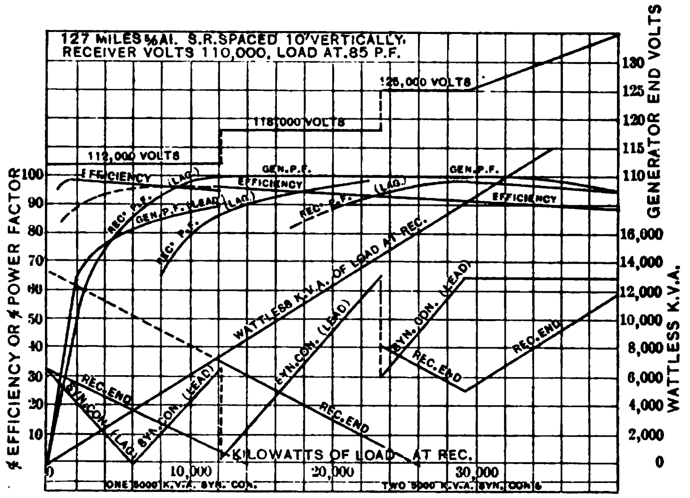


FIG. 28—ILLUSTRATES THE RELATIONS OF TRANSMISSION EFFICIENCY, POWER FACTOR AT EACH END OF A LINE AND SYNCHRONOUS CONDENSER KV-A. FOR VOLTAGE DROPS OF 1. 7, 7.3 AND 13.6 PER CENT, RESPECTIVELY, FOR A LOAD FROM 0 TO 30,000 KW. AT 0.85 POWER FACTOR. NO TRANSFORMER CONSTANTS INCLUDED.

be from 96,000 to 135,000 volts, a range of 41 per cent, if a constant voltage of 100,000 volts at the receiving end is maintained.

WITH SYNCHRONOUS CONDENSERS

Refer to Fig. 28 and Table III. Fig. 28 illustrates the effect of varying the voltage difference in order to increase the voltage range of operation of the synchronous condenser: whenever the voltage drop is changed, the synchronous condenser will automatically take a new point in operation on its phase curve. It is assumed in this case that voltage regulators are used on the machines at both ends of the line.

TABLE II

Receiver end of line			Load	Generator end of line				Transmission line	
Kv-a.	Kw.	Power factor	Power factor	Voltage	Kw.	Kv-a.	Power factor	Efficiency	Volts difference
0	0	0.0	0.0	96,600	170	6,600	0.026 lead	0.0	3.4% rise
7,300	6,200	0.85	0.85	103,000	6,460	6,520	0.99 "	96.0	3.0% drop
14,400	12,200	0.85	0.85	109,500	12,900	13,200	0.978 lag	94.7	9.5% "
29,200	24,000	0.85	0.85	122,000	26,700	29,600	0.90 "	90.0	22.0% "
35,300	30,000	0.85	0.85	129,100	34,500	40,200	85.9 "	87.0	29.1% "
43,700	38,000	0.85	0.85	135,500	44,700	54,600	82.0 "	85.0	35.5% "

This table refers to a transmission line 127 miles long with 5/0 steel-reinforced aluminum, 0.66 in. diameter, spaced 10 ft. vertically, on the assumption that the load on the line is at 100,000 volts and 0.85 power factor.

TABLE III

Receiver end of line			Load	Generator end of line				Transmission line	
Kv-a.	Kw.	Power factor	Power factor	Voltage	Kw.	Kv-a.	Power factor	Efficiency	Volts difference
6,600	200	0.03	0.00	112,000	249	1,960	0.12 lead	80.4%	1.7% drop
7,300	6,200	0.85	0.85	112,000	6350	7,710	0.83 "	98.4%	1.7% "
12,000	12,200	0.999	0.85	112,000	12,920	14,200	0.91 "	94.5%	1.7% "
14,400	12,200	0.85	0.85	118,000	12,700	12,700	0.999 "	95.4%	7.3% "
24,000	24,000	0.997	0.85	118,000	25,850	26,000	0.995 "	93.0%	7.3% "
25,900	24,000	0.927	0.85	125,000	25,800	26,400	0.981 lag	93.1%	13.6% "
30,600	30,000	0.982	0.85	125,000	33,300	33,500	0.994 "	90.2%	13.6% "
39,400	38,000	0.965	0.85	133,000	42,700	44,400	0.962 "	89.0%	21.0% "

This table refers to the same line covered by Table II, illustrates the application at the receiver end of the line of two 5000-kv-a. synchronous condensers controlled by voltage regulators. It is assumed that the load has a constant power factor of 0.85 at 110,000 volts.

With both lines in parallel, a load of 39,400 kw. at 0.85 power factor and 110,000 volts can be carried at an efficiency of 94 per cent, which would require at the generating end 42,000 kw. at unity power factor (42,000 kv-a.) and a voltage of about 118,000. The power factor of the receiver end of the line would be 0.98. The voltage at both ends of the line would be held constant by the voltage regulators, in spite of variations in load. With one line, a load of 37,000 kw. at 0.85 power factor and 110,000 volts can be carried at 89 per cent efficiency, which would require at the generating end of the line 42,000 kw. at 0.98 power factor (42,500 kv-a.) and a voltage of 132,000. The power factor at the receiver end of the line would be 0.96. It is assumed that, in the case of transmitting more than 18,000 kw. over one line, both synchronous condensers would be available.

As shown in the calculations plotted in Fig. 28, for a range of station load from 0 to 42,000 kw. using one line, automatic voltage regulation could be maintained with voltage difference of 1.7 per cent from no-load to 12,000 kw., of 7.3 per cent from 12,000 kw. to 24,000 kw., and of 13.6 per cent from 24,000 kw. to 30,000 kw. Above 13.6 per cent voltage difference, the synchronous condenser could be operated fully loaded and the generator voltage raised as indicated in the voltage curve. Definite values of voltage difference are given. As may be seen from an inspection of Fig. 28, considerable latitude is available in the setting of this voltage difference. Regulating rheostats can be applied in the circuits of the potential transformers for the voltage regulators, either on the generators or on the synchronous condensers, or on both, and by adjusting them the voltage settings of the regulators can be changed without any adjustments in the regulator itself. In actual operation only such settings should be given, whenever practicable, as bring about the voltage difference that causes the best efficiencies and most favorable use of the available kv-a. capacities of the generators and transformers.

Comparing the use of one line to deliver all the power, 42,000 kw., with and without the synchronous condensers, we have the following: voltage regulation at generator end of line 18 per cent against 39 per cent, generator kv-a. 42,500 against 50,600, receiver kv-a. 37,800 against 42,000, and efficiencies 89 per cent against 85 per cent. The voltage regulation without the synchronous condensers is excessive for successful operation; the application of 13,500 kv-a. in synchronous condensers reduces the kv-a. in the generating station and its transformers by 8000 kv-a. and in the receiver transformers by 4200 kv-a., the price of which offsets the investment for the synchronous condensers; and furthermore, the application of the synchronous condenser, due to the improved efficiency, increases the output of the line 1400 kw.; allowing 400 kw. for losses in the synchronous condensers, a net saving of 1000 kw. is obtained.

Flexible Generator and Receiver Voltages

At a distribution center, it is very desirable to have a flexible voltage, which can be increased as the load comes on, because with power feeders, the voltage drop due to load may thus be compensated for within proper limits, and with lighting feeders, the feeder regulators are enabled to operate within limits of accurate regulation.

A flexible generator and receiver voltage may be accomplished by means of a synchronous condenser and series booster, both machines arranged on the same shaft. The excitation of the synchronous condenser should be controlled with a voltage regulator and that of the booster by hand control. At the generating stations, the excitation should also be controlled by a voltage regulator. At both stations, regulating rheostats should be installed in the circuits of the potential transformers of the voltage regulators. By means of these regulating rheostats, the voltage setting of the busbars can be changed at the will of the operator.

Fig. 29 shows a one-line diagram of a system. In station C are shown a synchronous condenser and booster for each line. The line constants are assumed the same as those used for calculating curves in Fig. 28. Referring to Fig. 28, it will be seen that, for a given voltage difference and an assumed power factor for the load, the kv-a. of the synchronous condenser will vary definitely with the kilowatt load, and by changing the voltage difference, the kv-a. of the synchronous condenser may be changed: thus, controlling the voltage difference, gives a control of the kv-a. load on the synchronous condenser. An equivalent effect to this can be established by holding the voltage constant in both stations and compensating for voltage by means of a synchronous booster.

The function of the synchronous booster, therefore, is to effect voltage compensation, the amount of buck or boost being controlled by the field excitation. When used in connection with a synchronous condenser, controlled by a voltage regulator, a means of controlling the wattless kv-a. is effected. Without the booster, for a given voltage setting at the generating and receiving stations, the wattless kv-a. supplied by the synchronous condenser will depend upon the kilowatts load for a given assumed power factor. With the booster, whenever the synchronous condenser comes up to its overload limit, the field exci-

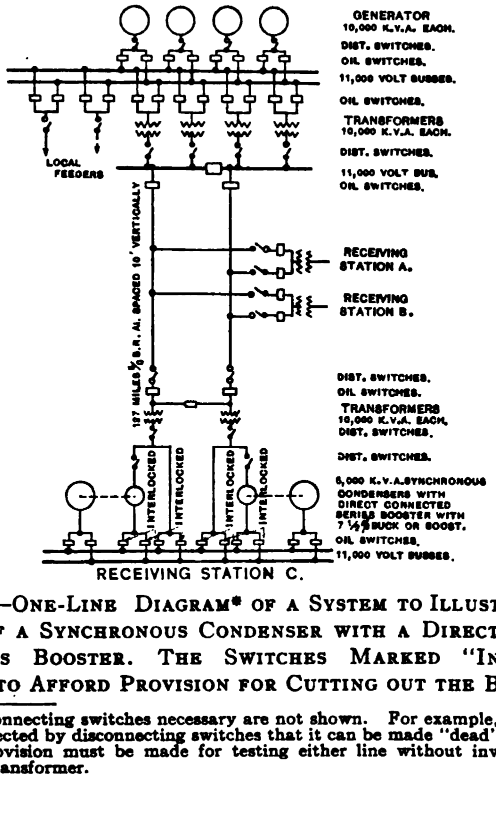


FIG. 29—ONE-LINE DIAGRAM* OF A SYSTEM TO ILLUSTRATE AN APPLICATION OF A SYNCHRONOUS CONDENSER WITH A DIRECT-CONNECTED SYNCHRONOUS BOOSTER. THE SWITCHES MARKED "INTERLOCKED" ARE SIMPLY TO AFFORD PROVISION FOR CUTTING OUT THE BOOSTER.

* All the disconnecting switches necessary are not shown. For example, each oil switch must be so protected by disconnecting switches that it can be made "dead" to allow repair work. Also provision must be made for testing either line without involving but one generator and transformer.

tation of the booster can be changed to reduce its kv-a. load, without affecting the voltage at either the generating or receiving station, and conversely, when the voltage setting at either station is changed, tending to bring the synchronous condenser kv-a. to an undesirable value, the booster's excitation can be changed, effecting the proper voltage compensation to bring the kv-a. to a suitable value; thus a flexible voltage can be accomplished at both ends of the line, while fulfilling the important requirement that suitable power factor correction may be effected.

In Fig. 29, receiving station C illustrates a wiring arrangement for a synchronous booster installation. Not only does the 11,000-volt distribution bus in station C have a flexible voltage, but so does that at the generating station, and in addition to this, the power factor of the transmission lines is under the control of the operator in station C. Furthermore, this operator is in a position to control the kv-a. of the synchronous condensers so that the power factor in each transmission line is maintained at such values as will secure suitable voltages at stations A and B, assuming that they are supplied from different lines.

Conclusion

1. The necessity of reactance in the circuits of modern systems is well recognized. Its importance has arisen with the growth of large systems, and is essential to prevent destruction by short circuits; to reduce within reasonable limits the size of oil switches, by diminishing the rupturing capacity which they must meet; etc. Transmission lines themselves, due to the spacing of conductors, furnish considerable reactance, but in addition to this, it is customary among operating companies to specify that the generators and transformers be built with considerable inherent reactance. To meet the requirements of some systems, even external reactances are supplied. Now, on the other hand, the development of industrial loads requires the extensive application of induction motors, whose exciting currents cause low power factor. Should these exciting currents be supplied over circuits involving considerable reactance, excessive voltage drops occur. Fortunately, synchronous machines can be located at the distribution centers to supply locally the necessary exciting currents, and by this means maintain uniform voltage regulation. It is not always economically practicable to locate synchronous generators or synchronous motors at the desired points. Hence, the synchronous condenser has been developed. This machine has come to fill an important place in the operation of transmission systems.

2. The combination of synchronous condenser and series booster offers an ideal method of voltage and power factor control. Not only can a flexiblè voltage be maintained at both ends of a transmission line, but an independent means of power factor control is secured for the transmission line itself, by which

a definite voltage control is established for some intermediate point in the transmission line. This certainly offers a very considerable advantage, in that it is frequently necessary to have a long transmission line with one or two intermediate stations.

3. A desirable feature of synchronous condensers on a high-voltage transmission system is that they offer protection against those voltage surges that arise due to a sudden loss of load, which might throw the generating stations on the unloaded transmission line with their generators on heavy field excitation. Under such a circumstance, due to the effect on the generators and transmission lines of the leading current set up by the charging current, a destructive voltage would occur. Over-voltage devices may be applied to the generators to give protection; however, with synchronous condensers on the receiving ends of the transmission lines, and each one equipped with an over-voltage device, an ideal solution of the problem is effected, since some of the synchronous condensers could be on the line at all times with their excitation under control through the desired range.

Discussion on "Operation of Transmission Lines" (Hagood), San Francisco, Cal., February 28, 1913.

Herbert W. Crozier: We are certainly much indebted to Mr. Hagood for the interesting paper we have had tonight, particularly the slides, which show great care in preparation.

I wish to ask him a question. With regard to the use of synchronous condensers, what do you find is the most satisfactory method? Suppose you have a large amount of power to deliver to consumers which would involve the use of synchronous motors driving direct-current generators,—would you prefer to make those synchronous motors just large enough to carry the load and have an additional machine synchronous to the condenser, or would you prefer to make the synchronous condensers large enough to take care of this regulation? That is the first question, and then the other is, what would you do if you had synchronous converters? That would be a different phase of the same subject.

Lee Hagood: With the synchronous machine, whether it is a generator or a motor, it can effect power factor correction and at the same time do work. I did not dwell very much on that point, because so many think that is the only way, economically, to use synchronous machinery for power factor correction. The ideal way of course is to let the synchronous machines do 70 per cent in kilowatts and about 70 per cent in wattless kilovolt-amperes, that is, rate these machines on a 70 per cent power factor basis. This applies to synchronous generators, frequency-changer sets, and synchronous motors. It is not always practical to locate these machines at the points where power factor correction is desired, hence the necessity for synchronous condensers.

The characteristics of a synchronous converter are such that its design does not lend itself to operation other than at 1.00 power factor at full load. It would not be safe, in general, to give operators instructions relative to using synchronous converters for power factor correction on light kilowatt loads, as a departure might lead to damage to the machine.

Herbert W. Crozier: Then I gather from your statement that the synchronous converter is fixed by the direct-current voltage, so any small amount of voltage control available would be fixed by the direct-current voltage, which presumably you would desire to keep constant.

Lee Hagood: That is one of the difficulties.

Herbert W. Crozier: But in regard to motor-generators driven by synchronous motors, if we take, for instance, a unit which would have a 1000-kw. direct-current machine, then the idea would be to purchase the motors considerably larger?

Lee Hagood: Yes.

Herbert W. Crozier: So as to take 70 per cent. You make the motor part 30 per cent greater?

Lee Hagood: Yes. It is not a good thing, in all cases, to put a voltage regulator on the motor, unless it is relatively large for its purpose. Even then the voltage regulator must be restricted in its operation so that no danger is incurred of the machine falling out of step by attempting to take a heavy kilowatt load on a weak field.

Robert Sibley: I notice that it has been announced that the Chicago, Milwaukee and St. Paul Railway will electrify some 500 miles (805 km.) of its line through Montana, and the proposal is to purchase power from the Great Falls Power Company at half a cent per kilowatt, the company agreeing to wholesale the power from the main transmission line. However, the power company also reserves the right to put in regulating devices at the receiving end. I would like to ask Mr. Hagood how such devices would differ in railroad work from such installations as he has shown us this evening.

Lee Hagood: As long as they are motor-generator sets it would be quite feasible to accomplish automatic voltage regulation provided the conditions were such that the machines would not have to run on weak fields, but if they are converters, I do not see how it would be practical.

Sometimes motor-generator sets are operated with fixed field excitation, or it is compounded so that as the load comes on the excitation is increased.

Herbert W. Crozier: One example of this phase control, that has worked out very successfully on the coast, is the Sierra and San Francisco Power Company, delivering power to the United Railroads. On their transmission line, 135 miles (217 km.) long, the same voltage is obtained at both ends of the line. The motor-generators are all 1500 kilowatts, I believe, and the regulation is obtained by controlling their fields. I believe they have now installed voltage regulators at a number of the stations and are getting very satisfactory voltage. This condition of course is one that does not always occur in regular commercial practise, because the power company has full control of a very large amount of synchronous apparatus at the receiving end. The ordinary condition is where there are many consumers taking current and using it any way they please, and it is up to the power company to make such regulation as it is able to. In such cases the synchronous condensers discussed tonight are apparently a very excellent feature. One thing that impressed me greatly was the fact that by the introduction of a synchronous condenser, results are obtained amounting to 25 to 30 per cent additional power delivered over the transmission lines, which certainly would appeal to our commercial friends as a considerable saving in capital expenditures necessary for transmission lines or for additional machinery. Heretofore we have been forced to buy generators to take care of the wattless current—and then more generators; and the use of the synchronous condenser in the substation is certainly a very valuable feature.

One thing that impressed me more than any other was the curve shown, if I remember the figures correctly, where 25 per cent more power could be delivered over the same transmission line with practically the same regulation.

A Member: Under what conditions, if any, is the use of synchronous condensers warranted by the decreased power loss due to the necessity for transmitting less wattless current through the line, apart from the question of voltage regulation? Another question. Is it possible to build synchronous condensers simply for regulating purposes so that for a given kv-a. capacity they are cheaper than a synchronous motor of the same capacity would be?

Lee Hagood: In regard to the first question, in the case of the 22,000-volt line (see Figs. 3 and 7 of the paper), at unity power factor the line losses are 7.5 per cent for 2500 kw. That includes the transformer loss—I mean the copper loss in the transformers. Since the losses vary as the square of the power factor we would get 15 per cent at 0.70 power factor. Now the synchronous condenser, to bring the power factor to 1.00, requires about 4 per cent on losses. That would give us a net under full-load conditions around 3.5 per cent. But not only that. We brought the power factor at the generators from 70 up to unity, and that meant that we reduced their core losses, armature and field copper losses. An approximate estimate of the saving is about 2½ per cent, giving a total net of 6 per cent. In the case of the 110,000-volt lines (see Figs. 22 and 23), the losses without the synchronous condenser—the efficiency of the line for delivering 30,000 kw., is 85 per cent, whereas with the condenser on the line the efficiency is 90 per cent. In this case the synchronous condensers have a loss of 1.3 per cent, giving a net saving of 3.7 per cent for the transmission line.

In regard to the design of the synchronous condensers, they can be designed with small air gaps, and with light bearings and bearing supports, since no mechanical load is carried; in view of these features they can be made cheaper than for the same rating as a generator or motor. Some day, perhaps, they may be built for much higher speeds and this will reduce their cost considerably.

Clarence L. Cory: I have been exceedingly interested in this paper, especially because not more than a year or so ago, one of the points, only touched upon in the paper, was gone into rather completely; and more than twelve years ago another point, which has been touched upon in the paper, was gone into carefully.

I will mention first the point that was considered only a short time ago.

We have heard this evening a very complete discussion of what the synchronous condenser or the synchronous motor, either very lightly loaded or doing perhaps 70 per cent of its full-load work, may accomplish in the improvement of transmission conditions. A little over a year ago an investigation —a rather complete one—was made of the design of a 127-mi. (204-km.) steel tower transmission line having two three-phase circuits, maximum loss not to exceed about 6 per cent, as the power was purchased by the power user at the generating station and the tower line built by the power user, payment for the power to be made at the generating station. The amount of power required was to vary between 6000 and 10,000 kw., to be used primarily for induction motors, these induction motors to drive machinery for the crushing of about 20,000 tons of ore a day. Due to the consideration that other transmission lines were possibly to be interconnected, the voltage transmission was limited to about 80,000 volts. However, there were no taps for power along the line, nor was there more than the one generating station to be considered at that time.

The preliminary investigation indicated that 4/0 copper—the equivalent of that in stranded copper or aluminum, or aluminum of any sort—3/0 or 2/0, might be used. As has been shown this evening so very clearly, the problem in this case was not necessarily the very best possible regulation at the expense of other things; not necessarily the greatest efficiency in transmission at the expense of regulation; not necessarily the cheapest possible transmission system as regards cost of conductors at the expense either of efficiency or regulation or both. The power factor of the load was known to be somewhere around 80 per cent, due to the very large number of small induction motors. The frequency was fixed at 60 cycles, inasmuch as the generating station to be used, would be, in the future, one of a number.

Not by any means in the complete manner that has been developed in this paper, but by practically the same general principles, the general conclusion was reached that the installation of synchronous condensers at the receiving end would not only reduce the copper from 4/0 to 3/0, but it would improve the voltage regulation and at the same time increase the efficiency of transmission. A very careful analysis, using the complex quantity formula for the investigation of the power factor along the line from the generating station to the receiving station 127 miles distant, indicated that with the proper operation of these synchronous condensers doing anywhere from 50 to 70 per cent of their kilovolt-ampere capacity, it would be possible to get the best operating conditions.

Electrical energy in the form of direct current may, of course, be transmitted without introducing any of the undesirable effects due to the inductance or electrostatic capacity of the line. We start out with a given number of amperes at our generating station, using direct current, or theoretically with alternating current, with zero frequency, and we find absolutely the same current at the receiving end, neglecting leakage; and the ideal of the transmission engineer with alternating-current power would be to produce a condition somewhat approximating that possible with direct current. We know that is not practically possible, at varying loads, because in the matter of transmission the direct current theoretically has an advantage over the alternating current. However, I can now see, having had this evening an opportunity to look at these curves, that there were instances where such problems might have been very directly solved by the proper use of these curves rather than by going through the rather complex computations that are otherwise necessary.

The other instance that I had in mind, occurring more than a decade ago, was a case where the installation of a comparatively small synchronous motor in a mine in California was made use of to improve the power factor of the load. One of the early transmission companies, fearing induction motors would lower the power factor, offered all consumers who would use their alternating current at approximately unity power factor, the advantage of receiving 920 watts for a horse power, whether real or apparent. A well-known engineer in San Francisco said, "Very good. If we can get 920 apparent watts for what we ordinarily pay for 746, we will see to it that we get 920 real watts,"—which was accomplished by the installation of a synchronous motor. This synchronous motor, at such times as the power was off—and in those days the transmission lines gave more trouble than they do today—was belted to a steam engine, and when the power was off the synchronous motor was operated as a generator and the mine was kept free from water. It was necessary to prevent the mine being flooded even if the transmitted power should be off only for a few hours. The result was that for a number of years the synchronous motor was ordinarily operated without doing any work whatever, and the power factor of the load was maintained at 100 per cent. The result was that the buyer of the power did receive 920 real watts, for which he only paid the price of 746 watts. I do not remember how many times that condenser or unloaded synchronous motor paid for itself every year, but I remember that it was used for a number of years.

I am only mentioning this because in this case the particular advantages of the operation of synchronous motors though lightly loaded or practically unloaded, were availed of to improve the conditions of transmission—improving the conditions in regard to both efficiency and regulation of voltage; and while I have not perhaps had an opportunity of fully studying the paper that has been presented this evening, I can see that in the first case mentioned the solution of that problem might have been made graphically from the curves presented by Mr. Hagood instead of being worked out in that one isolated case.

I would like to add this other point. Due to the careful investigations of Dr. Steinmetz's formulas for distributed capacity, also introducing the factor which it is possible to introduce in Dr. Steinmetz's formulas, namely, the amount of energy loss due to leakage between conductors, and going one step further than Dr. Steinmetz has gone, representing the results of some of his formulas graphically, it is possible to determine the power factor at any place you wish in your transmission line from your generating station to the point of delivery. If your line is 300 miles long it is only necessary to go through an elementary graphical construction as you would in determining stresses and strains in a structure, to ascertain what the power factor is at any point, simply substituting in the formula, whether the point under discussion is one mile from the generating station or 299 miles. So there we see the opportunities and the possibilities of further ascertaining the facts, and the desirability of introducing these synchronous condensers.

There is another point brought out that appeals to me especially. Not very many years ago it was generally understood that if we attempted to transmit power great distances where the charging current would be abnormal there was only one thing to do, namely, to reduce the frequency, if the voltage were maintained of sufficient magnitude to reduce the line losses. But one can see by viewing the curves that have been shown by Mr. Hagood that this is not necessarily true. Possibly we will be able to solve a great many of our problems at 60 cycles that we thought a few years ago could only be solved at one half that frequency or even one-third or one-sixth.

Not only are these few isolated instances that I have mentioned covered by the work that has been done by Mr. Hagood, but his work covers all other problems of a similar character.

L. P. Jorgensen: In substations where no motor power is required, the synchronous condenser is undoubtedly the most practical device that can be used successfully for voltage control; but in stations where the principal use for the power is to drive motors these motors should be so selected that part of them are synchronous and part induction. It is seldom possible, nor desirable, to have all motors on a transmission system of the synchronous type, as the starting requirements may be such as to exclude synchronous motors, and it may not be practical to provide direct current for excitation. Where large motors are to be dealt with, the extra outlay and complication for excitation does not amount to so much, and it is therefore of advantage to equip them with devices for voltage control to compensate for the lagging current of the smaller induction motors and other apparatus. The voltage in use at present on some of the largest transmission lines has reached the limit. This limit is set by the amount of corona loss permissible, so it is not possible to improve per cent of voltage drop by increasing the voltage, as this cannot be done. The critical voltage depends upon size of conductors and elevation above sea level, and the limit of voltage has already been reached in several cases. The limit at sea level is somewhat above 120,000 volts where the amount of power is large enough to require conductors larger than 4/0. A few months ago I had occasion to visit a place and do some work in connection with a power transmission where 50,000 volts was the upper limit of transmission voltage, principally because of the high altitude, the average of which was 14,000 ft. (4270 m.). The lowest point on the line is the power house, located at 12,200 ft. (3720 m.) above sea level, and the highest substation is located at nearly 16,000 ft. (4880 m.) elevation. The line runs for the greater part of the distance of 120 mi. (193 km.) over a comparatively flat plateau of 14,000 ft. (4270 m.) elevation. This place is called the roof of the world, and is up in the Andes mountains in Peru. The load on this system is mostly motors driving copper mining machinery to the extent of about 10,000 h.p., requiring a conductor of No. 1 size cable. The motors are divided up between synchronous and induction in such a way that the power factor will be under complete control by means of automatic voltage regulators adjusting the excitation of the synchronous motors.

Herbert W. Crozier: I want to ask a question of Mr. Jorgensen. Did I understand you to say that 50,000 volts was the limit?

L. P. Jorgensen: Yes, because only 10,000 h.p. was to be transmitted. You could not afford to take larger than No. 0 cable, and at that diameter you cannot get above 50,000 volts.

J. P. Jollyman: One of the chief functions of the synchronous condenser in the ordinary transmission system is, as Mr. Hagood has pointed out, to help hold up the voltage. As the load on our system grows, frequently we run into conditions where excessive voltage drop limits the amount of the load which may be successfully carried at some point.

There is also another factor in the holding up of voltage which Mr. Hagood only touched on very briefly, but which to those of us who are connected with 60-cycle systems is a very important matter, and that is the charging current of the line, and its effect in balancing or neutralizing the lagging current due to the ordinary commercial load. The effect of this charging current is very considerable. We have at the present time in this vicinity a number of high-tension transmission voltages—60,000 and 100,000. At 60,000 volts the charging current amounts to about 18 kv-a. per mile. In other words, a company such as the one with which I am associated, operating some 1200 mi. (1930 km.) of 60,000-volt line, has a charging current of over 21,000 kv-a., and this is sufficient to raise 29,000 kw. of 0.80 power factor load to unity. That of itself has a very important effect in holding up the voltage of such a system. Now a 100,000-volt system has a very much higher charging current. As the voltage and the current go up your kilovolt-amperes go up. The charging current for a 100,000-volt line is about 52 kv-a. per mile. In other words, a 300-mi. (483-km.) line would have some 15,000 or over 15,000 kv-a. wattless leading current component, which would assist in balancing the bad power factor of the ordinary commercial load. This wattless component on 300 miles of 100,000-volt line would balance 21,000 kw. at 0.80 power factor load, bringing it up to unity. I believe that the charging current which, as you see, is no small amount on systems which are operating, has a very important effect in helping to hold up the excitation. The excitation on a transmission system is somewhat analogous to the mechanical condition of the cantilever beam. If the excitation is supplied entirely at the generating station it is somewhat analogous to a cantilever beam supported at one end and loaded at the other. Such a beam must necessarily sag at the loaded end. The voltage cannot be anything but lower at the receiving end where you have purely load and are supplying no excitation in the way of synchronous machinery. The variations in voltage with the variations in load are somewhat analogous to the variations in the sag of such a beam with a varying load. To steady that beam you may supply some support at the free end. In other words, you may apply some synchronous machinery at the receiving end. It is obvious that you may *steady the variations* in the deflections of that beam with very much less support than would be required if you wanted to raise the beam up to the same level that it was held at the fixed end; and that is, as near as I can suggest, a mechanical analogy to the transmission problem in the operation of synchronous condensers. It is obvious that a beam supported at both ends will have less sag if it is loaded uniformly than if it is supported at one end as is the case in the transmission system. If you have synchronous apparatus at both ends the voltage drop will be less throughout the system. I think it would be well for any one having to do with the design of transmission systems to consider always the effect of their charging current, and to bear in mind that it is a very considerable amount, very closely approximating 18 kv-a. per mile for 60,000 volts at 60 cycles; and 52 kv-a. per mile for 100,000 volts and 60 cycles. There have been some transmission systems projected recently where the charging current is going to prove a very serious problem to handle properly. My own opinion in the matter is that you should not ordinarily attempt to operate a single transmission circuit with generators which are less in capacity than the charging current. In the system on which we are working at the present time the kv-a. capacity of a generator is approximately double the charging current on the line, and I anticipate that we will not be very much bothered by variations in load or bringing the empty line up to voltage.

Herbert W. Crozier: Mr. Jollyman apparently has given this matter of the charging current a very considerable amount of thought. The point I want to draw attention to in connection with the use of synchronous condensers for the regulation of the line is the matter of safety. We all know that the loss of load due to excitation and opening of circuit breakers and other things causes a very considerable rise of voltage at the substations; and the use of the synchronous condenser controlled by the automatic voltage regulator is certainly an element of safety, because there is something there which will make a change in the conditions in the end, assuming of course that the synchronous condenser is so installed as to remain connected to the transmission line. I remember two or three cases where very disastrous results occurred, due to the opening of the end of a transmission line, causing the wrecking of lightning arresters and general destruction of such apparatus as still remained connected.

L. N. Peart: We have a 60,000-volt line approximately 200 mi. (322 km.) long, and I can confirm some of Mr. Jollyman's remarks very strongly, because I have just made some experiments on the power factors of this line, and also some corrections obtained by using some turbines at one end of the line as synchronous condensers. We have about 200 miles of 60,000-volt line, fed at one end by a hydroelectric station, and at the other end by a steam auxiliary. The hydroelectric end is 16,000 kw. capacity; the steam end is about 8000 kw.; and close to the steam end at the outer end of the line we have the oil fields, which furnish a very highly inductive load. The power factor at the oil fields loads is approximately 70 per cent, and when we first started the transmission line the power factor of the transmission line at the power house was 62 per cent leading. That was before we had much oil field business. As the oil field business came on—and I might say that the power factor of the oil field load is not much better than between 60 and 70 per cent—it never gets over 70—the power factor at the transmission end at the power house gradually approached unity, and at present, it is very close to that. Now operating the turbines in Bakersfield at 8000 kw., allowing them to float on the line with a little steam and adjusting the exciting current, we are able to keep the voltage regulation of the system very nearly constant, that is, we can hold the voltage in Bakersfield and at the power house end practically the same, with a rise of voltage in the middle of the line of about 8 or 10 per cent. Recently, due to the failure of some of the hydroelectric machinery at the generating end, the water end, it was necessary to carry the entire load from the Bakersfield end. You will appreciate that without any feeder voltage regulators on the system, the step-down transformer stations are connected through the proper taps to the line so that we get the proper voltage grading; in other words, near the power house the transformers are on a higher tap and gradually decrease down the line to take care of transmission drop. Feeding from the steam end, matters were reversed, and it was a question whether we were going to get any voltage regulation; but operating the hydroelectric generators as synchronous condensers, and feeding from the Bakersfield end with the steam generating machinery, we were able to keep suitable voltages at all points. That, I think, furnishes a very good illustration of Mr. Hagood's point.

J. P. Francis: There is one other point in the operation of a synchronous motor as a condenser that it is well to consider, and that is, that by limiting the range of the exciter voltage, so that when the voltage of the transmission line is too high the regulator is lowering the voltage of the motor, the excitation of the synchronous motor will not be reduced too greatly, causing the machine to fall out of step if the load increases suddenly. Also, the upper limit of the exciter voltage should not be high enough to burn the regulator contacts when the synchronous machine is boosting.

R. C. Powell: Mr. Jollyman mentioned that the charging current in the line should be taken into consideration. The point is, as I understand it, that the charging current will help out the generators but not the line.

Clarence L. Cory: It ought to help out both.

R. C. Powell: That is, with a simple line.

H. Y. Hall: I think Mr. Jollyman's point was more in respect to regulation. There is no question but that the charging current of the line will help out the regulation.

R. C. Powell: I don't believe I can agree with you, for the reason that the regulation will be the difference between the voltage and no-load voltage. At no-load the voltage at the receiving end will be higher than at the transmitting end, due to the charging current. Now if the load comes on, the charging current is constant, and the voltage rise due to the charging current is constant, so where you would start with 100,000 volts with no-load and get 120,000 at the receiving end, at full load you would have 20,000 drop, and you would have 100,000 at each end; but your regulation is the same.

J. P. Jollyman: I would say that the principal effect on the distributing system is naturally the improvement of the power factor of the load that the generators carry. The charging current is roughly one-half as effective in helping out the line as would be a synchronous condenser at the receiving end. The charging current is distributed, but it is not far, in general, from the effect of either the equivalent leading current load at the center of the line, or one-half of the load at the extreme end of the line. You will frequently find that a transmission system which starts out purely as a transmission affair, and transmits a very large percentage of its power over practically its entire length, grows into a system in which more or less load is taken off along the line; and in that case the charging current to a certain extent neutralizes the lagging current of the load, the load being somewhat distributed, and the charging current being entirely distributed. The principal effect of the charging current, as I stated, is an improvement in the generator power factor, which is a very important matter; and it is perhaps in its effect on the efficiency of transmission roughly half as effective as a concentrated synchronous condenser of the same capacity at the load point.

A Member: I would like to ask Mr. Jollyman if the value he gave of 18 kv-a. for a 60,000-volt line and 52 kv-a. for 100,000 volts, has a certain regard for the spacing, and cannot that be varied by altering the spacing?

J. P. Jollyman: I think if you will consider the practical cases of transmission at 60,000 volts you will find that the spacing is usually, in modern construction, about five or six ft. (1.5 to 1.8 m.), and for 100,000 volts the spacing is nine or ten ft. (2.7 to 3 m.), and is frequently in a vertical plane, which means that the average spacing of the phases is about one and one-third times the distance between the two wires. I have found by plotting the values of the charging current in relation to the ratio of the diameter of the wire to the spacing, that the change in the value of the charging current over the range of actual construction such as is generally employed by operating companies, is very small. If I remember correctly, the charging current per mile (1.6093 km.) for 10,000 volts over the range of construction ordinarily employed varies from about 0.028 of an ampere to about 0.031. The values that I gave were based on 0.03 of an ampere for 10,000 volts per mile. The charging current does not change very rapidly over the range of spacings, with the size of wire ordinarily employed. If you have a rather small spacing and very large conductors it would be a little different from what it would be if you had large spacings and very small conductors; but the change is really so small, and there are so many other things that enter into the exact determination of this value, that these figures are sufficiently accurate for ordinary calculation. You would not select the size of a generator down to anything smaller than 500 kilowatts, and these values will give you the charging current within a few hundred kilovolt-amperes of the actual value.

A paper presented at the 281st meeting of the American Institute of Electrical Engineers, New York, March 14, 1913.

AIR AS AN INSULATOR WHEN IN THE PRESENCE OF INSULATING BODIES OF HIGHER SPECIFIC INDUCTIVE CAPACITY

BY C. L. FORTESCUE AND S. W. FARNSWORTH

I. Introduction

The breakdown strength of air between conductors or terminals of different sizes and shapes when expressed in volts per cm. of separation, is a very variable value and of little significance. The great variation is not due to any difference in the strength of air, for that is a physical constant, which, according to the results of many able investigators, has a value of between 30 and 38 kilovolts (maximum value) per cm. The variation is due rather to the influence of such factors as the shape and size of the terminals or conductors, and also to the position and potentials of these bodies relative to neighboring bodies. The case of comparative breakdown distances between spheres and needle points for a given voltage, furnishes an excellent illustration of this point. For example, with 25 cm. between spheres 25 cm. in diameter, the air breaks down at 260,000 volts, whereas, it requires 67.3 cm. to withstand the same voltage between needle points. In the first case the volts per cm. are 10,400, whereas, in the second case the volts per cm. are 3870 (9800 volts per inch). Looking at air as a means of insulating a potential of 260,000 volts, it is evident that it serves much more efficiently in the case of the spheres than in the case of the needle points. When serving at its maximum efficiency, it will stand from 30 to 38 kilovolts (maximum) per cm., and for the purposes of this paper, the lower value will be assumed.

In most practical applications of air as an insulator, the problem is complicated by the presence of other dielectric media having a higher specific inductive capacity than that of air. There is, in general, a false conception of the parts played by the two media, namely, the solid dielectric and air, in performing their functions in the insulator. With increased voltage, the part the air plays becomes more and more important and at present-day voltages should no longer be disregarded. While the breakdown voltage of air alone, as expressed in volts per cm., is a very variable quantity, the breakdown voltage of air over the surface of a solid dielectric when expressed in the same terms (a value commonly called " creepage " voltage) is still more variable and of even less significance.

As there are ways, as illustrated above, of using air alone more efficiently than is ordinarily done, so are there ways of using the combination of air and a solid dielectric more efficiently. It is the purpose of this paper to show the conditions that determine the disruptive strength of an air path along the

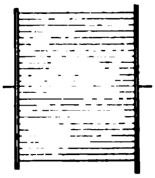


FIG. 1

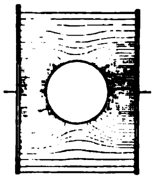


FIG. 2

surface of a solid dielectric of higher specific inductive capacity, and what steps must be taken to insure the most efficient use of the two dielectrics in combination.

II. THE ELECTRIC FIELD IN THE PRESENCE OF SOLID DIELECTRICS

The effect of introducing an insulating body into the static field between two conductors is to increase the stress in the air path at the surface of the conductor and at the surface of the insulating body, unless certain laws governing the proper shaping of the body, which will be stated later, are observed. This statement may be easily verified experimentally, by placing a glass sphere in the electrostatic field between two parallel plates maintained at different potentials. If the static field is intense enough, the introduction of the sphere will cause corona to form at the surface of the sphere and at the contiguous parts of the two plates; the air between the plates will consequently break down at a much lower voltage than if the glass sphere were absent.

Fig. 1 shows the condition of the static field before the introduction of the glass ball. Fig. 2 gives an idea of the condition of the field after the glass ball has been introduced. It will be seen that the lines of force concentrate in the glass ball and are more dense at its surface and at the portion of the surface of the two plates nearest the ball than anywhere else.

A close analogy to the action which takes place when the glass sphere is introduced into the electric field is obtained by placing a steel ball in the magnetic field between two large poles of opposite polarity. In this case the field may be mapped out with iron filings.

Fig. 3 shows a method of plotting the equipotential surfaces of the current flow between two conductors which are similar to the equipotential surfaces in the electric field between terminals of similar shape. To obtain the equipotential surfaces or current flow, current is passed between the two terminal plates suspended in a conducting liquid. Between the plates is suspended a metal ball. An insulated exploring lead is taken from any given point on the calibrated resistance placed across the supply voltage and carried into the liquid, where it terminates in an uninsulated point. The needle is moved about, always keeping the galvanometer zero, thus indicating the location of points of the same potential as the chosen point on the resistance. The movement of the exploring needle is recorded by means of a pantograph.

Fig. 4 shows one of many charts taken according to the method shown in Fig. 3. The bath used in this case was salt water, and the two terminal electrodes a cylindrical rod and a torus ring in the relative positions indicated. The wavy lines show the observed equipotential surfaces. The small circles indicate points of definite observed potential. The smooth lines show theoretical equipotential surfaces of indicated potential for the given terminals. Having obtained the equipotential surfaces, the lines of flow may be drawn in at right angles to them. If in Fig. 3 the sphere should be steel and the bath mercury, since the conductivity of iron is about ten times that of mercury, the field obtained would resemble very closely the field for the glass sphere in air, the specific inductive capacity of glass being about eight.

The conditions that exist at the surface of the dielectric of high specific inductive capacity in a field of force in air are brought about as follows: A given difference of potential will cause a very much greater electric flux in the solid dielectric

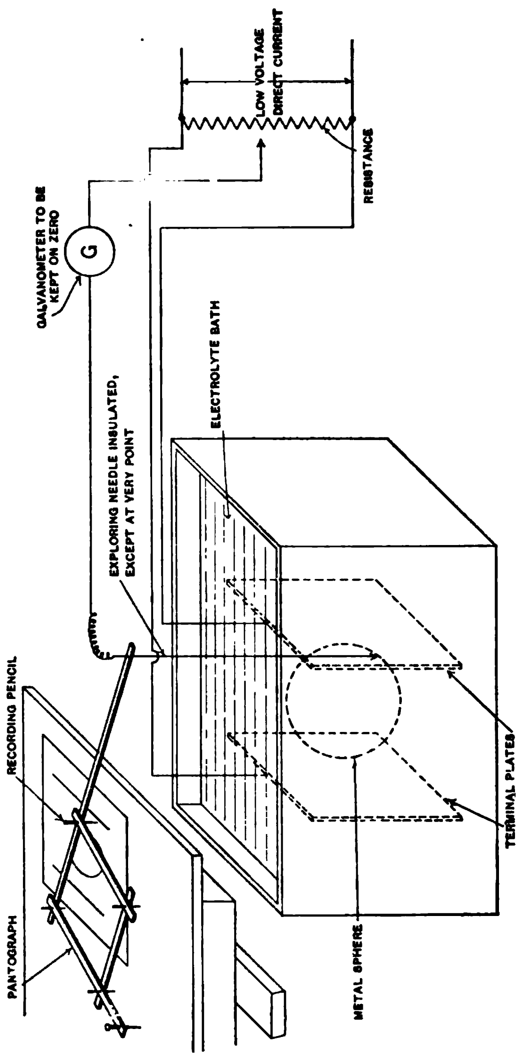


Fig. 3

than in air. At the surface of separation of the air and the solid, the tangential component of the intensity must be the same in the air as in the solid. Since it requires K times the force to produce the same dielectric flux in the air as in the solid, where K

is the value of the specific inductive capacity of the solid dielectric, the component of intensity in the air normal to the surface will be K times that in the solid dielectric. The intensity at each point of the surface of separation is increased from what it was

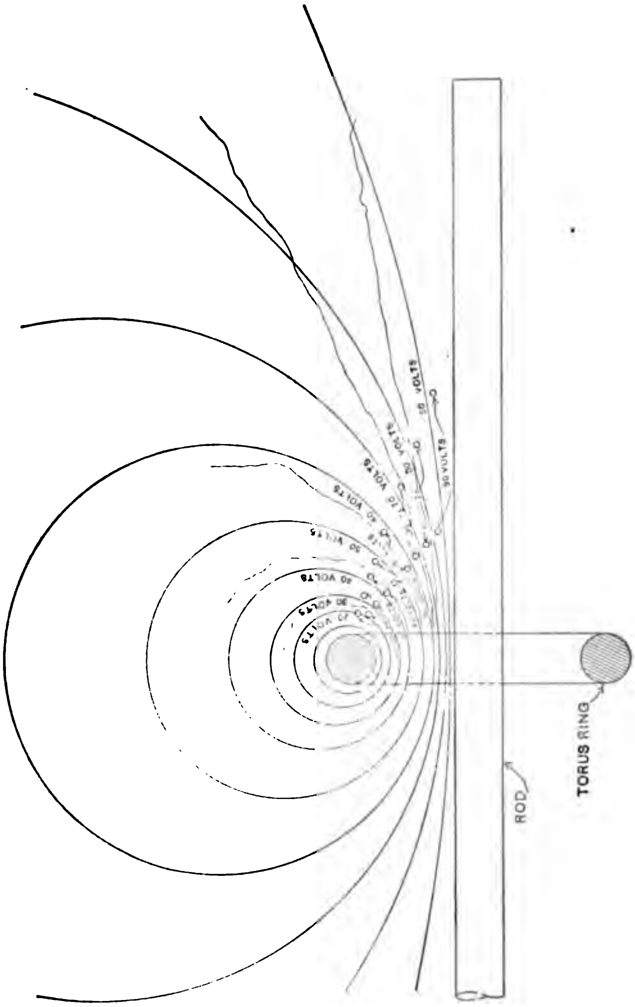


FIG. 4.

before the introduction of the dielectric and is more nearly normal to the surface. If the solid body extends from one conductor to the other, we may have a condition in which the tangential component of the intensity will be nearly uniform, but the maximum intensity being in a direction more nearly normal to the surface than before the introduction of the solid, its value is greater at every point. The air path along the surface is, therefore, weaker than before the introduction of the dielectric. If, however, we form the dielectric so that its surface is tangential to the lines of force at every point, then there will be no normal component of intensity; the tangential intensity at each point of the surface will be the same as before the intro-

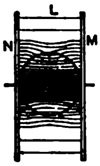


FIG. 5

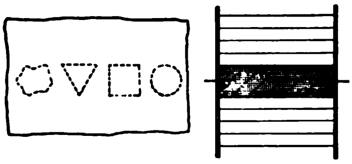


FIG. 6

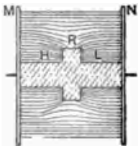


FIG. 7

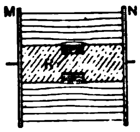


FIG. 8

duction of the solid, and the strength of the air path will remain unchanged.

To illustrate this by simple examples, let us consider the sphere to extend from one plate to the other, then the field will have the form shown in Fig. 5. Comparing this with Fig. 1, it is seen that the stress along the surface of the sphere adjacent to the plates has been very much increased. Suppose that we substitute for the glass sphere a glass cylinder, or any solid shape with a surface

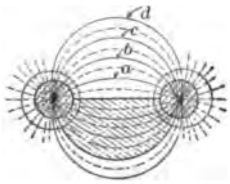


FIG. 9

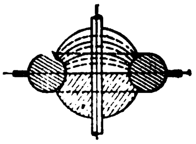


FIG. 10

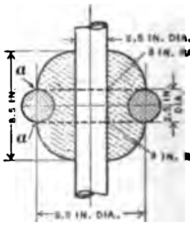


FIG. 11

generated by a line perpendicular to the planes as in Fig. 6, then there will be no increase of stress either at the surface of the solid or at the surface of the plates. In the first case, the introduction of the solid has decreased the breakdown strength by a large amount, but in the second case the strength is unimpaired by the introduction of the solid. Fig. 7 shows two planes, *M* and *N*, having a piece of insulating material extending continuously between them with a projection in the middle such as

[FORTESCUE AND FARNSWORTH]

FIG. 12
(Circular light spots due to shiny background and have nothing to do with the discharge.)

[FORTESCUE AND FARNSWORTH]

FIG. 21
(Circular light spots due to shiny background and have nothing to do with the discharge.)

is commonly employed to give increased " creepage " distance. In the light of the previous explanation, it is seen that the air in the path *HL* will be stressed to a higher value than if the projection *R* were not present. That is to say, the very thing which is ordinarily sought for, namely, an increased distance over the surface, is shown to be detrimental in this case rather than beneficial. The higher the specific inductive capacity of the material, the more detrimental is the projection.

Should a groove be placed in the insulating material as in Fig. 8, another very poor condition is obtained, as the air at *R* will be stressed beyond its limit at a comparatively small difference of potential between *M* and *N*.

The cross-hatched portion of the lower half of Fig. 9 shows how the insulation would have to be shaped to insulate two parallel wires with a maximum efficiency. The surface could be made to conform to any one of several flow lines, *a*, *b*, *c*, etc.

III. Practical Application of Theory with Results of Tests

Fig. 10 shows a means of insulating a terminal rod passing through a casing. Experimental pieces of this design were made up to dimensions shown in Fig. 11. The material used was a mixture of shellac, whiting and various gums. The bushing was cast in an accurately machined mould. A maximum breakdown of 145 kv., effective value, was obtained, and when good care was taken to have a clean surface, the breakdowns were seldom less than 135 kv.

Fig. 12 shows a breakdown at 137 kv. on one of these bushings and it will be noticed that there are no evidences of corona at any point of the surface.

Fig. 13 shows the calculated intensity curve for the surface path. Assuming a maximum allowable intensity of 19.7 kv. per cm. (50 kv. per in.), effective value, at the points of highest intensity which are at the surfaces of the ring and rod, by integrating the area under the curve, we obtain the voltage necessary to break down the path, which on the above assumptions is 142 kv. It will be noted from the curve that the minimum intensity is 9.1 kv. per cm. (23.2 kv. per in.) or slightly less than half the maximum value. The distance over the surface is 11.9 cm. (4.7 in.) so the average volts per cm. for a breakdown voltage of 145,000 is 12.2 (31 kv. per in.).

The dimensions of ring and rod chosen were such as to give a maximum breakdown voltage over the surface for a mean diameter of a torus ring of 21.6cm. (8.5in.). The surface of the solid was made to conform to the flow lines, leaving the torus ring on line *a*, Fig. 11. Any other set of flow lines might have been chosen. Had a set been chosen leaving outside of lines *a*, there would have been difficulty in casting the bushing due to the back draft on the mold. If a set of lines had been chosen leaving inside of lines *a*, there would have been a pocket to catch dirt.

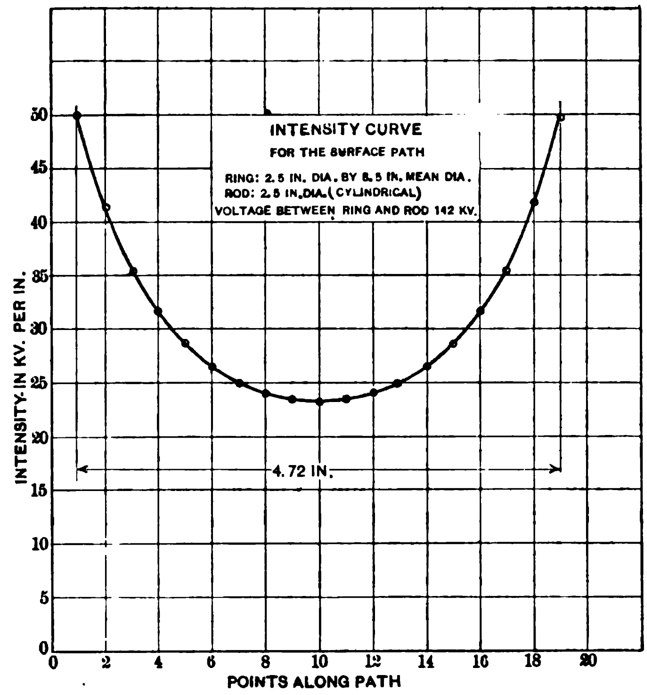


FIG. 13

The law of similar fields applies to these cases, so if all the dimensions of Fig. 11 be doubled, the breakdown voltage should be doubled.

As a matter of interest, the approximate intensities existing through the insulating material along the line of minimum separation between ring and rod were calculated, and are given in Fig. 14, considering a voltage of 142,000 applied. The intensity is seen to be less at the ring than at the rod, and for the given voltage, the intensity at the rod is 46 kv. per cm. (117 kv. per in.)

effective value. Ordinary insulating material should be capable of standing this voltage without being unduly stressed. This is the more evident when we consider that air itself is capable of standing 21 to 27 kv. per cm. (54 to 68 kv. per in.)

The above analysis of the intensities in the solid dielectric suggests that to find the real strength of insulating materials, some form of terminals should be used which will permit calculation of the stress at the point of maximum intensity when breakdown occurs.

In order that the great gain which can be made by properly designing the surface of the solid dielectric may be more evident,

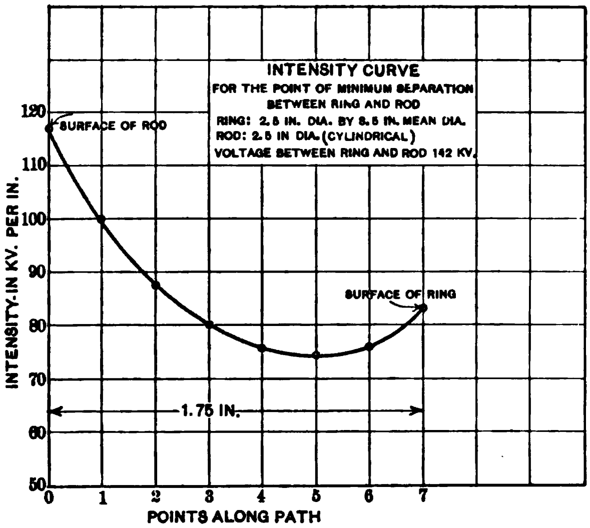


FIG. 14

attention is called to the breakdown over the surface of a former standard transformer bushing and an ordinary line insulator. Fig. 15 shows the transformer bushing which was formerly a standard for 66,000-volt work and which would flash over its 81½ cm. (32 in.) of surface at a voltage of about 145,000. In the bushing shown in Fig. 13, a distance of 11.9 cm. (4.7 in.) breaks down only when this same voltage is reached. Fig. 16 shows an ordinary line insulator having a surface distance of 63.5 cm. (25 in.) which breaks down at 119,000 volts.

By changing the shape of the terminals it is possible to design a bushing which will have a higher average intensity over the surface before breakdown of the air will occur. Fig. 17 shows an arrangement similar to that of Fig. 11, which accomplishes this. Allowing the same mean diameter of torus ring, 21.5 cm.(8½ in.), as used in Fig. 11, and the same maximum intensity in the air, we see from Fig. 18 that the calculated breakdown voltage will be 181 kv. The minimum intensity is 13.2 kv. per cm. (33.5 kv. per in.) as against 9.15 kv. per cm. (23.2 kv. per in.) for the bushing shown in Fig. 11. Fig. 19 shows the calculated maximum intensities in the solid, which are 179 kv. per cm. effective value, (455 kv. per in.) at the surface of the rod. Judging from our tests, it seems reasonable to expect that what are ordinarily considered as good insulating materials, will be capable of standing this intensity. This piece has not been made up, however.

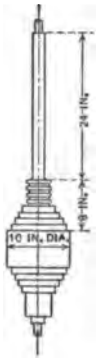


FIG. 15

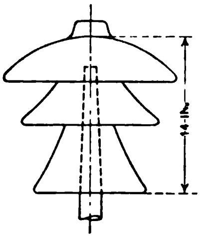


FIG. 16

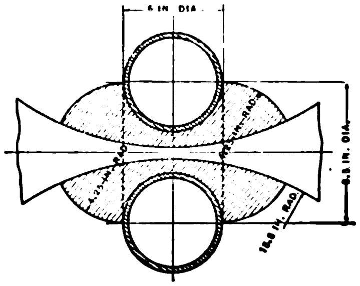


FIG. 17

Fig. 20 shows a different design, using a confocal system of ellipsoids and hyperboloids of revolution, having foci which are 5.08 cm. (2.0 in.) apart. A piece was turned out of hard rubber to the given dimensions and the average breakdowns over the surface have been above 160,000 volts; thus giving an average of 9.4 kv. per cm. (23,900 volts per in.) for a surface distance of 17.0 cm. (6.7 in.). These values are striking when compared with the breakdown voltage of 145,000 volts for 81½ cm. of surface of the bushing shown in Fig. 15. In that case the average kilovolts per cm. are 1.78 (4.54 kv. per in.).

This piece was placed where it would collect dust and dirt such as it would be likely to in indoor service and was tested after three months. It showed no deterioration in breakdown strength, even with this heavy coating of dust.

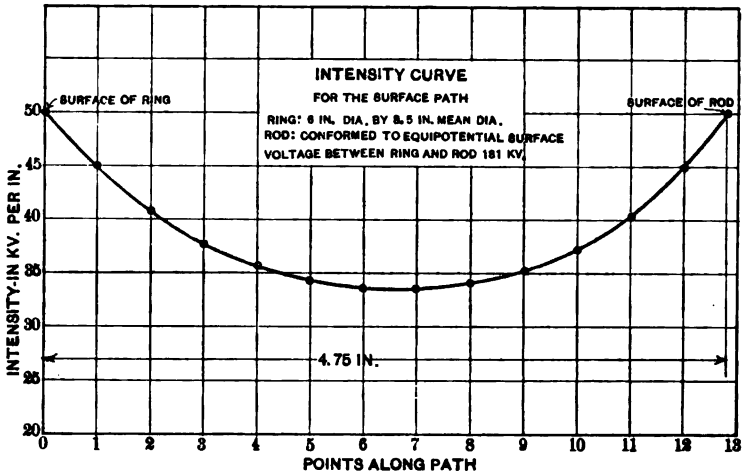

INTENSITY CURVE
FOR THE SURFACE PATH
RING: 6 IN. DIA. BY 8.5 IN. MEAN DIA.
ROD: CONFORMED TO EQUIPOTENTIAL SURFACE
VOLTAGE BETWEEN RING AND ROD 181 KV.
SURFACE OF RING
SURFACE OF ROD
4.75 IN.
INTENSITY-IN KV. PER IN.
50
45
40
35
30
25
20
0
1
2
3
4
5
6
7
8
9
10
11
12
13
POINTS ALONG PATH

FIG. 18

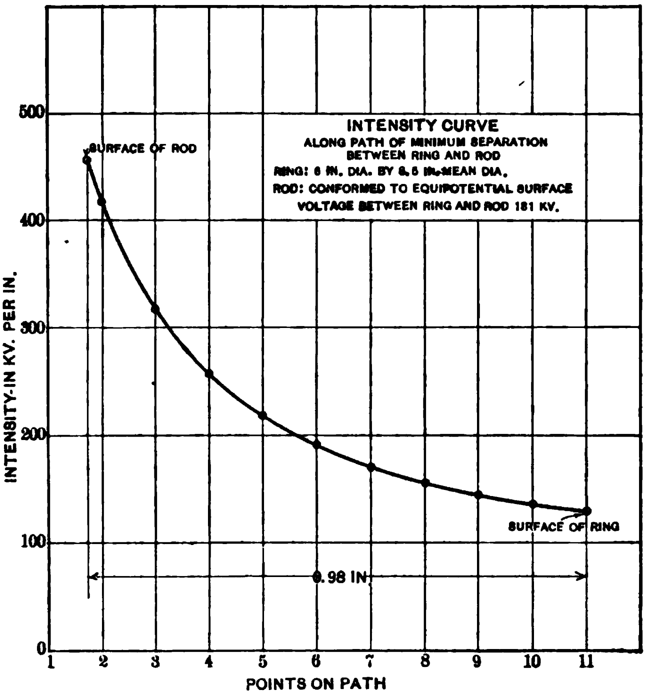

INTENSITY CURVE
ALONG PATH OF MINIMUM SEPARATION
BETWEEN RING AND ROD
RING: 6 IN. DIA. BY 8.5 IN. MEAN DIA.
ROD: CONFORMED TO EQUIPOTENTIAL SURFACE
VOLTAGE BETWEEN RING AND ROD 181 KV.
SURFACE OF ROD
SURFACE OF RING
0.98 IN.
INTENSITY-IN KV. PER IN.
500
400
300
200
100
0
1
2
3
4
5
6
7
8
9
10
11
POINTS ON PATH

FIG. 19

Fig. 21 shows a rather crude, but none the less instructive and convincing experiment. The arrangement of parts and their dimensions are shown in Fig. 22. The illustration shows how the longer path, having insulation stepped off in the conventional way, broke down without distressing the shorter end in the least.

Fig. 23 shows a possible adaptation of this principle to an insulator which is shown as of the suspension type, but which might be slightly modified and made pin type.

The applications of the principle of shaping terminals and dielectric spoken of thus far, have embodied new designs of each. In the condenser type bushing we may have an ideal even distribution of potential on the surface. The full benefit of this

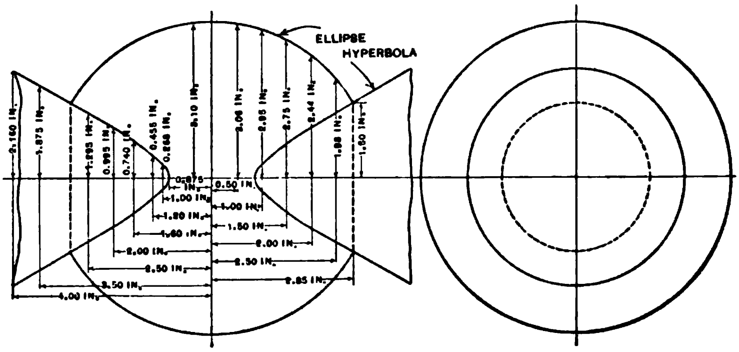


Fig. 20

distribution cannot be obtained, however, unless the external field is properly cared for. As an excellent illustration of this statement, the following test on a large terminal is given. This particular terminal had a maximum diameter of about 30.5 cm. (12 in.) and was about 203 cm. (80 in.) high. As originally designed, a metal disk was provided on the top end, which was 40.6 cm. (18 in.) in diameter with a 3.8 cm. (1.5-in.) pipe welded on its outer edge. Under test, corona was visible from the edges of the disk at 300,000 volts; at 400,000 volts there was a great deal of corona, and the small metal rings at the ends of the tin-foil coatings at the top steps were beginning to glow; at 430,000 volts is was deemed inadvisable to either hold the voltage or to raise it any higher for fear of a complete breakdown. A new disk was designed for the top of the terminal and the dimensions selected were 152.5 cm. (60 in.) diameter and 30.5 cm. (12 in.) thickness, with edges having a radius of 15.25 cm. (6 in.). The disk was made of wood and covered with tinfoil. The results obtained with this new disk were rather startling. At no voltage up to 575,000, the maximum voltage tried, was there any appreciable corona. There were occasional bunches of bluish fan-shaped haze emanating from some piece of dirt or rough spot on the disk toward the iron framework of the building which happened to be near the terminal. The body of the ter-

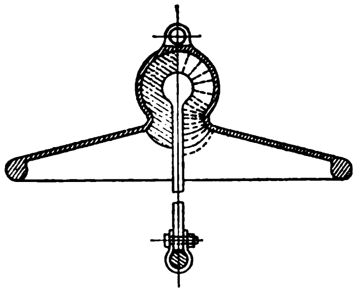


Fig. 23.

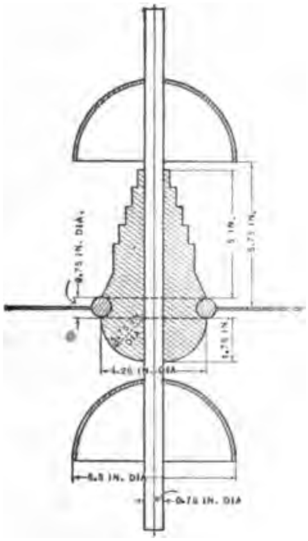


Fig. 22.

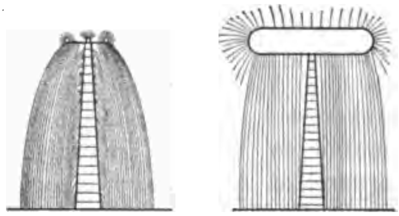


Fig. 24.

minal itself showed not the slightest sign of a glow on any of the steps. Thus, by means of a very inexpensive change of design, the terminal was able to stand 575,000 volts as satisfactorily as it had stood 300,000 volts previously. This was accomplished simply by relieving stresses in the air at the top end of the terminal, and also redirecting the field adjacent to the surface of the terminal, so that the equipotential surfaces in the air were evenly distributed, corresponding to the even distribution secured by the condensers. Fig. 24 gives some idea of the distribution of the field under the two conditions.

IV. Summary and Conclusions

We have given, in this paper, a new conception of the functions which air and a solid dielectric perform when used in combination for insulating purposes. Based on this conception, a large number of tests have been made under commercial conditions, which show that it is possible to use air more efficiently than has been customary in the past. Breakdowns of an air path over a surface have been obtained which average as high as 9.4 kv. per cm., effective value, (23,900 volts per in.) over a distance of 17.0 cm. (6.7 in.). The conditions of design are such that these same averages may be maintained at any voltage, by increasing all dimensions of the structure proportionately.

A maximum efficiency of air path over a surface is obtained when the surface of the dielectric is made to conform to the flow lines between the terminals. The strength of such a path is independent of the specific inductive capacity of the dielectric. The principal thing to be considered, therefore, is the proper shaping of the terminals in order that points of high intensity may be eliminated and a high average intensity obtained for the given path.

It seems to the authors that there exist great possibilities of improvement in present designs of terminal bushings and high-tension insulators, when considered from the standpoint outlined in this paper. It was with a view to stimulating research along these lines that this paper was written.

Acknowledgment is made to Mr. K. C. Randall, for his participation in the early investigations on this subject.

Several of the authors' associates have closely followed the tests described above and have reviewed the results. Our thanks are due to Messrs. H. J. Ryan, C. F. Scott, L. W. Chubb, J. A. Sandford, Jr., H. B. Smith, B. G. Lamme, H. E. Clifford and C. E. Skinner for the interest they have taken in this investigation.

A paper presented at the 281st meeting of the American Institute of Electrical Engineers, New York, March 14, 1913.

THE APPLICATION OF A THEOREM OF ELECTROSTATICS TO INSULATION PROBLEMS

BY C. FORTESCUE*

I. Introduction

In the early days of the electrical industry, the problem of insulation was solved by taping the conductors to be insulated with some kind of textile material saturated with an insulating compound. Present day methods of insulation, while showing marked improvement over those in the past, still retain the influence of early tradition, and are largely based on rule of thumb. This condition of affairs may be ascribed to the very rapid development of high-tension transmission systems, and the consequent immediate demand for apparatus insulated for high voltages. The development has been so rapid that engineers have had no time for a proper study of the problem.

One aspect of the problem that has received but little consideration is the proper grouping of conductors, so that they shall assist in insulating one another. To illustrate this idea by an example that will be familiar to everyone, consider two spheres 25 cm. in diameter and 25 cm. apart, at potentials + 200,000 and − 200,000 and a small thin disk of conducting material connected to ground and placed near the spheres. The disk may be placed in a number of positions in which it is insulated from both of the spheres, but there is only one set of positions in which it will cause no disturbance of the electric field, namely, those lying in the zero potential surface or plane of symmetry midway between the two spheres. If one of the spheres be removed, the balance will be disturbed and the remaining sphere will discharge to the disk. Thus the presence of the sphere of potential + 200,000 has served to insulate the sphere of potential − 200,000 from the grounded disk. Very thin wires may be brought into the field of the two spheres and maintained at a difference of potential many times larger than their normal disruptive voltage.

In such cases as these the ordinary rules of procedure followed in insulation methods are seemingly violated. Thus sharp edges and fine wires are more effectively insulated than thick rounded edges and large wires. In practical work, conductors usually have edges where the intensity becomes high, and therefore, if by some method, these sharp edges can be insulated as effectively as large rounded edges, a valuable advantage will have been gained.

The object of this paper is to emphasize a principle of electrostatic theory by which the individual units of a system of conductors may be arranged to protect one another. The statement of the principle and several illustrations are given in Section II following.

II. Statement of Principle of Electrostatic Theory with Illustrations

If a region in any particular electric field be isolated or cut out by any number of closed surfaces, then the electric field in this region will remain unchanged, whatever change may take place in the external electric field, if the potentials at all points on the enclosing surfaces are maintained at their original values.

It follows from the above that the intensity due to the surface distribution will, at each point of the region, be the same as that of the assumed electric field. The potential in such a region will be unaffected by any change in position or electrification of external bodies, since it is uniquely determined by the surface potential distribution which remains unchanged.

It is also obvious from this principle that two or more regions, completely separated from one another by any number of surfaces, may each have a different electric field, if at every point of the dividing surface or surfaces between any two of the regions, the fields in these regions have the same potential.

The truth of the principle just stated is almost self-evident, for it is known that when the potentials of a system of conductors are given, there is only one possible solution of the electric field consistent with the proper conditions. If, therefore, a solution be known that will give the same potential at each point of the bounding surfaces of a given region, as the assigned potentials, then this must be the only possible solution. It is not so easy to see that the field within the region is independent of any external influences. This may be deduced from the fact that, in order to produce a potential within the region, an external system must also produce a potential at each point of the surfaces, and, since the potentials at the surfaces are maintained constant, the effect of external bodies within the region must be zero. Changes in the external electric field have the effect, however, of altering the charges at each point of the surface. In other words, the effect of any change in the external system is to produce a change in the capacity of the system of charged bodies at the surfaces of the region, in such a way that there is a change in the surface charges, the potentials remaining constant, of such a nature as to completely annul the effect within the region of the change in the external system.

The analytic theorem upon which this principle is based may be found in Maxwell's " Electricity and Magnetism," 1904 edition page 136, Article 99.(b). A more general proof af the same theorem is given in Kelvin and Tait " Treatise on Natural Philosophy," 1903 edition, Vol. 1, Chapter 1, Appendix A (c). A full discussion of Green's problem, which has some bearing on the above principle, may be found in Volume 2 of the same work, Articles 499 to 508 inclusive. Reference may also be made to Jean's " Electricity and Magnetism," Articles 186, 187 and 188, and to Webster's "The Theory of Electricity and Magnetism," the last paragraph of Article 86.

A few simple illustrations of electric fields produced in accordance with this principle may serve to give a more vivid conception of its use.

Consider two parallel plane conductors, forming two portions of the surface of a solid dielectric. To produce within the body a uniform field corresponding to that between two infinite parallel plates having the same potentials as the two conductors, all that is necessary is to produce at each point of the surface of the dielectric a potential equal to that of the uniform field at that point. The whole surface of the dielectric might be considered as mapped out in equi-potential contour lines infinitely close together and each line may be imagined to become a conductor which will be supposed to be connected to an external source of the proper potential.

A dielectric sphere in a uniform field furnishes an interesting application of this principle. If the potential of the field be $V = R\,x$, and if the center of the sphere be at the origin and its radius be a, any distortion of the field due to the difference in specific inductive capacity of the sphere from air may be prevented by maintaining, at the surface of the sphere, a potential distribution,

$$V_s = R\,a \cos\theta$$

Under this condition, the potential both inside and outside the sphere will be,

$$V = R\,x$$

Without the surface distribution, the potential inside the sphere would be,

$$V_1 = \frac{3}{K+2} R\,x$$

and that outside the sphere would be,

$$V_0 = R\,x \left\{ 1 - \frac{K-1}{K+2} \left(\frac{a}{r} \right)^3 \right\}$$

The maximum value of the intensity at the surface of the sphere in the air would be,

$$R_0 = \frac{3\,K}{K+2} R$$

Thus if $K = 4$, $R_0 = 2\,R$. The intensity therefore has been doubled at the maximum point by the introduction of the sphere.

It is interesting to find the surface distribution of electricity produced on the sphere by the potential distribution, V_s over the surface. Let V_2 be the internal potential due to this surface distribution of electricity. Then

$$V_2 = R\,x - V_1$$

$$= \frac{K-1}{K+2} R\,x$$

The surface density required to produce this internal potential is

$$S = \frac{3\,(K-1)}{4\,\pi\,(K+2)} R \cos\theta$$

The external potential due to this surface density will be

$$V_3 = \left(\frac{a}{r} \right)^2 \frac{K-1}{K+2} R\,a \cos\theta$$

or since

$$r \cos \theta = x$$

$$V_3 = \left(\frac{a}{r}\right)^3 \frac{K-1}{K+2} R x$$

and this when added to V_0, the external potential obtained without the potential distribution, gives the potential outside the sphere when the surface potential distribution is determined according to the principle. That is,

$$V_0 + V_3 = V = R x$$

A section of the equipotential surfaces of the electric field in the neighborhood of a dielectric sphere ($K = 4$) in a uniform field, with and without the proper potential distribution, is shown in Figs. 1 and 2.

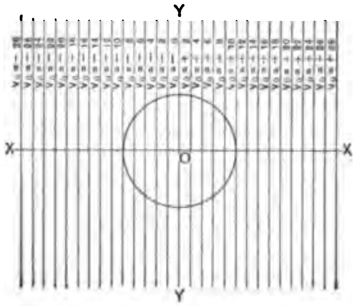


FIG. 1—SECTION BY PLANE PASSING THROUGH AXIS OF X OF EQUIPOTENTIAL SURFACES OF DIELECTRIC SPHERE IN A UNIFORM ELECTROSTATIC FIELD.

The sphere has the proper potential distribution over its surface to insure no distortion of the field.

Cylindrical conducting bodies and insulating structures are of common occurrence in practical problems. Suppose it be required to insulate a short external cylinder from a long internal concentric cylinder. A line may be drawn from the ends of the external cylinder to the nearest point on the ends of the inner cylinder, see Fig. 3. The surfaces of revolution formed by these two lines and the two cylinders may be taken as bounding surfaces of the enclosed region, which will be assumed to have within it a potential

$$V_1 = B \log_e r + A$$

where r is the distance of the point, whose potential is V_1 from the axis of the cylinders and B and A are constants determined from the potentials of the two cylinders. The equipotential surfaces of the field will intersect the surfaces of revolution formed by the two lines in circles, the planes of which will be perpendicular to the axis of the cylinder. If each circle were a conductor connected to an outside source having the same potential as that of the equipotential surface on which the circle lies, the distribution over the surface so produced would cause the potential within the enclosed region to be equal to

$$V_1 = B \log_e r + A$$

If, instead of the external cylinder, a disk be substituted of infinite radius with a hole in it of the same diameter as the cylinder, and with its plane at right angles to the axis of the cylinders, and if the internal cylinder be provided with a similar

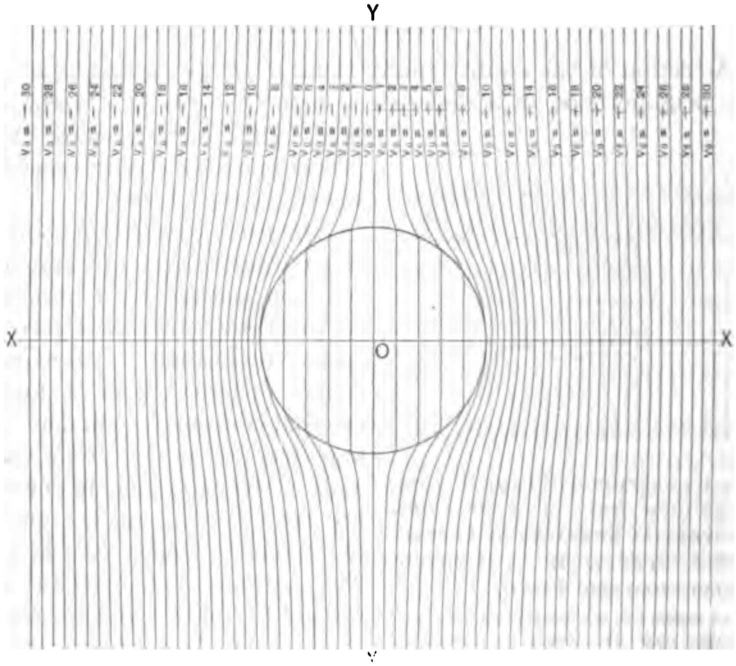


FIG. 2—DIELECTRIC SPHERE (K = 4) IN UNIFORM ELECTROSTATIC FIELD

Section by plane passing through axis of X of equipotential surfaces in neighborhood of sphere. The value of the potential of each surface is given in the figure.

disk at each end; taking z as measured from the point of intersection of the axis of the cylinder and the plane of the middle disk, a field may be assumed between the middle and upper disks, having its equipotential surfaces parallel to the disk, and the potential of the field will be

$$V_0 = C + D z$$

C and D being determined by the potentials of the middle and outer disks.

The region between the inner cylinder and middle disk may be assumed to have an electrostatic field, the equipotential surfaces of which are cylinders concentric with the inner cylinder, and whose values are given by

$$V_1 = B \log_e r + A$$

The surface of separation between the two regions having potentials V_1 and V_0 will have for its equation,

$$r = e^{\frac{C - A + Dz}{B}}$$

$$= a \left(\frac{b}{a} \right)^{\frac{2z}{l}}$$

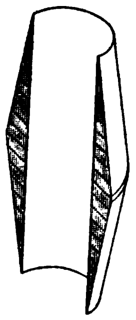


Fig. 3

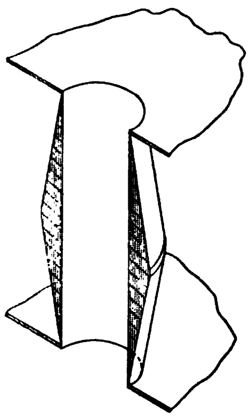


Fig. 4

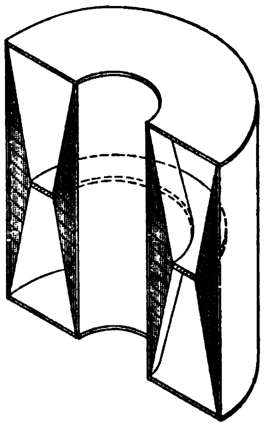


Fig. 5

where

$a = e^{\frac{C-A}{B}}$ = radius of hole in outer disk

b = radius of inner cylinder.

l = length of inner cylinder.

If the three disks are limited in diameter, the middle one being less than the other two, and if the region between the two outer disks is completely enclosed by an external cylinder extending from one to the other, the outer edge of the middle disk may be insulated from the external cylinder in a similar manner. These three examples are illustrated in Figs. 3, 4 and 5 respectively.

III. Practical Applications—Limitations

In practical applications of this principle, the means for producing a potential distribution over a surface are limited. A finite number of steps must be used and the potentials must be applied to strips of metal of finite widths in such a way that the intervening surface between two adjacent strips shall have the proper potential at each point. However, with proper care in laying out the various parts, a fairly close approximation may be made to ideal conditions.

The practical application of the principle that at once suggests itself as being most suitable, is the insulation of high-voltage transformers. Accordingly, the problem of insulating high-voltage transformers, particularly such as are used for testing purposes, will first of all be considered. Application to line insulators will then be taken up and miscellaneous uses to which the principle may be adapted.

IV. Core Type Transformers

Before considering the application of the principle to the insulation of transformers, it will be well to consider the mechanical characteristics of the materials available for insulating transformers. Moulded material, on account of the large bulk required in high-voltage transformers, is probably out of the question. At present, the most suitable materials are obtainable in the form of cylinders and plates. The insulation of high-voltage transformers will therefore consist of structures of concentric cylinders and parallel plates of solid insulation interspersed with oil. The forms of electric field that are most suitable with such insulation structures, namely, the uniform field and the field of logarithmic potential, give also the best conditions of stress in the materials and are adapted to the natural form of the winding and core structure of transformers.

The simplest form of high-voltage transformer has one terminal connected to ground through the core and case and the other is brought out through a high potential bushing in the cover. Fig. 6 shows the diagram of connections for such a transformer designed in accordance with this principle. The high-tension winding of this transformer is made up of a number of discoidal-shaped coils, arranged and connected so that the potentials of the coils progressively decrease from the center coils outward. Thus, the coil marked, "terminal coil" has the highest potential, while that marked "ground coil" has the lowest potential.

Each coil is so placed that it lies on the equipotential surface, corresponding to its potential, of an assumed uniform field in the region included between the middle high-potential coils and the outer or low potential coils. The whole high tension winding thus assists in producing in the external region in close proximity to the windings, and in the part of the internal regions which are unoccupied by the cylindrical barriers, what substantially amounts to a uniform electric field.

The external surface of the cylindrical insulating structure which serves to insulate the core and low-tension winding from the high-tension winding is shaped according to the internal field of logarithmic potential and the external uniform field, as

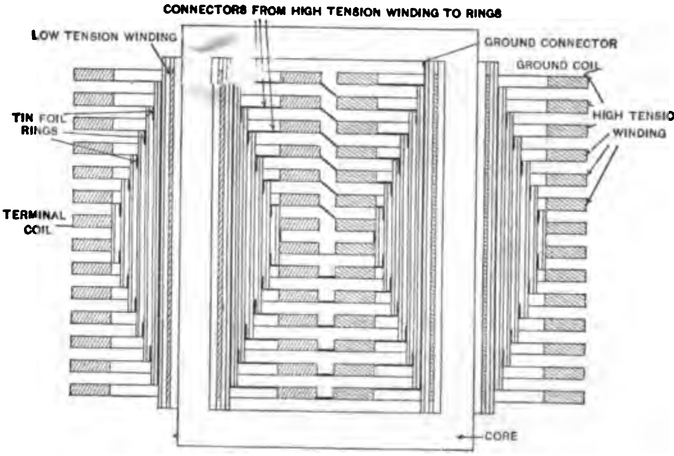


Fig. 6—Insulation Scheme for Transformer Designed to Operate with One End Grounded

shown in the foregoing illustrations given in Section II. See Figs. 4 and 5. The potential distribution over the surface of this cylindrical structure is produced by connecting unclosed rings of tin foil on the insulating cylinders to the proper point of the high-tension winding. The top and bottom rings on each cylinder are connected together by a thin strip of copper, which also serves to connect an upper to a lower coil. Thus the ring and its connecting strip lie on the proper equipotential surface of the system, and therefore produce no disturbance in the electric field. Moreover, the ring on account of its width, helps to produce the proper potential at each point of the surface of the cylindrical barrier lying between it and the adjacent ring.

The outlet bushing also is designed so that when in position the end of each conducting layer will lie on the equipotential surface of the system having the same potential, thereby producing the minimum disturbance. This outlet bushing is not shown in Fig. 6.

Fig. 7 shows a transformer built according to this scheme partly assembled. Fig. 8 shows the same transformer with coils and iron assembled, but without external barrier. Fig. 9 shows the transformer with external barrier partially assembled, and Fig. 10 shows the transformer complete in tank. No attempt was made to produce an artificial distribution of potential over the surface of the external barrier of this transformer which was made of fullerboard, on account of the impossibility of obtaining true cylindrical surfaces with such material. After this transformer was completed, it withstood successfully the following tests applied in succession:

High Tension Potential Above Ground By Ratio.	Time of Application.
400,000 volts	1 minute.
450,000 "	½ "
500.000 "	1 "
525,000 "	1 "
573,000 "	10 seconds

The potentials indicated by needle point spark gap were 35 per cent higher than given by ratio.

At a later date, the transformer was excited so as to give by ratio a potential of 623,000 volts effective above ground without the terminal in place.

The form of transformer next in degree of simplicity is designed to operate with the middle point of its high-tension winding grounded. Instead of one transformer designed to operate in this manner being used, two transformers, like that already described, may be used, and while it is somewhat more costly to make two such transformers, there are advantages in having two units that can be used for testing independently. When two transformers are used in this way, the difference of potential obtainable with the same conditions of insulation stress is double that of one. Thus two transformers, each capable of producing 500,000 volts to ground when so combined, will be equivalent to a one million-volt transformer with the middle point grounded. The design of a transformer to operate with the middle point grounded may be made according to the scheme shown in Fig. 11.

[FORTESCUE]
FIG. 7—TRANSFORMER PARTLY ASSEMBLED

[FORTESCUE]
FIG. 8—TRANSFORMER WITH EXTERNAL BARRIER REMOVED

FIG. 9—TRANSFORMER WITH EXTERNAL BARRIERS PARTIALLY ASSEMBLED

[FORTESCUE]

FIG. 10—TRANSFORMER COMPLETE IN TANK

A transformer is sometimes required capable of operating with either end of the high tension winding grounded. A design for such a transformer may be made according to the scheme shown in Fig. 12. The insulating structure between the groups of coils on different legs may be made up of flat sheets, in which case the surface potential may be such as to give a uniform field. Instead of a structure of flat sheets, a cylindrical structure, completely surrounding the groups of coils on each leg, may be used, in which case, the proper potential distribution is such as will produce in the structure, a field of logarithmic potential. Besides acting as insulation between the two groups of coils, the cylindrical structure forms part of the insulation to ground, but it is

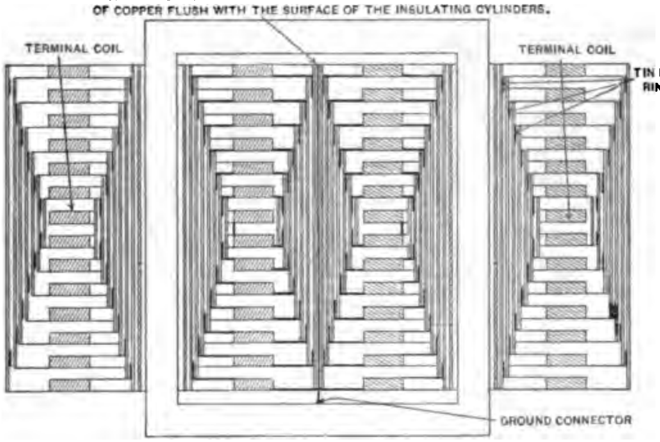


FIG. 11—INSULATION SCHEME AND COIL ARRANGEMENT FOR TRANSFORMER DESIGNED TO OPERATE WITH MIDDLE POINT GROUNDED

hardly practicable on account of the high cost of insulating cylinders of very large diameter.

V. CORE TYPE TRANSFORMERS FOR POLYPHASE CIRCUITS

Conditions of operation on polyphase circuits are somewhat different from those on single phase circuits. A brief description will be given of the changes required to adapt the schemes for insulating transformers shown in the last section to polyphase circuits.

For grounded three-phase star systems, the scheme shown in Fig. 6 may be used unchanged. For ungrounded star systems this design may be modified by the addition of extra insulation between core and windings to protect against stresses due to one wire becoming grounded. The insulation included in the coil grouping, in this case, should be considered as a separate region, because external bodies may have their potentials changed relatively to the winding. Accordingly, the potentials of the windings themselves will also be raised or lowered relatively to external bodies. Therefore, to maintain the electric fields in the coil grouping and included insulating structures unchanged, it is necessary to consider all this space as a separate region. The

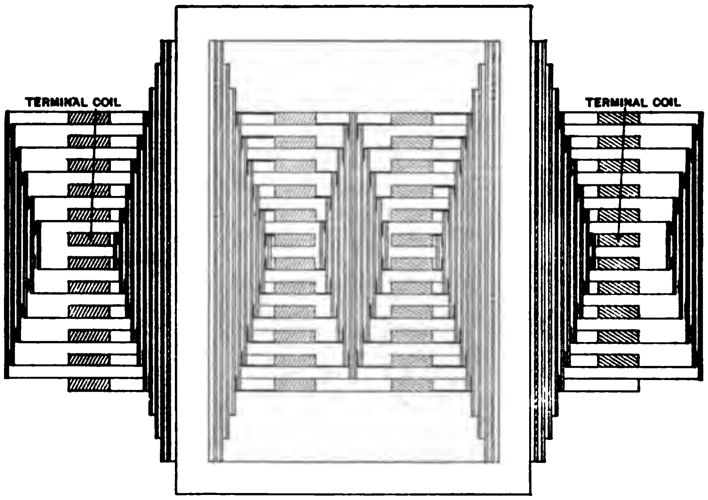


FIG. 12—INSULATION SCHEME FOR TRANSFORMER DESIGNED TO OPERATE WITH EITHER END GROUNDED

intensity at each point within this region will then be independent of the potential of the coils relative to ground. For the region external to the winding included between the cylinder and coils of lowest potential of the winding system and the core, the proper potential distribution over the surface may be obtained by means of a system of concentric cylindrical condensers of equal capacity, varying in length by equal steps, according to the principle used in the condenser-type terminal. Fig. 13 shows a scheme of insulation for such a transformer.

For delta connection, transformers may be a modification of design shown in Fig. 12, additional strength between windings and core being secured to take care of stresses when one winding becomes grounded.

Figs. 14 and 15 show polyphase transformers designed according to the principle.

VI. Shell Type Transformers

Figs. 16 and 17 show the arrangement and connections of the high and low-tension coils of a shell-type transformer designed to operate with one end permanently grounded. Here the coils themselves correspond to the equipotential surfaces of one region, their potentials decreasing progressively from No. 1 to No. 9. In the regions included between the coils and iron, the surface

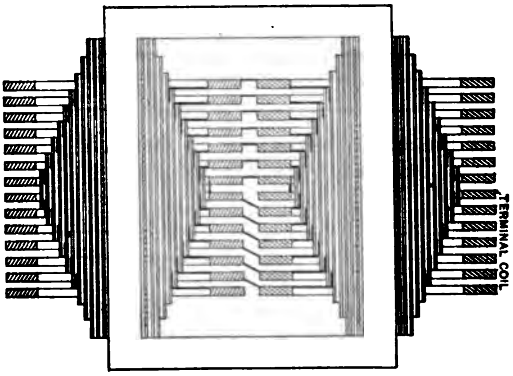


Fig. 13—Insulation Scheme for Transformer Designed to Operate on Star System with Ungrounded Neutral

potential is determined by the coils themselves, so as to produce approximately a uniform field. Fig. 16 shows a section through the middle of the group at right angles to the plane of the core laminations, while Fig. 17 shows a section of one side of the coils parallel to the plane of the laminations. A single group only has been shown. If more groups are used, each group may be wound in the same way, and insulated from the adjacent low-tension winding and iron, according to the potential of the outer coils.

A shell-type transformer, designed to operate with its middle point grounded, may be made up of two groups connected as shown in Fig. 18, the inner coils of each group being the high-potential coils, and the outer being connected to the iron and case and grounded.

For polyphase systems, modifications of the designs shown above may be used. These modifications are of the same nature as those required to adapt core type single-phase design to designs for operation on polyphase circuits, and need not be gone into any further.

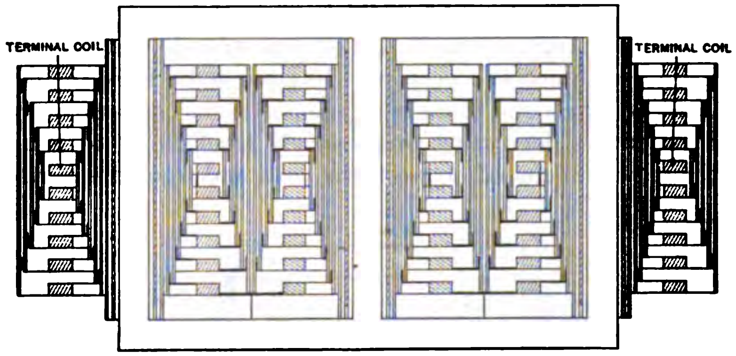


FIG. 14—THREE-PHASE TRANSFORMER TO OPERATE ON CIRCUIT WITH NEUTRAL POINT GROUNDED

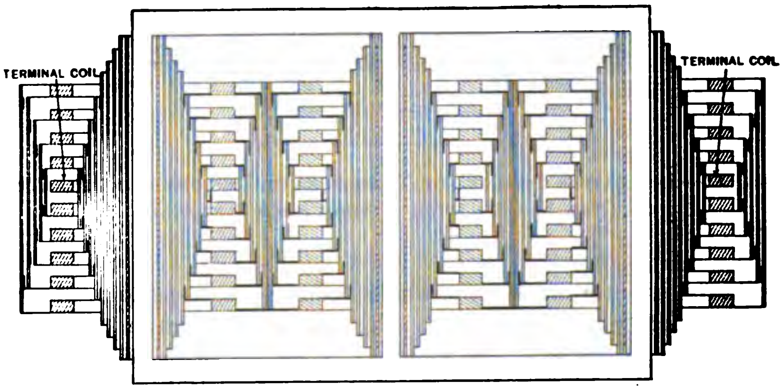


FIG. 15—THREE-PHASE TRANSFORMER DESIGNED TO OPERATE ON UNGROUNDED SYSTEM

VII. APPLICATION OF THE PRINCIPLE TO THE DESIGN OF OUTLET TERMINALS AND INSULATORS

In core-type transformers, designed according to the principle, the outlet terminal forms part of the insulation system of the transformer, and must be designed in accordance with the same principle. One way of doing this would be to produce within the body of the terminal a logarithmic potential by means of rings of tin foil connected to the proper points of the transformer winding. This method, however, is not convenient. A better plan is to obtain the required surface potential distribution by means of a system of concentric condensers.

The condenser type terminal, as it is commonly called, affords a conspicuous example of the successful application of the principle stated in Section II of this paper. As commercially made, this terminal consists of a series system of concentric cylindrical condensers of equal capacity, so arranged that the ends of the conducting cylinders form, at the outer surfaces of the terminal, a potential distribution corresponding to that of a uniform field, the equipotential

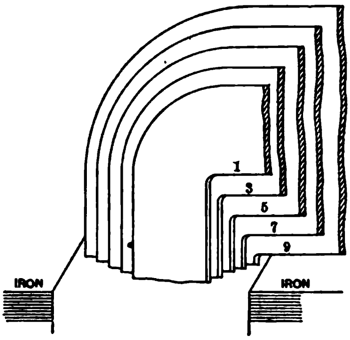


FIG. 16

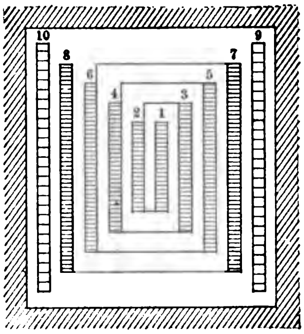


FIG. 17

surfaces of which are at right angles to the axis of these cylinders. To conform therefore to the principle stated in Section II of this paper, the space external to the terminal must be bounded by two infinite parallel conducting planes perpendicular to the axis, one at the tip and the other at the end of the conducting cylinder of largest diameter, and connected to the conducting cylinders at these points. In practise, this condition is approximated by placing a disk-shaped conducting body at the end of the middle cylinder, and making the surface of the cover through which the terminal passes as free from projections and ridges as possible.

A comparative test has been made on a terminal of this type, first with a small disk, and then with a large one, which illustrates in a remarkable way the importance of the proper application of the principle. The terminal when tested with a small disk, at a potential above ground of 300,000 volts, showed signs of corona, which extended to the rings at the edges of the tin foil cylinders at 350,000 volts. At 400,000 volts, the whole terminal was lit up with corona. It was not found advisable to raise the vo'tage above 420,000 volts for fear of rupturing the terminal. The small disk was then replaced by a large one five ft. (1.52 m.) in diameter and one ft. (0.304 m.) thick with semi-circular edges. With this disk the terminal showed no signs of corona up to 573,000

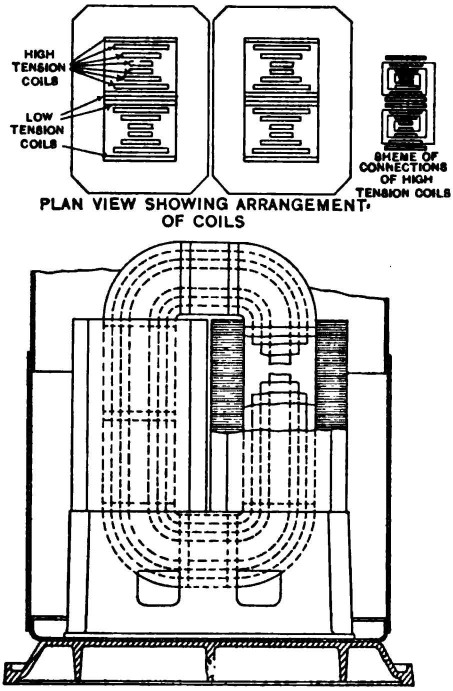


FIG. 18—SHELL-TYPE TRANSFORMER DESIGNED TO OPERATE WITH MIDDLE POINT GROUNDED

volts. These potentials are those obtained by ratio. By a needle spark gap, the values indicated were 35 per cent higher.

There are other ways of constructing a terminal, depending upon the same principle. For example, instead of using a system of concentric cylindrical condensers, a system of parallel disk condensers of equal capacity might be used, the diameter of the disks being such that the distribution of potential over the surface of the internal region of the terminal will correspond to a logarithmic potential, with proper relations between the inside conducting cylinder and the inside edge of the outer disk. The intensity within the terminal may be limited to any assigned value. A terminal designed in this manner would occupy more space than the condenser type terminal, but might have advantages where it is desirable to make the overall length as short as possible.

A transmission line insulator consists of a system of condensers in series. To obtain an equal division of potential difference between insulators, is not the only requirement of a successful design. This condition may actually be obtained, and yet the system of insulators will break down at a very much lower potential difference than the sum of the breakdown strengths of each insulator. The strength of the system may be increased by applying the principle stated in this paper. Accordingly, if the

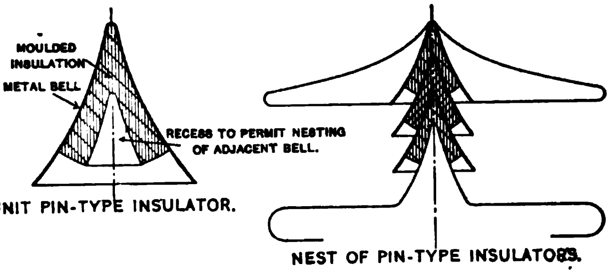


FIG. 19

units of which the insulator is composed are equally spaced, and have each the same capacity, the high- and low-potential conducting surfaces which are insulated from each other by the insulator should be so designed as to give as nearly as practicable a uniform field corresponding to the potential distribution at the outer surface of the insulator. For example, Fig. 19 illustrates an element of an insulator, which consists of a metal bell, so designed that it may be used as the electrode of the adjacent insulator. A dielectric compound is moulded into the above mentioned bell, so that it will have a recess for the adjacent bell to nest into. The surface of the dielectric, which is intercepted between two adjacent bells, is moulded so as to conform to the lines of force of the electric field between the two adjacent bells. A nest of these may be used as a high-voltage insulator, and each insulator will take up approximately an equal share of the difference of potential, but in order to obtain the highest efficiency from the insulator, an arrangement of parallel discoidal-shaped electrodes, like those shown in Fig. 19, must be used. The external field will then be substantially uniform, and agree with the division of potential between the individual insulators. The efficiency of the combination is thereby increased in exactly the same way that the efficiency of a condenser type terminal as an insulator was increased by the addition of the large disk, namely, because it completed the surface distribution of the region surrounding the terminal in accordance with the principle.

A unit insulator, made according to the above plan, conforms to the law of similar fields. Thus, if the dimensions are doubled,

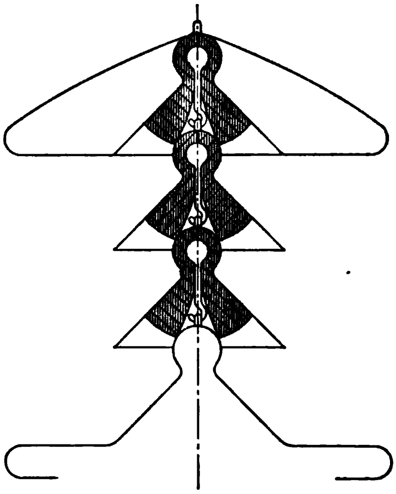


FIG. 20

the breakdown voltage is doubled. The cost of an insulator of this kind will vary approximately as the cube of the voltage. If, by some such method as that outlined in the last paragraph, the efficiency of a nest of insulators can be made to equal that of each individual unit, and if this can be done at a reasonable cost, then the cost of an insulator of this type will vary directly with the voltage for which it is designed. It is therefore obvious that an insulator, consisting of a nest of small insulators, would cost less than a large single insulator designed for the same voltage.

The principle may be applied to suspension type insulators, also, as shown in Fig. 20. Other types of insulators besides those shown may also be assisted by the application of this principle, but the advantage to be gained is not so evident as in the type shown.

VIII. Miscellaneous Applications

The well known principle of the guard ring is a particular case of the principle stated in Section II, and so also is the method of protecting instruments by means of grounded metallic coverings. The principle gives the method by which a given electric field may be maintained constant within an enclosed region, independent of the influence of external bodies. It may therefore be used to protect systems of charged bodies from the influence of other bodies. There seem to be many possible applications of the principle in high-voltage investigations.

IX. Conclusions

The principle stated in Section II of this paper has been used to insulate a high-voltage transformer. This transformer has been used to produce a potential above ground which the author believes is the highest yet obtained by electrical apparatus. The principle is capable of wide application, and seems to afford a solution of some of the most perplexing problems confronting electrical engineers.

DISCUSSION ON "AIR AS AN INSULATOR WHEN IN THE PRESENCE OF INSULATING BODIES OF HIGHER SPECIFIC INDUCTIVE CAPACITY" (FORTESCUE AND FARNSWORTH) AND "THE APPLICATION OF A THEOREM OF ELECTROSTATICS TO INSULATION PROBLEMS" (FORTESCUE), NEW YORK, MARCH 14, 1913.

Percy H. Thomas: There are two principal conclusions or principles in these papers which we should carefully distinguish. First, where solid dielectrics are combined with air in the insulation of terminals of any kind, advantage is gained if the solid insulator is shaped in a particular way; that is, if the outline of the solid material follows what may be called a line of electrostatic force extending from the positive to the negative terminal. Second is the intelligent control of the static potential at all points between the two terminals. This principle is illustrated by the so-called "condenser" transformer terminal. Instead of applying 100,000 volts between two terminals and allowing the potential to take its own natural distribution, Mr. Fortescue inserts a number of conductors at various points between the terminals, and then impresses on each particular intermediate conductor the potential he thinks it ought to have. The net result is a gain of three or four hundred per cent in the voltage which can be withstood by the same length of air gap. The advantage comes about in this way—we get a relatively high voltage resisted by a definite air gap, when the fall of potential is distributed equally through the whole length of the air gap. By devising some scheme which will make every portion of the air gap work to its maximum limit, we succeed in getting the greatest effectiveness that is possible out of the available separation.

The one critical thing which determines this distribution of potential along the air gap, in other words, which determines the electrostatic intensity of the electrical field through the gap, is the shape of the terminals. The electrical field has a uniform intensity along a line of force only where it is between terminals in the form of wide parallel planes. Wherever the terminal exposes an edge, or form of that sort, there will be a concentration of the lines of force, that is, a high intensity where these lines concentrate and a low intensity somewhere else. In this case the only part of the air gap that does its full duty is that at the point of greatest concentration. From this it can be seen why it is desirable to establish the potential at a number of intermediate points between two terminals separated by insulating material. Suppose we have two terminals a foot in diameter, separated by a distance of two feet. Evidently the field between will not be uniform, since these plates are relatively small in dimensions in proportion to the distance between them. If, however, we place two similar plates one inch apart, the stress between them will be substantially uniform at all points. Therefore, if we interpose between the two terminals separated by the space of two feet, a number of similar plates, each spaced an inch from its neighbor, and if we at the same time impress upon each intermediate plate a potential a little higher than the potential of the previous plate, so that the total potential is divided evenly between them, then will all the space between the original plates be stressed uniformly, and the maximum insulation strength possible will be obtained.

I would like to call attention to one point, since we have recently been discussing the relative effect upon insulation of stresses at ordinary frequencies and at high frequencies. The principles and methods set forth by the authors do not involve the frequency directly; that is, the ideal arrangement for one frequency will be the ideal arrangement for another frequency, at least so far as the theoretical principle involved is concerned. Considering, for a moment, the first principle as above defined, of using such a shape of solid dielectric as will give an outline which will follow the direction of the lines of electrostatic force, we must recognize two disturbing influences.

The first is the effect of external fields. If we design an insulator of this type on the theory that the line of electrostatic force will no longer be the one which we assumed, the surface chosen will no longer be a correct surface for giving the maximum efficiency. This is a matter which, in some applications of this principle, will be extremely important. Mr. Fortescue has given one excellent example in the condenser type terminal. The necessity of having a large plane on top of the terminal for the best effect, rests in its effect in eliminating the disturbance of the stress due to external fields.

The second disturbing point is the matter of current leakage over the surface. We have a chance for surface leakage of current from one terminal to the other. The resistance value of the leakage path taken by this current may vary from that representing the electrostatic distribution of potential. As a result, the fall of potential along the surface will not necessarily be the same as that produced by the electrostatic field. In this case, the result will be that a charge will accumulate on the surface of the insulation which will change the distribution of the static field to bring it into relation with the drop in potential of the leakage current; that is to say, it would not be safe to plan the insulation from a consideration merely of the electrostatic field, where a considerable surface leakage may be expected.

Since our electrical knowledge has grown up from two or three different and independent beginnings, for instance, from the electrostatic point of view and the electromagnetic point of view, there are usually two ways of looking at the same phenomenon. Mr. Fortescue has considered one method of looking at these electrostatic insulation problems, and a good many have used another method of looking at exactly the same thing, these two methods being, perhaps, mathematically equivalent. For the particular class of problems Mr. Fortescue discussed, his method is best. For some other applications the other method may be the best. Take the magnetic circuit: we used to look upon it as determined by the magnetic charges located on the surface of the magnetic material, or, in other words, as determined by the resultant effect of the free magnetism appearing at the various surfaces where magnetic flux enters iron or other material of high permeability. Later, however, we began to look at the magnetic circuit as analogous to the electric circuit, and to apply a law analogous to Ohm's law, namely, the law that the magnetic flux in a magnetic circuit is the magnetomotive force divided by the reluctance.

Suppose we have a rod and insulate it from the conductor at any potential, as in a bushing. We know that the metal *a* (Fig. 1) and the rod *b* are at a difference of potential, separated by the insulation *C*. This has long been known as an unsuitable form for resisting high-frequency voltage. One way of explaining this fact is to assume that the point *A* in the sketch represents a small condenser, one plate being the conductor inside the insulating tube and the other plate being the outer surface of the tube. It is well known that any condenser cannot have any potential

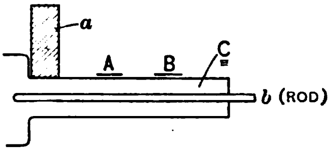


FIG. 1

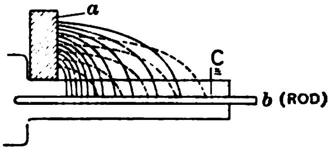


FIG. 2

impressed upon it until a charge is added to the condenser. The surface of the insulation at this point *A* will be the potential of the inner rod, until some charge appears on its surface, that is, until the full potential to be insulated has been impressed between the point *A* and the plate *a*. If, now, this potential is sufficient to break down the air to the point *A*, this point becomes charged to the full normal potential on the terminal and the voltage of *a* is brought to the point *A*. It is, therefore, impressed upon the remaining portion of the insulation *C*, and we may assume a similar jump to a second point, *B*, nearer the end, which will bring the potential of the plate *a* to the point *B*, and so on until a discharge is produced over the whole distance. This has been one customary way of explaining the known inherent weakness of this form of insulation.

In Mr. Fortescue's explanation you have it somewhat as follows: If there were no insulating bushing, the lines of force would be distributed as shown in dotted outline in Fig. 2. If the insulation is now introduced, the resistance of this material to the electrostatic lines of force becomes less, and the lines of force become crowded and more numerous near the plate *a*. This means a greater intensity of field at the point *A* as is shown by the lines of force represented by full lines, which, in the case previously assumed, starts the breakdown of the air at this point and a fresh concentrated distribution of the field of force will occur, leading to a total breakdown. These two explanations are different ways of expressing the same mathematical and physical situation and illustrate what I started to say, namely, that some of the phenomenon explained by Mr. Fortescue with great clearness have been previously understood and explained from a different point of view.

The idea of greatest interest, perhaps, and the one which will help us the most, is the second of Mr. Fortescue's principles, that of establishing arbitrarily the fall of potential through the insulation in the manner that we wish to use.

I wish to ask Mr. Fortescue if he will answer one or two questions in regard to Fig. 7, in the paper by the joint authors, as illustrating the effect on the distribution of the field of having a little ring *R* added to the solid dielectric between the two plates *M* and *N*. This produces a concentration of field and increases the intensity at certain points. I would like to ask him whether, as a net result, the total breakdown strength is less than it would be without that little ring. It is conceivable that locally the stress on the air might be greater, as it undoubtedly is, and yet, in view of the added distance that the spark must pass, that the net result would be an increase in the total resistance of the apparatus.

That leads to another question: If we have a distribution of surface of a solid dielectric between two terminals, which produces an excessive concentration of field at one point, does the local breakdown, in fact, have any more effect than removing so much of the total gap? That is, has a breakdown at one point a positive deteriorating effect upon other parts of the gap?

Another caution is needed. If we succeed in determining the form of our field and succeed in getting a form of insulation which will give us the very maximum possible strength from a given separation of terminals, and there is any disturbance in these ideal conditions, the results will be far more harmful than would have been the case with a less favorable design of the potential distribution. It is generally easier to upset a perfectly ideal condition than one where reliance is placed on thick material and wide spacing.

I infer, from what Mr. Fortescue says, that we must give up the idea of having a positive and a negative terminal in the same high-tension transformer when we go up to perhaps 100,000 volts.

Ralph D. Mershon: These papers are admirable. They are not only valuable and interesting—they are fascinating. I emphasize this, because a little later I shall criticize one of them, and I would not have you think I undervalue the paper, or the beautiful work it presents.

Referring to the joint paper, there are a number of things about it, aside from the general subject, that appeal to me very strongly. For instance, the scheme of Fig. 3, using a pantograph. I do not know whether that is original with the authors, but whoever devised this scheme has done an effective and ingenious thing. I am surprised and disappointed that they did not get results agreeing more closely with theory than those shown in Fig. 4. Possibly the discrepancies are due to the e.m.f. of decomposition of the salt solution.

On page 899, the question of "creepage" distance is brought up. Mr. Thomas has already referred to it and I hope the authors will bear rather forcibly upon it when they make their closure. It seems to me there is a possibility of the action Mr. Thomas suggests. When in the past a corrugated surface has been used, I think it was not primarily used for the purpose of insulation, in the sense that the authors of the paper have assumed. I think that usually the surfaces would have insulated properly, without grooves, so long as they were clean, and that the grooves were used in order to increase the length of path and, therefore, the resistance of any dirt that might get on to the insulator. The authors speak of the fact that they have tested insulators with dirty surfaces. I have made tests with insulators purposely sprinkled with dust, in the endeavor to simulate the conditions of practise, and have found the results not at all the same as those obtained with insulators that had gathered dirt in their own peculiar way.

Mr. Thomas referred to the matter of frequency, and stated that surfaces made in the way described by the authors would not be affected by frequency. It seems to me there may be some question about that, especially with the very high frequencies ascribed to lightning.

The effects of high frequency have been emphasized in my mind by some experiments I saw not long ago at Leland Stanford University. Prof. Ryan is now doing some work with sustained high-frequency current, using a Poulsen arc and getting voltages up to 35,000 and frequencies as high as 1,000,000. It is startling to see what the high frequencies do and the difference between their effects and those of ordinary frequencies, both as to distribution of voltage over an insulator, and the effect on the insulator itself. For instance, 35,000 volts, at a frequency of 250,000, will in a few seconds go through an inch of porcelain just as you might poke a skewer through a piece of cheese.

I want to second Mr. Thomas's request that the authors give us some idea as to the effect of surrounding objects. For instance, what will be the effect on transformer terminals if the transformer is in a small cell? Is the conductivity of the ordinary masonry wall sufficient to materially change the distribution? Suppose some time we wanted to install a transformer within a cell made of something that is a better conductor than masonry—what precautions must we take in the case of terminals built in this way?

Now, turning to the paper of Mr. Fortescue—as I said before, it presents a very beautiful piece of work. But it is a pity, I think, that a piece of work as valuable as this is not more clearly presented. It seems to me that this paper ought to be worked over very carefully, and so modified as to make it more readily intelligible to those who may have occasion to refer to it in the future. I sincerely hope that Mr. Fortescue will endeavor to work it over in that way.

The trouble with investigators and engineers is that they are usually so saturated with their subject that they cannot realize the fact that the man to whom they are trying to impart information is not similarly saturated. They have gradually come to the point of saturation themselves, and having arrived there are so thoroughly permeated with the subject that they cannot realize the fact that they got their information by slow processes. In explaining they omit many of the mental steps by which they arrived and which should be included, so that those who read what they produce may clearly understand. It is not enough to write engineering and scientific papers so that they can be understood; they should be so written that there can be no possibility of their being misunderstood. To my mind, the best example we have of that sort of writing is the works of Silvanus P. Thompson. I think he is a past master of instructional writing. I have never read anything of his that did not conform to the criterion just laid down, and I think we can all take him as an example.

It is not quite clear to me whether the treatment of the insulator at the end of Mr. Fortescue's paper is an overlapping of the treatment in the joint paper, or not, and I would be glad if Mr. Fortescue would make this a little clearer.

Comparing the two papers, the one that appeals to me most is the joint paper. Although the results outlined in Mr. Fortescue's paper are undoubtedly of very great value, and exceedingly interesting, the method of accomplishment, is, so to speak, more brutal than that of the other. The method of Mr. Fortescue's paper is like nailing a thing down with a railroad spike, whereas that of the other is like tying it up with a silk thread. I think that is the reason why this joint paper is so much more fascinating to me than the other, although the results of the other paper are, no doubt, just as valuable, perhaps in some ways more so.

C. O. Mailloux: I may not be able to contribute very usefully to the discussion of these papers, having seen them for the first time only as I came in the meeting. Nevertheless, I feel prompted to say something in justice to the authors, because the impressions produced upon my mind by these papers are quite different from those seemingly produced upon the preceding speaker. As I listened to the presentation of the abstracts of these papers, my interest in and admiration for them grew rapidly; and I thought it only right to let the authors know it.

I have a high opinion of these papers. I think the Electrophysics Committee is to be congratulated upon having presented them. I regard them as possessing the highest interest, both theoretically and practically. Indeed, I am tempted to characterize them as being, in a sense, epoch-making. It seems to me that they afford another interesting example of the fact that great practical results may be reached through highly theoretical and even somewhat abtruse considerations.

We have all had occasion to realize that pure theory may, and occasionally does, prove a clear-sighted prophet and a most helpful guide by whose assistance results of the highest practical value are attained in a relatively simple manner, sometimes after all previous efforts to secure them by means more direct and adequate have failed completely. The paper of Mr. Fortescue seems to me to be one of that rare and valuable kind. As I listened to the abstract, I remembered that five years ago, in a discussion of engineering education, before this Institute, I called attention to the importance and desirability of an engineer being trained so as to be able, on occasion, to attack and to solve problems by entirely theoretical methods. I cited, as examples, two cases where a triumph in electrical science and engineering was achieved as the result of the solution of a differential equation. It is worth while to recall them here as edifying deeds. The first of these triumphs was achieved more than fifty years ago by Kelvin, who, by finding " particular solutions " of a differential equation, which has since become known to us as the " telegraphers' equation," rendered submarine telegraphy possible, and in fact was able to foresee, forecast and specify all the essential facts, features and difficulties involved in submarine telegraphy. The second triumph was achieved a half-century later by Dr. M. I. Pupin, who, by finding the general solution of the same equation, made clear for the first time new physical features and characteristics, and modernized long-distance telephony, by rendering very easy that which had seemingly been impossible before. In connection with this very subject, I might add that the excellent work of Dr. Kennelly in the study of the characteristics of long transmission circuits—constituting interesting extensions and applications of the same theoretical principles—furnishes another example of the way in which theory may blaze out a path which proves to be the shortest cut to the best practical methods.

Mr. Fortescue's paper has, in my opinion, the rare merit of being at the same time intensely theoretical and intensely practical. In this paper, theoretical considerations are used very intelligently to assemble and co-ordinate certain facts, to develop conclusions from them, and to point out the way to very interesting and far-reaching practical applications.

The preceding speaker finds the paper lacking in clearness. The abstract of the paper given by the author seemed to me to be lucid enough, especially after reading and noting the statement of principle of electrostatic theory, given in the paper. It so happens that the principle here referred to could be presented and made clear in many different ways; and this paper is of a kind whose clearness may depend as much on the habits of mind and thought of the person who reads it, as of the one who wrote it. I say this in all seriousness, and without wishing in the least to disparage the type of mind or the habits of thought or study of any person whatever. Some persons, it is known, can see theoretical principles very clearly from a geometrical or pictorial point of view; others see them best from a mathematical or analytical point of view; still others see them best, or perhaps only, through a physical description; and, lastly, there are those who can see them from all points of view. Personally, I think it would have been a good idea to have put in an appendix to this paper, a resume of the mathematical theory referred to on page 909. Had this been done, those of you who have studied the Theory of the Potential Function would doubtless have recognized some other old acquaintances besides "Green's theorem". The sight of an equation may suffice, often, to put one on speaking terms again with an idea or a principle that one already knew either analytically or geometrically or physically, or in all three ways. In the case under discussion the ideas and principles are much better known than might be supposed at first. They are as simple as they are beautiful. The author should be congratulated for having placed before us something which has been in our reach for a long time, and which, though as simple as the egg-trick of Columbus, yet has been unnoticed and passed by, by most of us. It is not at all difficult to understand the theory which is involved. You are all really quite familiar with it already, I am sure. We have had to do, in this case, with equipotential *surfaces*, which in a space of three dimensions become equipotential *envelopes*. A toy balloon of thin rubber, inflated with gas, gives an ocular demonstration of a " surface " constituting an "envelope " which may be, in a sense, called "equipotential ". In that case we have, on both sides of the envelope or shell, an equilibrium of *pneumatic* " pressures," whereas, in the case of an equipotential electric envelope, we have equilibrium of electric " pressures " or potentials on the two sides of the envelope. In electrophysics, we have equipotential " envelopes " or " shells " of two principal kinds, namely one in which the potentials are electrical forces or pressures, which are familiar to us by the indications or effects which they can produce on a voltmeter, and also another kind in which the forces are magnetic. In both cases, the surface passing through all points of the same potential is an equipotential surface, and this equipotential surface becomes an equipotential envelope in the case of an isolated electrified or magnetized body. The iron sphere used by Lord Kelvin as the "envelope" of his " ship-galvanometer " to make it independent of the magnetization of the earth or of the ship was, in a sense, an equipotential magnetic shell. It also answered the purpose of a " barrier ", to borrow a term from the theory of the potential function. Mr. Fortescue has done something analogous. Starting from and guided by considerations which usually would interest only the mathematical physicist, he has evolved from intensely theoretical ideas an intensely practical conclusion and method. I think that the analogy of the toy balloon which I have just referred to will enable us to get a pretty good idea of what he has sought to do theoretically and what he has accomplished practically. Let us suppose that while the little balloon is being inflated, it shows a tendency to bulge and to form excrescences at one or more places. It would still be an equipotential surface, but there might be more strain on the thinner and weaker parts which are bulged out excessively. If now we find some way of applying a counter-potential or a balancing potential, it will reduce the bulge and make the balloon as a whole spherical once more. We would thereby reduce the excessive strain produced at the weaker points. This would be accomplished, for instance, if the toy balloon were enclosed in a hollow metal sphere or spherical shell, because the bulging parts coming in contact with the inner surface of the shell, would be prevented from further bulging and straining. In analogous manner, Mr. Fortescue seeks to apply balancing or compensating potentials to different points of an electromagnetic winding, so as to keep the equipotential envelope from bulging too much or to keep it within bounds, so to speak. This is undoubtedly a very interesting and captivating feature of his paper. I consider it a theoretical treat as well as a practical feat.

F. W. Peek, Jr: These papers bring out in a very interesting way the dielectric circuit. How in design we must not think of voltage and insulation thickness alone, but of dielectric flux.

When energy was first transmitted electrically low voltages and large currents were used. Therefore, it was not necessary to bother much with a study of insulation, but the magnetic field was of necessity investigated and studied. We would not think of building apparatus in which the magnetic circuit was badly out of balance or in which the lines were very much crowded in one place. Yet balance of the dielectric circuit is of much greater importance than balance of the magnetic circuit.

In these days of high-voltage engineering the dielectric circuit must be considered in the same way the magnetic circuit is considered—and not as "charges" or as something connected with rubbing glass rods on silk, pith balls, etc., as, unfortunately, it is still taught in some of our colleges. Naturally in all intelligent high-voltage designs the dielectric circuit is considered.

The dielectric circuits and magnetic circuits are in many ways analogous. Thus to establish the magnetic field there is the magnetomotive force, to establish the dielectric field the electromotive force; corresponding to the magnetizing force or magnetomotive force per centimeter of circuit there is the electrifying force or gradient or volts per centimeter of dielectric circuit; magnetic flux density; dielectric flux density, etc. Insulation ruptures when the dielectric flux density exceeds a given value, or the force producing the flux exceeds a given value. The flux density at a given point is proportional to the gradient at that point, that is, $\frac{de}{dx}$. The gradient may thus be considered as a force or stress on the insulation, and the flux density the resulting electrical strain or displacement. This is analogous to Hooke's law of mechanically elastic bodies. When the force or gradient exceeds the elastic limit, rupture occurs. The permittivity or specific capacity of a dielectric is a measure of its " electrical elasticity ". The permittivity for air is (1) and for glass (5). Thus with the same gradient or electrical force the displacement or flux density is five times in glass what it is in air. But it takes about twice the force or gradient to rupture glass, therefor at rupture glass is displaced ten times as much as air. Thus air is electrically stiff and glass is electrically elastic. In most forms of insulators, leads, etc., air is unfortunately necessarily in combination with a solid dielectric. When glass or some other solid dielectric is placed in series with air the solid, for the same displacement or flux density, requires a lesser gradient or stress; most of the stress is thus put on the weaker air, which ruptures.

Naturally in design, then, it is our general object to so proportion the insulation that the stress or gradient is proportional to its strength; that is, for a given insulation, to make the flux density uniform, and for a combination of insulations, to so arrange them that in the combination the one does not weaken the other but that the stronger actually relieves the weaker, or in other words so that the flux density is greater in the one of greater permittivity and strength. This must apply to the internal strains as well as the so-called leakage strains.

The authors have shown that by making the solid insulation tangential to the lines of force the field is not distorted and the stress on the air is not increased by the introduction of the solid. This is a step in the right direction, but it however does not necessarily give the best form for the solid, for although the addition of the solid does not put extra stress upon the air, it also does not tend to relieve the air at the point of greatest flux density.

For internal strains the arrangement of conductors should be considered and is, as for instance, in a transformer by arrangement of conductors and coils, where extra metal for this purpose is not generally necessary. For instance, if two conductors of a given diameter are taken and placed a given distance apart the insulation may break down at say 85,000 volts; if these conductors are now split up into six conductors an arrangement may be made which will require 100,000 volts to rupture.

It is proper and necessary to consider all such features in design, but as in all engineering work there must be a compromise; apparatus must be designed not only to meet normal conditions, but also to meet abnormal conditions. We have, so far, only considered high voltage at low frequency.

Suppose, now, coils, insulators, etc. are designed so that the stress is everywhere evenly distributed. To do this the insulation must be thinned out at one point and thickened at another point. Now in case of high frequency the whole voltage may be localized at the point where the insulator was decreased in thickness. Hence ideal insulation for normal frequency may in no way be practical but break down immediately with the first surge or lightning stroke. This means that the design must sometimes be modified from the " ideal "—not always or even generally, but it is a point which must always be considered. It can be readily seen how this localization of potential can happen, especially in certain types of apparatus as at the point of thin insulation the potential may pile up due to the reactance and capacity combination at this point. It is of utmost importance in design to consider this and modify the " ideal " design accordingly when necessary, and undoubtedly this has been considered by the authors of these papers.

Then in the design of leads, bushings, etc., the details must often be modified to take care of weather conditions. In transformers in order to get an "ideal" design the connections may be too complicated. In leads or insulators consisting of a large number of metal parts care must be taken that the " multigap effect " does not exist as in the well known lightning arrester of that name.

The condenser lead is ideal in many respects, especially at normal condition; though the practical design is possible, and a fact, it is not as simple as would appear. For instance, if the capacities of the cylinders are made equal, true balance is not approximated, as each cylinder has a different capacity to ground and to the center terminal or tube. These to ground capacities and to lead capacities tend to neutralize each other, but can not always do this and may cause concentration of flux or potential at any point.

Regarding insulators built as in Fig. 20 in the paper on "Insulation", if a single unit gives balance or equal strain the combination of equal units as shown cannot possibly do this, but potential will be greater on some units than on others. To get equal balance not only must the flux be uniform, but the total flux must be the same on each unit. As can be seen for the combination, each unit has a different capacity to ground and to line. This means that the total flux is not the same on all units, and therefore, potential is not equally divided. This does not condemn the design, however, but such a combination may be worked out with different sizes of units as it also can be in the commercial insulator of today. In a design of this sort the high-frequency multigap effect must also be guarded against. That balance can not exist has previously been shown for commercial insulators.*

I wish to say again that these papers are of exceeding interest

***Electrical Characteristics of the Suspension Insulator*, TRANSACTIONS A.I.E.E., Vol. XXXI 1912, p. 907.

and importance because they bring forcibly before engineers the fact that a consideration of the dielectric circuit has a greater and more practical sphere than that of the pure physicist and mathematician, and that it must be considered as the magnetic circuit has for years been considered by practical engineers.

A. E. Kennelly: The papers before us are not only important from a practical standpoint, but they also indicate defects in our working theory of electrostatics.

We are accustomed to say that air breaks down between parallel plates when the intensity reaches 3000 volts per mm., or 30,000 volts per cm., and we are in the habit of thinking of it in gradient of potential. The reason, perhaps, that we select that method of expression, is that we have a unit that is easily expressed;—*i.e.*,volts, and the inch or centimeter, so that we have a unit easy to apply. But it is equally correct to say that air breaks down when the static flux-density exceeds a certain critical value. That is a very important generalization. The reason we do not put it that way is, perhaps, because we have no name for the unit of electrostatic flux density. We have a unit for the magnetic flux density—the gauss. Personally I call the unit of electric flux density the stat-gauss, which is, of course, objectionable slang, but it has, at least, the merit that it is self-explanatory. It would seem very desirable to name the static unit of flux the " Kelvin ", to correspond to the maxwell. It would seem very proper that Lord Kelvin's great name should be attached to the science of electrostatics in which he did such magnificent work. If we assume that the gradient of impressed potential in air, which produces breakdown, is 30,000 volts per cm., we realize that this corresponds exactly to 100 stat-volts per cm., since the static volt is just 300 volts. One hundred stat-volts per cm. would produce in air a flux density of 100 kelvins per sq. cm., that is to say, the critical flux density producing breakdown in air is, by the foregoing reasoning, 100 kelvins per sq. cm.

These papers may be summed up by saying that where you have two insulators associated together, their contour must be so selected that at no place does the electrostatic flux leave the air to go into the other medium of greater specific inductive capacity, because if it does, there is danger of producing an undue flux density, too many kelvins per sq. cm., in the air, tending to overstress it and break it down locally.

Philip Torchio: I am very much interested in these two papers, and I would like to point out that these theorems and their application are not confined only to high voltages, in the order of 100,000 volts, but also apply to very moderate generator voltages of 4000 volts up to 20,000 volts. I want to show several effects that we obtained in making a voltage test on dielectrics in series with air as applied between a cable insulated with 10/32 in. (7.9 mm.) of rubber, one in. (25.4 mm.) of air space and a needle point mounted on a ground plate, this plate and the conductor being respectively connected to the terminals of the transformer, as in Fig. 3.

When we applied 5400 volts at 25 cycles, Fig. 4, the needle point became luminous with corona. At 13,000 volts, Fig. 5, the braid also became luminous at the point opposite the needle. At

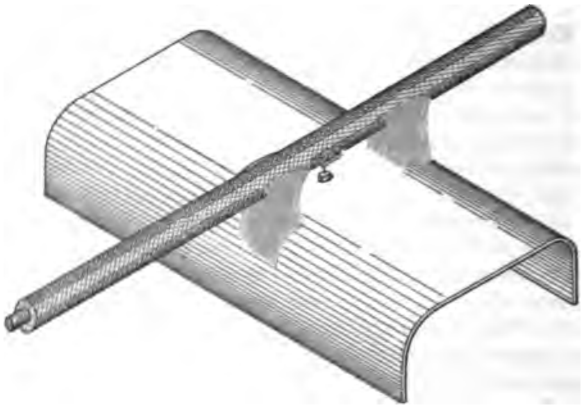


FIG. 3—GENERAL ARRANGEMENT
One inch between cable braid and needle point mounted on ground plate. Transformer connections omitted.

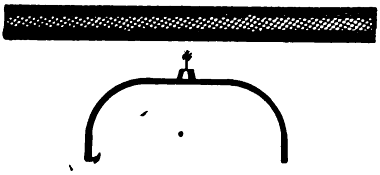


FIG. 4—5400 VOLTS
Corona at needle point.

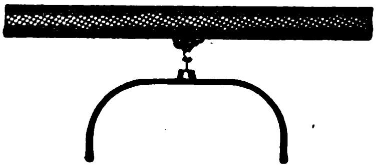


FIG. 5—13,300 VOLTS
(27,700 volts, streamers, needle point to braid.)

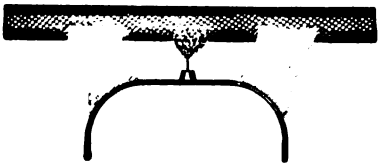


FIG. 6—34,200 VOLTS
Corona, bands from curved surface of braided cable, separate from, and apparently repelled from needle point streamers.

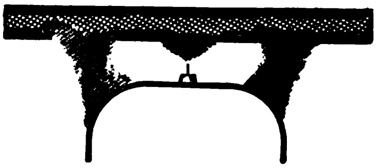


FIG. 7—42,100 VOLTS
Very vigorous streamers from needle point. Occasional discharge streamers through border corona.

27,700 volts the two luminous zones grew together, and discharge streamers took place between the needle point and the braid. At 34,000 volts, Fig. 6, a curious phenomenon occurred; at the borders of the round edges of the plate, at about two in. (50.4 mm.) separation between plate and braid, two separate coronas formed, having a rounded shape, which seemed to be repelled outward by the field of the streamers at the needle point. At 42,000 volts the streamers between needle points and braid, Fig. 7, were very violent and also occasional streamers took place between border coronas. In this case the cable withstood for about five minutes 42,000 volts under those conditions without failure and then the test was stopped. In other cases we had failures.

I have also an interesting set of photographs, giving a photographic record of this corona effect at very moderate voltages. These photographs will be found accompanying my discussion of Mr. Lamme's paper of last January. The explanations then given, indicate the importance of overcoming the dangers of corona for voltages of only a few thousand volts when air is in series with dielectrics of higher specific inductive capacity.

J. Murray Weed: The authors of these papers are to be commended for their efforts to deal in a scientific manner with subjects in which the development of design practise has been so largely the result of experimental and " cut and try " methods. Scientific treatment is to be sought for in dealing with all engineering problems. Some of these problems are so complicated, however, and the general problem of insulation is emphatically one of them, that it is necessary to urge the utmost caution in interpreting the dictates of theory. This caution needs to be exercised, not only in connection with the more complicated problems of insulation, but the danger also exists of making errors in the interpretation of the fundamental laws which affect the results in the most simple cases, such as the parallel wires, and the terminal rod and concentric ring, dealt with in the paper on "Air as an Insulator ".

I cannot agree with the authors in the statement that seems to receive most emphasis as a fundamental proposition in this paper, *i.e.*,— that the maximum efficiency of air path over the surface of a solid insulator is always obtained when that surface conforms to the flux lines as they would exist if the insulator were not present. Maximum voltage for a given length of surface path will be obtained in this manner in a uniform field,(between parallel planes), but not in the field of the parallel wires, or of the rod and torus.

The cylindrical surface recommended for the parallel wires, as represented by the bottom line of the cross-hatched portion of Fig. 9 of the paper, and the surface generated by the revolution of arcs of circles for the rod and torus, as shown in Figs. 10 and 11 of the paper, may be made more efficient by flattening out the middle portions of these arcs, and using smaller radius of curvature at the ends.

Confining our attention now to the rod and torus, the explanation of this is as follows: Referring to Fig. 8, a portion of the ideal insulator recommended by the authors has been removed, giving a more nearly conical shape. That zone of the field between the surfaces *a* and *b*, the middle portion of which was occupied by that part of the insulator which has been removed, would, if the insulator were not present, be most intense at and near the surfaces of the rod and ring. The substitution of the solid dielectric, of uniform specific capacity, throughout this zone, with surfaces conforming to the original flux lines, will not alter the distribution of the stress, although the density of the flux will be multiplied by the specific capacity of the material. As stated in the paper, the strength of the air path will remain unchanged. It will break down, however, at a much lower voltage than that corresponding to the rupturing stress in the middle portion of this field, the rupture beginning at the surfaces of the rod and ring, and extending outward as more of the total stress is brought upon the intermediate portions of the field.

If, instead of filling the entire zone between *a* and *b* with the solid dielectric, we fill only those portions adjacent to the rod and ring, where the stresses were greatest, the solid dielectric being in series in the flux path with the air further away from the terminals, where the stresses were much less, the flux density in the zone will be increased in proportion to the reduction in the total reluctance of the path. The stress in that portion of the zone still occupied by air will therefore be increased, but its maximum value may still be much lower than was the stress at the surfaces of the terminals before the solid dielectric was inserted. Since the total potential difference between the terminals does not change, the stress near the surfaces of the terminals has been reduced by the insertion of the solid dielectric, by an amount corresponding to its increase in the air in series with it. But these statements give only a first approximation to the conditions which actually exist.

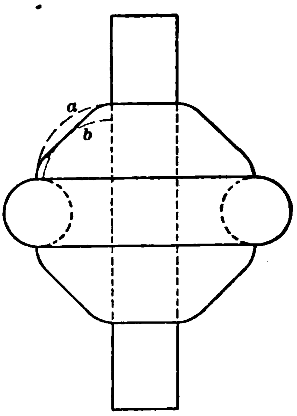


FIG. 8

This solid dielectric, in addition to being in series with air in its flux path, is in parallel with other air at the surfaces of the rod and ring in the adjacent flux paths. The reduced stress in the solid dielectric causes it to absorb some of the flux from these adjacent air paths, thus tending to equalize the stresses in these paths at the places where these stresses were maximum.

The above explanation shows how a modification of the ideal shape recommended in the paper, making it more nearly conical, by the actual removal of some of the material, and actually shortening the length of the air path, will reduce the maximum stress in the air, and increase the voltage required to break down the air path. The authors have shown merely how to place solid dielectric in the field without reducing the strength of the air path over its surface, whereas it is possible, by a modification of the surface which they have recommended, to make a large increase in the breakdown voltage, though the length of the path over the surface is reduced.

For the purpose of proving the above deductions, the following tests were made: Insulators were turned from hard rubber, with dimensions as shown in Figs. 9 and 10. Voltages were measured by voltmeter and ratio of transformation, with a very close approximation to sine wave impressed. Three consecutive tests on the insulator in Fig. 9 gave 52.5, 53, and 52.5 kv. respectively over the surface of 3.5 cm., which is an average voltage of 15.1 kv. per cm. In like manner, three consecutive tests for the insulator of Fig. 10 gave 62.5, 62.5 and 62 kv. over the surface of 3.29 cm. This

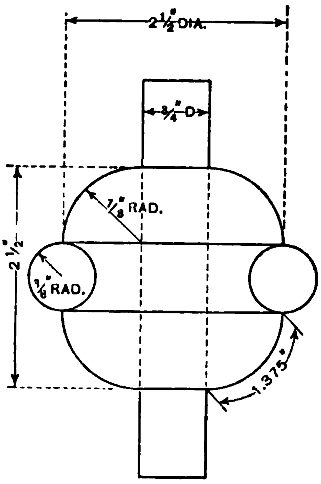


FIG. 9

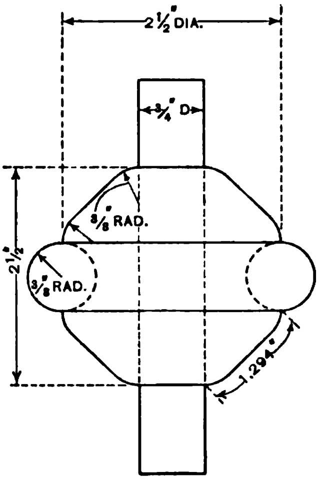


FIG. 10

average voltage per cm. was 19 kv. or about 25 per cent higher than the voltage per cm. for the insulator of Fig. 9.

Tests were also made on the insulators of Figs. 11 and 12, of same over-all dimensions as those of Figs. 9 and 10, the length of air path of Fig. 11 being 3.63 cm. and that for Fig. 12 being 4.25 cm. The average total breakdown voltages for both of these insulators was the same as for Fig. 9.

Some explanation is due for the extraordinarily uniform and high results obtained in these tests. The fit obtained between the ring and the insulator was not good, and in a preliminary set of tests breakdown occurred at about half the voltages recorded above, and was preceded by the formation of corona at the joint between the ring and insulator. It was intended to fill the crevice at this joint with paraffin, and repeat the tests, but in the meantime some tests were made of the effect of a copper shield on one end of the rod, by redistribution of the flux. For this test the other end of the rod and insulator, to a point midway on the ring, was immersed in oil. Accidentally, the entire insulator was dipped, thus filling all the crevices with oil, which was held in the crevices by surface tension after one end of the insulator was brought above the oil again. The test gave a result like those recorded above, which was at first attributed to the shield, but a repetition of the test after the removal of the shield gave practically the same results. All of the insulators were now tested in this manner, being completely immersed in oil before the test, and then lifted so as to bring one end of the insulator and part of the surrounding ring above the surface. The oil made perfect joints between the ring and rod and the insulator, eliminating all corona, and giving uniform and consistent results.

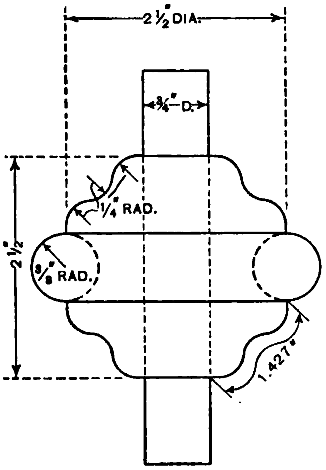


FIG. 11

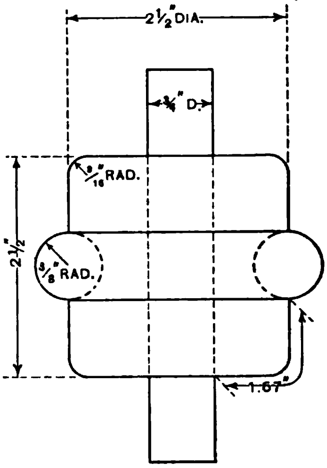


FIG. 12

No improvement was obtained with these insulators by the use of shields, or disks, for re-distributing the flux at the surfaces of the rod and ring, although several shapes were tried. On the other hand, some of the shields caused the breakdown to occur at considerably lower voltages.

In these tests, it was noticed that the oil clinging to the surface would, during the test, draw up about the rod, on top of the insulator, into a slightly elevated cone. In my opinion, this indicates a more nearly ideal form for the surface of the insulator. If we had a viscous fluid insulator, free from the effect of gravity, which could shape itself under the influence of the electrostatic field, we should see the perfectly ideal form, giving the most efficient surface for any given terminals. It is probably impractical to determine this shape analytically, even for the simpler cases, but I would define it in a general way by saying that it will be obtained from the insulator conforming to the flux lines in air, recommended in the paper, by filling more of that portion of the field where the stress is greatest with the solid dielectric, and less of that portion where it is least, in such a way as to obtain as nearly as possible a uniform potential gradient over the entire surface. This will require an artist, with a good imagination, and the results of his work must be tested in comparison with the results of other similar efforts.

No one can be more in favor than I am of using analytical and scientific methods, in so far as they are applicable, in the design of insulation or insulators. It is my opinion, however, that this branch of engineering must continue to be an art as well as a science. Analytical methods cannot be applied directly in any but the most simple of practical cases, as that of grading insulation for cables. They may be used, only cautiously, as a general guide, in more complicated cases.

The fundamental error that has been pointed out above began to grow in this paper in the consideration of Figs. 2 and 5 of the paper. It was pointed out in connection with these figures that higher flux densities were produced at the surface of the ball than those which existed before the ball was introduced. It was not noted, however, that over a portion of this surface the flux density, and stress, are much reduced. It should have been surmised that, if it is possible to introduce solid dielectric into a uniform field in such a way as to increase the stress at some points on the surface, and to reduce it at others, it may be possible to do the same thing in a non-uniform field, making the reduction in stress in that part of the field where the stress was maximum, and the increase in that part where it was minimum.

In their argument on page 897, the authors are evidently thinking of a uniform field, and some particular kind of surface, as their reasoning here is not at all general.

There are some other points in this paper which deserve further consideration. For instance, in some tests which I have made, the projection *R*, at the middle of the spacing strip *H L*, in Fig. 7, has increased the breakdown voltage between the plates about 10 per cent above that obtained with straight strips (no projections). The corresponding increase in total length of surface path was 66 per cent. While the efficiency, if measured in average voltage per unit length of the path, following the contour of the surface, is considerably reduced, the voltage which can be obtained with a given distance between plates is increased.

The inference in the paper that surfaces conforming to the various flux lines of Figs. 9 to 11 (of the paper) will have equal efficiencies, will evidently not hold. The voltage gradient will be most nearly uniform, for instance, along the straight line connecting the centers of the wires in Fig. 9. The uniformity along any of the other lines will be less and less, as we pass further from this straight line, that is, with a given average stress, the maximum voltage gradient at the surfaces of the wires is increased. The average voltage per unit length of path which will start breakdown of the air over the surface of the insulator will, therefore, be less and less, the further the surface is from the straight line or plane. On the other hand, the total volts required to produce breakdown will be greater.

The results obtained from tests of the insulators shown in Figs. 15 and 22, as reported in the paper, while striking, are easy to understand. The great length of tube at the top end of the insulator, in Fig. 15, adds a relatively small amount to the strength of this bushing, since as soon as there is over-stress over the large part of the bushing, from the tank cover to the lower end of this tube, a discharge will take place to this point, and will then travel progressively along the tube with relatively small increase in voltage.

In the case of the insulator shown in Fig. 22, the density of the dielectric field is increased at the surface of the torus by the increase in the length of the insulator, and also by the metallic shield above the insulator, since both of these things produce an increase in the flux leaving the rod, which must terminate on the ring, without any reduction in the reluctance to the flux at the end of its path adjacent to the ring. Correct design must make corresponding reductions in the reluctance at both terminals, and avoid over-stressing the air at either end, or at any point, of the flux path.

Considerations such as those given above, lead to a better understanding of the limitations of the condenser type bushing, as it is built. The layers of dielectric are so thin that, if the surfaces at the ends of these layers be made to conform to the shapes which would give the most efficient surface, whatever that shape would be, the length of the leakage path is altogether too short to stand the voltages for which they are designed. This makes it necessary to extend each of those layers a considerable distance beyond the end of the metallic layer at its outer surface, but it is not extended at the end of the flux path where the stress is maximum, *i.e.*—over the edge of the outer metallic cylinder. The increase in surface is, therefore, altogether out of proportion to the resulting increase in the total strength of the air path.

In fact, the greater these extensions, beyond a certain limited amount, the worse will be the final result for the lead. This will be understood when it is considered that the capacity between the intermediate layers of these leads is greater than that between the innermost layers, and that the extension of the varions layers presents additional surface for electrostatic flux to the tank and surrounding objects. The flux connecting the intermediate and outer layers to the surrounding objects, added to that passing through the outer layers themselves, must be supplied through the smaller capacity of the inner layers. This produces higher voltages across the inner layers. This results in the production of corona at the edges of these layers, which is cured by the use of the very large shield at the top of the lead, as reported by the authors of the paper.

This improvement is due to the fact that the shield supplies the flux to the tank and surroundings, which otherwise originates from the surface of the lead. If, without the shield, the top of the lead (innermost layers) were not overstressed before the intermediate and outermost layers, the shield would afford no such benefit. If the top part of the lead were understressed as compared with the bottom portion, the shield at the top would be a positive detriment.

From some tests which I have made on a condenser type lead, I can say that, though the voltage distribution across the various layers of this lead were ideal in normal operation, it may be far different when subjected to a high-frequency disturbance. In these tests, high frequencies, ranging from 10,000 to 50,000 cycles, were impressed upon the lead. With about 100,000 volts across the entire lead, as measured by a sphere spark gap, the voltage across the outermost layer, measured in the same manner, was 40,000 volts. The average voltage per layer across the eight outer layers was over 14,000 volts. The average voltage over the intermediate and inner layers was about 5000 volts per layer, becoming rather higher for the inner layers. The arithmetical sum of the voltages across all of the layers was more than twice the voltage measured across the entire lead.

Referring now to the paper on " A Theorem of Electrostatics ", I would say that the applications made of the theorem referred to are very interesting. No objection can be made to the forced distribution of potential illustrated in Figs. 6 to 18, except that equivalent results may be more economically obtained in other ways.

The assumption that a uniform field between parallel disks may be made to articulate with a field radiating from a cylinder, in the manner described in the paper, with reference to the condenser type lead, except by the aid of conductors connected to an external source of potential, is erroneous. This will be understood if we stop to consider that under these circumstances we would have two fluxes converging upon the conical surface of division between these two fields, *i.e.*, that of the uniform field coming downward from the upper disk, and that of the logarithmic field radiating from the lead. It is impossible for these fluxes to terminate upon this surface except by the intervention of conduction supplying current from outside sources of the proper potentials. These are, of course, actually present, in the form of the metallic layers of the condenser lead, but the fact that they are able to supply this extra current and still have the correct potential distribution, proves that this potential distribution would not be right without the shield or disk at the top, which calls for the extra current. This merely confirms the explanation given in my discussion of the other paper, that the voltages across the inner layers of the lead are too great without the disk.

In conclusion, I wish to say that I thoroughly appreciate the serious efforts which have been made in these papers to place the design of insulators and insulation upon a rational basis, and I do not wish to discount their value. By means of investigations of this kind, combined with careful experimental work, progress will be made in the ability to design apparatus successfully and economically for high voltages. Careful study will show us, however, that the subject is by no means so simple as it has appeared in the presentation of these papers.

Harris J. Ryan (by letter): In their paper on " Air as an Insulator when in the Presence of Insulating Bodies of Higher Specific Inductive Capacity ", Messrs. Fortescue and Farnsworth have isolated and well exemplified the principle that determines the proper proportions of solid high-voltage insulations so that the adjacent atmosphere will not be overstressed. This principle is applied by means of a two-fold expedient as follows:

1. Beginning with air as the insulating medium the high-voltage electrodes are so shaped that the tubes of established electric force spread a minimum. Thus the electric field is made to approach uniformity as much as practicable, and the maximum stress in the air is kept everywhere at a minimum.

2. Between the electrodes a solid dielectric is fitted and formed precisely as the tubes of electric force. Such solid dielectric is employed for a double purpose: (a) for mechanical support, and (b) to eliminate the air in the most intense portion of the electric field wherein, if it remained, it would be overstressed.

The electric field in the solid dielectric is greater than in the air displaced, by its specific inductive capacity as a multiplying factor. The voltage stresses have, however, nowhere been altered, so that the electric field, through the air surrounding the solid dielectric, remains unchanged and the stresses in it are everywhere the same with as without the presence of the solid dielectric. This is a simple expedient and the results are correspondingly certain.

We now know that a number of available solid dielectrics possess an enormous electric strength, (1,000,000 volts per cm.). Only a small part of this strength can ordinarily be utilized in practise on account of the necessary presence of air, moisture, oil and other fluids capable at comparatively low electric stresses of functioning as electrolytes, *i.e.*, as *ion carriers*. Such electrolytic or ionic conduction rapidly destroys the solid dielectric through heat and molecular erosion and not by electric overstress.

We are much indebted to the authors, who have uncovered in simplest terms the principle that controls the minimum stressing of the troublesome fluids (gaseous or liquid) in which all solid high-voltage insulators must be immersed to do their work.

Fourteen years ago, with the aid of Mr. E. F. Scattergood, use was made of the *sectored guard surface* to construct at Cornell a 90,000-volt " dry-insulated ", 10-kw., 125-cycle transformer. Up to a year ago this transformer was in regular service for high-tension testing purposes in the laboratory where it was built, and it may be in use at the present time. Compared with Mr. Fortescue's thorough and effective designs for insulating very high voltage transformers our 1899 effort is of course a very crude one. However, our experience taught us to understand that one can depend upon the sectored guard surface to reduce all intense electric stresses developed in very high voltage transformers to safe working values. Other work at the time demonstrated to me the great value of the guard surface as a general expedient in high-voltage insulation. I heartily concur in the author's conclusion that " the principle is capable of wide application, and seems to afford a solution of some of the most perplexing problems confronting electrical engineers ".

The results of Mr. Fortescue's own work as given in this paper abundantly demonstrate the correctness of such conclusion.

C. Fortescue: A number of important questions have been brought up in this discussion. Some of these questions indicate that further investigation along certain lines, for instance on the effect of high-frequency electric stresses on commercial dielectrics, would be of great value. Not having made any tests myself on the effect of very high frequencies on such materials as are commonly used for insulating purposes, what I say in this respect must be largely based on theoretical grounds.

Replying, then, to Mr. Thomas's first question as to the effect of frequency—if, as is assumed in the joint paper, the dielectric is homogeneous and non-conducting, the stress at any point is derivable from a potential which is a solution of Laplace's equation, provided that the applied potentials are constant. When the applied potentials are periodic, the potential is no longer a solution of Laplace's equation and the line integral of electric force taken round a closed circuit in the electric field will not be zero as in the case of a steady stress, but will be proportional to the rate of charge of the line integral of the vector potential taken round the same circuit. This relation and a similar one associating the line integral of magnetic force round a closed circuit with the rate of change of electric displacement through the circuit, are the relations from which Maxwell derived his equations for the propagation of electromagnetic waves. It is evident, therefore, that with varying potentials the potential at a point will not be a single valued function depending only upon the potentials of surrounding bodies, as in the case of steady potentials, but it will be subject to a time variation, or, to put it another way, the equipotential surfaces will change in form during the cycle. With slow cyclic changes in potential this effect will be negligible, but with very high frequencies it cannot be ignored. Another factor that is negligible at commercial frequencies but becomes of great importance at high frequencies is molecular loss in the dielectric. With sustained high frequency these losses may, by the effect of local heating, ultimately produce breakdown.

The proper remedy for both these conditions is to be found in the method of designing insulators outlined in the joint paper, that is, to so design the insulator as to have as near uniform stress as possible over the surface, under normal conditions, and allow a liberal factor of safety to take care of abnormal stress conditions.

Replying to Mr. Thomas's second question as to the effect of external bodies—the remedy appears to me to be obvious, namely, to make the electrodes of the insulator of such a form that they will shield the surface of the insulating body from external influence. In other words, the electrodes must be designed so that the field between them will be but slightly affected by external bodies. In some cases, as for instance in outlet terminals, external bodies form one of the electrodes. The case cited by Mr. Thomas is one of these and the large plane on top of the terminal only completes the design of the terminal according to the premises assumed, as is fully explained in the second paper.

Regarding the matter of conduction over the surface, which is the subject of Mr. Thomas's third question, I have observed that soot is precipitated from the atmosphere wherever there is corona. Corona is probably present under working conditions in nearly all the present forms of commercial insulators, which accounts for conduction over the surface being considered of so much importance in these designs. It may be well to add that corona itself causes conduction and therefore in such insulators, even when free from dirt, there may be conduction over parts of the surface. In the forms of insulators discussed in the first paper corona is entirely absent up to the breakdown point. Thus there is no conduction over the surface due to corona itself and no tendency for smoke to be precipitated over the surface in the form of soot. The test piece exhibited has been tested with three months' natural deposit of Pittsburgh dirt and its strength was unimpaired.

The presence of corona will, I believe, always tend to weaken the air in the neighborhood of an insulator, and therefore should be avoided if possible. When corona is present in an insulator or air gap, as the potentials are increased at the electrodes the outer surfaces of the corona formation will advance, the maximum intensity at the surface of the formation remaining unchanged until the formation reaches a certain critical point, the form of the electric field for which is such that further formation cannot take place without an increase in the flux intensity; this is the point of breakdown. Even at this point arcing may not actually take place, on account of the high resistance of the corona, but breakdown is manifested by the presence of streamers or sparks passing between the electrodes. When the electric field is nearly uniform the introduction of any disturbing element, such as that illustrated in Fig. 7, will cause breakdown at a very much lower value than if the disturbing element were absent. This proposition has been demonstrated by actual tests.

I have in a previous part of this discussion shown that air insulators of the type under discussion may be designed so as to be inappreciably affected by external bodies. By way of answer to the latter part of Mr. Thomas's discussion it may be well to add that with a proper design and suitable factor of safety there would be less likelihood of trouble with the ideal form of distribution than with the other.

I wish to say in reply to Mr. Thomas's last question that there is no reason why a million-volt transformer with two terminals of opposite polarity should not be made. The transformer described in the second paper, from an insulation standpoint, is the equivalent of such a transformer. The point I wanted to bring out in this paper is that two 500,000-volt transformers with one end grounded, is a better proposition for a test room then one one-million-volt transformer with terminals of opposite polarity, because the majority of tests are made with one end grounded, and with the first proposition two transformers are available for testing purposes. These two transformers when connected in series in the primary are exactly equivalent to the one-million-volt transformer.

Mr. Mershon's questions as to the effect of high frequency, dirt and the presence of neighboring bodies, are answered in my reply to Mr. Thomas's questions.

The scheme of Fig. 3 referred to by Mr. Mershon was suggested by Mr. Chubb. The causes of the discrepancies in the results are several; possibly one of them is that suggested by Mr. Mershon. Others are the effect of the sides of the tank and the effect of the ends of the cylinder. The equipotential surfaces given are only a first approximation to the true surfaces with an infinitely long cylinder passing through a ring.

I am glad Mr. Mailloux called attention in his discussion to the great gains that have been made in the past by the use of theory. The remark is often heard that practise and theory do not agree. This attitude of mind is so ingrained in some that when they come across such a case of apparent disagreement, instead of looking into the matter and finding the cause of it, they accept it as a further proof of the unreliability of theory. Whereas, if it were early instilled in the minds of these men that theory and practise are not antagonistic, but that disagreement between them is the result either of incorrect observation or of incorrect or incomplete premises, far greater progress would be made.

I think with Mr. Peek that the theory of electric stresses is generally very inadequately presented in engineering courses. Too much stress is laid on charges and distribution of charges when potentials and distributions of potentials are the determining factors in practical application.

Messrs. Thomas and Peek lay great stress on the possibilities of disastrous effects due to high frequency. I think this matter of high-frequency surges is a good deal of a bogy. The number of failures of transformers attributed to high-frequency surges that have come within my observation during the past ten years has been so small that this factor has not appeared to me to be of much consequence as a cause of failure. However, as I have already pointed out, there is no reason why an insulator or transformer designed according to the principles given in the two papers should not at the same time be capable of withstanding high-frequency oscillations, and as a matter of fact these conditions are taken into account in the design.

Mr. Peek's remarks on the insulator shown in Fig. 20 seem to indicate that he has not read the second paper carefully or else he has not grasped the principle stated. Reasoning from Mr. Peek's point of view the condition for equal division of potential between the units of an insulator is that the flux leaving each electrode shall be the same; this means that the capacity of each electrode to ground must be inversely as its potential. This condition is approximated very closely if the high-potential and ground-potential electrodes coincide at their edges with two large parallel disks as shown in Fig. 20. The advantage of this method of obtaining equal distribution of potential over that suggested by Mr. Peek has already been pointed out in the paper.

Dr. Kennelly's remarks as to the importance of having a name for the electrostatic unit of flux are very interesting. When dealing with alternating stresses it is always necessary to indicate whether maximum or root-mean-square values are intended. This ambiguity would not exist if the flux density were given instead.

In reply to Mr. Weed's strictures on the joint paper, I wish to say that it was not the intention of Mr. Farnsworth and myself to give out as a fundamental proposition that the surface conforming with the flux lines that would exist if the insulating body were removed gives maximum efficiency. The investigation was undertaken to show that the breakdown voltage over the surface of an insulator depends on the strength of air. The authors came to the conclusion that creepage actually existed in most insulators on account of corona set up at the surface of the insulator due to the deflection of the flux from the air into the insulator. The natural remedy for this is to make the surface of the insulator conform to the flow lines that would exist in air alone between the two electrodes. By properly shaping the electrodes the breakdown path may be made very efficient. I agree with Mr. Weed that slight changes from this form may sometimes result in slightly higher efficiency, but on the other hand a radical change will result in lower efficiency. If the electrodes are shaped to give a good field, then the surface conforming to the flow lines will conform very closely to the best form.

It would take too long to discuss all the points brought up by Mr. Weed, particularly since the greater part of them are based on a misunderstanding of the object ot the paper. There is one point, however, in his discussion of the joint paper, that should be answered, and that is his report of a test he made to determine the truth of the statement as to Fig. 7 of our paper. The plain strip used by him should give a breakdown value equal to that between the plates when only air is present, otherwise the results are valueless. I do not think Mr. Weed's tests are as conclusive as he seems to consider them. Other observers independently of ourselves have obtained results that agree with our conclusions.

Mr. Weed's remarks about the condenser type terminal are merely a recapitulation of what has been said in the two papers in a more general way. I do not think Mr. Weed can have read over the second paper with care, otherwise he would not have found any such assumption as he indicates in the paragraph preceding the last in his discussion. It is particularly pointed out in this paper that in order to obtain the proper conditions of stress in a condenser type terminal the edges of the high-potential and ground-potential cylinders must be on two infinite parallel planes, but, as pointed out in the paper, even then the conditions will not be ideal, on account of the number of steps being finite, but practically the division of potential will be uniform at ordinary frequencies. The reliability of the condenser terminal has been conclusively proved in service.

A Series of Lectures before the American Institute of Electrical Engineers, New York, March 19, 26, April 2, 9, 16, 1913.

RADIOACTIVITY

BY EDWIN PLIMPTON ADAMS

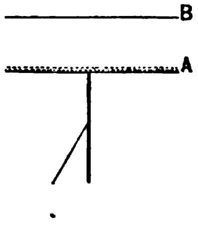
LECTURE I

The science of radioactivity has now reached a stage in its development at which we can speak of it with a good deal of confidence. Among those who feel a keen interest in the recent developments of science, but who have not had time to follow them in detail, and it is to such that I was asked to address these lectures, there is a rather prevalent feeling that the discoveries in this branch of physics and chemistry have been of a revolutionary character. One often hears the opinion expressed that the discoveries in radioactivity have completely overthrown many of the theories that we had come to look upon as firmly established. In particular, I have often been asked about the validity of the principle of the conservation of energy—the fundamental principle of the physical sciences—in the light of the phenomena of radioactivity. Can we still build upon that principle? What I shall attempt in these lectures is to show how far our old notions of the constitution of matter are still valid, what modifications we must make in them, and where the physical sciences stand at present in the light of the discoveries that have been made during the 17 years that have elapsed.since the discovery of radioactive phenomena. I hope to succeed in showing you that the results that have been obtained are much more constructive than destructive in their effect upon our views as to the constitution of matter. Instead of being revolutionary in their tendency they lead, viewed in a certain way, to a logical development of the physical sciences; for these discoveries give us information about the interior structure of the atom about which everything was largely conjecture. We are only at the beginning of what is bound to lead to a much fuller conception of atomic structure. The foundation for it has been built up, and it is this foundation that I wish to describe to you.

The discovery of the property of radioactivity may be said to have had its inception in the discovery by Röntgen, in 1896, of the rays which bear his name. Guided by the similarity between the greenish yellow luminescence observed on the walls of a Röntgen ray bulb when the rays are produced and the greenish phosphorescence of certain uranium compounds after exposure to light, Becquerel sought a connection between these two phenomena. In the course of his experiments Becquerel found that uranium and its compounds produced effects very similar to those of the Röntgen rays—namely, a photographic plate completely shielded from ordinary light, by covering with black paper, was affected when the uranium was brought near in the same way that it would have been if exposed to ordinary light; and furthermore, the air in the neighborhood of the uranium became a conductor of electricity. But there is one striking difference between the effects produced by these two agents. For the Röntgen rays to produce them there must be a continuous supply of energy from outside to keep the electrical discharge in the bulb; while the production of these effects by the uranium was found to depend in no way upon previous illumination or any other external source of energy.

Becquerel thus discovered a new property of matter—the property of radioactivity, and it was an important question to determine whether this property was exhibited by other substances than uranium and its compounds.

In the first place it must be shown how the radioactivity of different substances may be compared. The property which is generally made use of for this purpose is that of rendering air a conductor of electricity. As the currents to be measured are very small, smaller than can be measured by a galvanometer, some form of electrometer is employed. The simplest is the gold leaf electroscope.



Let A and B be the plates of a condenser of capacity C. A known amount of the radioactive substance is spread on A. If charged to a difference of potential V the quantity of electricity on one of the plates is $C\ V$. The current flowing between the plates is

$$C\frac{dV}{dt} = i$$

Suppose that over a certain range of motion of the gold leaf it is known that a fall in its potential of a volts corresponds to a fall through θ degrees. Then a fall through ϕ degrees in a second will correspond to a current

$$i = \frac{a\,C}{\theta}\,\phi$$

The electric current through the air does not obey Ohm's law, but exhibits the phenomenon of saturation; *i.e.*, as the difference of potential is increased from 0, the current first increases nearly proportionately to the potential, rising to a nearly constant value which it keeps until the difference of potential is sufficient for a discharge to take place. As long as the difference of potential is greater than a certain value (100–200 volts in usual cases) the currents measured in this way will be nearly the same over a very wide range of potential difference.

Now Becquerel found that the activity of the different compounds of uranium was proportional to the amount of uranium contained in them. If the activity of metallic uranium be taken as unity, then the activities of the uranium compounds will be expressed by numbers less than unity.

Of the elements known at that time, only one other was found to have radioactive properties. Schmidt and, independently, Madame Curie, found that thorium and its compounds were radioactive.

Madame Curie's discovery of the radioactivity of thorium was made in the course of a systematic investigation, first, of all the known elements it was possible to secure, and then of a large number of minerals and rocks. She found that the activity of the uranium minerals was larger than was to have been expected from their uranium content. Thus a certain specimen of pitchblende, an oxide of uranium, had an activity nearly four times that of metallic uranium. An obvious deduction from this discovery was that these minerals contained one or more hitherto unknown substances, in minute proportions to have escaped discovery by chemical analysis and of correspondingly high radioactivity.

The method employed by Madame Curie to test this hypothesis was to subject a large quantity (one ton) of the residues from the mineral pitchblende, after the uranium had been extracted, to chemical analysis. It was found that the radioactivity of these residues was four times as great as that of metallic uranium, from which it was concluded that the greater part of the conjectured substance was contained in them. In brief, the method of attack consisted in measuring the activity of all the products of a chemical separation in order to find which one of them included the looked-for substance. This one was then subjected to further separation and the process continued. The difficulties in this procedure were great, for owing to the phenomenon of induced or excited radioactivity, of which more will be said later, there was some uncertainty in the earlier stages of the separation as to which product contained the radioactive substance; and, in addition, it was found that there were several unknown substances, strongly radioactive, of different chemical behavior, which complicated the search very considerably. But we shall see that all these substances are closely related so that for the present it will be sufficient to speak of only one of them.

It was finally found that the product of separation containing the barium was enormously more active than any of the other products. No chemical method of making a further separation could be found, and so recourse was had to an assumed difference in solubility between the barium salt and the unknown substance. From a solution of two different substances that one of less solubility will crystallize first. The barium in the form of barium chloride was allowed to crystallize and the first crystals formed were found to be decidedly more active than those found later.

One of the characteristics of an element is the possession of a definite spectrum. When this very active barium chloride was examined spectroscopically before the fractional crystallization began, only the lines due to barium appeared. As the process of fractional crystallization proceeded new lines appeared, the barium lines grew fainter, until after a number of crystallizations the original barium spectral lines had disappeared, and a new spectrum had taken their place. This in itself was sufficient to prove the discovery of a new element, to which the name radium was given.

From a ton of the original material 120 milligrams of radium chloride were obtained in the way indicated. Its activity was estimated by Madame Curie at about a million times that of uranium. Madame Curie also succeeded in determining the atomic weight of the new element radium. The first number obtained, 225, has been corrected by more recent work, so that now 226.4 is accepted as the atomic weight of radium, taking oxygen = 16.

There being a greater difference in the solubilities of radium and barium bromides than of their chlorides, radium is now generally prepared in the form of the bromide.

Madame Curie and Debierne have lately been successful in isolating the metal radium itself by electrolysis with a mercury cathode, thus forming a radium amalgam. This amalgam was then distilled in an atmosphere of hydrogen. The mercury was completely driven off at a temperature of 700 deg. and the remaining substance, practically pure radium, had a shining white metallic luster. It rapidly turned black in air, and decomposed water. Chemically its behavior was thus similar to that of metallic barium.

We have now traced, very briefly, the processes leading to the discovery of this new element radium, and we must now study the physical properties of it in greater detail. We have seen that one of its most striking properties is that of rendering the air surrounding it a conductor of electricity. It will therefore be well to preface this study by a brief resumé of the theory of ionization of gases.

A gas in its normal condition is a non-conductor of electricity. It may be made a conductor by various agents, among others, the passage through it of Röntgen rays, of ultra-violet light, and, we have seen, by radioactive substances. But it was through the study of the electric discharge in vacuum tubes that our knowledge of the way in which gases become conductors of electricity came; and to Sir Joseph J. Thomson belongs the credit of having developed a consistent theory which has proved of the greatest service in all the recent developments of electric theory.

The leading idea in Thomson's theory is that negatively charged corpuscles—or electrons—are shot out from the cathode; that these corpuscles by their collisions with the neutral molecules of the gas ionize them; that is, split them up into positive and negative parts; the positive ions travel towards the negative electrode, the cathode, and the negative ions towards the positive electrode, the anode. We thus have a double transport of electricity through the gas—positive electricity in one direction and negative electricity in the opposite direction. This constitutes an electric current, flowing in the direction of the positive ions. The path of the cathode rays, the stream of corpuscles shot out from the cathode, is made visible by the light which is produced in the ionization of the gas molecules, and there is also a greenish yellow luminescence produced where the corpuscles strike the walls of the tube. An obstacle placed in the path of the cathode particles casts a shadow on the walls of the tube. By placing a diaphragm with a small opening in front of the cathode, a narrow beam of the cathode rays may be separated. When a magnet is brought near, the beam is deflected in the direction that a stream of negatively charged particles shot out from the cathode should be. The negative charges carried by these corpuscles may also be demonstrated by bending the beam into a vessel connected to an electrometer which becomes negatively charged. In short, every test that may be applied shows that the cathode rays are a stream of negatively electrified corpuscles. The cathode particles have one striking property which at first caused many investigators to doubt their material nature. This is their ability to penetrate thin sheets of metal and other substances. But now that we know their very high velocity, this property is not so surprising. The sudden increase in the electric current through a gas which occurs when the potential difference rises to a certain value, the "sparking potential," receives a ready explanation in this theory. The ions produced from the gas molecules are under the influence of the electric field. If the electric field is strong enough it will give the ions a sufficiently high velocity so that they become ionizing agents themselves—producing fresh ions by their collisions with the molecules of the gas.

The real confirmation of the hypothesis of the material nature of the cathode particles was furnished by actual measurements of their velocity, the charges they carry, and their mass. As the same methods have since been employed in the study of the radiations from radioactive substances, we may well devote a little time in considering the principles involved in such measurements.

Let us suppose that we have a corpuscle of mass m, charged with e units of electricity and moving in a straight line with uniform velocity v. As proved by Rowland's classic experiment, a charged body in motion is equivalent to an electric current. Suppose that a magnetic field of intensity H is set up in a direction at right angles to the motion of the corpuscle. If there were an electric current, i, flowing along a wire in the path of the corpuscle, we know that on each element of length ds of the current there would be a mechanical force $H\,i\,ds$, this force being perpendicular both to ds and H. The current element $i\,ds$ is to be replaced by the product of the charge and its velocity, $e\,v$. So that the force acting on the corpuscle at right angles to v is $H\,e\,v$. The acceleration normal to the velocity is v^2/ρ where ρ is the radius of curvature, or in our case the radius of the circle into which the path of the corpuscles is bent. We thus have

$$H\,e\,v = \frac{m\,v^2}{\rho} \tag{1}$$

Let us next suppose that an electric field of intensity X is set up parallel to the magnetic field, at right angles to v and to H. This exerts a force $e\,X$ on the corpuscles, and by varying X we can make this force just balance the force exerted by the magnetic field so that the corpuscle will be undeflected. We then have $H\,e\,v = e\,X$

or

$$v = \frac{X}{H} \tag{2}$$

which determines the velocity of the corpuscles, and by (1)

$$\frac{e}{m} = \frac{X}{H^2\,\rho} \tag{3}$$

which determines the ratio of the charge to the mass of the corpuscle. In these expressions X and H may be determined by the usual methods of measuring electric and magnetic forces. To determine ρ, it is necessary to measure the displacement of the spot of light made by the narrow beam of cathode rays on a phosphorescent screen when the magnetic field is applied; by geometry the radius of the circular path may then be found.

The velocity of the cathode particles found in this way was as high as 1.2×10^{10} cm. per second, roughly one third the velocity of light. Probably the most accurate value of e/m found by this method is 1.8×10^7 when the charge is measured in electromagnetic units. It is interesting to compare this with the ratio e/m for the charge carried by a hydrogen atom in the electrolysis of water. Let N be the number of molecules in one cu. cm. of gas at 0 deg. cent. and at 760 mm. of mercury. If e is the charge on an atom of hydrogen $2\,N\,e$ is the whole charge in one cu. cm. One electromagnetic unit of electricity liberates 1.04×10^{-4} grams of hydrogen. If m is the mass of hydrogen atom $2\,m\,N$ is the mass of one cu. cm. of hydrogen. So that

$$\frac{e}{m} = 9.6 \times 10^3$$

We shall show that there is strong reason for believing that the charge on a corpuscle is the same as the charge carried by an atom of hydrogen in electrolysis. The mass of the hydrogen atom, which was, up to the time of the discovery of corpuscles, the smallest mass known, is thus seen to be about 1800 times greater than the mass of a corpuscle.

Measurement of the Charge Carried by Ions

We have seen that the cathode particles have the power of ionizing gases through which they pass. This process consists in the splitting up of an atom or molecule into a positively and a negatively charged ion. Now it was discovered by C.T.R. Wilson that these ions act as centers of condensation for water vapor. In perfectly dust-free air, saturated with water vapor, no cloud of water drops will form unless the air is suddenly cooled by an expansion to something like eight times its initial volume. But if dust is present very much smaller expansions will suffice to produce a visible cloud on expansion. Wilson discovered that in dust-free ionized air sudden expansion to 1.25 the original volume was sufficient to produce a cloud in the air, each ion presumably becoming the nucleus of a water drop. These drops fall under gravity and their rate of fall may be determined from the rate of subsidence of the top of the cloud after the expansion has taken place. A calculation, by Stokes, gives as the rate of fall of a sphere in a fluid

$$v = \frac{2}{9} \frac{g a^2 \rho}{\mu}$$

g being the acceleration due to gravity, a the radius of the drop, μ the coefficient of viscosity of the fluid, air in our case, and ρ the density of the sphere. This expression thus enables us to determine the radius of the water drops. The whole volume, q, of the water vapor deposited per unit volume may easily be determined by thermodynamical methods; and

$$q = \frac{4}{3} n \pi a^3$$

where n is the number of drops per unit volume. This enables us to determine n, which, if we assume that each ion acts as a nucleus for one drop, gives us the number of ions per cubic centimeter. The total charge carried by all the ions of one sign may be determined by measuring the current through the air when a potential difference is applied; then the charge on a single ion is deduced at once.

This method was modified by H. A. Wilson so as to eliminate many of the uncertainties inherent in Thomson's original method. The cloud was produced between the plates of a parallel plate condenser and the rate of motion of the top of it observed alternately when under the influence of gravity only, and when in addition an electric field was applied. The force on the drop due to the electric field is $X\,e$ and due to gravity

$$\frac{4}{3}\pi\rho g a^3$$

$$\frac{X e + \frac{4}{3}\pi\rho g a^3}{\frac{4}{3}\pi\rho g a^3} = \frac{v_1}{v}$$

v_1 is the velocity when an electric force is applied; v, with no electric force.

$$v = \frac{2}{9}\frac{g a^2 \rho}{\mu}$$

$$e = \sqrt{2}\, g\pi \sqrt{\frac{\mu^3}{g\rho}}\, \frac{v^{3/2}(v_1 - v)}{X v}$$

Another modification, in which the motion of a single drop is watched, was made by R. A. Millikan, and the results of his measurements are probably the most accurate that we have. In the two methods we have sketched the cloud must be observed immediately after its formation, since it rapidly disappears by evaporation. Millikan used oil drops, formed by means of an atomizer and blown into a chamber from which they dropped through an opening into the space between the plates of a parallel plate condenser. This space could be made airtight after a drop had fallen into it so as to prevent air currents disturbing the motion of the drop. The oil drop was specially illuminated and was observed through a telescope. In its formation the drop became charged with frictional electricity so that its motion could be controlled by the electric field in the air condenser. The evaporation from it was so small that a single drop could be observed for several hours. An ionizing agent, Röntgen rays or radium rays, acted on the air in the condenser and the oil drop occasionally picked up an ion. The instant it did so its motion suddenly changed, and from the change in its motion the magnitude and sign of the charge it had picked up could be found. In addition to the more accurate method of observing, Millikan employed a correction to Stokes's formula for the rate of fall of a sphere in air which was developed by Cunningham for the case of very small spheres. The results of Millikan's experiments were that the charges on the oil drops were always exact multiples of an elementary charge, that is, the oil drop picked up one or more of these elementary charges. Its magnitude was found to be $e = 4.774 \times 10^{-10}$ in electrostatic units.

We have seen that determinations of the velocity and the ratio e/m have been made directly on the corpuscles forming the cathode rays. On the other hand determinations of e, the elementary charge, have been made only on ions, both positive and negative, which are formed by various ionizing agents. The evidence that the cathode particles carry this elementary charge is perhaps not conclusive, but the fact that the ratio e/m is found to be the same for the corpuscles however they are produced, and that ionization in certain cases must be considered as the result of a corpuscle entering into a neutral atom, thus giving the ion the charge of the corpuscle, gives us very strong reasons for believing that all corpuscles carry this negative elementary charge.

The accurate determination of the charge on an ion leads to a knowledge of many important physical constants. It is known by experiment that when 1 electromagnetic unit of electricity is passed through water 1.04×10^{-4} grams of hydrogen are liberated. If N is the number of molecules in a cubic centimeter of any gas at a pressure of 760 mm. of mercury, and at 0 deg. cent. temperature, ρ the density of hydrogen, and e the charge on a hydrogen atom in electrolysis,

$$2 \frac{N e}{\rho} 1.04 \times 10^{-4} = 1$$

or

$$e = \frac{\rho \, 10^4}{2.08 \, N}$$

Now N may be estimated from the results of the kinetic theory of gases. The value of N so obtained is, on the average,

$$N = 3 \times 10^{19}$$
$$\rho = 9 \times 10^{-5}$$

from which it follows that

$$e = 1.4 \times 10^{-20}$$

in electromagnetic units or

$$4.2 \times 10^{-10}$$

in electrostatic units.

This is so near the value of the charge in the ion, determined in the way sketched above, that we are led to believe that the charge on the ion of hydrogen in electrolysis is equal to the elementary charge determined on gaseous ions. As the latter determination is one in which we have more confidence than estimates based upon the kinetic theory of gases, we are led to determine N from the known value of e. This gives

$$N = 2.7 \times 10^{-19}$$

Now if m is the mass of an atom of hydrogen

$$m = \frac{\rho}{2\,N}$$

Thus the mass of an atom of hydrogen is

$$1.67 \times 10^{-24}$$

and from this the mass of the atom of any element can be determined from its atomic weight in terms of hydrogen as unity.

Radiations from Radioactive Substances

We now are going to consider the radiations from radioactive substances in particular. To trace in detail the course of the discovery of the nature of these radiations would require too much time, and so we must limit ourselves to an outline of what is known about them today and the evidence upon which our knowledge rests.

Beta Rays. We consider first the so-called beta rays. These rays are in all respects identical with the corpuscles forming the cathode rays in a vacuum tube, except that, among them, there are some that travel with very much higher velocities than any that can be produced in a discharge tube. Their equivalence to the cathode corpuscles is shown by measurements of e/m and v for them—measurements that are made in exactly the same way, by magnetic and electric deviation, as for the cathode particles. Further, the charges on the ions produced by them are also the same as the charges on the ions produced in any other way. Later on we shall have to speak of a variation of the ratio e/m with the velocity, but it is a variation which is conditioned only by the velocity and does not indicate that these corpuscles themselves are in any way different from the others.

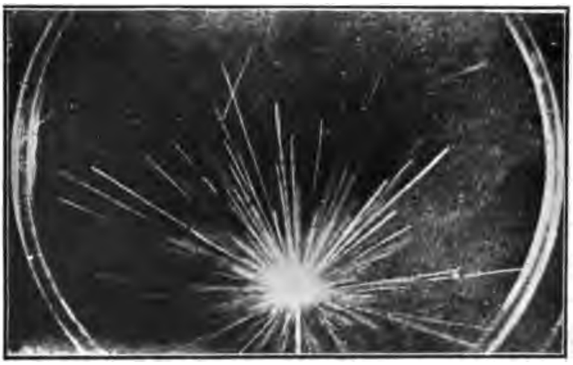
To show the penetrating power of the beta rays, as well as the charge they carry, Strutt devised a striking experiment. A quantity of radium in a thin-walled glass tube was supported by an insulator inside a highly exhausted bulb. The beta rays penetrated the glass tube, leaving the radium positively charged. A pair of gold leaves was carried by the glass tube containing the radium, and by means of a wire sealed into this tube the gold leaves were kept at the same potential as the radium. As the radium gained a positive charge by the expulsion of the negative corpuscles the gold leaves diverged until they touched the walls of the bulb which were coated with tinfoil, connected to earth. They then discharged to earth, and collapsed. This process repeated itself over and over again.

On passing through matter the velocity of the beta particles is reduced. For this reason, even if it should be assumed that all the beta particles from a given radioactive substance are emitted with the same velocity, those which are emitted from a thick layer of substance will have different velocities, since they pass through different thicknesses of the substance. The most rapidly moving beta particles have velocities very near the velocity of light, differing from the latter by about one per cent. It is not surprising, therefore, that they should be able to penetrate considerable thicknesses of solid matter. W. Wilson found that on passing through 2 mm. of aluminum the velocity was reduced from 2.86×10^{10} cm. per second to 2.00×10^{10} cm. per second.

In their passage through matter the beta rays lose their energy. In gases this loss of energy is caused by ionization, as it requires work to form ions from neutral atoms. In solids, also, there is good evidence that their conductivity is increased by the ions formed in them by the beta rays. The rays thus have their energy decreased so much that ultimately they are unable to ionize the atoms through which they pass.

Some very remarkable results have recently been obtained by C. T. R. Wilson, who succeeded in photographing the paths of the beta particles through gases. In passing through a gas positive and negative ions are formed, and these ions act as centers of condensation of water vapor. The path of a beta particle is thus marked out by the water drops formed on the ions which it pro-

PATHS OF BETA PARTICLES [ADAMS]



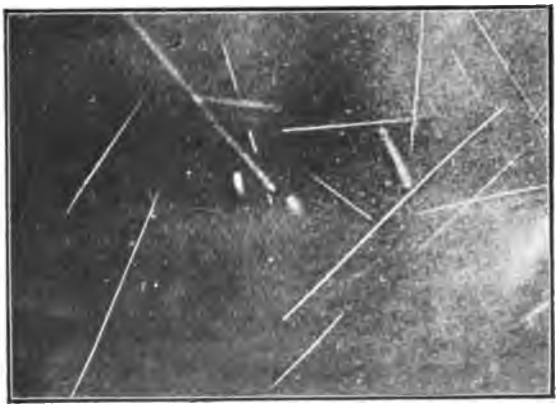
PATHS OF ALPHA PARTICLES [ADAMS]

also cease. These scintillations may be counted, and, assuming that each point of light is caused by the impact of one alpha particle, an estimate may be made of the rate of emission of alpha particles from various radioactive substances. But a more certain method of arriving at the same result was devised by Rutherford. A small opening was made in a chamber in which a strong electric field was maintained. The alpha particles are shot out in all directions from a radioactive substance, so that the ratio of those entering the opening in a given time to the whole number shot out in the same time was known from the geometry of the arrangement. On entering the chamber the alpha particles produced ions and thus an electric current was suddenly started in the chamber, which disappeared very quickly as the ions were removed by the electric field. Thus the entrance of each alpha particle into the chamber was marked by a sudden rise in the current flowing through it, and this was made known by the kick of the electrometer needle used to measure the current. The number of kicks in a given time gave the number of alpha particles entering the chamber in that time, and from this the whole number of alpha particles emitted could be deduced. As a single alpha particle does not produce ions enough to give a current which can be detected, the current was multipled by fresh ions produced by the action of the strong electric field on those already present.

Knowing now the whole number of alpha particles emitted from a radioactive substance, the charge on each is known as soon as we know the whole charge carried by the particles. Measurements of this kind have been made by Rutherford and others, and the results show that the alpha particle carries a charge opposite in sign but double the amount of the charge carried by the beta particle, or electron.

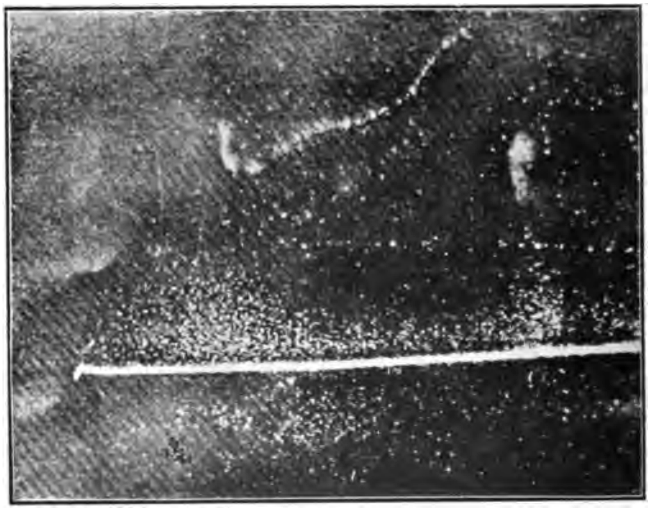
From the deflection of a beam of alpha particles in an electric and a magnetic field the ratio e/m, and the velocity v, have been determined. Knowing now the charge, e, the mass of a single alpha particle is known at once. The ratio e/m is the same for the alpha particles from all the radioactive substances, while their velocity is different. In this way it is found that the alpha particle has a mass equal to four times the mass of the hydrogen atom. This is very nearly the mass of the helium atom and it thus seemed that the alpha particle was a helium atom which had lost two negative corpuscles, leaving it effectively charged with a double positive charge. This hypothesis was put to the

PATHS OF ALPHA PARTICLES [ADAMS]



PATHS OF ALPHA PARTICLES [ADAMS]

PLATE XXI
A. I. E. E.
VOL. XXXII, 1913



PATHS OF BETA PARTICLES [ADAMS]
The broad line is the path of an alpha particle.

test by Rutherford. The alpha particles were allowed to penetrate through the very thin walls of a glass tube into a highly exhausted tube through which an electric discharge could be passed. By examining its spectrum the nature of the gas, if any, which collected in the tube could be determined. In a few days the whole spectrum of helium appeared. Control experiments precluded any other possibility than that the alpha particles, shot through the walls of the tube, and having their charge neutralized, became helium atoms. The special significance of this fact will be spoken of later when we come to consider the disintegration theory of radioactivity.

The recent experiments of C. T. R. Wilson, making visible the tracks of the beta particles through a gas by photographing the water vapor which condenses on the ions produced, have been spoken of. He obtained similar results with the alpha particles. His photographs, some of which are shown here, are even more striking than those obtained with the beta particles. The alpha particle, owing to its greater energy, is a much more efficient ionizer than the beta particle so that its track through a gas is marked out by many more ions on which the water drops collect. It will be seen that these tracks end abruptly, and this is accounted for by the fact that they have a definite range, beyond which their ionizing power ceases. Many of the tracks show a bend toward their end. This is probably the result of the reduction in their velocity which makes them more easily deflected from their straight line motion by collisions with the molecules.

Gamma Rays. The third distinctive type of radiation emitted by radioactive substances is the gamma radiation. There is no direct evidence, as in the case of the alpha and beta rays, that the gamma rays are formed of charged particles. The gamma rays produce photographic and ionizing effects; their most striking property is their relatively enormous penetrating power. A thickness of about two millimeters of lead is sufficient to absorb all the alpha and beta particles emitted by radium. Effects produced by gamma rays have been observed after they passed through 30 cm. of iron.

To account for the gamma rays there are two theories we must consider. The first is the ether pulse theory which was originated by Stokes to account for the Röntgen rays. When a charged particle is in uniform motion through the ether there is no radiation of energy from it. Once get it moving and it will continue to move in a straight line with constant velocity. Accompanying a charged particle at rest, there is an electric field which is everywhere directed along the radius drawn from the particle. In other words, lines of force go out from it, if positive, and enter it, if negative, equally distributed in all directions. There is no magnetic force while the particle is at rest. Now suppose the particle moves with uniform velocity in a straight line. It carries its lines of force with it, and as long as the velocity is small there is not much change from the equal distribution of lines in all directions. A charged particle in motion acts as an element of current; so that there is a magnetic field accompanying it. The magnetic force is at every point perpendicular both to the direction of motion and to the line drawn from the particle to the point. It is thus everywhere perpendicular to the electric force. Now, for higher velocities, the lines of electric and magnetic force crowd towards the equatorial plane, always at right angles to each other. For a velocity equal to that of light the electric and magnetic forces would all be concentrated in the equatorial plane.

Suppose now that a charged particle, moving with a velocity very nearly equal to that of light, is brought to rest within a very short distance by collision with an atom or by any other means. Just before its collision it carried a field of electric and magnetic force with it and this field was concentrated near the equatorial plane. Now suppose that the corpuscle is brought to rest in a very short distance. A pulse of intense electric and magnetic forces, at right angles to each other, spreads away from the corpuscle with the velocity of light, the direction of propagation being normal to the electric and magnetic forces. The thickness of this pulse is proportional to the distance in which the stopping took place, that is, to its acceleration. A succession of such pulses arising from the stopping of many corpuscles will thus give rise to electromagnetic waves in the ether which differ in no respect from waves of light except that the wave length is much less than that corresponding to light of the shortest wave length that we know anything of. On this view, the gamma rays from radioactive substances and the Röntgen rays are identical in their nature. Differences in effects produced by these two radiations are to be expected, since the beta rays, the origin of the gamma rays, have velocities which are much greater than the cathode rays which give rise to the Röntgen rays.

The other view as to the nature of gamma rays is due to Bragg. He regards the gamma rays as made up of material particles, a negative corpuscle united to an equal positive charge, so that the combination is neutral electrically, the whole travelling with a velocity very nearly equal to that of light. This view of the gamma rays explains as well as the pulse theory the absence of any deflection of the gamma rays by electric or magnetic fields; it was originated by Bragg to account for certain dissymmetries on the incident and emergent sides of matter through which the rays pass—effects which are difficult to account for on the ether pulse theory. The study of the whole matter is greatly complicated by the secondary rays produced when gamma rays fall upon matter; these secondary rays are partly secondary gamma rays and partly secondary beta rays.

On the whole, the evidence seems to favor the ether pulse theory. Recent experiments by Laue and others seem to show the existence of diffraction effects when Röntgen rays pass through certain crystals. Whatever view we take of the gamma rays it seems well established that the Röntgen rays and the gamma rays are of the same nature. In the experiments referred to, a narrow beam of Röntgen rays was passed through a thin plate of a crystal, and the rays then fell on a photographic plate. In addition to the central spot produced by the rays directly transmitted, there were other spots arranged in more or less concentric circles around it. If we suppose, as we must, that in a crystal the molecules are regularly spaced, then a crystal plate will act as a diffraction grating. On the view that the Röntgen rays are ether pulses, these results receive a satisfactory explanation, while it would be difficult to explain them on any other view of the Röntgen rays. Accepting this view, estimates of the wave length of the Röntgen rays gave numbers in the neighborhood of 10^{-9} centimeters, about what was to be expected from the mode of their production.

LECTURE II

Introduction

It has been suggested to me that the meanings of some of the terms employed in the first lecture were rather uncertain to some of the audience, and the following chart has been prepared which may help to keep in mind the particular meanings of the terms employed throughout the lecture.

Cathode Particle **Beta Particle** **Corpuscle** **Electron**	A negatively charged particle, known only in motion, carrying the elementary electric charge: $e = 4.774 \times 10^{-10}$
Ion	A general term for a charged atom or molecule, or a cluster of atoms or molecules. May be charged either positively or negatively.
Alpha Particle	A positively charged atom of helium, carrying double the elementary charge, e.
Gamma Rays	Probably ether pulses produced by accelerations of the beta particles. Like Röntgen rays.

The four terms: cathode particle, beta particle, corpuscle, electron, all mean exactly the same thing. The distinction between them arises from the fact that the particles appear under different conditions. In the first place, the cathode particle is so called because it appears as if it were shot out from the cathode in a vacuum tube when an electric discharge is sent through. The beta particle is emitted by radioactive substances; the corpuscle and electron, which mean exactly the same, are general terms for both of these particles, and they are used when we wish to speak of them in general.

You will notice that I have not said anything on the chart in regard to the mass of these particles. This subject we shall have to come back to in the next lecture; but, roughly speaking, we can say that the mass of one of these particles is 1/2000 of the mass of the hydrogen atom; the hydrogen atom was, up to the discovery of these particles, the smallest known mass. Where I have used the word " atom," I mean exactly what chemists have always considered the atom to be; that is, the smallest mass which enters into chemical combinations.

Ion is a general term for any charged particle; frequently it is employed when the cathode or beta particle is meant. But usually by ion we mean a charged atom, or molecule, or a cluster of atoms or molecules which stick together around a central charge, which may be either positive or negative. Thus in the electrolysis of liquids, the current is carried by positive ions moving in one direction and negative ions moving in the opposite direction.

The alpha particle, about which we spoke last week, is a positively charged atom of helium; its mass is four times the mass of the hydrogen atom, and it carries a charge which is twice that of the cathode particle.

In regard to gamma rays, we saw last week that there is a good deal of evidence that they are electromagnetic disturbances, in every respect similar to light waves, except that their wave length is very much shorter than the wave length of any known light—something of the order of 1/1000 of the wave length of the shortest waves with which we are familiar.

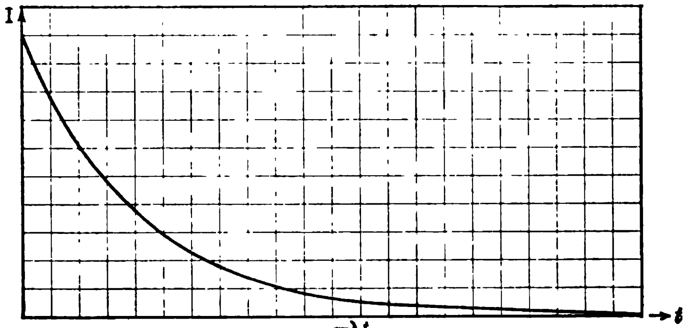
The Disintegration Theory of Radioactivity

We have now given a brief account of the discovery of radioactive substances and of the chief characteristics of the radiations emitted by them. We proceed to consider an explanation of radioactive phenomena.

The most striking fact connected with the early study of radioactivity was the apparently continual emission of relatively large amounts of energy with no evident source of this energy. If the principle of the conservation of energy was to be retained, a source of this energy had to be found. The earliest hypothesis was that of Lord Kelvin, who assumed that space was filled with an unknown type of radiant energy and in some manner the radioactive substances were able to transform this unknown energy into known kinds of energy, and so make it apparent. The difficulty with this explanation was that it explained nothing, but merely let us keep formally the principle of the conservation of energy.

The clue to what we now believe to be the true explanation of radioactive phenomena was furnished by a more detailed study of the radioactive element, uranium—the first element which was discovered to be radioactive. It was found by Sir William Crookes that uranium could be separated into two constituents, one of which was inactive, and the other, much smaller in amount, was relatively intensely radioactive. His method of separation consisted simply in precipitating a solution of uranium nitrate by ammonium carbonate. The greater portion of the precipitate was redissolved by an excess of the reagent, but a residue, consisting chiefly of impurities in the uranium salt, was left undissolved. This residue carried with it the radioactive constituent of the uranium, and was given the name uranium X. The redissolved uranium was found to be inactive. But it is important to observe that tests of the radioactive property were made by the photographic and not the electrical method. Now it is the beta and gamma rays which produce, under usual conditions, the whole photographic effect. The alpha rays are so easily absorbed that the covering used to protect the plates from light also shields them from the alpha rays. The activity measured in these experiments may thus be called the beta-ray activity. On allowing the inactive uranium and the active uranium X to stand for a time it was found that the former recovered its initial activity after a few months, while the latter in the same time lost its activity. By making quantitative determinations of the radioactivity by the electrical method, using only the beta rays, Rutherford and Soddy determined the law of the decay of activity of the uranium X and the rise in activity of the uranium. Let I be the activity at any time, and Io the initial activity. The law of decay was found to be

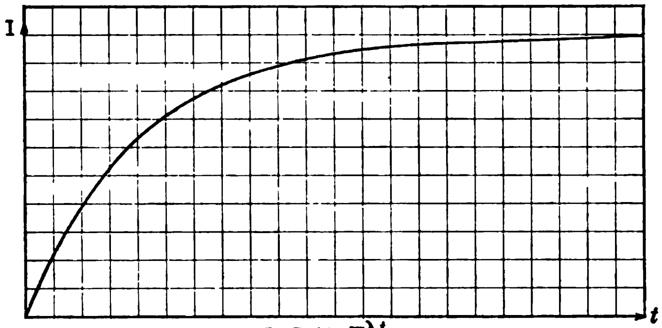
$$I = I_0 \epsilon^{-\lambda t}$$



$I = I_0 \epsilon^{-\lambda t}$

and the law of recovery

$$I = I_0 (1 - \epsilon^{-\lambda t})$$



$I=I_0(1-\epsilon^{-\lambda t})$

where λ is a constant. The illustrations herewith show graphically the forms of the curves expressed by these two equations.

Results exactly analogous were obtained with thorium, from which a constituent thorium X was separated. In this case the decay of activity of the thorium X and the recovery of activity of thorium were much quicker than in the case of uranium. This means that the constant λ has a larger value for thorium than for uranium.

These results led Rutherford and Soddy to the disintegration hypothesis of radioactivity, which we shall now describe.

The old idea of the atom was that of an unchangeable mass, the smallest mass that could be thought of. There were as many different kinds of atoms as chemical elements. Of all the elements hydrogen has the lowest atomic weight and hence the hydrogen atom was supposed to be the smallest mass that could exist. It is true that there had been speculations in regard to the possibility of considering all atoms to be built up of various combinations of one substance, but there was no certain experimental foundation for such speculation and little, if any, progress was made in this direction. It was quite customary to look upon the atoms as hard elastic spheres of the various chemical elements. The study of the spectra of the various elements showed that no such simple view of the atom could be accepted; that the atom must be considered as a dynamical system in order to be capable of giving the very complicated system of spectral lines which most elements exhibit. But spectroscopy has not told us very much about the nature of the dynamical systems which we must take the atoms to be. Up to the time of the discovery of radioactivity, it was supposed that these dynamical systems forming the atoms were all stable systems—

under no conditions could one of them ever change into another of different type. This we may take as the essential feature of the old idea of the chemical atom—its absolute stability considered as a dynamical system.

The disintegration hypothesis discards this requirement of stability for the atom. We must look upon the radioactive elements as formed of the least stable atoms; non-radioactive elements as formed of the most stable atoms. This is certainly not a violent assumption to make, although it is a radical change from former views. If we knew the structure of the atom, all the influences acting upon it, it would be possible to predict just what each atom would do under any conditions. But we do not know this and so we have to make use of probability methods, just as in the kinetic theory of gases we make use of probability methods to find the mean velocity of the molecules.

Let us look upon the atoms as containing alpha and beta particles. We shall have more to say later as to the structure of the atom from this point of view. The atoms being unstable, in a given time a certain number of them will disintegrate, expelling one or more alpha or beta particles, or both. The atom thus changes into an atom of a different kind. Let us assume that the number of atoms which disintegrate in unit time is proportional to the whole number of atoms of its own kind present. If we start with N_0 atoms of a certain kind, the number N after a time t will be given by

$$\frac{dN}{dt} = -\lambda N$$

This expresses that the rate at which the atoms disintegrate is proportional to the whole number present at any time. We can integrate this equation at once, and get

$$N = N_0 \epsilon^{-\lambda t}$$

Thus, it is only after an infinite time has elapsed that all the atoms disintegrate. It will help our understanding of this theory to apply it to a definite case. Let us suppose that all the uranium X has been separated from a given amount of uranium by chemical means. We know that uranium X emits beta particles. Assume then that an atom of uranium X disintegrates into a non-radioactive atom and that the disintegration of each atom is accompanied by the expulsion of one beta particle. The number of beta particles emitted in a given time will then be equal to the number of atoms which break up in that time, and this is proportional to the number of atoms present. Therefore the beta ray activity I at any time is proportional to N, the number of atoms of uranium X present at that time; we thus have

$$\frac{dI}{dt} = -\lambda I$$

$$I = I_0 \epsilon^{-\lambda t}$$

where I_0 is the initial beta ray activity. This is the law of decay found experimentally by Rutherford and Soddy. Now consider the uranium after the uranium X has been removed. It has no beta activity, but it is continually forming uranium X. Suppose then that an atom of uranium breaks up, expelling an alpha particle, and as a result changes into an atom of uranium X. Its beta ray activity depends upon the number of atoms of uranium X which have been formed. Let N, as before, be the number of atoms of uranium X present at any time t. The number of atoms of uranium is enormous; if the number that disintegrate during a few days is very small compared to the whole number, we can regard the rate of production of atoms of uranium X as constant. Call this q. Then

$$\frac{dN}{dt} = q - \lambda N$$

That is, the atoms of uranium X are produced at a rate q and disintegrate at a rate λN. So the net rate of increase is the difference between these two rates.

The integral of this equation is

$$N = \frac{1}{\lambda}\left(q - \epsilon^{-\lambda (t+c)}\right)$$

where c is the constant of integration
when

$$t = 0 \quad N = 0$$
$$t = \infty \quad N = N_0$$

So that

$$N = N_0 (1 - \epsilon^{-\lambda t})$$

Now since the beta ray activity of uranium is due to the uranium

X produced from it, the beta ray activity at a time t after the uranium X has been removed is

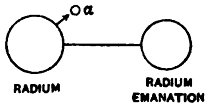
$$I = I_0 (1 - \epsilon^{-\lambda t})$$

and this is the law Rutherford and Soddy found experimentally for the rise in activity of uranium after removal of uranium X.

After a long enough time has passed, five to six months in this case, the amount of uranium X becomes sensibly constant. In other words, there is radioactive equilibrium between the uranium and uranium X. Just as many atoms of uranium X are produced per second as are destroyed per second. It is not a true equilibrium because we have neglected the number of uranium atoms that disappear in comparison with the whole number. But the uranium atoms disintegrate at so slow a rate compared to the rate of disintegration of the uranium X atoms that the equilibrium between the two appears to be complete.

This hypothesis of the actual breaking down of the chemical atom and the resulting formation of a new atom did not meet with immediate acceptance. Chemists, in particular, were inclined to look upon the production of uranium X from uranium as a case of an ordinary chemical reaction. But this required the assumption that uranium itself was not an element, but a compound which slowly broke up spontaneously into its constituents, just as, under the influence of heat, most chemical compounds break up. But all known chemical reactions are subject to external conditions; temperature and pressure have a marked influence on their rate. The decay of uranium X, on the other hand appears to be absolutely independent of all external conditions. It goes on at the same rate at the temperature of liquid air as at the highest temperatures that may be obtained in an electric furnace. And the same holds true for all other radioactive processes. They appear to be inherent properties of the atoms themselves, and no means have yet been found of retarding or accelerating them.

We have now had two examples of radioactive disintegration—uranium and thorium. Let us now turn to radium and see how the disintegration hypothesis works out in this case. We have seen that radium has all the characteristics of a chemical element —a definite atomic weight, a definite spectrum, and distinct chemical properties so that it may be separated from other elements. Now in studying the radioactive properties of radium it was soon found that there were many irregularities in the ionization produced by the rays from it. Air currents blowing over the uncovered radium, in particular, had a marked influence on the conductivity of the surrounding air. The ionization was much less from radium that had been freshly evaporated from solution than from the radium after it had stood for some days. Heating the radium produced a temporary diminution in its activity. It seemed as if a gaseous emanation, itself radioactive, was constantly forming in the radium and being occluded in it, so that heat and solution removed the store of this occluded gas. A simple way of studying this supposed gas was found in bubbling air through a solution of radium and collecting this air in a closed vessel. It was found that the conductivity of the air was enormously increased, that its conductivity gradually diminished, falling to half its initial value in about four days, but not according to a simple exponential law. It was found that objects immersed in it became themselves temporarily radioactive, particularly if they were negatively charged. This is the phenomenon known as induced or excited radioactivity. When radium was dissolved in water and air bubbled through the solution there was at first a very large amount of the emanation carried over. After a short time things reached a steady state. This may be explained by the disintegration hypothesis, if we assume that the radium atoms in disintegrating form atoms of emanation. This process goes on continuously and in the solid radium the emanation becomes occluded. In equilibrium there will be just as many atoms of the emanation formed in a second as disintegrate in a second—very little escapes from the solid radium. When the radium is in solution and air is bubbled through, the emanation is removed as fast as it is formed. The first stage in the disintegration of radium may then be represented:



We have spoken of this radium emanation as a gas and we must now examine the evidence for this assumption, and see what kind of a gas it is. If this assumption is correct it should have a definite spectrum, a definite density, and it should be possible to liquify it by increasing the pressure or lowering the temperature, or both. First, as to its spectrum. The emanation from a large quantity of radium was collected, freed from impurities and introduced into a spectrum tube. A wholly new set of spectral lines, unlike the lines from any known substance, was found. In the course of a few days this spectrum gradually weakened and disappeared, the spectrum of helium finally taking its place. This is of course exactly what we should expect if the emanation disintegrates with the expulsion of alpha particles, for the alpha particles are charged atoms of helium.

Rutherford succeeded in liquifying the emanation at a temperature of -65 deg. cent. at atmospheric pressure. The vapor pressure of the liquid emanation has been determined for a wide range of temperature. Thus, in this respect, it behaves as an ordinary gas.

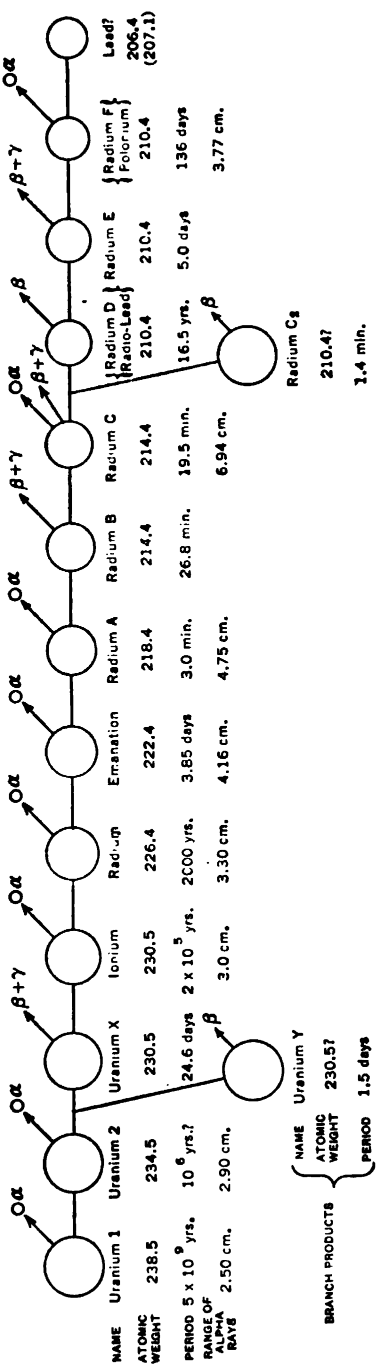
The determination of the density of the radium emanation was accomplished by Sir William Ramsay. It was a problem requiring extraordinary experimental skill for its solution. The method consisted in actually weighing a known volume of the emanation. When we consider that the weight of the emanation was of the order of $\frac{1}{1000}$ of a milligram it will readily be seen that a very accurate determination was impossible. The density was found to be 111.5 times that of hydrogen, and assuming that the emanation is a monatomic gas, this makes the atomic weight 223. The atomic weight of radium is 226; and if, then, the radium atom emits in disintegrating a single particle, whose weight is 4—the atomic weight of helium—this should leave for the atomic weight of emanation, into which the radium disintegrates, the value 222. This is in surprisingly good agreement with the number found when we remember the extremely small amount of the emanation available.

We are thus justified in looking upon the radium emanation as a gaseous element differing in no respect from an ordinary element except that its atoms are far less stable. Let us now see what becomes of the emanation after it disintegrates. We have seen that solids immersed in the emanation become temporarily radioactive. This effect is specially marked if conductors charged to a high negative potential are immersed in the emanation. Everything is exactly the same as if a solid substance were deposited on the metal surface. For example, this active deposit may be dissolved in acids, leaving the metal non-radioactive; while if the solution is evaporated to dryness the residue is found to be radioactive, its radioactivity diminishing in the same way it would have if left on the metal. The active deposit may also be removed by sandpapering the metal; the dust removed is then radioactive.

When the decay curve of this excited radioactivity is drawn, it is found not to be a simple exponential curve like the curve of decay of uranium X. Its form depends upon the time the metal has been immersed in the emanation. By analysis of the curves obtained for different times of immersion in the emanation and by measuring separately the alpha and beta ray activities, it is found that the following succession accounts for the observations. The atom of radium emanation emits an alpha particle, and becomes then an atom of a solid substance, called radium A. Radium A has a short life; in three minutes half of it has disappeared. In disintegrating, the atom of radium A emits an alpha particle and changes into radium B. This is also a solid substance disintegrating to half its value in 26.5 minutes. Radium B emits only beta particles. The mass of the beta particle is very small so that it can be neglected compared to the mass of an atom; thus the atomic weight of radium C is the same as that of radium B from which it is formed. Radium C emits both alpha and beta particles; and also gamma rays, which we have seen are to be expected when beta rays of high velocity are emitted. In 19.5 minutes half of the radium C disappears.

If only a small amount of the radium emanation was present to begin with, after a few hours the radioactivity of the active deposit becomes too small to measure. This is the result we should have if radium C on disintegrating forms a non-radioactive substance. If, however, large amounts of radium emanation are employed it is found that radium C does not end the series, but that another substance, radium D, is formed, which has a long life compared to radium C. In 16.5 years, half of the radium D changes into radium E. In this change, beta particles are emitted by the atoms of radium D. The atoms of radium E disintegrate to half their number in 5.5 days, emitting beta particles, and forming atoms of radium F. This substance emits alpha particles only, decaying to half its amount in 136 days, and is the last member of the series that we are certain about.

Radium D and radium F are of particular interest in that they have been found to be identical with radio-lead and polonium respectively—two radioactive substances found in the chemical analysis of pitchblende, which were first supposed to be independent substances. We see that they are members of the family which is formed by the disintegration of radium.



You will notice that for the last member of this family I have put down lead, with an interrogation point. The evidence for this is not conclusive, but there is enough to make it seem probable. In the first place, assuming that every time an alpha particle is emitted the weight of the atom is reduced by 4, the atomic weight of helium, the last member of the series should have an atomic weight equal to that of radium, less four times the number of alpha particles emitted in the complete disintegration. Looking at the diagram we see that there are five changes which are accompanied by the emission of an alpha particle. So the atomic weight of the last member of the series should be

$$226.4 - 4 \times 5 = 206.4$$

The element that has an atomic weight nearest this number is lead, whose atomic weight is 207. The discrepancy between these two numbers may possibly be accounted for in this way. There is some evidence that the atoms of radium *C* break down in two different ways. Some of them yield radium *D* and the others yield a substance called radium C_2. On the diagram this is called a branch product. If radium C_2 with a probable atomic weight of 210.4 disintegrates without emitting any alpha rays the final product will have the same atomic weight. As the amount of radium *C* that forms the branch product is small compared to the amount that forms radium *D*, the mean atomic weight of the two final products would be somewhat greater than 206.4 and might therefore be assumed to be equal to the atomic weight of lead. This view requires us to regard what we call lead as a mixture of atoms of two different kinds, of chemical properties so similar that no way has ever been found of separating them. There is nothing inherently improbable in this view and it is suggested as one way of identifying the final member of the radium family as lead. Indirect evidence that this is so is found in the fact that in many minerals there is a definite relation between the amounts of radium and lead present. This could hardly be explained in any way than that lead is formed trom radium.

Let us now examine the other end of the series. It has been well established that in uranium minerals there is a fixed ratio between the radium and uranium present. It is found that for every gram of uranium there is an amount of radium equal to 3.4×10^{-7} grams. A very few minerals show exceptions to this ratio; but these exceptions are satisfactorily accounted for by decompositions or chemical replacements for which there is other evidence. As uranium has the highest atomic weight of all the known elements we naturally take it as the parent element in the whole series.

We saw that the discovery leading to the disintegration hypothesis was that from uranium a substance, uranium *X*, continually produced by the uranium, could be separated by chemical means. There is evidence, however, which there is not time to give here, that uranium does not disintegrate directly into uranium *X*, but into an intermediate substance, uranium 2, the disintegration of each atom being accompanied by the expulsion of an alpha particle. We know of no method by which uranium 1 and uranium 2 can be separated. We may look upon what we call uranium as in reality a mixture of two substances, uranium 1 with atomic weight 238.5 and uranium 2, with atomic weight 234.5; the latter is present in so small a proportion as not to affect the mean atomic weight, which is that of uranium as we know it (238.5). Between uranium *X* and radium there is one member, ionium, discovered by Boltwood. The atoms of ionium disintegrate, with the expulsion of an alpha particle, into atoms of radium.

We must now see how it is possible to determine the rate of decay of the various members of the series. For those that decay rapidly—say to half their initial value in a few months or less—the rate of decay may be determined by analysing the curves obtained by measuring their radioactivity over a sufficiently long period. But this method obviously cannot be employed for those substances which decay very slowly. The period of radium may be determined in the following way. I showed in the previous lecture how the whole number of alpha particles emitted in a second by a radioactive substance can be counted. If we assume that each radium atom on disintegrating emits a single alpha particle, then the number of atoms of radium that break down in a second is equal to the number of alpha particles emitted by it in a second. The ratio of the number that break down per second to the whole number present is the constant λ in the equation

$$N = N_0 \epsilon^{-\lambda t}$$

Now Rutherford has found that the whole number of alpha particles emitted in a second by one gram of radium is 3.4×10^{10}. We now must find the number of atoms of radium in one gram. A cubic centimeter of hydrogen has a mass of 9×10^{-5} grams. If there are n molecules in a cubic centimeter of any gas, there are $2n$ atoms of hydrogen in a cubic centimeter. So that in one gram of hydrogen there are $\frac{2n}{9 \times 10^{-5}}$ atoms. In one gram of radium there will accordingly be

$$\frac{2n}{9 \times 10^{-5} \times 226.4} \text{ atoms}$$

Taking $n = 6.12 \times 10^{19}$ we find the number of atoms in one gram of radium to be

$$3 \times 10^{21}$$

Therefore

$$\lambda = \frac{3.4 \times 10^{10}}{3 \times 10^{21}} = 1.1 \times 10^{-11}$$

Now, remembering that $N = N_0 \epsilon^{-\lambda t}$, and putting $N = \frac{1}{2} N_0$ we find

$$\epsilon^{-\lambda T} = \frac{1}{2}$$

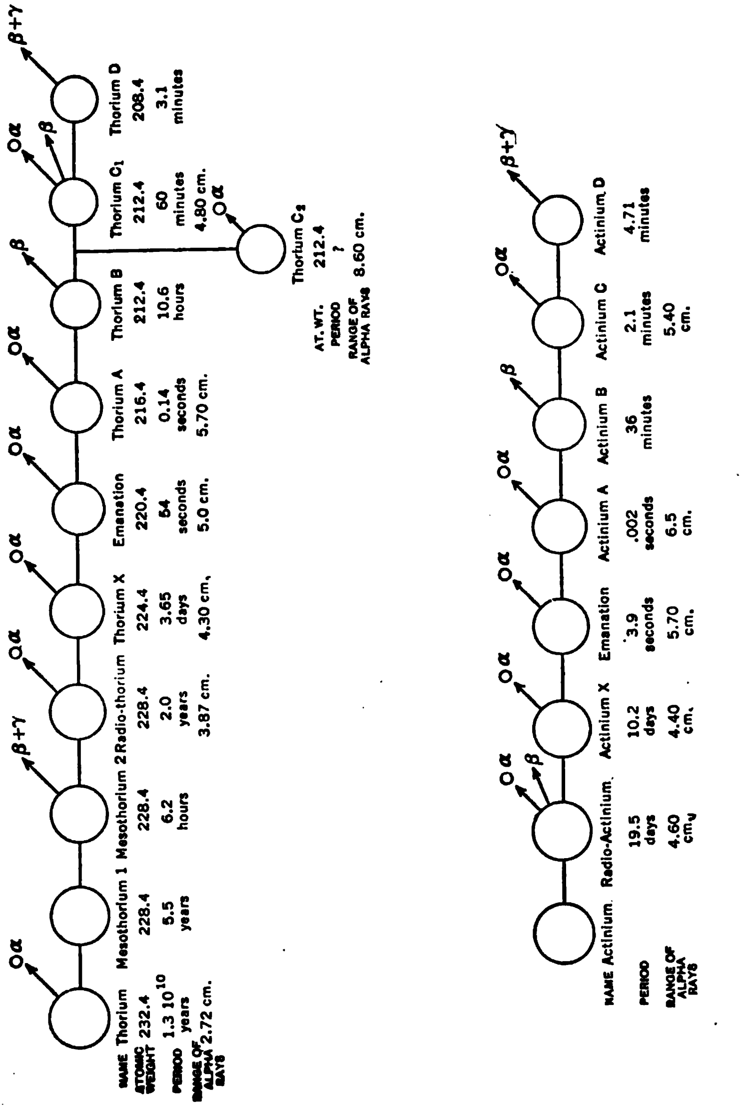
where T is the time for the substances to be half transformed.

Therefore

$$T = \frac{\log 2}{\lambda} = \frac{0.693}{\lambda}$$

For radium, therefore, $T = 6.3 \times 10^{10}$ seconds or 2000 years.

This same method may be applied to any substances which emit alpha rays that can be counted.



Since the uranium atom emits altogether three alpha particles in changing to radium, the atomic weight of the latter should be

$$238.5 - 3 \times 4 = 226.5.$$

This is in excellent agreement with the value found for it by Madame Curie.

In the diagram illustrating the radium series is given, in the last row of figures, the range of the alpha particles emitted by the various members of the series. These numbers give the distance, in air, that the alpha particles traverse before their energy is reduced sufficiently for them to lose their ionizing power. It is seen that with one exception the shortest-lived products emit alpha rays with the highest velocities. This is what one would expect if the disintegration of an atom is anything like an explosion. The shorter the life of an atom, the more violent one would expect the explosion to be, and consequently the alpha particle would be ejected with a higher velocity than if the explosion were less violent. By determining the range of the alpha particles from any product we thus have an independent way of getting an estimate of the life of the product.

There are two other known series of radioactive elements, the thorium series and the actinium series. There is not time to discuss these in detail, but accompanying this are two diagrams similar to the one for the radium series, which give our present knowledge regarding them. There is some evidence that both these series are derived from uranium; the members of both series are found in uranium minerals, which point to their derivation from uranium. It may be that they come from branch products in the main uranium—lead series. But, so far, any direct evidence for this view is lacking.

Heating Effect of Radium

We are now going to consider an important property of radioactive substances—their heating effect. We have seen that radioactive substances are continually sending off particles with a high velocity. Some of these particles are stopped in the substance itself and some in the walls of the containing vessel. As in the case of ordinary projectiles, when brought to rest, the kinetic energy of their motion is changed into heat, so we should expect a radioactive substance to keep itself at a higher temperature than its surroundings. We can easily calculate the rate of heat production in any radioactive substance as soon as we know the number, mass, and velocity of the particles emitted by it in a given time. For each particle of mass m and velocity v has an amount of kinetic energy equal to $\frac{1}{2}\ m\ v^2$. If n particles are emitted in a second the whole kinetic energy is $\frac{1}{2}\ m\ n\ v^2$, and if all these particles are brought to rest the heat produced in mechanical units is $\frac{1}{2}\ m\ n\ v^2$. To get thermal units, that is, calories per second, we have to divide by the mechanical equivalent of heat, 4.2×10^7. Let us apply this to calculate the amount of heat produced by one gram of radium when it is in radioactive equilibrium with the emanation, radium *A*, *B*, and *C*. This will be the case a week or two after preparation of the radium. The velocities of the alpha particles from the various members of the uranium family are given in the following table.

Product	Velocity of alpha particle	Kinetic energy of alpha particle
	cm. per second	ergs
Uranium 1	1.45×10^9	0.645×10^{-5}
Uranium 2	1.53×10^9	0.72×10^{-5}
Ionium	1.56×10^9	0.746×10^{-5}
Radium	1.61×10^9	0.794×10^{-5}
Emanation	1.73×10^9	0.915×10^{-5}
Radium A	1.82×10^9	1.01×10^{-5}
Radium C	2.06×10^9	1.31×10^{-5}
Radium F	1.68×10^9	0.866×10^{-5}

One gram of radium emits 3.4×10^{10} alpha particles in a second; so the number of particles emitted by one gram of radium with the three products in equilibrium with it is

$$4 \times 3.4 \times 10^{10} = 13.6 \times 10^{10}$$

The average kinetic energy of the alpha particles from these four products is 1.01×10^{-5} ergs. Therefore the kinetic energy which is transformed into heat per gram of radium per second is

$$1.01 \times 10^{-5} \times 13.6 \times 10^{10} = 13.7 \times 10^5$$

In heat units this corresponds to

$$3.26 \times 10^{-2} \text{ calories per second}$$

or to 117 calories per hour.

In this calculation two causes of heat production have been neglected. One is the absorption of the beta rays. Although their velocity is higher than that of the alpha rays, their mass is so much smaller that the kinetic energy of a single beta particle is only a small fraction of the kinetic energy of a single alpha particle. Then the kinetic energy of the recoil of the atom, resulting from the expulsion of the alpha particle, has been neglected. This may easily be calculated; for the momentum of the atom is equal in magnitude and opposite in sign to the momentum of the particle. The latter is known, as is the mass of the atom, and so the velocity of the atom is

$$mv/M$$

where m is the mass of the alpha particle (4) and M that of the atom after the alpha particle has been expelled; v is the velocity of the alpha particle. The total heating effect resulting from the recoil of the atoms is less than one per cent of the effect we have calculated. As a result, we find that the heating effect of one gram of radium a few days after its preparation is

118 calories per hour.

Now this is something that can be measured, by placing a known mass of radium in a calorimeter and observing the increase in temperature. Measurements of this kind give results very near the calculated value. In particular, one measurement of Rutherford gave as the heating effect of the radium emanation from one gram of radium, 94.5 calories per hour. The value calculated by the same method as that employed for radium is 94 calories per hour. These results therefore confirm the hypothesis that a single alpha particle is emitted from an atom when it disintegrates.

Another confirmation of the disintegration hypothesis is found in the agreement between the calculated and the measured rate of production of helium. Assuming each alpha particle to be an atom of helium, the number of atoms of helium formed in one second in one gram of radium in equilibrium with the emanation, radium *A*, *B*, and *C*, is

$$4 \times 3.4 \times 10^{10} = 13.6 \times 10^{10}$$

In one cubic centimeter of helium at a pressure of 760 mm. of mercury and at 0 deg. cent. there are

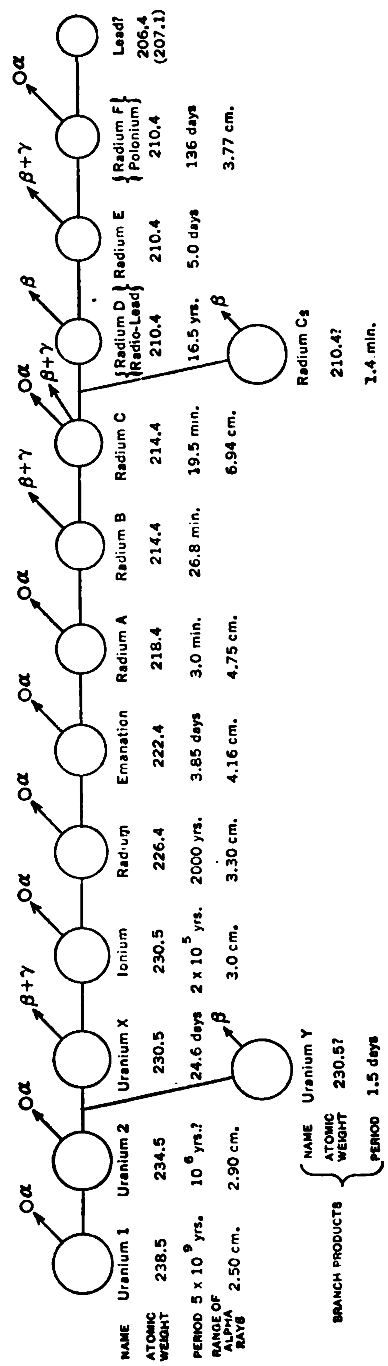
$$2.78 \times 10^{19} \text{ atoms}$$

Therefore in one year there should be produced 154 cubic millimeters of helium. Direct experiment, by Sir James Dewar, gave 169 cubic millimeters a year from one gram of radium. When one considers the difficulty of measuring the extremely small volumes of helium produced from a small amount of radium in a comparatively short time, the agreement between these numbers is surprisingly good.

Radioactivity and the Age of the Earth

The heating effect of radium and the other radioactive substances has an important bearing upon the problem of the age of the earth. You know that there has been a long-standing controversy between physicists and geologists about this problem. There are two principal methods of getting an estimate of the earth's age by physical reasoning. One method is to calculate the time required for the sun to reach its present size by contraction from an infinite sphere, radiating the heat thus produced at the rate determined by measuring the heat received in unit time on unit surface of the earth. The other method is to calculate the time it would take the earth to reach its present temperature, or rather its present rate of increase in temperature (about 1 deg. cent. in 100 feet) downwards from the surface. These methods give as the limiting age of the earth something between twenty and sixty million years. Now this is altogether too short a time to satisfy geologists, who base their reasoning partly on the fossils found in the different geological strata, and partly on the salt content of the oceans, assuming this to have been brought there from the land.

Lord Kelvin was careful to state that his low estimate of the age of the earth depended on no other source of heat energy being found. We now know that there is another source of heat energy, and that is the heating effect of radioactive substances. Nearly all rocks found on and near the surface of the earth are radioactive. The amount of radium contained in the common rocks in different parts of the world varies between wide limits. Some have a radium content as low as 1.2×10^{-13} grams of radium per gram of rock. But on the average, the surface rocks have a radium content of about 2×10^{-12} grams of radium per gram of rock; they also contain about 6×10^{-6} grams of uranium and 1.2×10^{-5} grams of thorium per gram of rock. If it is assumed that these proportions hold throughout the earth, there would result, from the heating effect of radioactive substances alone, more than ten times the amount of heat that is necessary to account for the present temperature gradient of the earth. So it is probable that the interior of the earth contains far less radium than is indicated by the surface. As a result of the discoveries



You will notice that for the last member of this family I have put down lead, with an interrogation point. The evidence for this is not conclusive, but there is enough to make it seem probable. In the first place, assuming that every time an alpha particle is emitted the weight of the atom is reduced by 4, the atomic weight of helium, the last member of the series should have an atomic weight equal to that of radium, less four times the number of alpha particles emitted in the complete disintegration. Looking at the diagram we see that there are five changes which are accompanied by the emission of an alpha particle. So the atomic weight of the last member of the series should be

$$226.4 - 4 \times 5 = 206.4$$

The element that has an atomic weight nearest this number is lead, whose atomic weight is 207. The discrepancy between these two numbers may possibly be accounted for in this way. There is some evidence that the atoms of radium *C* break down in two different ways. Some of them yield radium *D* and the others yield a substance called radium C_2. On the diagram this is called a branch product. If radium C_2 with a probable atomic weight of 210.4 disintegrates without emitting any alpha rays the final product will have the same atomic weight. As the amount of radium *C* that forms the branch product is small compared to the amount that forms radium *D*, the mean atomic weight of the two final products would be somewhat greater than 206.4 and might therefore be assumed to be equal to the atomic weight of lead. This view requires us to regard what we call lead as a mixture of atoms of two different kinds, of chemical properties so similar that no way has ever been found of separating them. There is nothing inherently improbable in this view and it is suggested as one way of identifying the final member of the radium family as lead. Indirect evidence that this is so is found in the fact that in many minerals there is a definite relation between the amounts of radium and lead present. This could hardly be explained in any way than that lead is formed from radium.

Let us now examine the other end of the series. It has been well established that in uranium minerals there is a fixed ratio between the radium and uranium present. It is found that for every gram of uranium there is an amount of radium equal to 3.4×10^{-7} grams. A very few minerals show exceptions to this ratio; but these exceptions are satisfactorily accounted for by decompositions or chemical replacements for which there is other evidence. As uranium has the highest atomic weight of all the known elements we naturally take it as the parent element in the whole series.

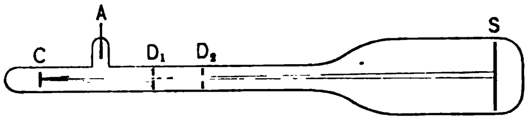
We saw that the discovery leading to the disintegration hypothesis was that from uranium a substance, uranium *X*, continually produced by the uranium, could be separated by chemical means. There is evidence, however, which there is not time to give here, that uranium does not disintegrate directly into uranium *X*, but into an intermediate substance, uranium 2, the disintegration of each atom being accompanied by the expulsion of an alpha particle. We know of no method by which uranium 1 and uranium 2 can be separated. We may look upon what we call uranium as in reality a mixture of two substances, uranium 1 with atomic weight 238.5 and uranium 2, with atomic weight 234.5; the latter is present in so small a proportion as not to affect the mean atomic weight, which is that of uranium as we know it (238.5). Between uranium *X* and radium there is one member, ionium, discovered by Boltwood. The atoms of ionium disintegrate, with the expulsion of an alpha particle, into atoms of radium.

We must now see how it is possible to determine the rate of decay of the various members of the series. For those that decay rise to the suspicion that the observed radioactivity of ordinary materials may result from the presence of a trace of radium. But in the case of the three metals mentioned, the fact that their observed activity seems to be independent of their source or method of preparation, leads to the view that they are themselves really radioactive substances. And, besides, the rays emitted by them have a character distinct from those emitted by any of the other known radioactive substances. Great care is needed in making experiments to determine the presence of minute traces of radioactivity; for laboratories in which radium emanation has been allowed to escape soon become contaminated by the active deposit that forms on the walls and on all solid objects, so that there is always the possibility of getting spurious effects.

In this lecture I have attempted to show that our old idea of the atoms of the elements as necessarily permanent, must be given up. Instead, we must look upon the atoms of the radioactive elements as unstable dynamical systems. And, probably, the atoms of all elements must be thought of in the same way. Those which appear to be non-radioactive may be thought of as formed of relatively stable atoms, disintegrating at too slow a rate to make it possible, by any means we now know, to detect radioactive effects from them. This view involves no violation of the principle of the conservation of energy. The energy emitted by the radioactive substances in the three kinds of rays is drawn from the internal supply of energy of the atoms.

LECTURE III

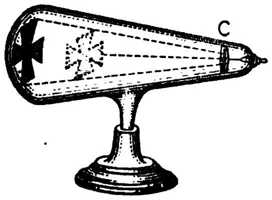
The purpose of this lecture was to illustrate by experiment some of the phenomena described in the previous lectures, and to give a little fuller account of some of the subjects touched upon. The first experiments shown had for their object the illustration of the chief characteristics of the cathode rays. By means



of an induction coil an electric discharge was maintained in a tube of the form illustrated herewith. *C* is the cathode and *A* the anode. D_1, D_2 are two diaphragms with small holes which serve to limit the beam of cathode rays, so that they produce a spot of light on the phosphorescent screen, *S*. By bringing a magnet near the tube the spot of light was made to move over the phosphorescent screen. The direction in which the spot moved was shown to be what one would expect if the cathode stream is a flow of negatively charged particles, travelling from left to right. For example, when the north pole of the magnet was brought near to, and above the tube, the spot moved forwards; when the north pole of the magnet was brought in front of the tube the spot moved down. In its undeflected position the spot of light was a small circular area. In its deflected position it had broadened out to a band. This was accounted for by the differences in the velocities of the particles. Recalling the expression for the deflection of a charged particle in a magnetic field

$$H\frac{e}{m} = \frac{v}{\rho}$$

it is seen that the faster moving particles are less deflected than the slow moving particles. As the cathode particles are shot out with all velocities within a certain range, the result is to broaden the spot of light into a band when a magnetic field is applied.



A tube of different form, as in the accompanying illustration, was next shown, which contained an obstacle, in the form of a cross which could be raised or lowered. When it was down, the cathode particles, striking the glass, produced a greenish-yellow phosphorescence. It was the similarity between this light, and the light emitted by certain uranium compounds after exposure to sunlight, that led Becquerel to make the first discovery of radioactive phenomena.

When the cross was raised its shadow was seen as a dark region on the glass, since the obstacle prevented the particles from striking the glass.

Next were shown experiments illustrating the conductivity of air traversed by the rays from radioactive substances. The electric currents passing through the air were measured by the rate of discharge of a parallel plate condenser when a radioactive substance was spread on the lower plate. And this was given by the rate of fall of potential of the insulated plate, when the other plate was charged to a definite potential. The current is given by

$$i = C\frac{dV}{dt}$$

where C is the capacity of the condenser, together with that of the electrometer and the connecting wire. In the experiment this capacity was about 100 in electrostatic units. The electrometer had a sensitiveness such that a potential difference of one volt produced a deflection of about 1000 divisions on the scale. Thus when the electrometer needle moved ten divisions a second, the current flowing through the air was roughly 10^{-12} amperes. It is feasible to measure, by this method, currents as low as 10^{-16} amperes.

Some uranium oxide was spread on the lower plate of the condenser and the motion of the spot of light over the scale determined. Then some of the powdered mineral pitchblende was tested and the spot of light moved about four times as fast, indicating that the radioactivity of pitchblende is about four times that of uranium oxide. A tube of radium, lent by Dr. Kunz, was then placed in the condenser and the spot of light rapidly moved off the scale. In this case the conductivity produced was due to the beta and gamma rays, since the alpha rays were absorbed in the containing tube. With an uncovered radioactive substance it is the alpha rays that produce the greater part of the conductivity.

The conductivity of air containing radium emanation was then shown. For this, a brass cylinder, connected to a 120-volt direct-current circuit, was employed. Insulated from the cylinder, and connected to the electrometer, was a wire inside the cylinder. When radium emanation was introduced into the cylinder, by bubbling air through a solution of radium, and blowing it into the cylinder, a very large increase in the rate of motion of the electrometer needle was seen.

Finally the phenomenon of excited or induced radioactivity was illustrated. A wire, charged to a negative potential, was suspended in a cylinder containing radium emanation, for two or three minutes, and then taken out, and its activity measured in the condenser used in the first experiments on radioactivity. It was found that the wire had a definite activity to begin with, and observations a few minutes apart showed that its radioactivity gradually decayed.

There are some points in connection with the previous lectures about which more should be said. We have seen that a wire placed in radium emanation becomes radioactive itself, and this was explained as caused by the deposition on the wire of solid particles of radium A resulting from the expulsion of an alpha particle from the atom of radium emanation. As the alpha particle is positively charged, we should expect the atoms of radium A to be negatively charged, and therefore to tend to concentrate on positively charged surfaces. Exactly the opposite is found to be the case. More of the active deposit forms on a negatively charged surface than on a positively charged surface, other conditions being the same. This difficulty has been cleared up by the discovery of what are called "delta" rays. These are negatively charged particles, which travel at too low a velocity to produce any ionizing effects, and which always accompany the emission of alpha particles. They were found in this way. Suppose a radioactive substance, like polonium, which emits alpha rays, but no beta rays, is placed opposite a metal plate, in a very high vacuum, so that the alpha particles will strike the metal plate without, on the way, hitting any air molecules to ionize. We should expect the metal plate to receive a positive charge—the product of the charge of the alpha particle by the number of particles that strike it. It is found, however, when the experiment is made, that the metal plate does not get positively charged. However, by applying a weak magnetic field in a direction parallel to the plate, the latter receives a positive charge. This is exactly the effect that would be produced if, with the positively charged alpha particles, negative particles are emitted. These delta rays travel with so low a velocity as not to produce any appreciable ionization; they are of the same nature as the beta particles except that their velocity is so small that they are very readily deflected by a magnetic field. If, then, when an atom of radium emanation disintegrates, with the expulsion of an alpha particle, it also expels a sufficient number of delta particles, the atom of radium A will be positively charged, and will therefore travel to the negative electrode.

The law of decay that has been found experimentally for a single radioactive substance,

$$I = I_0 \epsilon^{-\lambda t}$$

we have seen, can be deduced from the assumption that the number of atoms that disintegrate in a second is proportional to the whole number present. This may be looked at from another point of view. We may regard each of the atoms of a radioactive substance as equally likely to disintegrate. Not knowing the circumstances that cause an atom to disintegrate, and not being able to follow each atom throughout its whole life, we have to make use of the method of the calculus of probabilities in its simplest form. The calculus of probabilities is, perhaps, the most powerful mathematical method we have for dealing with problems involving atoms or molecules. Its justification lies in its success in accounting for many phenomena. The atoms all being equally likely to disintegrate, the number that do so will be proportional to the whole number. We might look upon the atoms as kernels of corn; the disintegration of an atom as the popping of a kernel. The illustration is not perfect, because as the corn gets hotter the rate of popping increases. But there is a stage which lasts for some minutes, at which the popping occurs fairly regularly. Now it is obvious that with only a few kernels to begin with, there would be no regularity in the popping. One kernel would pop, then several seconds might elapse and a number of them would pop almost simultaneously, and then there might be a fairly long interval before another one popped. But with a very large number of kernels to begin with, the irregularities are smoothed out. In the steady state referred to, as many kernels pop in one second as in the succeeding one. Thus with a very large number of kernels, probability becomes certainty. So it is in the familiar illustration of chance as applied to drawing a certain card from a pack. The probability of drawing a certain card at one trial is the ratio 1/52. But every one knows that it rarely happens that out of 52 trials the given card is drawn once and only once. It may not be drawn at all, or it may be drawn many times. If, however, there be a very large number of observers, each with a pack of cards, all drawing together, the likelihood that one out of every 52 will draw the given card becomes very much increased; so that with an enormous number of observers, this probability becomes a certainty.

The method, described in the first lecture, of counting the number of alpha particles emitted by a radioactive substance, furnishes a good illustration of this point. If the opening, through which the particles enter the ionization chamber, is made very small, then the ratio of the whole number that enter to the whole number emitted, is very small. The result is that the particles do not enter at equal time intervals. One particle enters, some seconds may elapse, and then two or more particles may enter almost simultaneously. So, to get definite results observations must be taken during a long time; the longer the time during which observations are made, the more accurate are the results in giving the rate at which the particles enter.

Now even in what we call infinitesimal amounts of matter there are enormous numbers of atoms. Therefore the probability that a certain fraction of them will disintegrate in a given time becomes a certainty that they will do so.

The particular fraction is the quantity λ which is characteristic of each substance. For a substance which is not at all radioactive λ is zero. The greater λ the more radioactive the substance. We have seen that the time required for a substance to disintegrate to one-half its initial value is related to λ by the equation

$$T = \frac{0.693}{\lambda}$$

Another interpretation of this important quantity is that its reciprocal tells us the average life of an atom. For if we start with N_0 atoms, the number at any time, t, will be

$$N = N_0 \epsilon^{-\lambda t}$$

The average life of an atom is therefore

$$\frac{1}{N_0} \int_0^{\infty} N_0 \epsilon^{-\lambda t} dt = \frac{1}{\lambda}$$

The meaning of "radioactive equilibrium" may require a little more explanation. Let us consider what is meant by saying that in a mineral containing uranium, equilibrium exists between the uranium and all its products. By referring to the chart which gives a representation of the whole known series of disintegrations, we see that uranium has a far longer life than any of its radioactive products. If we began with uranium and could remove all of its products from it, the latter would begin to form at once, and after a long enough time had elapsed, as many atoms of one of the products would form, in a given time, as disintegrated in the same time. Uranium would then be in equilibrium with its products. Since the uranium itself is breaking down, and, so far as we know, is not being continually formed from any element of higher atomic weight, the uranium itself would not be in equilibrium.

If there are two successive products, 1 and 2, so that 2 is formed by the disintegration of 1, the rate of increase of atoms of 2 will be given by

$$\frac{dN_2}{dt} = \lambda_1 N_1 - \lambda_2 N_2$$

for $\lambda_1 N_1$ is the number of atoms of 1 that break down a second; that is, the rate of formation of atoms of 2; $\lambda_2 N_2$ is the rate of disintegration of atoms of 2. So the difference gives the net rate of increase of 2. If there is radioactive equilibrium between 1 and 2, $dN_2/dt = 0$; that is, the number of atoms of 2 that form from 1 per second must be equal to the number of atoms of 2 that disappear per second. So that

$$\lambda_1 N_1 = \lambda_2 N_2$$

or

$$N_2 = \frac{\lambda_1}{\lambda_2} N_1$$

If A_1 and A_2 are the atomic weights of the two products, the mass of 1, M_1, in equilibrium with a mass M_2 of 2 will be

$$M_2 = \frac{\lambda_1}{\lambda_2} \frac{A_2}{A_1} M_1$$

or if T_1 and T_2 are the times for 1 and 2 to be half transformed, we can write this

$$M_2 = \frac{T_2}{T_1} \frac{A_2}{A_1} M_1$$

By the use of this equation we can find the amount of any product in equilibrium with its parent. Suppose we have a mineral containing one gram of radium. The amounts of the various products in equilibrium with this will then be given by the successive applications of this equation. In this way we can calculate the following table:

Name	A	T	M grams
Radium	226.4	2000 yr.	1
Emanation	222.4	3.85 days	$5.2\ 10^{-6}$
Radium A	218.4	3 min.	$2.8\ 10^{-9}$
Radium B	214.4	26.8 min.	$2.4\ 10^{-8}$
Radium C	214.4	19.5 min.	$1.8\ 10^{-8}$
Radium D	210.4	16.5 yr.	$7.9\ 10^{-3}$
Radium E	210.4	5 days	$6.5\ 10^{-6}$
Radium F	210.4	136 days	$1.8\ 10^{-4}$

We see that in a mineral containing one gram of radium we must expect to find 1.8×10^{-4} grams of polonium (radium F). The amount of the latter which can be obtained is therefore extremely small.

It is found that in a mineral containing one gram of uranium there is an amount of radium equal to 3.4×10^{-7} grams. So, in order to find the amount of any of the products of radium in equilibrium with one gram of uranium, we need only multiply the numbers in the last column of the above table by 3.4×10^{-7}.

We must believe that each member of the uranium series, and also each member of the thorium and actinium series, is a definite chemical element, with definite chemical properties and a definite spectrum. The reason why the properties of so few of these elements are known is now seen to be the result of the very minute amounts of them that are available. In the uranium series, we have fairly complete knowledge of the chemical properties and the spectra of uranium, radium, and the emanation. There is some evidence, also, of a definite spectrum belonging to radium F (polonium). Many of the chemical properties of the other members of this series are known, but this knowledge is by no means complete.

A great deal of interest has been aroused in the medical applications of radium. So far as we know, there is no other practical use for it. The rays emitted by radium produce burns which are similar to those produced by ultra-violet light and the Röntgen rays, and it is to be expected that they should possess a therapeutic value. I am not able to speak with any authority on this subject, but it is a fact that a good deal of work is being done—particularly in England and on the continent—along these lines, and announcements of success in the application of radium and the radium emanation are frequently made. Progress in this direction is retarded by the small amount of radium which is available. The principal source of radium is the mineral pitchblende, and the chief known deposits of this mineral are owned by the Austrian government, which also controls the manufacture of radium from it.

In conclusion I am going to give an approximate calculation of the amount of energy set free when one gram of uranium is transformed into the last member of the series, which we may take to be lead. In this transformation eight alpha particles are emitted, with an average velocity of

$$v = 1.68 \times 10^{9} \text{ cm. per second}$$

The mass of an alpha particle is four times the mass of an atom of hydrogen, or $m = 4 \times 1.7 \times 10^{-24}$.

Thus the average kinetic energy of the alpha particles emitted by uranium is $\frac{1}{2} m v^2$, or

$$9.6 \times 10^{-6} \text{ ergs}$$

and the kinetic energy of the eight alpha particles is therefore

$$7.7 \times 10^{-5} \text{ ergs.}$$

This is the energy set free when one atom of uranium breaks down into lead. In one gram of uranium there are

$$\frac{1}{238.5 \times 1.7 \times 10^{-24}} = 2.5 \times 10^{21} \text{ atoms.}$$

Therefore the energy set free when one gram of uranium turns to lead is $7.7 \times 10^{-5} \times 2.5 \times 10^{21} = 1.9 \times 10^{17}$ ergs.

Now suppose it were possible enormously to increase the rate at which this change takes place. Instead of millions of years, suppose it took place in one second. The rate at which work would be done is then

$$\frac{1.9 \times 10^{17}}{7.46 \times 10^{9}} = 2.5 \times 10^{7} \text{ horse-power.}$$

Uranium thus has an enormous store of energy contained in its atoms. We must believe that all the elements contain equivalent amounts of energy in their atoms. But no means is known of drawing upon this supply of energy, nor even of hastening its liberation from those elements which do disintegrate spontaneously.

It is the province of science to discover the laws of nature; the province of engineering is to apply them. And therefore we must leave to the engineers the problem of making use of this enormous store of energy concealed in the atoms of all matter.

LECTURE IV

I am going to speak of a discovery connected with radioactivity which is of very far reaching importance. It has to do with the dependence of the mass of a corpuscle, or electron, upon its velocity. The bare statement of this discovery is startling when viewed from the standpoint of our customary ideas about matter. We are accustomed to look upon mass as an unalterable attribute of matter. Newton's laws of motion—which are the foundation upon which the science of mechanics rests—are based upon the idea of the constancy of mass.

The conception we have formed up to this point of a corpuscle —or an electron—is that it is a particle of matter charged with electricity. This conception involves two unknown quantities—matter and electricity. We all think we know what matter is. We are so familar with matter in its various forms that we are inclined to think it needs no explanation. Of electricity, on the other hand, we think we know less. Familiarity with electric phenomena comes into our lives much later than familiarity with matter, so that we feel that electricity is a mystery compared to matter. It is worth considering very briefly whether this is so or not—whether matter is really a simpler conception than electricity. According to chemists we have to assume some eighty or ninety different kinds of matter—the different chemical elements —so that matter is not a simple conception, by any means. But more than this, for a material view of the universe we have to assume the existence of another kind of matter which differs in its properties from any kind of matter that we are familiar with—the ether. It is true that Lord Kelvin's model for an ether formed of interlocking cog-wheels does help us in realizing the possibility of the existence of a substance with the apparently contradictory properties that the ether must have to fulfill its requirements. And, from another point of view, the attempt has been made to conceive of the atoms of the various chemical elements as built up of the ether. The best known of these hypotheses is the vortex-atom hypothesis of Lord Kelvin. Assuming the ether to be a perfect fluid, a vortex ring formed in it would have the one essential quality we had always believed the atoms to have, absolute permanency. But, with the discoveries in radioactivity, that is no longer an essential, as we have seen. The difficulties in the mathematical analysis involved in working out to its conclusion this hypothesis are too great for our present methods, and so it has remained merely a suggestion. As for the problem of interpreting electricity in terms of matter, that, too, has remained unsolved. We may speak of an electric charge as a singular point in the ether—a center of strain—but that is not an easy conception to form. And then again we have the baffling problem of gravitation. We know no more today why the apple falls than Newton did.

What I wish to emphasize in all this is, that the attempt to reduce the physical universe to its lowest terms in the form of matter has not been very successful, and while I believe that much more progress can and will be made in this direction, for the present we seem to have reached a point at which we do not know where to turn to make this progress.

Suppose, then, that of our two unknown quantities in the conception of an electron, matter and electricity, we try to explain matter in terms of electricity. For this purpose electricity is a conception that we do not need to explain. It is our fundamental conception from which we are going to attempt to derive matter and the physical universe in general. It is difficult to make myself clear on this subject—mainly because it is not easy for me to conceive of electricity not associated with matter. It is hard to form a conception of anything being more fundamental than matter. But for the purpose of building up a logical scheme for the universe we must try not to let our inherent prejudices blind us to accepting another possibility. And by continual thinking along these lines the time may come when electricity will seem just as simple a conception to us as matter does now.

The goal towards which science aims is the reduction of the physical universe, and all the processes going on in it, to the lowest terms, and for this object it makes no difference whether we take electricity or matter as fundamental. If we find that we can get a consistent scheme for interpreting all the universe in terms of electricity, then electricity will be the fundamental conception —and when that is done it will be time enough to see whether we can go back of our conception of electricity. I do not wish to be understood as promising to exhibit to you any such complete scheme, but I shall try to make clear to you the trend of a large part of modern physics.

The smallest electric charge that has ever been determined is the charge on an electron—or corpuscle. And the smallest mass that has ever been determined is the mass of an electron or corpuscle. Our first question, then, is, can we interpret the mass of a corpuscle in terms of its electric charge?

Suppose, then, that we have what we may call a particle of electricity. Two such particles will repel each other with a force inversely as the square of the distance between them. We cannot make the experiment of measuring the force between two particles of electricity—we can only measure the force between two bits of matter that are charged with electricity. But we first of all make the hypothesis that the force which we can measure is a force between the electric charges. Now merely to say that there is a force between two particles of electricity does not satisfy us. Our minds are so constituted that we must form a picture of something which will transmit this force. We cannot, to be consistent with the view we are now considering, assume a material medium—an ether—pervading all space. For that is equivalent to assuming a kind of matter as a fundamental conception—and we are going to try to get rid of that idea. So we must imagine another way of forming a mental image of the action between the two particles of electricity. Let us, therefore, follow Faraday in assuming that every particle of electricity carries with it lines of force. What these lines of force are we cannot say any more than we can say what electricity is. They are to be for our theory just as fundamental a conception as electricity itself; they are a part of the electric charge. It was proved by Maxwell that all the forces between charged bodies can be interpreted as resulting from a tension along the lines of force and a repulsion perpendicular to them. We shall therefore assume that the lines of force carried by every particle of electricity have this property—of being in a state of tension, like so many stretched elastic bands—and also of repelling each other sideways.

Let us now consider a corpuscle in motion along a straight line. We first suppose that the velocity of the corpuscle is small compared to the velocity of light. Under this condition the lines of force will end on the corpuscle uniformly in all directions. As we cannot suppose that a finite number of lines of force end in a mere mathematical point we shall suppose the corpuscle to be a sphere of radius a, with the lines of force along the radii of the sphere. This leads to a very serious difficulty. If we regard a corpuscle as a very small, but finite, volume of electricity, what is it that keeps it together? If we apply what we know to be the force between two charged particles of matter to the elements into which the finite volume of a corpuscle may be divided, these elements will repel each other, and the corpuscle will fly apart. But since all we really know anything of is the force between particles of matter containing very many corpuscles, we are not necessarily obliged to assume the same law of force between the elements of a corpuscle. An electric charge in motion is an electric current. An electric current produces a magnetic force, and by Ampere's law the strength of the magnetic force is at any point equal to

$$H = \frac{e\, v \sin \theta}{r^2}$$

where e = charge, v its velocity, r the line from the instantaneous position of the charge to the point at which the magnetic force is required, θ the angle between r and v. Now it is known that the whole amount of work we have to do in order to set up a magnetic field is

$$W = \frac{1}{8\pi} \int\int\int H^2\, dx\, dy\, dz$$

As we suppose the corpuscle to be moving with a velocity small compared to that of light, everything will be symmetrical about it, and we may write this expression

$$W = \frac{e^2 v^2}{8\pi} \int_0^{2\pi} d\varphi \int_0^{\pi} \sin^3 \theta\, d\theta \int_a^{\infty} \frac{dr}{r^2} = \frac{e^2 v^2}{3a}$$

In terms of mass, the amount of work we would have to do to get a particle of mass, m, moving with velocity v, is

$$W = \frac{1}{2} m v^2$$

Thus we must put

$$\frac{1}{2} m v^2 = \frac{1}{3} \frac{e^2}{a} v^2$$

$$m = \frac{2}{3} \frac{e^2}{a}$$

We have therefore interpreted the mass of a corpuscle in terms of its electric charge.

It should be said that there are other expressions for the mass of a corpuscle in terms of its charge which differ from the one we have obtained. The difference arises from the conception we form of a corpuscle. But they are all of this same form—proportional to the square of the charge and inversely proportional to a length, which we may call the radius of the corpuscle.

The expression we have just obtained holds only when the velocity of the corpuscle is small compared to the velocity of light. When the velocity is comparable to that of light the lines of force are no longer symmetrical with respect to the corpuscle and so our simple method of calculating the energy of the motion can no longer be applied. I fear you will not care to follow me through the rather involved mathematics that is required to work out the case of a corpuscle travelling with high velocity. There are in fact several different theories arising from different conceptions of a corpuscle. But all theories agree in showing that what we have defined as the mass of a corpuscle increases very rapidly with its velocity, and approaches infinity when the corpuscle has a velocity equal to that of light. In other words, it would require an infinite force to get a corpuscle moving with the velocity of light.

It is here that we must appeal to experiment to test the validity of these views. We have seen how it is possible to measure the ratio e/m for the corpuscles, and v, their velocity, by deflecting them from their straight line paths in magnetic and electric fields. Radium emits corpuscles—the beta rays—and there is a wide range in their velocity of emission. Experiments to show a connection between e/m and v for the beta particles from radium were first made by Kauffmann. The experiments have been repeated by others, using also the cathode particles in vacuum tubes. All these experiments show that the ratio e/m decreases with increasing velocity of the particles. For example, Kauffmann found that for a corpuscle travelling with a velocity 2.83×10^{10} cm. per second the value of e/m was 0.62×10^7, while for a corpuscle having the smaller velocity 2.36×10^{10} cm. per second, $e/m = 1.31 \times 10^7$. In other words e/m was more than halved for an increase in velocity of only 20 per cent. Now on the view that electricity is our fundamental conception we must suppose that e, the charge of the corpuscle, remains constant, so that the decrease of e/m with increasing velocity means an increase of the mass of the corpuscle as its velocity increases. This is what theory demands, but the experimental results that have so far been obtained are not certain enough to enable us to decide absolutely in favor of any one of the various theories that have been developed to account for these effects. But we can say that it appears probable that the mass of a corpuscle or electron can be interpreted as resulting from the motion of an electric charge.

Before leaving this subject something should be said about another possible explanation of the increase in mass of a corpuscle with its velocity. That is a proved experimental result, unless we are willing to suppose that the electric charge diminishes with increasing velocity. What is measured is the ratio e/m; this is found to diminish with increasing velocity, so that either the mass increases or the charge decreases. Well, now, in no view of matter—whether we take matter or electricity as a fundamental conception—is there anything else that would lead us to a possible connection between an electric charge and its velocity. We have seen that there is such a connection between mass and velocity in the view we have described, and there is also a connection between mass and velocity in our older view of regarding matter as the fundamental conception. This connection can be illustrated by a very simple analogy. Suppose we have a solid sphere whose mass is m, moving in a fluid. The fluid is supposed to be perfect, so that there is no loss of energy in friction. The work we would have to do to get the sphere moving with velocity v is $1/2\ m\ v^2$ if there were no fluid. But the sphere sets the fluid in motion, pushing it out in front. The calculation of the energy of this fluid motion is one of the simple problems of hydrodynamics; the result is that the energy of the fluid motion is $\frac{1}{2}m_1 v^2$, where m_1 is the mass of a volume of fluid equal to one-half the volume of the sphere. Everything will then be just the same as if the fluid were annihilated and the mass of the sphere increased to $m + m_1$. At rest the sphere has a mass m; its mass is effectively increased by m_1 when it moves through the fluid. Now according to this, the sphere has one definite mass when it is at rest, another definite mass when in motion and this is the same for all velocities. There is no variation of its mass as the velocity increases. If we suppose that the sphere is not a rigid but an elastic body, such a variation will be found. At rest, the fluid pressure on the sphere is equal in all directions—so that the sphere will not be deformed. In motion, the fluid pressure is unequally distributed over the sphere and it becomes flattened in the direction of motion. The work that has to be done to produce the motion of the fluid may still be written in the form $1/2\ m_1\ v^2$, but m_1 increases with the velocity—that is, the flatter the sphere gets the greater is its effective mass. On this view, then, we get the result that the mass of the sphere is effectively different for different velocities of motion—increasing with the velocity. If, then, we regard the ether as a perfect fluid, and the electron as a particle of elastic matter, we would get just the effect we wish to explain—that the mass of the electron would increase with its velocity.

This view requires the hypothesis of an ether with all of its apparently contradictory properties. But I have tried to show that as far as explaining the increase in mass with velocity we are not absolutely forced to change our inherent feeling that matter, after all, is the fundamental concept. It may be possible to develop these ideas into a consistent scheme which will be more satisfactory than a scheme which builds matter out of electricity.

Whatever view we take of the electron—whether we assume it to be a particle of matter charged with electricity, or a particle of electricity which is endowed with mass in virtue of its motion—we are forced to the conclusion that electrons have a leading part in the constitution of matter as we know it. So far as we have gone we have found electrons only in vacuum tubes when an electric discharge is passed through, and emitted by radioactive substances. But we must suppose them to have a very wide distribution. For example, when a metal surface is exposed to ultra-violet light, corpuscles are emitted that have all the properties of the electrons that we have studied. So also when metals are raised to high temperatures, electrons are emitted, to such an extent that large electric currents can be sent through the highest obtainable vacuum; the current is carried by the corpuscles shot out from a glowing metal. All these effects point to the existence of these electrons in all forms of matter. From whatever source they come, no differences have ever been found among the electrons, except a difference in velocity, which carries with it a difference in mass. Measurements of the ratio e/m for electrons from different sources, and by varying methods for velocities that are small compared to the velocity of light, do not differ from each other by more than can be accounted for in experimental errors and uncertainties.

We are thus led to the electron theory of matter which assumes that the atoms of the various chemical elements are built up, in part, of electrons. The atoms of the different elements will then differ from each other in the number and arrangement of the electrons they contain. We shall have more to say about this view of the constitution of the atom, but for the present I am going to speak of some of the consequences of such a view, first of all in relation to light.

The view that electrically charged particles play a part in light phenomena is not a new one. It had been suggested, and some of its consequences worked out, long before the discoveries that led to our knowledge of the actual existence of electrons. Now that the existence of the electron or corpuscle has been made certain, its charge and mass determined, we can use the electrons in optical theory with much more confidence than before. We know that the various chemical elements are characterized by having definite spectra. That is, the atom of any chemical element may be supposed, when it is made, by whatever means, to emit light, to emit light of only certain definite frequencies of vibration. The conception of an atom as an elastic solid, and of the frequencies of the light emitted by it as the frequencies of the free vibrations of an elastic solid, has never led to results at all satisfactory in giving vibrations of the frequencies that we have to account for. Let us see what an electron view of the atom will lead to. All the electrons have the same electric charge, and according to our conventions we call the charge negative. Calling it negative is of course of no significance; it is all a matter of the convention that when a piece of glass is rubbed with silk the glass is called positively, and the silk negatively, charged. And it is found that the electron has a charge of the same nature as the charge on the silk after being rubbed on the glass. Now the idea of an atom formed of negatively charged particles will not work. For the negative electrons are going to repel each other and so we have got to imagine something to hold them together to form an atom. We are thus bound to introduce another fundamental conception along with our fundamental conception of electricity, which, we see, corresponds to negative electricity. This new conception is positive electricity. An element of positive electricity we know nothing of, in the sense that we have a knowledge of an element of negative electricity—the electron. At one time it was thought that the alpha particle might be considered as such. We know that the alpha particle carries a positive charge; but now we know that it is a charged atom of helium and so has a structure comparable in complexity with other atoms, that is, is built up, at least in part, of electrons, so that the alpha particle cannot be regarded as a fundamental unit of positive electricity. We are therefore forced to assume some special form for the positive electricity and examine the consequences of our assumption.

Let us therefore take positive electricity distributed uniformly throughout a sphere whose size is comparable with the size of an atom; *i.e.* having a radius of the order of 10^{-8} cm. The simplest model for an atom will then be this sphere of positive electricity with a single electron inside it. In order that the atom may be neutral, we must assume the whole positive charge equal to the negative charge. If we call a the radius of the sphere and r the distance of the electron at any instant from its center, the force drawing the electron towards the center is equal to

$$\frac{e^2 r}{a^3}.$$

Now the motion of a particle in a circle about a center of force varying directly as the distance is known to be stable, and the time required for the electron to go round the circle once can easily be found.

The equation of motion of the electron is

$$\frac{d^2r}{dt^2} = -\frac{e^2 r}{m a^3}$$

This is the same equation that we have for the small vibrations of a simple pendulum. The time of complete vibration is therefore

$$T = 2\pi\sqrt{\frac{m a^3}{e^2}}$$

Now we have taken

$$a = 10^{-8}$$

we know that

$$\frac{e}{m} = 1.8 \times 10^7 \text{ in electromagnetic units, or}$$

$$5.4 \times 10^{17} \text{ in electrostatic units}$$

$$e = 4.7 \times 10^{-10}$$

so that

$$T = 4 \times 10^{-16} \text{ seconds}$$

wave length $= T \times 3 \times 10^{10} = 1.2 \times 10^{-5}$ cm.

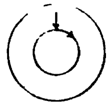
This is of the order of magnitude of the wave lengths we are familiar with in ordinary light. An electron circulating about inside a sphere of positive electricity would thus give rise to electromagnetic waves of the nature of light waves. It must not be thought that this model for an atom is anything more than a mere suggestion; for one thing, the energy of the electron would be radiated away in the form of light energy altogether too rapidly to allow us to consider this an actual atom. And such an atom would have but one frequency of vibration—only one spectral line—but it is certainly of interest that by such a simple mechanism we can get a wave length of light which is comparable to those that we know are present in ordinary light.

THE ZEEMAN EFFECT

Of all the applications of the electron hypothesis to optical effects, the most striking success has been in explaining the so-called Zeeman effect. Suppose we have, as a source of light, an incandescent gas, and examine the light through a spectroscope. A number of bright lines are seen, forming the spectrum of the particular gas employed. Now let the incandescent gas be placed in a magnetic field by placing the tube containing it between the poles of a magnet. It is found that, in general, the spectral lines are split up—where there was a single line with no magnetic force acting, on exciting the magnet this line has split up into a number of others. Spectroscopes of very high resolving power have to be used in order to make the distance between the lines appreciable. Some spectral lines split up in a very simple way—either into three lines or two lines, depending upon whether the direction of the magnetic force is perpendicular or parallel to the line of sight. Our simple model of an atom serves admirably to explain this phenomenon. To see how this is let us suppose that we have an electron describing a circular path about the center of an atom. Let it make n revolutions per second. On our simple view of an atom of positive electricity containing a single electron we have seen that

$$n = \frac{1}{T} = 2.5 \times 10^{-17}$$

Now suppose a magnetic force acts on the atom. The electron in motion acts as a current element and therefore the magnetic force H exerts a force on the moving electron which is perpendicular to both the direction of the magnetic force and the direction of motion of the electron. In the accompanying illustration let the outer circle represent the section of



the atom, and the inner circle the path of the electron, the arrow showing the direction of its motion. Suppose the magnetic force is directed up from the plane of the paper. Then there will be a force, acting on the electron, tending to drive it towards the center of the atom. This force is $e\,v\,H$, while the force drawing the electron to the center resulting from the uniform positive charge of the atom is $\frac{e^2 r}{a^3}$.

Together, these must give the centrifugal force $\frac{m\,v^2}{r}$. Hence

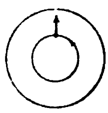
$$\frac{e^2 r}{a^3} + e\,v\,H = \frac{m\,v^2}{r}$$

But $v = n_1\,2\,\pi\,r$, where n_1 is the number of vibrations in one second under the influence of the magnetic field.

Hence

$$\frac{e^2}{a^3} + 2\,\pi\,e\,H\,n_1 = 4\,\pi^2\,m\,n_1^2$$

For an electron moving in the opposite direction, as illustrated herewith, we get in a similar manner



$$\frac{e^2}{a^3} - 2\,\pi\,e\,H\,n_2 = 4\,\pi^2\,m\,n_2^2$$

Subtracting these two equations, we get

$$n_1 - n_2 = \frac{e\,H}{2\,\pi\,m}$$

The difference in the number of vibrations per second for the two extreme lines into which the single line is split in the magnetic field is thus

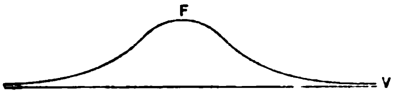
$$\frac{e\,H}{2\,\pi\,m}$$

and this may be measured optically. Knowing H, the strength of the magnetic field, the ratio e/m may be found. And the value for this ratio that is obtained in this way, using a number of different sources of light, and a number of different spectral lines, is so nearly the same as the value of e/m determined by measurements on cathode particles in vacuum tubes, beta rays from radioactive substances, the corpuscles emitted by metals when exposed to light, or when heated, that we must conclude that light emission is produced by charged particles of the same kind. It is impossible to over-estimate the importance of this result. It furnishes the clearest evidence of the universal existence of the electron and the important part it plays in physical phenomena.

There are more complicated types of the Zeeman effect, where a single line is split up into more than two or three components, and while these cases cannot now be explained satisfactorily, there can be no doubt that the true explanation will be found to depend upon the disturbance in the motion of electrons produced by a magnetic field.

In the theory of light that regards light as the transverse vibrations of an elastic solid ether, and also in the electromagnetic theory of light, there has been a difficulty in accounting for dispersion. Waves of different frequencies travel in material substances with different velocities. The hypothesis was made by Sellmeyer, and developed by many others, including Helmholtz and Lord Kelvin, that there were particles inside matter capable of executing definite vibrations, and that their vibrations so modified the propagation of light through the bodies as to produce the effect of dispersion. These hypothetical particles now have a definite reality. They are the corpuscles or electrons of which we have been speaking. I do not wish to be understood as implying that we are even near a complete theory of the propagation of light through matter, but the fundamental importance of the electron in optical theory is established beyond doubt.

I now wish to consider a point in the general theory of radiation about which we are still very much in the dark. It has to do with the emission of light—or any form of radiant energy—by bodies, when their temperature is raised. The discussion of this question is much facilitated by the introduction of the conception of a " black body." By that is meant a body which absorbs all the radiation falling upon it and reflects none. While an absolutely black body is not known, it is easy to get so close an approximation to one, that an ideal black body is not a difficult thing to imagine. When a black body is raised to a definite temperature it radiates energy from the shortest known light-waves to the longest known heat waves. The intensity of the radiation, arising from any small range of frequencies, can be measured, and when this is done a curve something like the illustration is obtained; ν is the frequency, the number of vibrations of the radiation in 2π seconds, and F is the intensity of the radiation.



The radiation has a maximum value for some definite frequency, and falls off for both higher and lower frequencies. As the temperature is raised, the maximum intensity is displaced towards the higher frequencies—the shorter wave-lengths. It is important to explain why it is that these curves have the form they have, and to deduce an expression which will enable one to calculate the energy, corresponding to any frequency, emitted by a black body at any temperature θ. Such an expression, which does fit the experimental results with great accuracy, was deduced by Planck in 1900:

$$F(\nu\,\theta) = \frac{h\nu^5}{4\pi^4 C^4} \cdot \frac{1}{\epsilon^{\frac{h\nu}{2\pi k\theta}} - 1}$$

C = velocity of light, h and k absolute constants.

To arrive at this result Planck assumed that the radiating body contained elementary "vibrators" which we can now interpret as vibrating electrons. In addition he had to make the assumption that when energy is absorbed or emitted by an oscillator, it is done, not continuously, but in finite, though very small, elements. These energy elements, the energy quanta, the Germans call them—are something of which it is very difficult to form a conception. We have grown used to thinking of elements of matter: I have tried to show at the beginning of this lecture that we may be forced to conceive of elements of electricity not associated with matter, but I am not going to try to convince you of the possibility of thinking of energy—wholly dissociated from either matter or electricity—flying around in discrete elements. Of course if we find that this hypothesis does explain phenomena that none of our usual views explain, and in addition explains all that is explained by our present views, then we shall be forced to accept the energy-quanta as a working hypothesis. Now, in the first place, Planck has recently shown that his radiation formula can be deduced on the assumption that radiant energy is continuously distributed through space in wave motion, but that a vibrator, when it emits energy, does so in finite elements, although it absorbs energy continuously. It does not seem wholly improbable that a model for an atom can be thought of which will behave in just this way, and if this can be shown, the conception of the energy-quanta will be unnecessary so far as accounting for the radiation law is concerned. There can be no question about the energy-quantum hypothesis explaining certain phenomena—especially in connection with the specific heat of bodies, and the photo-electric effect, that have never received any other explanation. But it is by no means certain that this hypothesis is necessary to explain these phenomena. Aside from the difficulty of conceiving of these energy-quanta there is one very serious difficulty in accepting this hypothesis. In accepting it we are obliged to give up the wave theory of light. We should have to go back to what amounts to Newton's corpuscular theory of light, Newton's corpuscles being replaced by these particles of energy. Now so far as we know, such a theory cannot account for some of the most striking properties of light—interference, diffraction, polarization—things which the wave theory of light explains so beautifully. The success of the wave theory of light, not only in explaining known phenomena, but in predicting new phenomena, has been so great that the energy-quantum hypothesis requires very much more evidence in its favor than has yet appeared, before it can be accepted.

Positive Electricity

I now wish to speak about one of the most puzzling features connected with our present views. It has to do with the nature of positive electricity. Negative electricity, we have seen, may be regarded as having an atomic structure, being composed of discrete particles, called electrons or corpuscles. And I have tried to convince you that these corpuscles have a real existence, as real as anything we know about. Negative corpuscles, whatever their origin, all are identical, save for differences in their velocities. One would rather expect to find something similar for positive electricity. But nothing of the sort has ever been found. Positive electricity, whenever it appears, is always associated with atoms or molecules of the chemical elements. We have already spoken of a sphere of positive electricity, but this was introduced solely for the purpose of enabling us to build up an atom of ordinary matter out of electrons—we had to assume some means of holding the negative electrons together, to keep them from repelling each other by the forces between like charges.

So far, we have become acquainted, in nature, with positive electricity, only in the alpha particles; measurements of e/m and e for them showed them to be atoms of helium with two elementary positive charges. And we also have more direct evidence that the alpha particle on losing its charge becomes an atom of helium. We must now speak of some other occurrences of positive electricity. As from radioactive substances both negative and positive charges are emitted, so in vacuum tubes, when an electrical discharge is maintained in them, we find both cathode rays, which are formed of negative particles, and the anode rays, which are formed of positive rays. If the cathode is perforated with a number of holes, a luminosity is observed behind it. Experiment shows that this luminosity is caused by a stream of positively charged particles, and determinations of the ratio e/m for these particles have been made by methods similar to those employed for the cathode particles. The results of measurements of this kind show that e/m is very much smaller for these positive particles than for the negative corpuscles. This may be interpreted either by assuming that the charges the positive particles carry are much smaller than the charges carried by the corpuscles, or that their mass is very much greater. Now, no negative charge has ever been found smaller than the charge carried by the corpuscle. It seems reasonable to suppose, therefore, that this is also the smallest positive charge that can exist. Assuming this, the observed values of e/m give values for the masses of the positive particles which indicate that they are either single atoms of various chemical elements or aggregates of two or more atoms. For example, the largest value of e/m found is just what would correspond to a single hydrogen atom, carrying a single positive charge. With mercury vapor in the discharge tube, values of e/m are found which would correspond to four mercury atoms, with a single positive charge. And atoms of other chemical elements are detected in the same way. Sir J. J. Thomson, who has contributed a large part of our knowledge of these positive rays, regards the method of determining e/m for these particles as an extremely sensitive method of chemical analysis. The presence of minute traces of a gas may be shown—a far less proportion than would be required to make its presence known by spectroscopic means.

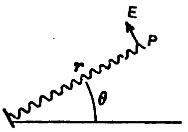
The view which these results lead us to take of the atoms of the elements, is, then, this. We assume positive electricity distributed throughout a volume, for simplicity a sphere, of atomic size. In this sphere of positive electricity there are just enough negative corpuscles to neutralize the positive electricity. Such an atom may expel one or two negative corpuscles without necessarily changing its character thereby—that is, it still remains an atom of the same element, but now positively charged in virtue of having lost some of its negative electricity. The corpuscles may subsequently be picked up again, when the atom becomes once more electrically neutral.

Let me summarize briefly the main points of what I have tried to exhibit. In the first place we considered the negative corpuscle, showing that its mass might be interpreted as resulting from a negative charge in motion, although we were not absolutely forced to this conclusion. The universal occurrence in nature of these corpuscles was next brought out, and their fundamental importance in phenomena connected with light proved; the energy-quantum hypothesis was next briefly touched upon, showing why it had been introduced. Finally the nature of positive electricity was discussed, leading to a provisional view of the structure of the atom.

LECTURE V

Before beginning on the subject announced for this lecture I must, in order to answer questions that have been raised, give a little further account of some of the things dealt with in the previous lecture. You will remember that I described a model of an atom, consisting of a sphere of positive electricity, with a single negative electron moving in a circle inside. The question is, why is it that this emits light? We believe now in the electromagnetic theory of light, according to which light consists of a periodic electric force and a periodic magnetic force, at right angles to each other, propagated by wave motion in a direction at right angles to both the electric and magnetic force. A mathematical calculation, which it would take too long to give here, shows that for points which are at a great distance from the vibrating electron compared to the dimensions of its orbit, this is just the state of affairs that we have—magnetic and electric forces, perpendicular to each other, propagated outwards in waves with the velocity of light. It may puzzle you to understand why it is that the velocity of light enters into the solution of a purely electrical problem like this. It is because we measure electric and magnetic forces each in a particular set of units—in a problem like the present one, where both electric and magnetic forces enter, they must be reduced to the same system of units. This requires the introduction of a factor, which experiment proves is equal to the known velocity of light in space. Theory shows that the electromagnetic effects are propagated with a velocity equal to the ratio of the units in the two systems, and this ratio is the velocity of light.

A convenient way of thinking of these effects is in terms of lines of force, which we may imagine are carried by the electron, are, in fact, a part of it. Think of these lines of force as so many stretched threads, one end of each attached to the electron. We must assume that transverse waves travel along these strings with the velocity of light. Now suppose that the electron moves in a circular orbit. We know that motion in a circle may be compounded of two motions in straight lines perpendicular to each other. It is only the component at right angles to the line of force which is effective in causing its transverse vibrations, and so we may neglect the other component. Therefore at any point at a distance from the electron the line of force will vibrate back and forth in a direction perpendicular to itself when at rest. The sketch herewith illustrates roughly the spreading out of the transverse electric force along one line of force. We therefore have a vibrating electric force, which at every point is perpendicular to the line joining the electron to the point. Accompanying this there is a magnetic force perpendicular both to the line joining the electron to the point and to the electric force. This therefore results in a spherical wave of electric and magnetic force spreading out from the electron.



Metallic Conduction

Let us now consider what influence the discovery of the electron has had upon our views of the conduction of electricity through metals. First of all it may be well briefly to consider our previous notions of metallic conduction. It had to be assumed that electricity was something—a substance or an effect—that travelled freely through conductors. The force driving the electricity was called the electromotive force, and the flow of electricity the electric current. Between these two quantities a simple relation, Ohm's law, was found to hold, which we may express by

$$i = k E$$

or, in words, the electric current is equal to the conductivity k, times the electromotive force. The conductivity k is a specific constant for each kind of conductor. Experiment shows, however, that the conductivity of a conductor is not constant, but depends on its physical state. Thus on heating the conductor, in general, the conductivity diminishes; some metals shows the opposite effect, and this makes it possible to make alloys whose conductivity varies very little with the temperature. In a magnetic field, again, the conductivity may increase or decrease, depending on the conductor and the direction of the magnetic force with respect to the current. Light falling on certain conductors, notably selenium, changes very decidedly their conductivity. Then, too, there was found to be a very close connection between the electric conductivity and the heat conductivity of metals. The old view of an electric current offered no explanation of this variation of conductivity with the physical state of the conductor. It merely had to be accepted as a fact. The best conductors of heat were found, in general, also to be the best conductors of electricity. There was no explanation of this fact on the older view. It is true that Wilhelm Weber towards the beginning of the last century offered an hypothesis in regard to the electric current in which the current was regarded as the flow in opposite directions of positive and negative electric particles, and by making additional hypotheses in regard to the nature of these particles many of the known phenomena might be accounted for. But as long as there was no evidence of the reality of these particles this view never received general acceptance, nor was developed to any considerable extent.

We are still far from being able to give a completely satisfactory explanation of the various phenomena with which we are concerned, but a considerable amount of progress has been made in applying the notion of electrons to metallic conduction, and I shall attempt to give some account of this in its simpler aspects.

The view we have been led to take of an atom is that it is a volume of positive electricity containing many negative electrons. Now we have good reasons for believing that in a metal, in addition to the electrons contained in its atoms, there are also many electrons not contained in them, but continually moving about among the atoms. We may look upon these electrons, the free electrons we shall call them, as pursuing zig-zag paths among the metallic atoms, colliding with them—perhaps entering into them, while others are shot out from the atoms. On the average there will be a certain number, N, of these free electrons per unit volume. These have a perfectly irregular motion, there are just as many moving in one direction as another. Now suppose an electric force is applied to the conductor. This we know causes a current to flow through it. Let E be the electric force. There is now a force $e\,E$ acting on each electron driving it in a direction opposite to the direction of the electric force. Negative electricity flowing in one direction is equivalent to positive electricity flowing in the opposite direction. We thus get an electric current flowing in the direction of the electric force. It is worth while to calculate the strength of this electric current on the simplest view we can take. Let v be the velocity of the electrons before the electric force was applied. The velocity of an electron in a direction opposite to that of the electric force is given by

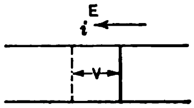
$$m \frac{d^2x}{dt^2} = e\,E$$

$$\frac{dx}{dt} = \frac{e}{m} E\,t$$

at any time between two collisions of the corpuscle. But at the beginning of the free paths the corpuscles had no average velocity in this direction; at the ends of their free paths the electrons thus have a velocity $e/m\ E\ T$ where T is the time the electron is free. So the average velocity in this direction is

$$\frac{1}{2}\frac{e}{m}\,T\,E = V$$

In a unit volume of the metal we have N free electrons; each carries a charge e, and each moves with this average velocity in a direction opposite the electric force. Therefore the electric current is equal to all the charges that cross the section of the conductor in unit time. Let the cross-section of the conductor have unit area. Then the number of electrons which cross it in unit time will be equal to the number that at the beginning were contained in the volume V, *i.e.*,



$$\frac{1}{2}\frac{e}{m}\,T\,E\,N$$

Each has a charge e, so that the current is

$$i = \frac{1}{2}\frac{e^2}{m} T N E$$

We thus get Ohm's law, the conductivity being

$$k = \frac{1}{2}\frac{e^2}{m} T N$$

Let us now introduce an hypothesis which we shall attempt to justify by its consequences. It is that the electrons move about among the atoms of the metal just as the molecules of a gas move about, and just as the average kinetic energy of a molecule of a gas is proportional to its absolute temperature, θ,

$$\frac{1}{2} m v^2 = \alpha \theta$$

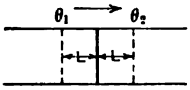
so the average kinetic energy of an electron is proportional to the absolute temperature, with the same factor of proportionality, α.

Then since $T = \frac{L}{v}$, where L is the average distance the electron goes between collisions—the free path of the electron—we can write

$$k = \frac{1}{2}\frac{e^2 L}{m v} N = \frac{e^2 v N L}{4 \alpha \theta}$$

Experiment shows that the conductivity of most pure metals varies very nearly inversely as the absolute temperature. This is what our formula tells us, provided that $N L v$ does not vary with the temperature. On our hypothesis, v varies as the square root of the temperature, so that we may assume that $N L$ varies inversely as the square root of the temperature. We have not time to consider the different views that have been held on this matter; the point I wish to emphasize is that the view of metallic conduction that has been briefly described, does lead, first of all, to Ohm's law, and it does account for a variation of conductivity with the temperature. The old idea of the electric current made no attempt to give any explanation.

Let us now find the conductivity for heat of our metal. For this purpose we must calçulate how much heat goes through any unit cross-section in a unit time. Let the temperature decrease as we go from left to right. The number of electrons that cross any cross-section in unit time is $\frac{1}{6} N v$. For v is the distance they travel in unit time and as they move in all directions one-sixth of those in a volume v will get through. Each electron has an amount of energy $\frac{1}{2} m v^2 = \alpha \theta$. For θ we must take the temperature at the last collision. On the average, the last collision was at a distance L to the left of A. Call the temperature at this place θ_1. Hence the heat crossing A in unit time from left to right is



$$\frac{1}{6} N v \alpha \theta_1$$

Similarly the heat crossing A from right to left is

$$\frac{1}{6} N v \alpha \theta_2$$

Hence the net amount of heat that crosses A from left to right is

$$\frac{1}{6} N v \alpha (\theta_1 - \theta_2)$$

or

$$\frac{1}{3} N v \alpha \frac{\theta_1 - \theta_2}{2 L} L$$

$\theta_1 - \theta_2$ is the difference in temperature at two places distant $2 L$ from each other; $\frac{\theta_1 - \theta_2}{2 L}$ is therefore the rate of fall of temperature as we go along the bar. Hence the heat conductivity is

$$c = \frac{1}{3} N v \alpha L$$

The ratio of the electric to the heat conductivity is

$$\frac{c}{k} = \frac{4}{3} \frac{\alpha^2}{e^2} \theta$$

This result is of particular interest because the only quantity in it which can vary with the temperature is θ, the temperature itself. The ratio of the heat to the electric conductivity should then increase proportionately to the absolute temperature. Suppose we put in the known values of the quantities in this equation, We have found that

$$e = 1.6 \times 10^{-20} \text{ in electromagnetic units.}$$

Now α is known from the kinetic theory of gases. It is equal approximately to

$$1.8 \times 10^{-16}$$

Then at 0 deg., $\theta = 273$

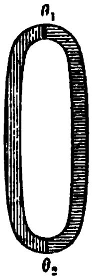
$$\frac{c}{k} = 4.6 \times 10^{10}$$

Experiment shows that this ratio, for a large number of metals, is about $6 - 7 \times 10^{10}$. In our calculation, it has been assumed that both the thermal and electric currents are carried only by the negative corpuscles. It is almost certain, however, that the thermal conductivity is partly due to the motion of the atoms themselves inside the metal. This would tend to increase the ratio c/k, bringing it nearer to the known value determined by experiment.

This result is as remarkable as those obtained by the application of the theory of electrons to optical phenomena that we considered in the last lecture. It furnishes very strong confirmation of the view that the electron plays an important part in electric conduction in metals.

Thermoelectric Effects

If a circuit is made of two different metals, and the two junctions are kept at different temperatures, it is known that a current will flow around the circuit. Let us see how the corpuscular theory accounts for this effect. We have taken the view that the corpuscles move about in a metal just as molecules of a gas in an enclosed space. And just as the pressure which a gas exerts is a result of the impacts of the molecules, so there will be a pressure at any point in a metal due to the impacts of the corpuscles. To calculate this pressure we need only to consider the momentum which the corpuscles bring up to a unit area in unit time. On the average we can say that one-sixth of all the corpuscles are moving in a given direction. Each has momentum $m\,v$. The momentum of the corpuscles striking a unit area of the wall in unit time will therefore be



$$\frac{1}{6} m\, v^2 N$$

If we assume that the corpuscles are reflected from the wall without loss of energy, each carries away an amount of momentum $m\,v$, but in the opposite direction from that it brought up to the wall. Therefore the wall receives in unit time the momentum

$$\frac{1}{3} m\,v^2 N$$

But this must be the pressure the corpuscles exert. Hence

$$p = \frac{1}{3} m\,N v^2 = \frac{2}{3} N \alpha \theta$$

If now the number of free corpuscles in unit volume is different for different metals, at a junction between two metals there will be a difference in the pressure of the corpuscles on the two sides and so the corpuscles will move from one metal to the other. To make the case definite suppose that N_1 is greater than N_2 and θ_1 is greater than θ_2. Then at the hot junction there will be a difference in pressure of

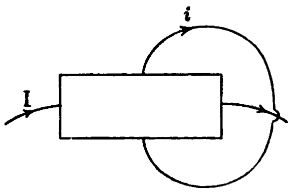
$$\frac{2}{3}(N_1 - N_2)\,\alpha\,\theta_1$$

driving the corpuscles from 1 to 2. At the other junction there will be a difference in pressure

$$\frac{2}{3}(N_1 - N_2)\,\alpha\,\theta_2$$

also driving the corpuscles from 1 to 2. But this is less than the former difference in pressure and so there will be a continual flow of corpuscles from 1 to 2 at the warm junction and from 2 to 1 at the cool junction. There will be therefore an electric current flowing in the opposite direction.

The other thermoelectric effects—the Peltier effect, the Thomson effect—may be explained qualitatively equally well by the corpuscular theory.



Let us now consider some effects of magnetism on electric conductivity. According to the views of Maxwell, if a conductor carrying an electric current is placed in a magnetic field, a mechanical force is exerted on the conductor, but there is no effect on the electric current.

It was discovered by Hall that if the conductor were in the form of a thin plate, through which a current I was sent in the direction indicated, and a magnetic force applied perpendicularly to the plate, then a current was produced in the circuit i. With no magnetic force acting, there is no current in this circuit. The existence of such a transverse current can easily be accounted for on the corpuscular view. Acting on a corpuscle there is a force at right angles both to the direction of the magnetic field H, and to the direction of motion. This force deflects the corpuscles from the right to left motion so that some of them move through the branch circuit, thus producing an electric current in the opposite direction. We meet, however, with a difficulty here.

According to what we have just seen, the Hall effect should have the same sign for all metals. But as a matter of fact for some metals the transverse current has the direction indicated by this theory; for other metals the transverse current has the opposite direction. This variability could be accounted for if we assumed that, in addition to the negative corpuscles, the current was also carried, in part, by positive charges; these would result in a transverse current opposite in sign to the current carried by the negative corpuscles and so, depending upon which effect was the greater, the Hall effect would have one sign or the opposite. But we have very strong evidence that positive charges are always associated with atoms and so we cannot assume them to have mobility enough in a metal or other solid to take an appreciable part in electric conductivity. We must therefore seek another explanation. The explanation is to be found, I think, in the structure of a metal. We have assumed far too simple a structure for the interior of a metal. In our calculations we have considered only the free electrons—that is, those that are outside of the atoms. It is probable that the corpuscles in the atoms play a part in these effects, although we have no theory yet which will account quantitatively for experimental results.

A similar difficulty is met when we attempt to account for the change in the conductivity of a metal when in a magnetic field. If the magnetic force is applied at right angles to the direction in which the electric current flows, then as a result of the deflections of the paths of the electrons it is easily seen that the conductivity of the metal will be decreased. This is the result that is observed, except that with the magnetic metals, iron, nickel and cobalt, weak magnetic fields decrease the conductivity; but on increasing the magnetic force the effect changes in sign, so that with strong fields the conductivity is increased. On this simple view there should be no effect on the conductivity when the magnetic force is parallel to the current. Experiment shows, however, that, in general, there is a decrease in the conductivity in this case. Qualitatively, both these departures from our simple theory can be explained by assuming that the effect of the magnetic field is to produce a rearrangement among the atoms so as to alter the free paths of the electrons. That some such effect should exist is probable. Recent work in metallography has shown that metals have a very complicated structure—being in fact aggregates of crystals. The view we have taken of a metal is undoubtedly far too simple and it is much more to be wondered at that this simple view should lead to so many striking confirmations than that departures from it are observed.

A number of attempts have been made to explain magnetism on the electron hypothesis. At first sight it seems an easy thing to do. We generally look upon a magnetized body as an assemblage of small magnets which are all made to turn more or less in one direction when a magnetic force is applied. Now the fundamental theorem in electromagnetism is that an electric current flowing in a small closed circuit is equivalent, as far as external effects go, to a small magnet, whose axis is perpendicular to the plane of the current. As an electron in motion is equivalent to an electric current, the electrons circulating in closed orbits inside the atoms might be expected to make the atoms themselves elementary magnets. This would be just the result necessary to account for the magnetism of such substances as iron and also the so-called para-magnetic substances—which have similar magnetic properties but in a much smaller degree. However, when one considers the possible symmetrical distribution of electrons in the atoms, it is found that the atoms will not be elementary magnets. There will still be an effect arising from the magnetic field on the motion of the electrons inside the atoms and such as to correspond to diamagnetism—the property exhibited by copper and many other substances. The difference between paramagnetism and diamagnetism may be shown by taking two rods—one of iron and one of copper, suspended in a uniform magnetic field. The iron tends to set itself so that its length is along the direction of the magnetic force, while the copper tends to set itself at right angles to the magnetic force. Thus, on this view, the atoms of all substances would be diamagnetic. To account, then, for paramagnetism, it is necessary to assume that the atoms, owing to a lack of symmetry, are effectively elementary magnets, as a result of the circulating electrons. The theory has been developed along these lines by Langevin, who finds that the diamagnetism will be independent of the temperature while paramagnetism varies inversely as the temperature; and this agrees, in general, with experimental results. We thus have every reason to believe that a real explanation of magnetism is to be found in the electron theory.

It will not be out of place here to say a word regarding the essential difference between a conductor of electricity and an insulator. In a conductor we have seen that we may assume that, in addition to the electrons circulating in the atoms, there are also free electrons. A metal atom may be looked upon as one from which one or two electrons can escape, and these escaped electrons then become free electrons. An atom of an insulator, on the other hand, we may regard as one in which the electrons are more firmly held, so that in a perfect insulator there will be no free electrons.

At the beginning of these lectures I told you that I was going to try to show how far recent discoveries had changed our old ideas about the constitution of matter. It may therefore be well to give a very brief summary of the most important results.

In the first place we have learned that the atoms of the various chemical elements are complicated structures. One constituent which is common to all atoms is the negative electron or corpuscle. In addition we have to assume positive charges as well—as to what their nature is we are still very much in the dark. As for the number of corpuscles in the atom, various lines of reasoning give results that show that this number must be comparable to the atomic weight of the atom. One method of getting an estimate of the number of corpuscles in an atom is by calculating the absorption that a stream of beta particles undergoes when traversing a known thickness of matter. This is something that can be determined experimentally, and by assuming that the absorption of beta particles is the result of collisions with the corpuscles in the atoms, an estimate of the number of the latter may be obtained. So that we cannot assume the whole mass of an atom to be the sum of the masses of the electrons contained in it. We are led, therefore, to assume that the mass of the atom is concentrated in the positive part of the atom. We may take a provisional view of the atom something like this—a number of positive charges with just enough negative corpuscles circulating around them to neturalize the electric charge. Atoms built in this way will have different degrees of stability. Such an atom may lose or gain one or two corpuscles and still remain the same kind of an atom. It will then correspond to a very stable element. Or an atom may be so unstable that on losing an electron a complete rearrangement takes place, resulting in a wholly different atom. That will correspond to a radioactive atom. This view of the atom leads to a natural explanation of chemical combination. Say that we have two different atoms A and B. A may lose an electron, leaving it positively charged. B may gain the electron lost by A. B is then negatively charged. A and B will then be attracted to each other and they may combine to form a molecule of two atoms A B.

The weakness in the position where we stand at present lies in our ignorance of the nature of positive electricity. Negative electricity distributed in corpuscles, we know a great deal about. We have seen something of the universal importance of the negative corpuscle in optical, thermal, and electric phenomena.

We have seen that the mass of the negative corpuscle may be interpreted in terms of an electric charge in motion, that its mass increases with increasing velocity. So we are led to think that perhaps all mass is electrical in its nature. We cannot, however, have much confidence in this view until something analogous is proved for the unknown positive charge.

Besides the change in our ideas regarding atomic structure which has resulted from the study of radioactivity, there is another much more revolutionary trend in modern thought concerning the physical universe. This has to do with the very foundation of the science of mechanics. If what has apparently been proved for the negative corpuscle, that its mass increases with its velocity, holds for all matter, then our familiar laws of mechanics, which have been accepted as holding absolutely, can be regarded only as approximations. It is only when the velocities we consider approach that of light that there will be any appreciable departure from the motion given by applying Newton's laws. According to our usual ideas, if a constant force acts on a body, a certain definite momentum, the product of the mass by the velocity, will result. If the force acts twice as long, double the momentum will result. The mass being constant, there will be no limit to the velocity that the body may acquire.

It is only necessary to apply the force for a sufficiently long time. But this is not so if the mass increases with the velocity. Doubling the time the force acts will not double the velocity, since the mass also increases. In the case of a corpuscle, when we regard its mass as resulting from a charge in motion, the mass becomes infinite when its velocity is equal to that of light. If the same be held to be true for all matter, if, in other words, all mass is of an electromagnetic origin, then the velocity of light is the highest velocity we can conceive of. This is a result which is startling when viewed from the standpoint of our usual ideas of mechanics. According to this view, the velocity of light in space is a constant of fundamental importance in the whole domain of the physical sciences.

All of this is intimately related to what has come to be known as the principle of relativity. If it is assumed that the velocity of light in space is the highest velocity that can be attained, it must be impossible to detect absolute motion of a system by any experiments made in that system. Suppose, for example, the velocity of light be measured on the earth, first in the direction in which the earth is moving about the sun, and second, in a direction at right angles to the motion of the earth. In the former direction the velocity of the earth would be added to the velocity of light, and the sum would be a velocity greater than that of light. But this, by hypothesis, is impossible. Hence it would follow that by no experiment in a moving system can we detect the absolute motion of that system. All that can be detected is the motion of one system relative to another.

A number of experiments have been made with the object of detecting absolute motion. The best-known of these is the Michelson-Morley experiment, in which a difference in the velocities of light, traveling along and perpendicular to the motion of the earth, was looked for. All such experiments have given negative results. And these negative results have led to the enunciation of the principle of relativity, according to which the absolute motion of a system cannot be determined by any experiment made in that system. In this form, the principle of relativity is a principle of negation, like two other fundamental physical principles—the principle of the conservation of energy and the second law of thermodynamics. The former denies the possibility of perpetual motion, and the latter denies the possibility of transferring heat from a cold to a hot body by any self-acting mechanism.

From a certain point of view, the principle of relativity, stated in this way, would seem to be a necessary consequence of the electromagnetic origin of mass. If all matter can be considered to result from the motion of electric charges, it must follow that absolute motion cannot be determined. For without motion, there could be no matter, and without matter there would be no possibility of detecting motion.

However, the necessity of introducing the principle of relativity with all of the consequences, so opposed to what we have always assumed as fundamental truths, that have been held to follow from it, cannot be said to be proved. Speculation regarding it has proceeded rather farther than experimental results warrant. The subject is mentioned here only for the purpose of showing how deeply into our fundamental conceptions of the physical universe the study of radioactivity has led.

In conclusion, let me repeat what may be taken as the main results of the study of radioactive phenomena. First, we are forced to believe that the atoms of the elements are dynamical systems of varying degrees of stability. In the second place, the negative corpuscle, or electron, has been shown to be of fundamental importance in the whole domain of the physical sciences. The difficulties in developing further these views of matter, for which a foundation has been laid, center about two large problems: the nature of positive electricity, and the old problem of the ether.

A paper presented at the Pittsburgh Meeting of the American Institute of Electrical Engineers, Pittsburgh, Pa., April 18, 1913.

PURCHASED POWER IN COAL MINES

BY H. C. EDDY

The ideal corporation may be defined as a cooperating aggregation of individuals of specialized ability.

The practical application of this idea has produced industrial companies whose activities frequently become so extensive and so diversified as to make it desirable to separate them into component parts, each exercising those functions which it is especially fitted to perform. Thus we find the huge industrial organizations of today made up of a number of departments, each complete in itself and each virtually a corporation, in the sense of the definition given above.

This division and segregation of corporate activities is practicable only in comparatively few instances, as the cost of thoroughly competent heads of departments becomes too great a burden for the companies of less than extraordinary size to carry. In the majority of cases it becomes good business policy to carry out this division of effort by depending upon outside agencies, whose sole function is to supply, in the most economical way, some certain requirement.

Thus the function of the public utility companies becomes that of supplying transportation, the facilities for the transmission of intelligence and the distribution of electricity, water and gas, each a highly specialized branch of service calling for the expenditure of much capital, technical knowledge and broad-gage thought, to reach the fullest possible development.

There is then a sound fundamental basis for the existence of an organization whose sole business is to make and sell power in both large and small quantities.

However sound the abstract theory of the central station as a power merchant may be, its continued existence depends upon its ability, in practise, to supply power on a basis which shall be reasonably profitable to both parties concerned in the transaction.

An analysis of the conditions of use of power in the bituminous coal mines in Pennsylvania and Ohio discloses certain general facts which are decidedly favorable to the purchase of power, when available, as compared with the operation of independent plants.

There are but few mines in these fields that operate more than sixteen hours per day, and many that run but a single shift. The double shift basis of operation is, however, sufficiently common to be considered typical. The load curve of such a mine shows that approximately two-thirds of the total kilowatt hours per day of twenty-four hours, is used between 7 A. M. and 4 P. M., the remaining one-third being used between 6:30 P. M. and 1 A. M. During the day run the demand is quite variable, fluctuating between wide limits for short periods. The widely different conditions existing in the individual mines makes it practically impossible to give accurate values to the various elements of load. In one mine the grades may be in favor of the loads to be hauled by the locomotives, while in another case the reverse may be true, tipples may be above or below the level of the mine opening, fan operation may be required continuously at a high rate of air discharge or the reverse. Notwithstanding all these variables, the general characteristics of the total load are quite uniform in being removed from the central station peak.

The application of alternating-current motors to mine equipment is not general, practically all mines operated by electric power using direct current at either 250 or 550 volts. This necessitates the use of either synchronous converters or motor-generator sets when power is purchased. When the latter are used it is the general practise to specify synchronous motors, on account of the somewhat better efficiency to be obtained, as compared with induction motors, and also for the improvement of power factor.

The advantages to the central station which justify low prices for mine power, may therefore be summed up as follows:

The considerable amount of power used.

The "off-peak" load.

The extensive application of synchronous motors, tending to raise the plant power factor, with the attendant advantages.

From the standpoint of the mine operator the advantages of purchased power are more numerous.

The most important consideration is that of cost. It is obvious that in this presentation of the subject, no comparisons of actual figures can be made, but it lies within the province of the writer to indicate the essential reasons for a relatively high cost of operation of independent mine power plants.

The controlling element lies in the load factor, which may be defined as the ratio of average use of the equipment required to meet the maximum load conditions.

The daily load factor is much higher than the monthly ratio, due to the fact that during the month the average number of working days ranges from 15 to 20, due to car shortage, market conditions and temporary labor difficulties. The annual load factor is even lower, due to the same general conditions, but upon a more extended basis.

The result of this condition is that the investment in power plant and equipment is idle for much of the time, and as interest, depreciation and taxes are continuous charges, the result is that the actual output of the plant carries a very high fixed charge per kilowatt hour.

This condition does not exist when power is purchased, except in so far as it applies to the current transforming apparatus. The cost of equipment per kilowatt of capacity being much lower than the cost of complete plant equipment, there is a substantial saving to be effected in this item of power costs.

The actual manufacturing cost, exclusive of overhead charges, under conditions of widely varying load and intermittent use of generating equipment, becomes much higher than would be found with exactly the same apparatus working more continuously.

The individual mine plant is usually located at the least favorable point, considered electrically, *i. e.*, at the mouth of the mine. As the mine is developed the electrical center of the load recedes from the plant location and the losses in the distributing system constantly increase. The extent to which it is advisable to increase the investment in copper to minimize voltage losses can be determined only by a careful survey of the conditions applying to each particular installation.

Aside from the actual copper loss, the low voltage obtainable at the point of delivery of current brings in its wake a high maintenance cost for motors on locomotives and coal cutters, chiefly in the form of armature and commutator repairs. These troubles are directly traceable to the abnormal volume of current required by reason of the less than normal voltage. Aside from this actual expense there is the loss of possible output due to reduced capacity of motors brought about by the unfavorable conditions of current supply. This loss is far greater than the actual cost of repairs and its magnitude is often unappreciated by mine operators.

These conditions may be remedied to a great extent, if not practically eliminated, where power is purchased, by placing converters or motor-generators so that the mine distributing system may be fed at several points, thus materially reducing line losses, equalizing line voltage, and bringing it up to the normal working voltage of the motors in use. As the mine is developed and new conditions arise with respect to the distributing system, the location of the conversion equipment may readily be changed. This flexibility is impossible with a complete steam plant.

In any successful concern the growth of its operations is ordinarily greater than originally expected. The coal mining industry is no exception to this general rule, and operators are periodically faced with the proposition of extending and enlarging individual plants to meet the greater demands for output. Usually this problem is solved by adding boilers, engines and generators, with a resulting greater investment of capital than would be required by the addition of a motor generator set. The capital invested in plant equipment in excess of the cost of motor generators would earn much more per year, if put into strictly mining machinery.

The operators of mine plants are ordinarily handicapped by the character of labor available for power plant operation. In some cases mines are so located with regard to living conditions that really skillful engineers may be obtained and kept. In more instances, however, the conditions of work and locality of the mines do not prove attractive to the best men except as a temporary expedient. It is more often than not, a case of a more or less regular procession of engineers through the cycle of being hired, endured and "fired." As a natural result the average mine plant receives less than an ordinary amount of skilled attention, when, by reason of the severe conditions under which it operates, it should receive more. Under such circumstances it is to be expected that the cost of maintenance and repair will be high. Aside from labor conditions the item of boiler repairs and replacement is usually excessive on account of the bad water conditions that are so commonly found in the coal districts.

Where power is purchased, the care required by conversion apparatus does not call for an expert man in constant attendance. The ordinary daily care required may be furnished by the necessary switchboard operator. In some cases this duty can be safely assumed by the repair shop men, thus eliminating entirely any cost for attendance.

It is obvious that the employers liability hazard, so far as power supply is concerned is greatly reduced when power is purchased than when a steam plant is operated.

The tendency of labor and other costs entering into the production of power is upward. This can only be met and compensated for in the case of the central station through the use of generating equipment of the very highest economy, by the securing of business of a diversified character of use, enabling the operation of the plant under good load conditions each hour of the twenty-four, and through quantity production. These safeguards against increasing power costs cannot be obtained by the individual plant operator except through the protection afforded by a long term contract with a substantial and responsible central station organization.

The advantages gained by the mine operator by the purchase of power are direct and may be summed up as follows:

Reduction of fixed charges on investment.

Reduction of actual operating costs due to the fact that only power used is paid for, without stand-by charges due to intermittent operation, and by reason of the higher efficiency of electrical apparatus at any load, as compared with steam generating equipment of the character available for mine work.

Material reduction of distribution losses.

A considerable increase in the output of mining machines and locomotives due to maintenance of speed through normal voltage.

Flexibility of location of motor-generators, enabling them to be placed at points giving the best operating results, and to be readily and cheaply moved as conditions change.

Reduction of labor costs for attendance.

Elimination of high maintenance, repair and replacement costs for boilers, piping and engines.

Reduction of cost of superintendence, enabling the mine superintendent to devote his entire attention to securing output.

Reduction of liability.

Insurance against constantly increasing power costs, through term contracts at fixed rates.

Additional coal available for sale.

A paper presented at the Pittsburgh Meeting of the American Institute of Electrical Engineers. Pittsburgh, Pa., April 18, 1913.

CENTRAL STATION POWER FOR COAL MINES

BY C. W. BEERS

The purchase of central station power by coal operators for use in and about the coal mines, appears paradoxical owing to the apparent cheapness of fuel at the mines, yet some companies have found it economical to do so, and up to the present time these companies do not regret making contracts for the purchase of power. One large coal company in the anthracite field has closed a contract with a large central station for a long term of years, and with considerable advantage to itself.

To correctly understand the reasons why a large producer of anthracite should find it economical to purchase central station power, it is necessary to have a clear understanding of the ordinary steam production, and the uses to which it is applied in and around the various collieries.

About eight or ten years ago, the writer was discussing with the mechanical engineer of a coal company the seemingly large amounts of steam used in various collieries, as the cry of the colliery people was constantly for more steam, although the installation of new steam consuming devices was in no way proportional to the constantly increasing amount of steam generated.

The mechanical engineer in reply advised that it was simply a waste of money to install more boiler capacity, and made the remark that the surest and best method of increasing the boiler plant capacity was to get busy with the pumps and engines, meaning that if the pumps and engines were kept in suitable repair, or rejected, and an entire new outfit substituted, that the existing boiler plants would be largely in excess of the actual steaming capacity required, and would operate with better load factors with a consequent reduction in steam expense.

A statement of this kind, coming from a liberal minded engineer, is the pure unadulterated truth.

There are old-fashioned pumps in the mines today working on 24-hour service that vary in age from 40 years down, and as long as they are able to push water they apparently fill the bill regardless of the fact that they can digest easily 160 lb. (72.5 kg.) of steam per water horse power. Pumps on long duty service are seldom touched on account of the time necessary to make suitable repairs, and when repairs are made the question is not "how economically will the pump operate," but rather "how short a time will it take to make repairs." One can imagine in what condition the cylinders, pistons, valves, etc., are in, and with tight packing and a poor water end it is not a hard problem to guess where the steam goes.

The same is true of engines. There are fine specimens of old time workmanship and material in service 8760 hours per year. Fans usually must be kept running at any cost, and owing to the inability to shut down the engines to make necessary repairs, the pistons, rings, and valves become badly worn with the result that large quantities of steam are used with a remarkably bad distribution. More than one fan engine shows 90 lb. (40.8 kg.) of steam per indicated horse power. These statements are advanced to show the condition of much of the machinery in use today. Colliery operations are usually conducted with the idea of getting maximum coal output, and little attention or money is spent in keeping the machinery in repair so that it may work at maximum economy.

A great source of loss in the present colliery steam plants today is in the boilers themselves. This is due to the fact that the firemen employed are not very intelligent and their wages are not particularly high. The result is that while fuel is comparatively cheap, little effort is made to use it economically; oftentimes the grates are ill-adapted for the kind of fuel used and this is due to the fact that the fuel varies largely in quality from time to time. Draft arrangements are not always suitable, with the result that much energy goes up the stack. The boiler units are usually small in size, working at large overloads, and no arrangements are made to have them operate at their highest efficiency.

As far as the boiler losses are concerned they are subject to easy control, and one large operating company has made great strides in this direction by expending intelligent effort on the type of boiler used, the grates employed, and the quality of fuel burned.

Long steam lines poorly designed are responsible for much waste of steam. Leaks are seldom repaired, and owing to exposed locations the pipe covering is usually in bad condition.

From the above rough sketch of average conditions it is seen that large steam consumptions are invited, and it is a conservative statement to say that for every effective horse power-hour used in and about the coal mines, 25 to 30 lb. (11.3 to 13.6 kg.) of fuel are burned under the boilers.

During the last eight years the average value of the fuel used under colliery boilers has increased in value from 35 cents per ton to 75 cents per ton and it is still advancing.

Today, an ordinary boiler plant of 700 to 1000 horse power rated capacity has a steam cost of approximately 15 cents per 1000 lb. (453.5 kg.) of steam generated. This figure drops to about 12 cents per 1000 lb. in plants of 2000 to 3000 boiler horse power capacity.

The constantly increasing cost of steam is a condition that the operator is beginning to appreciate, and as a result he is looking for a cheaper form of power. The large savings introduced by the original small electric haulage plants proved suggestive, and as a result many collieries today are electrically operated in whole or in part.

The correct design of a modern central station plant for colliery operations is a rather difficult task, and it requires that the future of the mining operations be clearly forecast. This is an exceedingly difficult thing to do, and as a result, the tendency is to curtail the initial expense as much as possible owing to the uncertainty of future developments. Hence the plant is started on a more or less limited basis with the idea of expansion.

This is good practise, and the engineer being eager to show good economy installs apparatus that permits of a good load factor on the plant, and as a result shows low cost of power at the switchboard. As soon as the plant is loaded, additional apparatus must be installed. The station then will operate at a reduced load factor for some time, although there has been no reduction in the steaming expense; hence the cost per kw-hr. delivered to the switchboard has naturally increased.

This method produces a variation in the kilowatt-hour cost from time to time and may result in the ultimate installation of five or six machines in the plant. Idle time periods and idle hours during the working day require that some machines work at under-load and this with full steam capacity on the boilers, hence the load factor naturally decreases, and as a result the average kilowatt-hour cost is fairly high. This condition must be so since the boiler plant does not show a proportionate decrease in cost of steam as the load falls off. The continued acquisition of generating capacity along these lines ultimately results in a high cost per kilowatt installed.

The cost per kilowatt installed varies somewhat for each particular case and for a mining central station of two 500-kv-a. turbo-generator units the cost per kilowatt installed was found to be $110.07. This included a 100 deg. superheater for each boiler unit (1200 boiler-horse power was divided into three 400-horse power units). Stokers were also included. The plant operation was based on a 50 per cent load factor. The following is an estimated tabular statement of the fixed charges per kilowatt installed:

Item	Installation cost per kw.	Fixed charges per yr. per kw. Installed
5 per cent int. on station cost	110.07	5.50
10 " " dep. and repairs on machinery	37.70	3.77
5 " " " " " " switchboard	3.00	.15
10 " " " " " " light arrester	.30	.03
10 " " " " " " superheater	4.80	.48
5 " " " " " " bldgs	6.00	.30
10 " " " " " " coal and ash handling devices	5.00	.50
5 " " " on boilers	42.00	2.10
Boiler repairs (800 h.p. at $2.25 per yr.)		1.80
50 per cent dep. and repairs on condenser (mine water)	3.20	1.60
5 per cent dep. and repairs on steam piping	2.00	.10
10 " " " " " " feed water heater	1.50	.15
2 " " taxes and insurance on plant cost	110.07	2.20
Superintendence, etc.	-	.32
Total		$19.00

On a basis of 50 per cent load factor we would have the following operating cost per year per kilowatt:

Fuel at 75 cents per ton	7.62
Boiler room attendants	3.42
Power house "	2.00
One general electrician one-half time	0.75
Oil, waste, etc	0.20
Water	2.12
	$16.11

From these figures it will be observed that there is a constant fixed charge of $19.00 per kilowatt installed which is a constant, regardless of the load on the plant. On a basis of 50 per cent load factor there is a yearly charge estimated at $16.11.

At this point in the argument it is well to consider these values. Under the item of fixed cost the values of depreciation and repairs may be considered high. This is not the case. It must be remembered that the plant is installed as a mining plant to suit mining conditions, and not a main central station in some city. The care exercised in preserving efficiency, etc., is in proportion to the intelligence of the help employed, hence the plant may be considered to depreciate rapidly for two reasons: First, variations in the proposed life of the plant or additions may be required from time to time, with the result that present capacities may be hardly operated before the necessary additions are made, thus bringing about a condition of hard usage; second, obsolesence of equipment. The first reason naturally carries with it large repairs. For these reasons the above values are considered fair, and hence the fixed charge per kilowatt of $19.00 is considered fair.

The operating cost such as coal, water, labor, etc., are the real bones of contention, and at best, their estimate is simply a guess, and the nearer the load factor approaches unity the better the guess. It is at this point, the real crucial point in the kilowatt-hour cost, that many fail, simply because of the high value of the load factor assumed.

The investigation of a mine load shows that on account of hoisting, locomotive, and other variable power service, the load naturally varies largely in a plant of the above rating. The variable load being such as to cause the generators to be temporarily overloaded many times during the day. Also, for many periods during the day they are run at underloads, and neither condition tends toward the best economy, although the load factor based on the kilowatt-hours generated may be fairly high. The result is that a large portion of the kilowatt-hours developed are on ascending parts of the water rate curves of the prime movers, and hence, we approach a condition of good load factor on a reduced steam economy. It is such conditions as these that cause the ordinary mine central station to differ from the regular city central station in which the load varies at uniform rates.

Another condition that tends to destroy the calculated load factor is the idle day periods, and, when pumping must be taken care of, the absence of large quantities of water. It is estimated that 105 days per year are idle days, and naturally on these days the load factor is not nearly so good as on the regular working day, and particularly is this true if pumping is not required during a part of this time. The fixed charge of $19.00 still keeps on working silently and so does a large portion of the $16.11 due to operating expenses. The only items of this charge that show any real decrease are the coal and water. Therefore, we see that while it is possible to estimate the average kilowatt cost per year at a total of $35.00 or $0.008 per kilowatt-hour on a basis of 50 per cent load factor, there are other things at work that are apt to change this figure considerably.

It will be observed in the original estimate of $110.07 per kilowatt installed that no reserve or emergency equipment has been included. If such had been the case, then the fixed charge of $19.00 would have to be considerably increased, with a resulting increase in the estimated kilowatt-hour cost.

A number of calculations on mine power plants up to and including 1500-kw. capacity resulted in a close agreement of all the figures which may be expressed in concrete form as follows:

Cost per kilowatt installed..................	$110.07
Fixed charges per kilowatt per year..........	19.00
Operating charges per kilowatt per year......	16.11
Net cost per kilowatt-hour at switchboard....	.008
Load factor..............................	50%

A careful study of these statements by the engineer will bring to mind the following questions: " Why must I be saddled with a fixed charge of $19.00 per kw," and " how can I improve the load factor within safe station limits and reduce the kilowatt-hour cost?"

Some years ago a large central station of 40,000-kw. ultimate capacity, located in a mining region, endeavored to interest the company by which I am employed, in central station power, but lack of understanding of mining conditions on the part of the central station always interfered.

Finally it became apparent if the power company could be induced to sell to the mining company at a rate not exceeding eight mills per kw-hr. on a 50 per cent load factor basis, that the proposition would be a fine solution to the above perplexing questions, and it was with this idea in view that the mining company ultimately took up the consideration of central station power in earnest.

A close study of colliery conditions, such as the expected load factor, periods of high and light loads, peak loads, etc., indicated that if a complete understanding of conditions could be made clear to the power company a contract advantageous to all parties concerned would be considered.

Later a contract was executed to the satisfaction of all concerned, in which the charge per kilowatt-hour was based on load factor only.

Before the contract was signed the following points were taken up and thoroughly discussed:

A. On what basis current would be paid for.

B. Territory to be covered by the contract.

C. Location of meters for registering the power consumed.

D. Delivered voltage, power, and point of delivery.

E. Maximum demand charges.

F. What apparatus should be considered as "connected" load and the methods of rating the same?

G. The included rating of apparatus used intermittently.

H. The method of determining load factor.

K. Pole line charges—co-party lines.

L. Power factor.

M. Explanation of terms used.

O. What constitutes a substation.

The discussion of each of the above topics brought out the following arguments for their adoption, and at the same time illustrates the items that should be considered in any contract between a central station and a mine operator.

A. Current could be paid for either on the "straight maximum demand basis plus cost per kilowatt-hour," or on a varying rate depending on the load factor. The latter plan was argued and adopted, because it is a simpler method of handling all charges. It eliminates errors due to wrong reading of graphic meters, and hence prevents argument as to the demand. To the ordinary mind, it presents the idea of cheaper rates in a clearer manner than rates based on the demand system, as the only point to be observed is that the greater the load factor the less the rate; whereas the straight demand system has a tendency to curtail consumption due to the fact that the demand power may at times be cumulative, and hence the operator may feel worried as he sees the increase on his demand chart, although his kilowatt-hours may not increase.

B. The contract to be of benefit should be made to cover all territory that a private mining plant could ultimately cover in order that maximum results in load factor would be obtained.

C. The preferred location for meters should be on the secondary side of transformers. This is not absolutely necessary if the central station installed the meters.

In any event they should always be located in the customers' substation.

D. In this case the power company agreed to deliver direct into the customer's substation, consequently it seemed fair and equitable to permit the power company to deliver the power and voltage from its nearest available lines. This particular power delivery should always be specified by letter for any particular substation; experience has proven this to be satisfactory. If the customer was required to build his line into his own substation then he should have the privilege of determining his own voltage in order to suit his delivery requirements. This would eliminate the cost of probable transformers on the part of the customer in lieu of the investment required by the pole line.

E. Under (A) it was decided to use a sliding rate per kilowatt-hour rather than a charge based on maximum demand. On what would the maximum demand be based? Certainly not on the momentary maximum starting loads of motors, as the starting peaks of motors would scarcely be noticed on the load curve of a station of 40,000 kv-a.; neither could a two- or three-minute peak be used on account of the difficulty of properly analyzing curves for such a time limit, as errors would naturally be introduced by the thickness of the line; neither could a five-minute peak be used, because this would tend to eliminate hoisting and this would be unfair to the central station, as much hoisting is done on a one-, two-, or three-minute basis.

To settle this question, the central power plant officials visited many plants in the mining regions, and found from actual observation that the rating of the "connected" load was just about twice the average maximum demand that occurs on the plant. Experience proved this to be fairly close, hence for the term "maximum demand" a figure was used that was equal to one-half of the total "connected" load, rated in kilowatts.

F. Since the basis of cost was load factor and since the "maximum demand " as outlined above is used in lieu of station capacity it is necessay to correctly define the " connected load."

This to consist of all direct power consuming devices (no transformers, converters, motor-generator sets, etc.) and is equal to the sum of the name plate ratings of all motors, or lamps, or heating devices, etc.

Exceptions. D-c. hoist and d-c. locomotives to be rated on one hour nominal rated basis. A-c. hoists to be rated on their continuous basis, and where transformers are used for lighting only, then the full kilowatt rating of the transformers is used.

G. Suppose reserve equipment should be installed such as pumps to give protection in time of floods. This equipment would be in service only a few weeks total time per year. It was considered equitable to include this apparatus only for the month during which it was used. It is reasonable to state that in a private mining plant emergency conditions are given preference, and therefore other apparatus would not be worked; for this reason it was considered that such reserve equipment should not be carried from month to month as connected load.

H. The method of determining load factors was intimated in (F) and is as follows: Let the total manufacturers name plate rating of apparatus used during the month equal 500 kw. and let the total kilowatt-hours used during the month of thirty days equal 72,000; then the maximum demand is equal to 500 divided by 2, equals 250, and the average demand is equal to 72,000 divided by 30 days times 24, equals 100; therefore, 100 divided by 250 equals 40 per cent which is the load factor. Reference to the cost curve shows the rate to be approximately $.009, therefore the charge for that particular month would be 72,000 kw-hr. times $.009 equals $648.00.

K. If the coal company should require the power company to build a power line expressly to reach a substation, then it seems fair and equitable that the power company should be paid a charge on this line that will represent a total investment charge on the line. However, if the coal company uses power of a value in excess of this investment charge, then no pole line cost shall be included in the monthly bill, but if no power is used then the full investment charge is to be paid. This service was fixed at 15 per cent of the pole line cost.

Exceptions. If the power company should place extra customers on this line, then this 15 per cent line charge should be pro-rated among the various customers in proportion to their respective " demands."

L. At all times it is to the interest of the coal company to have the proper voltage. The installation of considerable amounts of induction machinery tends to destroy this feature, and may cause trouble to pumps, fans, and hoists, hence to protect itself it is good policy on the part of the coal company to use power factor correcting devices judiciously.

M. In order that no errors in calculations in load factors, etc. could arise through ignorance, the power company considered it advisable to make use of definitions that clearly explain the following terms:

Maximum demand.

Manufacturers name plate rating.

Load factor.

Day.

O. To suitably define the word "substation" it was determined that all operations that could be conveniently grouped under one colliery lease should be known as a substation.

From the above discussion of the elements of a power contract it is seen how essential it is that the central station people should be made to thoroughly understand colliery operations and the conditions relating to connected loads, and in addition they should be made to appreciate the fact that the day load is highly desirable as it reduces their station losses and increases their load factor to a very high degree, and when the night loads occur that they are usually of the constant duty kind.

Failures on the part of others to obtain satisfactory rates could no doubt be charged directly to this lack of knowledge on the part of the central station which is due to the failure of the mining company to properly co-operate with the central station people.

The company with which I am connected has been operating on central station power for about one year, and up to the present we have about 1000 kw. connected load consisting of fans, pumps, hoists, locomotives, and heaters, and provisions are being made to increase this amount in the near future by about 2500 kw. Plans are now under way to remove the present boiler plant from a colliery and operate entirely by central station power.

Since operating on central station power, a number of features have presented themselves that make it look like a very satisfactory arrangement. They are as follows:

1. Our average kilowatt cost is lower than the estimated kilowatt charges in the ratio of about 7½ to 8, and this kilowatt

charge is based on delivery at our substation meters, and not at the main power house switchboard as per the original data.

2. The company is more ready to consider additions to its power equipment due to the fact that main power plant costs have been entirely eliminated and do not appear in the estimate.

3. There is always a " readiness to serve " on the part of the central station and this is seldom true of the mining power plant.

4. There is absolutely no worry due to power plant operation.

5. In case of holidays etc. the monthly bill will increase slightly, which would not be the case in those plants operated by the mining company for in such plants labor and fuel decrease but little.

6. As electric operations increase, less demands are made on the colliery boiler plant with the result that coal will be sent to a ready market which otherwise would be burned under the boilers, and ultimately this will be no small amount of fuel.

7. The use of central station power affords a remarkably cheap method of reaching isolated banks, and isolated pumping problems. Operations such as small washeries are more or less temporary in character, and can be advantageously worked without causing the distress that might be occasioned when operated from a mine central station.

8. The service is reliable. Our service up to the present time does not total more than 15 minutes delay due to failure of supply and these failures were directly due to lightning.

9. The effect of efficiency is not particularly noticeable hence air gaps can be made larger which is highly desirable in mining apparatus as it reduces the danger of break-down.

In the above discussion the author has presented the case of mine central station, vs. public service corporation supply as it appears to him. Experience has been somewhat limited, but the longer the service is continued the more is the author convinced that the purchase of power from public service corporations offers advantages that should not be overlooked by mining corporations.

DISCUSSION ON "PURCHASED POWER IN COAL MINES" (EDDY) AND "CENTRAL STATION POWER FOR COAL MINES" (BEERS), PITTSBURGH, PA., APRIL 18, 1913.

K. A. Pauly: We are very fortunate to have secured these valuable papers on this very important subject of central station versus isolated plants for coal mines. Many of the points brought out might readily form the subjects for separate papers. Such papers are especially valuable because they give the views of engineers, who, because of their experience, are eminently competent to deal with the subject. I do wish, however, to point out and emphasize the agreement between these three engineers whose opinions are based on actual and independent experiences, representing both sides of the question. While there are doubtless large mining companies which can and do maintain highly efficient and economical electric generating and distributing systems, it appears to me to be a foregone conclusion that in general—and this is especially true of all but the larger companies—a public service corporation should be in a position to deliver power at a lower cost than it can be produced in an isolated plant and that, in addition, the service should be much more reliable.

We have, on the one hand, a mining company whose efforts are devoted to the production of coal at a minimum cost, and in which cost the item of power plays but a minor part. In addition to this the power cost is handicapped by a low load factor, resulting in high fixed charges and operating costs, which combine to give a high power cost. On the other hand, the one object of the public service corporation is that of developing cheap and reliable power. It maintains a corps of competent engineers who devote their entire time to the problems incident to the development of cheap and reliable service. They have at their command the necessary capital to introduce improvements and make extensions, which make for lower power cost. Assisting them in their efforts, they have the greatest diversity of loads to bring up their load factor. To sum it up, so to speak, the one is a coal manufacturer and the other a manufacturer of power.

There is but one detail to which I wish to refer in connection with these papers, and that is the question of steam and fuel consumption brought out in Mr. Beers' paper. The contention made by the advocates of the isolated plant, as has been pointed out, is that the price of coal is low. Fuel is but one of the items entering into the cost of power, and the low priced fuel is not necessarily the most economical to use. These inferior grades are high in ash content, require more labor in the boiler plant, and cause a considerably higher cost for maintaining the boilers which may nearly, if not quite, offset the low cost per ton for fuel. I am especially interested in Mr. Beers' statements with regard to the steam and coal consumption of the present, in many cases, obsolete steam equipments about the mines. We advocates of the application of electricity to mining operations have always contended that the actual steam consumptions are far in excess of the figures given by our steam competitors, but never before have we had so complete and positive a statement of the facts as is given in Mr. Beers' paper. Much of this inefficiency is due to wear with age. Contrast with this the almost constant efficiency with the electric motor, which varies but slightly with length of time in service. Again, a great deal of the high fuel consumption is chargeable directly to the low load factor, under which the small isolated plants are operating. I am familiar with a seven day test of a large mine hoist which gave approximately seventy-five pounds of steam per shaft horse-power-hour as the water rate of the engine, and showed that the fuel consumed during the idle period of the hoist was approximately 50 per cent of that required for hoisting.

George H. Morse: Mr. Eddy has remarked that the power plant of the mine is by natural causes necessarily located at the least advantageous point, that is, at the mouth of the mine.

Those are natural causes of disadvantage. I recently installed a fan plant in Eastern Ohio, which suffered from difficulties which came through artificial causes. I refer to the recent law passed in Ohio limiting the voltage to 250 volts in the mines. Two hundred and fifty volts may do for some mines, but there are other mines, which are of sufficient length to make it necessary to use 500 volts, if power is to be transmitted from the mouth of the mine to the interior of the mine. I have in mind one mine at Wheeling in which they have for several years used 220 volts. The mine has gone on increasing in length, and the next step was to introduce a booster, because the voltage was low and the motors heated, and now the booster is not sufficient, and at the mouth of the mine, there are perhaps a dozen large cables, of 600,000 cir. mils waiting to reinforce the copper. These conditions notwithstanding, the point I wish to emphasize is this, that the position taken by the legislature in Ohio, in permitting even 250 volts to be used in mines, is, in my opinion, wrong. Two hundred and fifty volts is, to the speaker's thinking, too high. In fact, if we could have the power introduced into suitable points in the mine at high and workable voltage, so far as primary is concerned, and then reduce it even to as low as 50 volts, I think that insulation troubles and death from electric causes would be very much reduced.

E. D. Dreyfus: The various papers that exemplify the advantages of "importing" power to the coal mine add another chapter to "exploded" theories. On first thought one would naturally suppose that coal as it was disemboweled from the earth could be most economically converted into power on the spot for use on the premises. But this now has been generally proven to fall far short of reality owing to the modifying and militating conditions which have been discussed. The ideas held by our forebears likewise conflict with the present developments, as is evident in referring to the saying originated by an English writer by the name of Fuller, in 1661, "To carry coals to New Castle " (the Pittsburgh coal district of Great Britain), which was used as an expression of a preposterous situation. This conception is no longer valid since we see on every hand power companies delivering the energy equivalent of the coal to the very heart of the mine at distances that already greatly exceed 50 miles. The evolution curiously does not stop at this point, for we are beginning to witness a gradual substitution of the " black diamond " for producing the power required in coal mining by a much more valuable resource in the nature of " white coal " or water power.

The march of progress in this direction has been very marked. In rather limited time, I have personally observed over 200 coal mining companies that have become users of central station power, and this I know is a small percentage of the whole, and the rapidity with which new mines are added to the lists seems to be mostly a factor of time in which the power companies find themselves able to extend their service to the scattered locations. The coal mine operator through his lengthy experience in the production and utilization of coal intuitively becomes a very capable judge of the unit value of power, which often makes " fine haired " figuring unnecessary with him. If ideal cost only were considered the increased use of purchased power by the coal mines would have been impeded, but the practical sense of the operator has fortunately prevailed. I mention these facts merely to indicate that we must go cautiously into the subject of general cost of power. To do it justice we will have to cover an almost limitless field. There are more variable factors and intangibles in power costs than many of us are able to appreciate, and to broadly compare rates, prices or cost without a definite accounting of conditions would be entirely improper. In the philosophy of power costs there must be a liberal reckoning with the various elements that go to make up the total expense. And by virtue of these conditions no two installations or situations in any two districts are directly comparable.

The Pittsburgh district has evidently been the pioneer in this country in applying central station power to the coal mine on a large and extensive scale, and other territories have been able undoubtedly to profit by the important development wrought in this region. But the economy of production has in the meantime advanced to such a degree that the tonnage costs under similar circumstances would be lower than in other localities even if they were to pay higher prices in some cases. However, on examination, and with load factor and surroundings balanced, it is believed that it will be found that the Pittsburgh district enjoys very excelent rates which correspondingly are at the lowest possible level when favorable operations either obtain or are assumed.

Finally we should regard the character of the mine electrical equipment as carefully as the cost and reliability of the central station supply. I can not urge too strongly the employment of the best talent in planning and executing the local installation. Central stations invariably prefer to serve mines in which the application of electrical equipment is properly made, rather than where the apparatus and methods are inefficient, although the amount of power consumed is greater in the latter case. They desire that the service be entirely satisfactory in every respect even if it results in a smaller income to them. Some of the good work that has been carried out under the supervision of our professional electrical engineers has been prominently reflected in the success and economy of the mines for which they have stood responsible.

T. E. Tynes: As to the points enumerated in Mr. Beers' paper, I fail to see any mention made as to whether the coal operator is reimbursed for interruptions to his service, incurred by the power company.

W. Partridge: I would like to call attention to Mr. Beers' figures in which he apparently shows that power can be produced for 8 mills. This may be true in Mr. Beers' case, with a comparatively large station of about 1000 kv-a., and with 85 and 50 per cent load factors, but in the mine plants with which we have had experience the load factor is nearer 20 per cent. Also most of the mining operators are willing to charge for their coal the price which they can get for it on the cars, which would be, at least $1. If you do a little figuring, changing the load factor, and changing the coal, you will, in small plants of 100, 200 or 300 h.p., get a cost in the neighborhood of 2 cents per kw-hr. Recently in Boston I ran across an engineer accustomed to install small plants in mills and factories. His ideas and mine on the subject of power costs were entirely different. He started out by saying that he thought it was absurd to think that at a coal mine, power could be bought from the central station cheaper than it could be manufactured, and then he asked what our rates were. I told him in the case under consideration the rate would be somewhere around 1.5 cents per kw-hr. He thought that that was too low, and that any company that was willing to make a contract to supply power for 1.5 c. per kw-hr., should be required to give a bond to the company with which the contract was made, for fear that the power company might wind up at any minute. Then afterward he got out his pencil and started to figure what he could make the power for, and before he got through he had it down to ¾ ct. per kw-hr., including the fixed charges for a little plant of about 200 h.p. He did not like to figure on a kw-hr. basis. His figures were on the h.p. basis, and one of his claims was that he could and was producing power in a number of plants in New

England on 1.5 lb. of coal per h.p-hr. I afterwards came to the conclusion that one of the ways in which he was fooling himself and his customers, was this. He would take an engine installation, of say, 300 h.p., and take the total cost of operating the engine for 10 hours a day throughout the month, and then assume that the engine was carrying full load during the entire period. Such a basis is easily exploded by taking indicator diagrams on the engine, and those who are familar with selling power on a kw-hr. basis know that the load factor on almost any type of machinery is generally nearer 20, 30 or 40 per cent than it is 100 per cent.

W. E. Dickinson: Practically everything has been said on the side of central stations that can be said, and I have nothing to add, but for the sake of argument I want to call attention to a few points that may suggest a debate here. I should like some one to take sides against the central station to open a debate, knowing the popularity of the central station in connection with coal mining operations.

Mr. Beers in his paper brought out the fact that much steam was used around the coal mine anyway. So, I ask, has he charged up in the fixed charges the expenses of that part of the steam plant used purely and distinctly and entirely for the electrical side of his power plant, or has he charged in repairs and depreciation, etc., for that part of the boiler equipment that is to be used generally for other purposes?

He has brought out the fact that power generated today at 8 mills has been satisfactory. I should like to know what central station company will sell power at 8 mills. I should be glad if any one here, who is willing to make a contract to sell power at 8 mills, for a fairly long period, will let me know where I can see him to arrange for the purchase of power on that basis, as I should like to jot it down for future reference. If you begin to bargain with a central station power plant for power, they are more apt to charge about 2 cents or 3 cents, than 8 mills or 8.5 mills. To purchase power at 8 mills is certainly unusual.

Mr. Beers spoke of it being only fair to pay 15 per cent on the first cost of the pole line. Has he added that into the cost of his power?

Then, his definition of the substation is rather a complex one. He speaks of the substation as being at any prearranged point in one colliery, for instance, at the entrance thereto; while Mr. Eddy speaks of the advantages of the central station because of the ability to move the motor-generator set from one point to another, and the probability of being able to duplicate motor-generator sets and of placing them at several points. I ask whether the attendants upon the motor-generator set or the synchronous converter would not make an additional cost to the coal company? Neither speaker seems to have considered this expense.

Now, another point that has been suggested as favoring the use of central station power is changing the feeding point. Would not that also be possible in using a local power plant, and would not that also entail additional cost?

The making of a long term contract may be beneficial to the coal company, but it may be detrimental to the coal company, under certain conditions, because of the fact that the long term contract is binding upon either company. If, in any way, the coal company should find it to its advantage to make some change in its equipment, it is bound down by a long term contract. The time may come when the erection of its own central station may be justified by the extension of its property.

I believe that those are about the only points I noticed especially that might be brought up for argument.

H. C. Eddy: Various companies that are incorporated under the title "American Gas & Electric Company" are entirely willing to sell power under certain conditions at 8 mills, or even slightly under 8 mills. The cost of producing that power is easily all of that, or perhaps more, and it might be a pertinent question to ask how we can make money by selling power at less than it costs to produce. The answer to that is very simple, it is composed of two words: Diversity factor.

You have an output in a certain given plant of a certain number of kw-hr. per month or per year, which costs to produce at the switchboard a certain amount, suppose we say it costs 8 mills to produce it. That, under ordinary conditions of load, means a comparatively low plant load factor for twenty-four hours, and it is the twenty-four hours' expense that is the most important. The result is that if we can find a customer who will use our facilities and our capacity and our investment, during those hours in the twenty-four when the majority of our customers do not use these things, we can afford very well to sell that power during those hours at about the cost of production, and make a profit, because it will reduce our total cost per kw-hr. for all that we turn out.

There was one other point that the gentleman brought out, and that was the matter of the change of location of motor-generator sets. He suggested that that might be equally feasible in the case of an individual mine power plant. It is, provided the individual mine power plant has the necessary equipment to transmit current for considerable distances with slight loss; in other words if the plant is equipped with alternating-current generators and can deliver current at anywhere from 2300 volts upwards, they can install motor-generator sets, with synchronous converters, and move them about with the same facility as though they were purchasing power, but the fact remains that there are only one or two coal mining companies that do busines on that basis. They are very large, and they have that sort of equipment. The average coal mine, however, has one or two 150-kw. generators, 220 or 500 volts. Occasionally you will find a mine that has a generator of 300 kw. capacity in one size, or something of that kind, but in this section of the country, with the bituminous mines, at least, they run to small units as a rule.

The result is that if they were to sacrifice the present equipment and replace it with the proper amount of alternating-current apparatus, and then were to go further and buy the necessary conversion equipment, their plant cost would continue to go up, and their fixed charges would be correspondingly higher, and their cost per kw. hr. would be increased, so that, even under those circumstances, it would be cheaper for them to limit their investment to the conversion apparatus only, and then place it where it will do the most good.

George R. Wood: I think Mr. Eddy underrates the average size and number of central station mining plants. There are today between 15 and 20 a-c. mining plants with substations, and I believe the average d-c. plant is 300 kw. or over.

There is one additional advantage in purchased power, compared with your own d-c. plant. In the latter case, as the mine advances and requirements increase, an addition to generating capacity means an investment of $80 to $100 per kw. With purchased power, only an additional substation is required, with investment of $15 to $20 per kw.

C. W. Beers: In regard to the power company paying the coal company for delays, I want to say that there is a penalty clause in our contract with the power company from which we purchase power, and under which they are responsible for delays.

They agree to pay our company a certain sum of money, to be reached by agreement, for any delays that occur which are serious enough to interrupt the operations of the mining company. We also have in that contract a clause which gives the power company the right to ask us for delays or interruptions, without charge. For instance, they may want to do something to their generator plant, and we could afford to shut down, say, from some period on Sunday until some later period on Sunday evening, or from Saturday evening until Sunday morning, or something of that kind, so that we are advised in advance to make these arrangements; but for unexpected delays in our operations, resulting from the power company failing to give us power, and which result in damage to us, the power company will pay us a certain sum of money.

In regard to the question of bringing up the case of cost of steam in electrically operated plants, we have no steam at those collieries that we propose to operate electrically. We may bring up the question of heating at these particular plants, but we have not got up to the point yet where we are really running any boiler plant for heating purposes at all. So far we have confined ourselves to a small test, that we are now conducting, in regard to heating by stoves or heating by electric power. We have not come to any particular conclusion on that point, but at one big colliery, that we are proposing to operate entirely by electricity, we will probably during the cold winter months run a small boiler plant.

With reference to the question as to what power company sells power for 8 mills, in the case of the power company I am buying from, we are paying monthly bills now as low as 5.3 mills per kw.

In regard to the question of pole line cost, whether or not we have included this in our annual costs, I want to say we have not included any pole line costs so far, because as soon as we get a line in we propose to load that line up. We want to keep above the minimum charge. We think that is our privilege to do. Where we do not use power, we pay the 15 per cent, but we do not propose to have a line go in and not use it. We expect to use every line and to load it up.

A paper presented at the Pittsburgh Meeting of the American Institute of Electrical Engineers, Pittsburgh, Pa., April 18, 1918.

SAFEGUARDING THE USE OF ELECTRICITY IN MINES

BY H. H. CLARK

In connection with the use of electricity in mining work there are three possible dangers—shocks, fires and explosions. The electrical accidents that occur most frequently are shocks. The conditions under ground are peculiarly favorable to the occurrence of such accidents. One can scarcely imagine conditions that are more conducive to the occurrence of electric shocks than the intimate association of bare conductors with many more or less untrained men standing upon the ground, or upon track rails, in limited spaces that are damp, dusty and poorly lighted.

Sources of Electric Shocks

Trolley wires in mines present the most fruitful sources of electric shocks. Trolley wires are necessarily bare conductors that extend for long distances throughout a mine. They are often installed less than a man's height above the track rail that is used as part of the return circuit, and they are often installed in this manner in places where men must work in making up trips (trains) of cars, as, for instance, at points where loaded trips are brought by electric locomotives to the foot of a rope haulage system.

Another source of danger from electric shock is the accidental charging of parts of equipment that are not supposed to carry electric current. Shocks of this character are obtained most frequently from the frames of coal-cutting machines. The frames of locomotives become charged to the same potential as the trolley wire, if, while the motor or headlights are in operation, the locomotive loses its ground by reason of oversanding or for any other cause. Under such circumstances a very severe shock can be obtained between the locomotive frame and the ground.

Fires Caused by Electricity

The danger from fires caused by electricity arises principally from defective installation and careless up-keep, or from injuries to equipment resulting from falls of roof or similar causes. A short circuit or ground that does not blow the circuit breaker or the fuses may produce heat enough to start a fire by leaking across coal or timbering. The blowing of an open fuse may be accompanied by sufficient heat to ignite combustible material that is close to the fuse. The presence of inflammable material around electric motors or starting rheostats may prove to be a source of trouble. Incandescent lamps produce heat enough to ignite combustible material if the dissipation of heat from the bulbs of such lamps is allowed to become restricted.

Explosions Caused by Electricity

Explosions may be caused by the ingition of explosives, mine gas or coal dust. Accidents due to the ignition of explosives by electricity may be divided into two classes—those that occur while handling and transporting explosives near electric circuits and those that are incident to the detonation of explosives by electrical means.

As to accidents of the first class (with a single exception mentioned hereafter) electricity is no more of a menace than any other source of flame or heat, but it is just as great a menace and should be treated accordingly. As much care should be used in handling explosives in the vicinity of electrical apparatus as though the flashes and sparks that it is capable of giving were constantly in evidence.

Any source of heat may attack an explosive from the outside, but electricity may, under certain conditions, do more than that. An explosive that is a conductor of electricity may come in contact with an electric circuit in such a way that current may be passed through the explosive itself, and although no spark may occur outside the package containing the explosive ignition may take place on the inside. The possibility of such an occurrence may seem to be extremely remote, but accidents have been reported for which no other cause could be assigned, and in which the existence of the above conditions was quite probable.

Since the drawbars of mine cars are electrically connected throughout the length of the trip it follows that whenever the locomotive loses its ground all the drawbars are raised to the potential of the trolley wire, unless some of the drawbars are in contact with the car axles or some of their connections. If the drawbars of a car loaded with metallic packages of explosives were raised to the trolley wire potential it can be easily imagined that the bolts of the car axles could become connected to the drawbar in such a way that the current would flow through the packages and possibly through the explosive itself.

The accidents that occur in connection with electrical shot firing are largely due to the accidental discharge of detonators in the vicinity of explosives, or to the premature ignition of shots after the holes are charged.

With regard to the accidental discharge of detonators in the vicinity of explosives; it is a cardinal principle of safety that detonators should be kept separate from explosives, and that batteries and other sources of electric energy should be kept separate from detonators.

With regard to the premature ignition of shots; it is not the best practise to shoot electrically under conditions that require one side of the detonating circuit to be connected to the earth, because wherever grounded systems of distribution are used, unexpected differences of potential exist in the earth in the vicinity of such circuits. If, therefore, one side of the detonator be purposely grounded, an accidental ground on the other side of the detonator may connect it across a potential sufficient to cause ignition. Premature ignitions have been reported which seemingly have been caused by the conditions just described.

Electric sparks will ignite mine gas and air mixtures that contain between 5.5 and 12.5 per cent of gas (methane). Between these limits (which are rather widely separated) a comparatively small spark is sufficient to ignite the gaseous mixture. For all practical purposes it is safest to assume that all sparks that occur around such electrical apparatus and circuits, as are used for power and lighting in a mine, are capable of igniting gas.

The study of the ignition of coal dust by electric arcs and electric flashes has been undertaken and carried on to some extent by European investigators. The results of their experiments indicate that electric flashes can ignite coal dust suspended in the atmosphere. The Bureau of Mines is now at work upon a similar investigation, which has not, however, progressed far enough to permit of the publication of results.

Conditions Surrounding Electrical Installations in Mines

Underground electrical installations are surrounded by many more trouble-causing factors than are met with above ground. Falls of roof sufficient to wreck trolley lines and feeder systems are of frequent occurrence. Dampness, dust and acid water in sufficient quantities to be detrimental to insulation are not uncommon. Some or all of these conditions must usually be considered in selecting mine electrical equipment. Apparatus that might operate satisfactorily in the absence of these elements will fail when they are present. The space available for installing and operating underground electrical equipment is usually limited, thus increasing the chances for accidental contact with the live parts of the electrical system. Another factor that will appeal especially to those not accustomed to underground work is the lack of light. Not only has this condition a direct bearing upon the accidental contact with the electrical apparatus, but it also has an undesirable indirect influence, because of the difficulties that it places in the way of property installing and inspecting equipment.

As compared with electrical installations above ground, those underground are temporary in character. Circuits and machines are put in place with the certain knowledge that sooner or later they must be removed and installed elsewhere. There is also a good deal of portable apparatus used, such as portable pumps, coal-cutting machines and drills. It is, therefore clear that the economical investment in installation is limited to a far greater extent than it would be upon the surface where equipment is usually permanently installed. This condition increases the natural difficulties of maintaining underground electrical equipment in a condition that is absolutely safe, but it has often occurred to the writer that one of the factors that has been most influential in delaying improvement in underground electrical conditions is the fact that the electrical dangers contribute only a small percentage to the annual death rate in mines. As an illustration; statistics show that less than 3 per cent of the men killed in and about the coal mines of the United States during the first eight months of the year 1912 met their death as the result of electrical causes. It is not that the number of men annually killed in mines by electricity is not undesirably great, but that the number of men killed underground by other causes is so much greater that it quite overshadows the electrical death roll. If the thirty-seven men who were killed by electricity in and about the coal mines of the United States during the first eight months of 1912 had been the only ones killed in connection with the mining industry, effective measures to improve the electrical conditions underground would no doubt have been taken immediately.

Prevention of Accidents Caused by Electricity

The problem of safeguarding electric mine equipment is not a simple one, and at first glance involves so many considerations as to appear hopelessly confusing. A logical first step in improvement of underground conditions would be to remove or to counteract as many unfavorable conditions as may be thus disposed of. As previously stated, scanty light, limited space, and the presence of dust and dampness are underground conditions that are favorable to the occurrence of electrical accidents. The influence of the first of these may be eliminated by providing lights at particularly dangerous places, such as partings and cross-overs. If electric wires are a source of danger at such places they can also be made a source of light to reduce that danger. Although it may be impracticable to eliminate entirely the effect of limited space, this condition may be counteracted by the erection of guards about apparatus. Dust and dampness are elements that can hardly be separated from the operation of a mine; in fact the presence of dampness is often desirable to offset the effect of dust. It is possible, however, to provide apparatus so designed and installed as to resist the action of dust and dampness and the more generous the factor of safety included in such design and installation the greater will be its resistance to undesirable influences.

The problem of safeguarding may be divested of some of its vagueness and put in concrete form by considering that if the electric current can be kept where it belongs—in the conductors designed to carry it—it can not give shocks, set fires, or ignite gas, dust or explosives. Electricity becomes actively dangerous only when it breaks away from its proper channels in stray currents or as sparks and arcs. So far as stray currents are concerned, the confinement of electricity in its proper place is primarily a question of insulation, a term that includes the covering of conductors, the insulators upon which they are supported, and the insulating material used in motors and accessory equipment.

It is sometimes argued that the insulating coverings of conductors deteriorate so rapidly that they provide an added element of danger, because they give a false impression of safety. This argument can not be regarded as universally applicable because its truth depends upon the kind of insulation used and the conditions of service. If bare conductors are used, they should be well installed and to some extent at least guarded, in order to confine the current. With the possible exception of high voltage cables, all conductors, bare or insulated, should be supported upon insulators that are mechanically strong as well as electrically efficient. If bare conductors are used, confinement of the current depends entirely upon the insulators. Moreover, dampness and dust can come into direct contact with the wire, a condition not consistent with the highest factor of safety.

In order to insure a high factor of safety in the insulation of motors and other electrical machines, they must be carefully selected with a view to the service that they are to perform. They must then be protected from moisture and dust, unless such protection is inherent in their design. Care in this respect will be rewarded not only by increased safety, but also by decreased cost of up-keep.

It must be admitted that the electric current can not be kept where it belongs in the sense of eliminating entirely such sparks and arcs as occur at fuses, circuit breakers, air-break switches, starting rheostats, and the commutators of direct-current machines. In this connection the factor of safty must be applied by arranging to confine the outbursts of current to a limited area unoccupied by anything that may be affected by heat or fire.

Assuming that in the installation and insulation of electrical equipment care has been exercised to insure the proper confinement of the current, the factor of safety may be increased by grounding the dead metallic parts of apparatus, by providing means for insulating the bodies of those who work upon such apparatus, and by barring from the vicinity of the current such elements as are explosive or combustible.

It is as important to maintain a high factor of safety as to obtain it in the first place and such maintenance calls for careful and frequent inspection by the mine electrician, whose responsibility can scarcely be overrated. The supervision of the electrical equipment of a mine is a task that requires unusual ability, sound judgment, and experience of a peculiar sort. To select suitable apparatus, to install it properly, and to maintain it free from interruption of service at a minimum cost demands ability. The requirements of safety add a further load of responsibility. It seems to the writer that the electrician holds the key to the problem of safeguarding the use of electricity in mining work. The electrician is the man that deals with the problem at the closest range and in the position of greatest advantage to observe dangers, to correct improper conditions, and to maintain a suitable factor of safety. The power to truly and effectively safeguard the use of electricity in mines rests more with him than with any other one man.

By the way of summary there are offered the following suggestions for reducing the number of accidents due to the use of electricity in mines:

1. Remove contributory causes.
2. Remove from the vicinity of electrical apparatus all elements susceptible to its influence (gas, dust, explosives, combustible material, etc.).
3. Keep the electric current where it belongs.
4. If, under certain circumstances, the current cannot be entirely confined, at least limit the area of its activity by using protective devices.
5. Insure a high factor of safety by (a) selecting material and apparatus with care, (b) installing equipment in a strictly first class manner, (c) inspecting equipment frequently and thoroughly, (d) maintaining it in good condition at all times.

Electrical Equipment that Promotes Safety

In the foregoing, electricity has been discussed as a menace to life and property. There are, however, some ways in which it seems possible for electricity to decrease the risks now attendant upon mining work. There is one piece of electrical equipment that may almost be considered as a safety device and there are three others that by substitution for more dangerous equipment and methods promote the safety of underground workers.

First may be mentioned the telephone, which is of use in spreading the news of trouble, in calling aid to the injured, and in assisting in mine rescue work after disasters. Next may be mentioned portable electric lamps for use of miners. The development of such lamps is just beginning in the United States. At the date of this writing no device has been fully developed and standardized for insuring absolute freedom from gas ignition by lamps of this sort. There can be no doubt, however, that in the near future some such device will be developed and then the electric lamp becomes safer than the locked safety lamp, although it has not the latter's ability to detect the presence of explosive gas. The statement that the electric lamp may be made safer than the safety lamp is based upon the fact that the parts of a safety lamp may be improperly arranged and ignition of gas occur as the result. The records show that this has happened on more than one occasion.

The greatest benefits to be derived from the electric lamp as a safety device will be had in those mines where the electric lamp supplants the open flame lamp and thereby eliminates a real fire hazard.

Next may be mentioned the firing of shots by electrical means. There can be no doubt that the firing of shots by properly designed and operated electrical shot firing devices and equipment, is safer than firing shots by fuses or other devices that ignite explosives by means of sparks or flames.

Finally, it may be suggested that electricity may partially do away with its own greatest danger by substituting storage battery locomotives for gathering locomotives operated from trolley wires. Although main line haulage by storage battery locomotives can hardly be advocated at present, the gathering of coal by storage battery locomotives seems, in many instances, to be a feasible proposition. The use of storage battery locomotives would entirely do away with the trolley wire from a large part of the mine entries that are now provided with this dangerous equipment. In addition to the greater degree of safety assured, storage battery locomotives would be more flexible to operate than are cable reel locomotives. The load factor on the generating station would be materially improved, satisfactory voltage regulation of the distributing system could be obtained with less copper, and the expense of installing and maintaining trolley wire and rail bonding would be eliminated in the entries worked by storage battery locomotives.

Discussion on "Safeguarding the Use of Electricity in Mines" (Clark), Pittsburg, Pa., April 18, 1913.

C. A. Lauffer: During the past four years I have been an advocate of the Schafer prone pressure method. This method bids fair to become the universal method employed for resuscitation. During this time it has been my privilege to instruct over two thousand men in giving artificial respiration, and I have done considerable writing and speaking on the subject of artificial respiration.

But in coming before you today, I came expecting to hear what somebody else was going to say. It did not occur to me that your chairman would call upon me. I presume that every gentleman here has read the report of the National Electric Light Association on the subject of resuscitation. The Committee appointed by that Association, which was composed jointly of prominent engineers, and eminent physicians agreed unanimously that the Schafer method is the most promising method to employ in efforts at resuscitation. When the manufacturers of mechanical devices produce something that every electrical workman can carry round in his vest pocket, then we will agree that the manual method can be supplanted by the mechanical method, but so long as mechanical devices are so large and so difficult to have when they are most needed, if we neglect instruction by manual methods, the present loss of life will continue to be disastrous.

As to the mechanics of artificial respiration by the prone pressure method, we know that in breathing, the main muscle which is used is the diaphragm. The diaphragm is arched, and above the diaphragm lie the lungs and the heart. Below the diaphragm, under these ribs, up to the fifth rib lies the stomach. On the other side lies the liver. Below these organs, behind, lie the two kidneys. Behind the stomach on the left side lies the spleen.

Now, when we give artificial respiration by the prone pressure method, we lay a man on his stomach and turn his head to the side. In turning his head to the side we prevent the mouth making contact with dirt or water. We turn his head to the side, and that permits any fluid in the mouth or air passages to run out. When we lay him on his stomach and turn his head to the side his tongue falls forward. There is no necessity of any device to pull the tongue forward, because gravity causes the tongue to fall forward. With the patient in that position, lying prone, with his arms spread out, or above his head, we proceed to make pressure on the lowest ribs. The pressure is made on the eleventh and twelfth ribs with the heel of the operator's hands, and the pressure is made far out towards the ends of the ribs. When pressure is made that way for three seconds, it is possible to empty the lungs, it is possible to drive out more air than is ordinarily driven out in our natural breathing, so that by making the pressure twelve times a minute, and completely removing our hands after each pressure, we see at once that we are giving the man more cubic inches of air than he gets in his natural breathing, and we are bringing back into play the diaphragm; we are forcing these movable organs of the abdomen up against the diaphragm in a manner that not only restores the action of the lungs but also assists in maintaining the action of the heart. We cannot give massage of the heart, such as is done sometimes during abdominal operations, manually, but we can give massage of the heart by throwing these movable organs up against the diaphragm twelve times each minute. There have been several instances where victims of electric shock, declared dead by physicians, were brought to by friends of the apparent victims. It very often happens that linemen and electric workmen have better muscles than doctors, and more courage and consequently more success; because, with the people who have the courage, the tenacity of purpose, this prone pressure method succeeds admirably in quite a number of cases.

J. S. Jenks: I will ask the doctor a few questions for the benefit of some of us here concerning the length of time it would be advisable to keep up such action, and the advisability of the use of stimulants, and methods of administering them, and methods of aiding and assisting the heart and lung action by hypodermics or some such thing as gases of different kinds, or the reëstablishing of breathing by slight electric shock.

I have given this matter considerable thought, and have tried to instruct and train our men in the manner in which they would be able to take care of their comrades in case of accident, and have supplied them with small cases containing hypodermics, atropin, nitro-glycerin, strychnin, etc., and gave instructions where they could procure such gases as would be advisable and advantageous in producing results in extreme cases.

Now, the thing that has impressed me—I have been at a number of cases of accidental shock—is the fact that everybody gets excited, that they go to work with a vim, and work at high speed, and do not take the time that is necessary. I want to emphasize that point of the time element. I have tried to tell our boys and instruct them to count three slowly after each operation, and if they do not do that, they get all worked up, and do not see results, and tire themselves out readily, and give up, and I believe many a good man dies just on account of a man's over-exertion in the early part of the attempt at resuscitation.

I would like Dr. Lauffer to elaborate on the time a man should continue to work. I have known of several cases where there was complete resuscitation after periods of one and three-quarter hours of continuous and unceasing efforts by fellow workmen and physicians and attendants. I have also known of cases where we had very extreme cases of burning and apparent death, and the victim was brought around in as short a time as three minutes through the aid of some stimulation.

C. A. Lauffer: The best gas to inhale is aromatic spirits of ammonia. That is readily available. If you do not have aromatic spirits of ammonia, use any kind of ammonia. If you use aromatic spirits of ammonia, the handkerchief can be saturated and held about three inches from the nose. If you use household ammonia, and it is full strength, try it on your own nose first—dilute it with water, or hold it farther away—and do not take an unfair advantage of an unconscious man. Then as to oxygen, I think aromatic spirits of ammonia is superior, provided you are in the fresh air. In the case of mine gases and various chemical combinations, it seems to me that oxygen, under certain circumstances, is superior to fresh air and ammonia.

No liquid should be poured into the mouth of an unconscious man.

Now, as to hypodermic medication, we never instruct the men to give that themselves. There is usually a doctor available, and the hypodermic drugs have to be administered with much care. The best drug is atropin, the second best drug is strychnin, and when you are not getting results, you give both; both right away, and as soon afterwards as you can, it is a good plan to give digitalin, cactin and camphor in oil, and then other hypodermic stimulants, such as the physician may have with him.

As to the amount of stimulation which you can give, that is a question which ought to be left with the physician. To a certain extent you can whip a mule and make it go, but after a while the mule will drop dead, and it is the same way in the matter of stimulating the heart. To a certain extent you want to stimulate but beyond that too many doses of strychnin are going to prove fatal.

I took no notes of your questions, and consequently I may miss some of them. There was a question asked as to the duration of the treatment. I have known results to be delayed for two or three hours, and in some cases I have seen results come within less than twenty minutes. I know of one case in particular where everybody thought that the man was dead. He was a man about fifty-seven years of age; he had fallen nine feet, lighting on the concrete floor, with his whole weight, on the back of his head, and he was a heavy man. He had a marked case of concussion, and was unconscious; he had no respiration or pulse, and with the stethoscope you could not hear his heart. His comrades knew about artificial respiration and kept busy, and my assistant reached the man and worked over him, giving him artificial respiration; and then he started to breathe and soon stopped again, and as soon as he would stop they would start again, and by keeping him on his stomach and giving artificial respiration by this method, in about one hour's time he came to sufficiently to vomit, and from that time was able to breathe normally. Under ordinary circumstances a man who sustains a fall on his head, or a man who gets a blow in his solar plexus, is going to die, because there is no one around who knows how to give artificial respiration; the general public must take up the subject of artificial respiration and learn how to give it, for you cannot expect doctors endeavoring to reach the scene, always to arrive in time. These people must be assisted. If a man has not breathed in two minutes, he is in as great danger as a man who has been without food for forty days.

Ralph D. Mershon: I noted the pressure you exerted was long-continued and slow, and I judge that is an important feature of the treatment.

C. A. Lauffer: That is the best way, begin the pressure gradually, and continue it until you feel the ribs give way under your hand. Some people are much firmer in their ribs and in their costal cartilages than others; and occasionally you run across a conscious man in these exercises who will exert all the force he has to prevent your giving him the artificial respiration, but when the patient allows himself to be perfectly passive, you will find that a few pounds of pressure will be ample. You can apply the pressure gently, gradually increasing, and when you have reached the limit of compression, then suddenly remove your hands. As you remove your hands, those organs which you have displaced upward by the pressure will drop down again; the diaphragm will descend and the air will rush in. As you make the pressure you raise the diaphragm and compress the lungs and drive out the air. As soon as you remove your hands, these organs fall back into their former places.

The question has been asked if there is any danger of breaking the ribs. I think not. I have never heard of it happening. I suppose if you put enough weight on you can do it, but the blows which break ribs are sudden blows. A man does not break ribs from lifting, and he does not have his ribs broken from such a gradual, firm pressure on his ribs. The only person who can experience any damage by the prone pressure method of treatment is somebody with cancer of the stomach or liver, or a far-advanced case of tuberculosis, but the people who are a living pathological museum are not the men who are engaged in the active pursuits of life.

George R. Wood: I would emphasize that for the purpose of the mining men, we can almost disregard the matter of hypodermics and the other administration of medicine. What the mining man should do is to send for a doctor and then start artificial respiration, and let the company doctor do the other things.

Graham Bright: Mr. Clark brings up the question of sand on the track insulating the locomotive and causing current to go back through the cars and create trouble. This is a point which has caused a great deal of contention between the manufacturers and the operators in that the operator sometimes wants a locomotive that is too light. The result of a light locomotive is that he has to pile sand on the track to get the cars up the grade, causing a great deal of dusty grit to work its way into the bearings. It would be very much to the advantage of the operator if he would either obtain a locomotive large enough, or cut the trips down, so that he does not need the abundant use of sand. Aside from this damage, and the damage or trouble mentioned by Mr. Clark, another trouble exists, in that you provide a sand path for the cars to run on, which, of course, increases the rolling friction of the locomotive and cars, and you also cut down the available voltage for the locomotive by increasing the electrical resistance between the wheels and track. As brought out in Mr. Eddy's paper, low voltage is the cause of a great many of the troubles that take place in mine locomotives and is the cause of much of the high upkeep, owing to the windings of the motors burning out.

Another point Mr. Clark has brought out is the question of storage battery locomotives for mine service. Any one who has had very much to do with storage batteries knows that they require rather expert attendance, and we can well imagine what will happen to the average storage battery that does not receive any more attention than the average mine locomotive gets. In the first place, many of you who have been in mines have no doubt noticed that a considerable number of the locomotives will have their side or end frames cracked or broken, showing that in mines as well as railways on the surface, we have not yet solved the problem of two locomotives running in opposite directions on the same track. You can imagine what would happen to the ordinary storage battery in one of these collisions, so that if we are going to go to storage batteries, we must select one which is mechanically strong and will stand these collisions which are liable to occur in the best regulated mine.

Another point about these storage battery locomotives is that there is a tendency among some of the smaller manufacturers to build a locomotive which they can offer for a very low price. The locomotive must necessarily be of light construction, which makes it totally inadequate to meet the severe conditions about a mine. If we are going to build a storage battery locomotive it must be amply strong to stand the rough usage incident to the mine service, and a locomotive to withstand that service cannot be obtained in the case of many of the storage battery locomotives in industrial service at present. Industrial service is generally outside in the open, or in well-lighted buildings, and the chance of collision is very small. They can get a better class of operators, because industrial plants are, as a rule, located in large industrial centers where the living is much better and it is a comparatively easy matter to get a good class of men to take care of these locomotives, so while the storage battery locomotive sounds like a good way out of some of our difficulties, in the way of protection from electrical trouble, there are a good many details to be worked out, before it can be made a successful gathering locomotive.

W. E. Dickinson: There is one subject that Mr. Clark treated in his paper that is of special interest to me, viz., the accidenta discharge of shots after the charge has been made. As most of you know, this matter was investigated to a certain extent in Kentucky, and it was thought to have been demonstrated that stray currents have fired shots when copper needles were used. I believe that it was stated that within a mine investigated, considerable electrical potential existed between certain points, sufficient, it was decided, to produce in the explosive a current sufficient to ignite the shot.

I do not understand how such a conclusion could have been drawn. It is an easy matter to find with a sensitive voltmeter a potential difference between two points in a mine. But it is difficult to get an appreciable current to flow through even an ammeter connected to these two points, owing to the internal resistance of the circuit through which the current must flow. Moreover, the explosives used in shot firing are of very high electrical resistance. Admitting that the current may travel through the copper needle, it must then find its way through the explosive and be large enough to generate therein sufficient heat to fire the shot. It is quite doubtful that even stray currents leaking from trolley to track would select the high resistance path of the explosive material. But it is very probable that leakage currents will enter poorly insulated detonator leads under favorable conditions and thus prematurely discharge a shot.

Mr. Clark said, however, that if currents are held within their proper channels by proper insulation, it is not possible for them to ignite shots. I can not entirely agree with him on this point. I believe that many accidents in mines are due to induced currents in short circuits, regardless of the nature of the insulation of the power lines.

Some time ago my attention was called to an accident which occurred in West Virginia, in which a shot was fired prematurely when a hand generator was to be used. The leads from the detonator had been untwisted and brought out separately into an adjacent passage. The generator was then placed between the rails of the motor track and connected to the detonator leads. When all preparations had been made as usual, the operator attempted to fire, the shot failed; another attempt to discharge the generator was made without success. The man was about to investigate the trouble when the shot went off unexpectedly. Fortunately, he was not seriously injured, and lived to describe the conditions.

There was a motor operating on the track, and you know that a motor in starting up will draw an immense current for a short time. This current sets up a rapidly changing magnetic field around the track and trolley. And such a field of flux, when favorably cutting a closed conductor of low resistance, may induce in a circuit a voltage sufficient to produce a current large enough to fire a shot. So, it occurred to me that a premature discharge of a detonator might be attributed to induced currents under favorable conditions. I do not know that this was the specific cause in the case described, but I could not attribute the accident to leakage currents occasioned by poor insulation. I feel sure, however, that under certain conditions it is possible for such an accident to occur. If a pair of untwisted wires are run from a detonator, for even a short distance, along a rail carrying a varying current, and one wire is on one side of the rail and the other wire on the opposite side, or one wire is close to the rail and the other located at considerable distance therefrom, the circuit being completed by the generator, no insulation would prevent the induction of current sufficient to fire an electric cap in the circuit. I need not go further into the technicalities of this point. But I wish to add that immunity from such a danger lies in the use of leads well twisted from battery to detonator. And this is a precaution to which we seldom pay sufficient attention.

Wilfred Sykes: I would like to draw attention to one point in Mr. Clark's paper, where he says: "If the 37 men who were killed by electricity in and about the coal mines of the United States during the first eight months of 1912 had been the only ones killed in connection with the mining industry, effective measures to improve the electrical conditions underground would no doubt have been taken immediately." I want to draw attention to the increasing attention that is now being given to the question of safety. Our steel mills at one time killed great numbers of men. In the last few years the people running the mills have come to the conclusion it is not very economical, and to protect their own interests they have given the question of safety a great deal of attention and spent a great deal of money upon it.

It seems to me that the number of accidents can be materially reduced by the work of the Bureau of Mines, if it obtains proper statistics and analyzes the causes of these accidents. The same thing is being done in other industries, and in this connection I wish to say—I am not quite sure of my figures—that about four or five years ago the number of accidents in steel mills in this country per ton of product was several times greater than they were in Europe, particularly in Germany. I understand at the present time, through the campaign which has been carried on, particularly by the electrical engineers, who have assumed the duties of safety engineers, that the number of accidents has been reduced until we have less accidents per ton of material produced than in Europe.

I think that the work of the Bureau of Mines, in which Mr. Clark has taken an active part, can materially reduce the number of accidents, by the collection of statistics, and by having proper regulations adopted in the different states. Perhaps, in that connection, we may some day see federal regulations controlling the mining industry, and then we will have a much better control over electrical work.

L. R. Palmer: May I ask Mr. Clark what is the method of testing the electric safety lamp as compared with the standard safety lamp, and how you can determine their relative merits in that line?

The last speaker mentioned the work done by the electrical engineers in the line of safety. Perhaps it might be well to present to the Chairman of this meeting the report of the First Cooperative Safety Congress, held at Milwaukee, Wisconsin, September 30 to October 5, 1912, under the auspices of the Association of Iron and Steel Electrical Engineers, with headquarters at the Hotel Pfister. It was an electrical engineer who first suggested that this Congress be held, and it was Mr. Gano Dunn who was our chief adviser and assisted in drawing up the resolutions that brought into being the National Council for Industrial Safety. Anybody who cares to see a copy of that report can get it by writing to any member of the committee which was appointed to manage the affairs of the Congress.

George R. Wood: Referring to electrical accidents mentioned by Mr. Clark, the tendency is to magnify electrical accidents on account of the comparative novelty of electrical mining, and to lose sight of the general improvement in mining conditions, and reduction of the general hazard by its use.

For example, in 1911, Pennsylvania State Reports show 265 fatal accidents to 66,000 pick miners, or 4 per 1000. There were 127 fatal accidents to 55,000 machine runners, loaders and scrapers, or 1.3 per 1000. Had Pennsylvania's total output been mined by hand, total number of pick miners would be 136,000, with, on the same ratio, 544 fatal accidents, against 392 at present.

The same reasoning applies to haulage, where one locomotive, with two men, replaces 5 to 20 mules, each with a driver.

The increased output per man employed, resulting from the installation of electrical machinery, results also in decreased liability to accident, on account of the reduction in number of men employed.

Almost every mining district in the country today is short of men, and it would be impossible to operate in a large proportion of the bituminous districts without the use of electrical machines, locomotives and pumps.

Harold H. Clark: Mr. Bright spoke rather discouragingly of the use of storage battery locomotives. It was far from my intention to suggest that everybody should at once adopt the storage battery locomotive even for gathering, but I think it undoubtedly would be a step towards safety, and as such I mentioned it. I will further say that I believe that one of these days a practical storage battery locomotive will be developed for gathering.

Mr. Bright's remarks concerning the necessary staunchness of the construction of such a locomotive are well considered. Such a locomotive, and the batteries used to operate it, must be very strong, and very simple to operate. There are storage batteries, I believe, for which strong claims are made along these lines.

Mr. Dickinson spoke of the stray currents, and spoke of the possibility of detonators being discharged by induction in the leads from heavy currents in the rail. I do not consider this to be impossible, but at first glance it would seem rather improbable.

In regard to what Mr. Sykes says about collecting, classifying, and publishing the accidents that occur underground: That is something that is now done by the State Inspection Departments and published every year, usually about a year after the close of the annual period in which the accidents occurred.

The Bureau of Mines several months ago published Technical Paper No. 27, the title of which is " Monthly Statement of Coal Mine Accidents in the United States, January to August, 1912, with statistics for 1910 and 1911."

Mr. Palmer asked about the electric safety lamp test. In the Bureau's Technical Paper No. 47, just received from the printer, are given the characteristics of portable electric lamps as compared to flame safety lamps.

Schedule No. 5 just issued by the Bureau outlines conditions under which the Bureau will test electric lamps to determine their permissibility for use in gaseous mines. These conditions call for a high standard of design and construction. The electric lamp is so safe anyway, that any device applied to it to make it safer must be very nearly, if not quite, 100 per cent efficient, and the tests that we are going to make on those lamps are based on the fact that they should be nearly perfect. We shall take these lamps and try in every way possible to make them ignite gas, and then if we pass them, we shall be reasonably sure they are perfectly safe for use in any mine, no matter how gaseous. Experiments that are reported in Technical Paper 47 seem to indicate that sparks that may come from storage batteries up to a 3-cell battery, or 6 volts, will not ignite mine gas. If such a battery were short-circuited a spark that would ignite gas might be produced if the short-circuit current of the battery were rather larger than the ordinary battery would give, something like 100 amperes for a 2-volt cell and 85 amperes for a 4-volt cell. The only danger that the Bureau believes to exist in connection with portable electric lamps is the ignition of gas by the glowing filament of the lamp, and prescribes that such lamps shall be provided with safety devices, so that if the bulb should be broken, the circuit will be broken, or the lamp short-circuited, so that the carbon filament will not glow at a temperature sufficient to ignite gas.

A paper presented at the Pittsburgh Meeting of the American Institute of Electrical Engineers, Pittsburgh, Pa., April 18, 1913.

ALTERNATING-CURRENT MOTORS FOR THE ECONOMIC OPERATION OF MINE FANS

BY F. B. CROSBY

It is no longer necessary for the advocate of electric drive to dwell at length upon its many advantages. The superior qualities of the electric motor are thoroughly attested by its all but universal adoption in every industrial field requiring the application of mechanical power at widely separated or otherwise inaccessible points. The question which confronts the engineer today, save in exceptional instances, is not that of relative merits of the several possible methods of transmitting energy to the point of application, but rather it is a question of the intelligent selection of the most suitable electrical equipment with due consideration of all factors bearing upon the particular case in hand.

Efficient motor design is essentially a compromise. No single type possesses all the desirable characteristics of the ideal machine. Recognizing the limitation of design and the physical impossibility of producing a universal motor, electrical manufacturers are giving more and more attention to the specific requirements of certain clearly defined classes of service. As a result of this specialization and the steadily increasing variety of forms manufactured, it is obviously of greatest importance that for any given duty the proper motor be selected, otherwise a motor which under conditions for which it is designed, would give entire satisfaction, may very likely under different conditions prove an utter failure or at best a needlessly expensive piece of apparatus. For example, induction motors can be readily designed to develop at reduced speeds:

a. Constant horse power, —increasing torque.
b. Constant torque,—horse power proportional to speed.
c. Torque decreasing with speed.

Moreover these motors can be given either a maximum or overload rating. The power required to drive a centrifugal mine fan varies approximately as the cube of the speed. Obviously to install for fan service a motor capable of developing constant horse power at reduced speeds or with any considerable overload capacity, does not represent sound engineering, either from the standpoint of first costs or operating characteristics.

It is the purpose of this paper to indicate certain considerations which should govern the selection of the mine fan motor and describe briefly those types which have been adapted to this service. No attempt is made to discuss the relative merits of the several types of fan on the market or forced draft as compared with the suction draft fan. It is assumed that the type of fan and horsepower required to operate it have been determined for any given application.

Two general conditions arise requiring (1) constant speed drive, and (2) adjustable speed drive. Until recently, for adjustable speed drives, it was practically necessary to install direct-current motors. This necessity no longer exists. In laying out new installations not handicapped by an existing direct-current system, alternating-current motors only need be considered.

In the following discussion the term " constant speed " implies no appreciable change of speed from no-load to full-load; " variable speed " implies speeds varying with the load, but constant at constant load; " adjustable speed " implies several independent speeds, each constant under varying load. In either case the polyphase alternating current motor in some one of its several forms fully meets all requirements. The advantages of alternating current from the standpoint of efficient transmission and facility of manipulation are well recognized, as are also the high efficiency, sturdiness, simplicity of operation and low maintenance charges of the induction motor.

I. Constant-Speed Drive

This is obviously the simplest condition met with and is found chiefly in connection with long railway tunnels, subways, and old mine workings in which the volume and pressure of air handled is practically constant twenty-four hours a day and every day in the year. For such service the standard polyphase induction motor is without a competitor. Properly installed, such a motor can be stopped or started automatically in emergency by remote control and will run constantly without attention other than occasional inspection of the oiling system. Whether the squirrel cage or phase wound rotor should be used depends upon the capacity of the generating and transmission systems and the maximum permissible peak loads during starting. The question of voltage depends upon location and capacity of the motor. It is rarely advisable to carry more than 220 or 440 volts underground. For motors of 300 h. p. or less, above ground, it is in general desirable to use 2300 volts, or less, although 6600 volts can be used for motors as small as 150 h.p. with slight sacrifice of power factor. If high voltage must be used for transmission to remote points, the cost of a low voltage motor with suitable step-down transformers will usually be about the same as for the high voltage motor, while the operating characteristics of the former will be somewhat better.

II. Adjustable-Speed Drives

The great majority of mine fan installations require an adjustable-speed drive and for this reason, as stated above, until comparatively recently it has been practically necessary to install direct-current motors with shunt speed characteristics.

Good ventilation is fundamentally essential in all underground mining operations. Not only must the temperature and the carbon dioxid (CO_2) content be kept down for most efficient working conditions but also especially in coal mines poisonous and explosive gases, such as carbon monoxid or " fire damp " (CO) and the scarcely less dangerous inflammable coal dust suspended in the air, must be continuously removed. Absolute continuity of service and reliability under all conditions are imperative requirements of fan service.

It is evident that in contrast with the conditions outlined in case I, the volume and pressure which the fan must deliver increases from day to day as the headings are advanced and the drifts extended. To accomplish this the speed of the fan must be increased from time to time until the limit of efficient operation is reached. It has become standard practise to install a fan of sufficient capacity to meet the ultimate requirements at maximum speed and operate it at reduced speed when first installed. Such a fan may be operated six or eight months or a year at the minimum speed and light loads before the first increase in speed is required. It is frequently years before the maximum capacity is required.

The average fan motor operates under practically constant load throughout long periods, consequently maximum efficiency is a consideration secondary only to the reliability of operation. The direct current shunt motor shows a fairly high efficiency throughout its range of operating speeds but its installation is subject to the disadvantages of the first cost, maintenance, and attendance inherent in a direct-current generating and distributing system. As compared with these disadvantages the many desirable characteristics of the induction motor have frequently led to its installation even at the cost of a compromise between efficiency and desirable operating speeds. Numerous schemes for obtaining speed control for polyphase induction motors have been developed. Among those of chief importance are the following:

1. Constant-speed motors with changeable pulleys.
2. Variable-speed motors with rheostatic control.
3. Multi-speed windings.
4. Concatenation.
5. Single cascade motors.
6. Dynamic regulation.
7. Brush shifting motors.

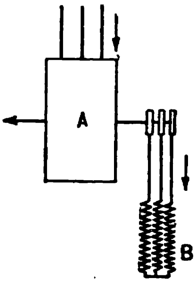


FIG. 1—INDUCTION MOTOR AND RHEOSTATIC CONTROL.

1. *Changing pulleys* is obviously a makeshift method subject to annoying delays and limitations such as practical ratio of pulley diameters, and distance between center lines of shafts, etc.

2. *Rheostatic Control.* When operated with negligible secondary resistance the polyphase induction motor is inherently a constant speed machine. Continuous speed control can be had by means of an adjustable resistance in the secondary circuit but the efficiency falls rapidly as the range of operating speeds is increased. (Fig. 4). In Fig. 1, A is the main motor and B is the external secondary resistance. Assuming that 50 per cent speed reduction by rheostatic control is required, it follows that the shaft output, with fan load must be approximately $(\frac{1}{2})^3$ = 12.5 per cent, and an equal amount of energy must be dissipated in the rheostat in addition to the losses in the motor itself. The effect on overall efficiency is obvious.

Another vital objection to rheostatic control lies in the fact that with external resistance in the secondary circuit, the speed varies with the load, accelerating under light loads and dropping again, to a value determined by the secondary resistance when the load comes on. Since at no-load the secondary current is negligible, no amount of resistance within reasonable limits will hold down the speed of the motor.

Fig. 2 shows increase in speed of motor corresponding to several initial speeds when load is changed from 1¼ to ¼ times full load torque.

3. *Multi-Speed Windings.* Where two definite constant

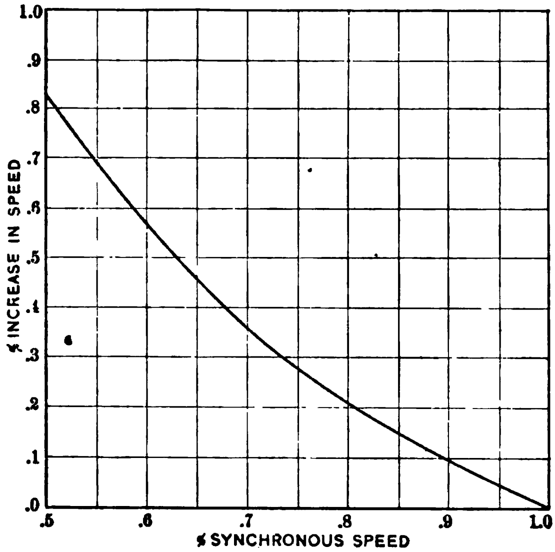


FIG. 2—INCREASED SPEED CORRESPONDING TO DECREASED TORQUE.

speeds are sufficient the induction motor can often be supplied with external connections by means of which the polar grouping can readily be changed to give the desired synchronous speeds. The cost of such a motor is but slightly more than that of a single speed motor provided a 2:1 ratio is employed for the high and low speeds. Where a ratio other than 2:1, or where three or four constant speeds are required, the condition can sometimes be effectively met by two separate windings in the same slots, one or both of these windings being arranged for external multi-polar grouping. Three separate windings are not permissible in practical design. Such motors are sometimes used with or without changeable pulleys and rheostatic control for intermediate speeds, but at best are a compromise, since the number of constant speed steps is limited by characteristics of design and by prohibitive costs. Rheostatic control is always objectionable for reasons noted above.

Fig. 3 shows diagrammatically the arrangement of stator and rotor windings for a two-speed changeable pole motor.

Fig. 4 shows typical efficiency and power factor curves for a two-speed motor with rheostatic control.

4. *Concatenation.* Another method of obtaining three or more constant speeds, particularly where low speeds are required, has been employed abroad for mine fan service. This scheme employs segregated electrical and magnetic circuits and is known as operation in "cascade" or "concatenation."

In general two single-speed motors, one of which at least has a polar wound rotor, are mounted on the same shaft. The primary of motor *A* is connected to the secondary of motor *B*. Each of these motors may have either single or multi-speed windings and may be operated independently of the other as well as in concatenation. The second motor may have either a phase-wound or squirrel-cage rotor. In case phase-wound rotors are used, speed regulation by secondary rheostatic control may be obtained in the usual manner, and with the usual objections.

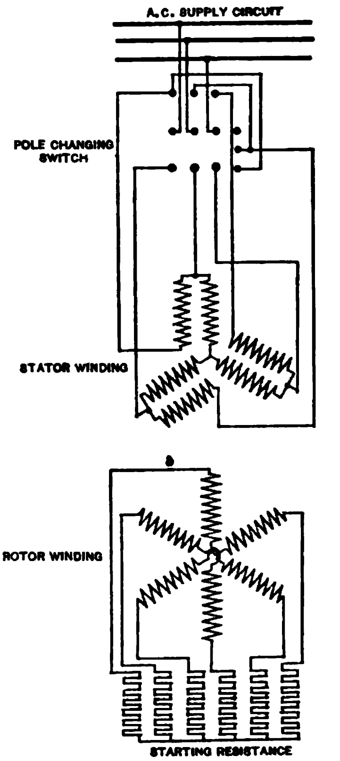


FIG. 3—MULTI-SPEED MOTOR WINDINGS

Two motors are connected in direct concatenation if they show a tendency to start in the same direction and in differential concatenation if they tend to start in opposite directions. The synchronous speed of motors in concatenation may be determined as follows:

$$\text{Speed} = \frac{\text{cycles} \times 120}{P_1 \pm P_2}$$

where P_1 = number of poles of first motor and P_2 = the number of poles of the second motor, the plus sign being used for direct and the minus for differential concatenation.

With the multi-speed pole changing motors it is necessary to open the primary circuit when changing from one speed to the other, this may be avoided in concatenated sets by introducing resistance in the leads between the two motors, the resistance being cut in or out step by step when changing speeds. (Fig. 5).

As noted with a polar-wound rotor, any reduction in speed by

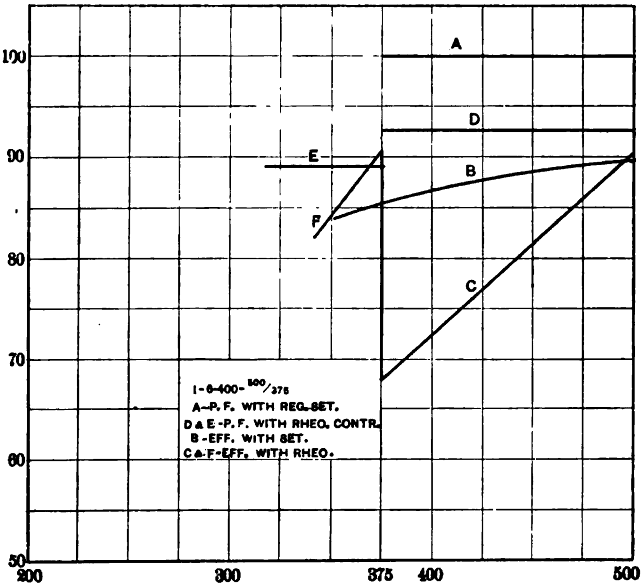


Fig. 4—Efficiency and Power Factor, Two-Speed Constant Horse Power Induction Motor

rheostatic control is accompanied by a proportionate reduction in efficiency, the power factor remaining practically constant. With concatenated motors the efficiency remains approximately constant provided there is no external resistance in the rotor circuit of the second motor, while, at the lower speed, the power factor drops, due to the fact that as compared with their normal individual ratings, the motors are underloaded when in concatenation.

5. *Single Cascade Motor.* The single cascade motor offers still another method of obtaining two or three definite fixed

speeds. This motor has an internally concatenated winding. The stator winding is of the usual full and half speed type. The single winding of the rotor is so arranged that its magnetizing effect is the same as would be produced by two separate windings. It is, however, a decided improvement over two separate windings, since all coils which in such cases would neutralize each other are omitted in the concatenated connection and grouped together for connection to the slip rings for use only at other speeds. When the primary of the first element is properly connected with two circuits per phase in multiple corresponding to the number of poles, these circuits are in exact opposition for the number of poles in the second element and form a perfect path for short circuiting the secondary of the second element. The stator winding, therefore, carries two currents simultaneously (1) a current from the line at full frequency and (2) induced current at frequency corresponding to the slip. The general arrangement of windings is shown in Fig. 6.

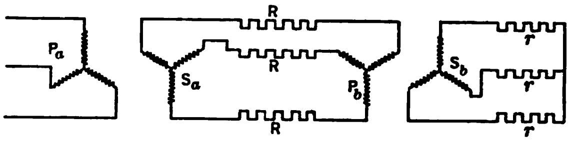


Fig. 5—Concatenated Winding

By inserting a variable external resistance between certain points in the stator windings and open-circuiting the collector rings, variable speed control can be obtained for the concatenated connection. Resistance across the collector rings gives ordinary variable speed characteristics for the other two speeds.

This type of motor is particularly adapted to the three speeds corresponding to the polar ratios 1-2 and 3, for example 4-8-12, 6-12-18 etc.

6. *Dynamic Regulation.* As stated earlier in this paper each of the foregoing schemes possesses inherent disadvantages which until recently have made it practically necessary to employ a direct current motor with shunt characteristics whenever close regulation was required for a large number of speeds each constant under varying loads.

Recently means have been perfected whereby shunt speed characteristics can be had with the standard polyphase induction motor together with high power factor and high efficiency throughout the range of operating speeds. The method employed is susceptible of several modifications and will be referred to inclusively as dynamic regulation. With rheostatic control the secondary energy is dissipated as heat whereas with dynamic regulation the major portion of this energy is returned to the system. Referring to Fig. 7, the external resistance *B* of Fig. 1 is replaced by the compensated commutator motor *B*, which forms one element of a two unit motor-generator set, the second element of which is a standard squirrel cage induction motor connected to the supply mains. This machine is driven slightly above synchronism by the commutator motor and operating as an induction generator returns to the system, energy proportional

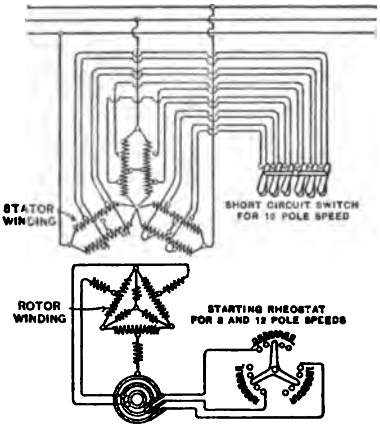


FIG. 6—SINGLE CASCADE MOTOR WINDING

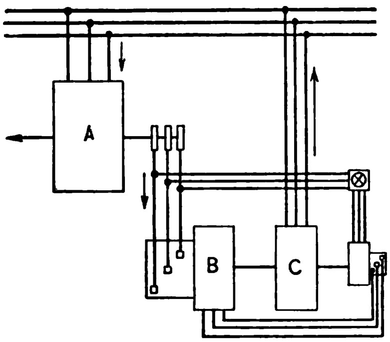


FIG. 7—INDUCTION MOTOR WITH REGULATING SET

to the slip of the main motor, less the losses in the set itself. The commutator motor receives energy from the secondary of the main motor at relatively low frequencies and in general must have a proportionately small percentage of the main motor capacity. Assuming that *A* is to drive a fan at 50 per cent of synchronous speed the horse power delivered to the fan will be approximately 12.5 per cent, of the rated capacity of the motor and the same amount of energy will be delivered to the regulating set.

If the connected load required constant horse power at the motor shaft, then neglecting losses, the motor input at 50 per cent speed would be 200 per cent, the shaft horse power 100 per cent and the energy returned to the system 100 per cent.

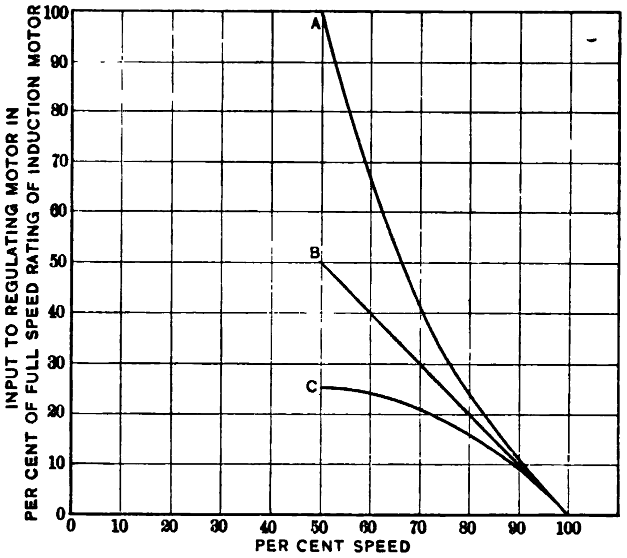


Fig. 8—Input to Regulating Motor

A—Constant horse power.
B—Constant torque.
C—Horsepower proportional to speed square.

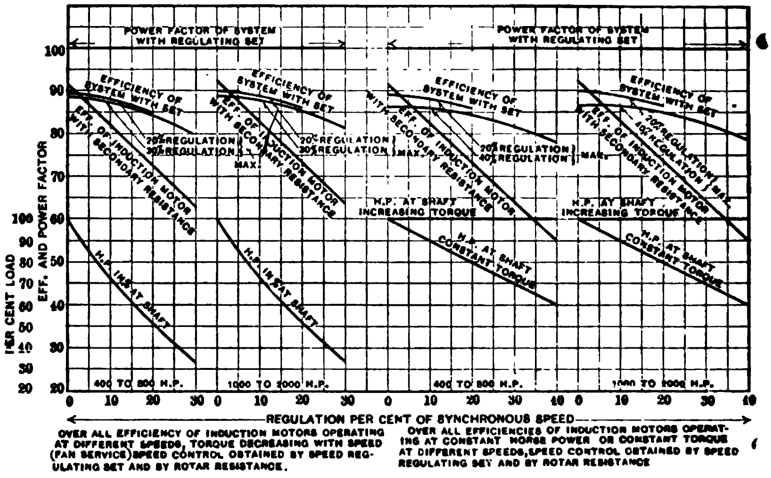


Fig. 9

If again the connected load required constant torque at the motor shaft, then neglecting the losses, the motor input would be 100 per cent, the shaft output 50 per cent, and 50 per cent would be returned to the system. This is shown graphically in Fig. 8. If X per cent regulation is required at constant torque the motor generator set must have X per cent of the normal horsepower capacity of the main motor. In general where the required torque varies with the speed the regulating set must have $\frac{X}{1-X}$ per cent of the normal h.p. capacity of the main motor. Fig. 9 shows typical curves of efficiency and power factor for the conditions indicated above with dynamic regulation and rheostatic control. Where standard regulating sets are employed they can be of relatively high speed and inexpensive design as compared with the slow speed main motor.

Fig. 10 shows a modification of the above scheme in which the commutator motor B is direct connected to the main motor shaft. In this case the slip energy of the main motor is transformed to mechanical energy and the torque of B added to the torque exerted by the main motor A. This arrangement is especially desirable where constant horse power must be maintained at the shaft. For fan service, however, the high-speed self-contained regulating set is usually cheaper and yields equally satisfactory results. Furthermore, the possibility of applying the standard regulating set to any standard induction motor with phase-wound rotor in event of future re-arrangement of equipment, is an important advantage.

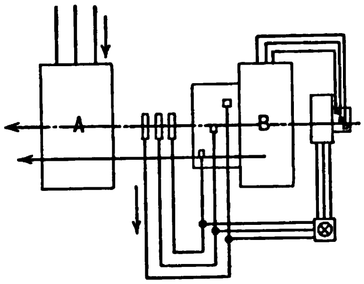


FIG. 10—DIRECT-CONNECTED COMMUTATOR MOTOR

In addition to the advantages of adjustable constant speeds under varying load and high operating efficiency the possibilities of power factor correction are often of great importance. If desired, unity power factor can be maintained on the main motor with all the usual beneficial results in improved regulation and increased energy capacity in power station and transmission system. Unity power factor correction naturally involves a somewhat more expensive set since the magnetizing current is supplied by the commutator motor instead of from the line. The standard sets should have sufficient capacity to raise the power factor of the main motor about 10 per cent, maintaining an average power factor of 95 per cent or in some cases 100 per cent without increased cost. In special cases it is possible to supply sufficient magnetizing current from the commutator motor to give the main motor a leading power factor thus obtaining a certain corrective effect for low power factor conditions on the external system. The kilovolt-ampere capacity, copper losses and first cost will be increased and the overall efficiency lower in this case.

The operation of the set with magnetic control is very simple. The induction generator is thrown across the line by means of standard compensator and brought up to speed as an ordinary squirrel cage motor. The main motor is started by closing the primary oil switch and accelerated by automatic current limit or

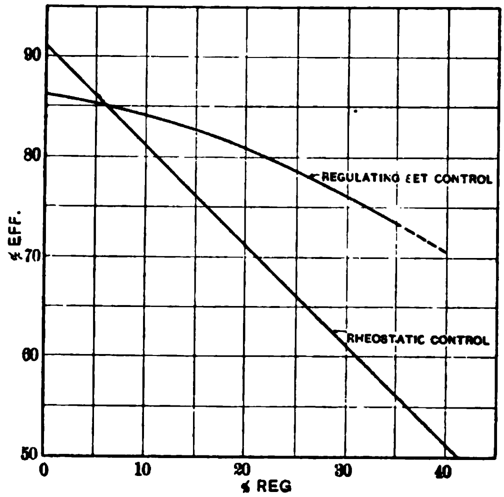


FIG. 11—EFFICIENCIES, 400-H.P. MOTOR

hand control. The speed of the main motor adjusts itself to the tension determined by the setting of the exciter field rheostat. Any further speed adjustment is obtained by the manipulation of this exciter field rheostat.

The range of speed regulation obtained is limited by the maximum frequency impressed on the commutator motor. In general for good design this should not exceed approximately 20 cycles, which will give about 30 per cent regulation on a 60 cycle motor. This limit varies somewhat with the size of motor involved.

This system was developed in Europe and in the past four years about thirty equipments have been put in successful operation for mine fans and rolling mills. Ten similar equipments have been sold in this country within the last six months.

Fig. 11 shows efficiency curves with regulating set and with rheostatic control for a 400-h.p. motor driving a mine fan. Two of these motors were actually installed and operated continuously at 40 per cent speed regulation by rheostatic control for nearly a year. Under these conditions the input to the fan would be 21.6 per cent, and the energy dissipated in the rheostat or delivered to the regulating set 14.4 per cent, of the full load rating of the motor. From the curves the relative over-all efficiencies with rheostatic and dynamic control are seen to be 51 per cent and 70.5 per cent respectively. The relative power consumption is therefore 169.5 h.p. and 122.5 h.p. or a net saving of 47.0 horse power per machine. Assuming that power can be purchased for one cent per kw-hr., the net saving due to

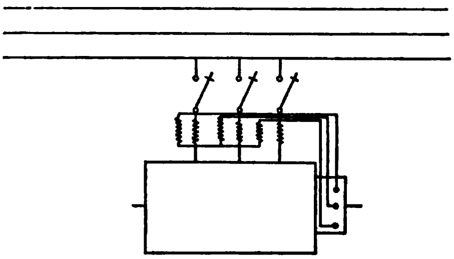


FIG. 12—CONNECTIONS FOR BRUSH SHIFTING MOTOR

the use of a regulating set with each fan 12 hours per day 300 days per year would be 0.01 × 47.0 × 0.746 × 12 × 300 × 2 = $2525.00 approximately. In many gaseous mines the fans must operate continuously 24 hours per day every day in the year, in which case the above net savings would become $6160.00.

7. *Brush Shifting.* For fan installations requiring motors of 100 h.p. or less capacity, in place of the regulating sets described above, the brush shifting polyphase motor can often be employed to advantage. Fig. 12 indicates the general arrangement of connections for this type of motor. The motor derives its name from the fact that it is started, stopped, reversed and controlled by merely shifting the brushes. With a certain brush setting no torque is developed, consequently the motor will not start when the line switch is closed until the brushes are moved from this position. The speed of the motor is proportional to the shrub

shift. Reverse operation can be obtained by moving the brushes in the opposite direction from the zero position, but for best commutation two phases should also be reversed for reverse operations. Brush shifting is accomplished by means of suitable worm gear and hand wheel conveniently located. Commutation is excellent throughout the entire speed range. Fig. 13, shows power factor and efficiency curves for a 60-h.p. brush shifting motor and for an ordinary induction motor with rheostatic control designed for fan service. These curves were plotted from actual test data.

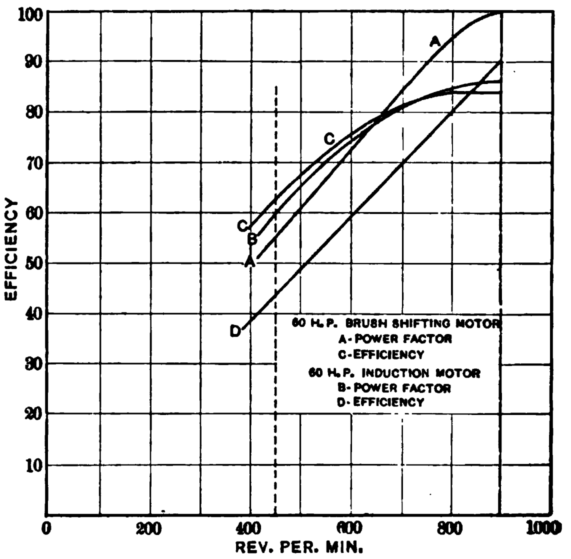


FIG. 13— POWER FACTOR AND EFFICIENCY FOR BRUSH SHIFTING MOTOR

The minimum permissible speed depends largely upon the torque requirements of the driven load. For fan service this type of motor will give stable operation at very low speeds corresponding approximately to 70 per cent slip below synchronism.

In conclusion it would appear to the writer that in the absence of an existing direct-current system, the advantages of the polyphase motor leave small ground for the application of direct-current motors for either constant or adjustable speed fan drive.

Discussion on "Alternating-Current Motors for the Economic Operation of Mine Fans" (Crosby), Pittsburgh, Pa., April 18, 1913.

C. W. Beers: The title of Mr. Crosby's paper is apt to convey, to my mind, something misleading. It deals with the motor side of the question and leaves out the fan side of the question, and I would like to give a few points in regard to the fan operation.

I have read the article by Mr. Crosby with considerable interest, as to me much of the subject matter as presented by Mr. Crosby is new. I was somewhat disappointed, however, to note the apparently small amount of attention that he paid to fans operating in the anthracite region, which, with few exceptions, require small driving power.

A survey of the fans in the anthracite region indicates that with few exceptions the fans are comparatively small. There are a few of 100 h.p., while the majority run below 50 h.p.

From the motor viewpoint of the article, as I have interpreted it, the application of variable speed motors as a fan drive is hardly the correct method of making use of the fan. Looking at it from the fan point of view, in the anthracite region I believe that the prevailing opinion of mining men is that the fan should run at its constant maximum efficient peripheral speed, regardless of the quantity of air to be delivered, although there are quite a few cases where this is not always carried out, due, no doubt, to certain peculiar property conditions.

Mr. Crosby has said that the great majority of mine fan installations require an adjustable speed drive. To this I do not agree unless the arrangements do not permit of splitting the air, or the air required is exceptionally small compared to the ultimate fan capacity, in which case I believe a better plan is to use a smaller fan until such time as a larger fan might be required. In the last paragraph on page 1075 it is said that as the workings advance from day to day that the volume and pressure increases. This is not true. The volume decreases and the pressure increases, and if the fan is speeded up, to give more air, then pressure begins to increase at a great rate, with a considerable increase in power, and if that plan of operation is continued, then the limit of efficient operation is soon passed, the fan becomes inefficient, and the applied power is spent in overcoming practically resistance only, and very little is spent on the air, which is an uneconomical arrangement.

To produce a change in the air, by means of speed change appears to me to be rather bad practise, unless mining conditions prohibit any other method. It produces very poor fan economy, because the fan is not running at its most efficient peripheral speed. And on account of keeping so c ose to the air quantity, the gases may not be properly cleaned out, and it places the care of the fan in the hands of a person who may not, at all times, be under the direct control of the mine foreman.

On account of rapidly approaching the limit of efficient air volume, the fan becomes too small, or else the power apparatus is overloaded in overcoming the mine resistance. The correct way to vary the air quantity of a fan is by changing the air courses, or by splitting the current, thus giving an increased volume and a reduced water gage, and where this plan is carefully followed out, the fan runs at constant peripheral speed, and with slight variation in power. This also results in high fan economy as the applied power is spent more on the air than on the mine resistance, which is not the case in adjustable speed drives.

It is the displaced air that should be the measure of the power expended, and not the amount of resistance to be overcome. To illustrate this point: In a certain mine, the gangway was used as the intake, and the airway for the return. The water gage was two inches, the number of cubic feet was 98,000 and the fan was running at 50 rev. per min. It was necessary that the air be increased. To accomplish this the gangway and old airway were both made to serve as the intake, the headings being used for return. The result was that the fan delivered 178,000 cu. ft. of air, and the water gage dropped to 6/10 in., the fan speed remaining practically constant.

It is a fact that any normal size fan motor may be overloaded by reducing the resistance to a minimum, and working the fan on free air discharge. Conversely, the motor load decreases, when the fan displacement is the minimum, and the resistance is the maximum. Hence, between these limits the quantity of air delivered may be made anything, simply by varying the amount of resistance to be overcome, and as previously stated, this could be made by changing the airways themselves or by proper splitting.

From a consideration of these facts, it appears to me that constant speed motors are the proper style of drive to use, and should be used wherever possible. It is true that the ultimate variation in power on the motor may be large, but even between these extremes, the overall efficiency does not vary so greatly, as the fan efficiency under these conditions is quite high and constant over a wide range in air quantity, provided the fan is operated at its most economical peripheral speed.

Mr. Crosby has said that reliability is the first condition, and then comes economy. As far as reliability is concerned, it seems to me that the simplicity of the ordinary squirrel cage motor could be well capitalized in the matter of economy, when compared to the more complicated adjustable speed drive.

Wilfred Sykes: I am very much interested in Mr. Crosby's paper, and I think it is well that this subject of using three-phase commutator motors for regulating induction motors should be brought up before a meeting of this class. In a paper delivered before the Institute in New York by Dr. Meyer and myself, a great number of other schemes were mentioned that have been worked out, using commutator machines. There are a great many combinations possible, and some of them perhaps simpler than that described by Mr. Crosby.

I notice in Fig. 7, showing a regulating set, that it really consists of three machines, and apparently the small machine on the right is an exciter.

Now, there is one point I want to bring out particularly, and that is the question of the amount of attention that you can give to a mine fan or to mining apparatus generally. The usual condition in a mine is that you start the machine working and you let it go and hope it will keep going, and until something connected with the machine breaks, nobody goes near it. In my mind it is questionable whether you can use a machine having the characteristics of the three-phase commutator motor under those conditions and get good results. These schemes have been mostly developed in Europe, and recently I saw a number of them operating and apparently working very well. I asked the operators a number of questions, the principal one of which was—"How much attention do you have to give to the machine?" And I got the same answer in all cases, that they had to look after these machines pretty well, give them more attention than they would to a straight induction motor. If that is the condition existing in Europe, where they have a very much better class of labor, looking after machinery around mines, where the attendants are much better educated, and have a much better knowledge of the characteristics of the machines, what is going to be the condition in this country? It seems to me we will have to make our machines here very much stronger in every way to meet operating conditions than do our European friends. I am convinced that for ordinary work you could not use the European induction motor and set it down under American conditions and get anything like the results achieved with the machines built in this country. Our operating conditions are different, and the machines have less skilled attention.

The question as to whether it is desirable to use any regulating arrangement at all, is open to quite a little discussion—I am referring now to fan motors. My impression is that in most mines possibly two speeds for the fan will meet all operating conditions, or will meet the conditions with very little sacrifice in economy, if you consider the yearly operating conditions. Now, you can get the two speeds either by a two-speed motor, or you can have some arrangement in which you have different pulley ratios. The mine fan has to be belted in nearly every case, because the fan speed is so low that the motor could not be direct-connected. In opening the mine you do not require so much air, but later, if you want to increase it, there is no hardship in changing the pulley ratio. A condition like that would not warrant the installation of an expensive regulating set. On the other hand, instead of the simple induction motor, you have, as shown in the sketch by Mr. Crosby, a three-machine regulating set, and I think when you come to consider, not only the first cost but the attention, and giving proper consideration to the question of reliability, that there is quite some question as to whether you are justified in the majority of cases in using any scheme of this kind. I personally would very much like to see this thing worked out satisfactorily. On the other hand, I think we must go slowly in applying the European experience to American conditions. I know where it has been tried in a good many cases, and we have had disastrous results.

One of the schemes that was mentioned in the paper that we read in New York on this question involved only a single regulating machine, or, in case the voltage of the line was over 440, a single regulating machine and transformer. Such an arrangement seems to be a simplification of the one indicated by Mr. Crosby, but I think it will be a matter of some time before we can say that these equipments will be really satisfactory for American conditions. I think the experiment of trying out some of these things is one worth while, and such experiments may lead to developments which will greatly enhance the value of the induction motor and avoid the use of the direct-current variable speed motor around mines.

In all of our work in mining it is aimed to cut down the amount of direct-current machinery as much as possible, and try to use simple apparatus. Whether it is advisable to add on a regulating set, especially a machine like the three-phase commutator motor, which has limitations, is a question which can only be found out by experience.

One point arises, and that is the use of the three-phase commutator motor on 60 cycles. Most of our mines use alternating current for 60-cycle power, and that makes it a good deal harder to make a satisfactory commutator machine than if we were using 25-cycles. Of course, the commutator machine is in the rotor circuit of the induction motor, and consequently the frequency on it is variable, depending upon the speed drop required. With a scheme of this kind the condition is not quite so bad, when driving a fan, as it would be if you had a constant torque, because the current decreases as your frequency increases, and that facilitates commutation. I think the question, however, as to reliability of these regulating schemes, is one that has to be given a great deal of attention, and one that makes it desirable to go a little slow in installing apparatus of this kind.

George R. Wood: My impression is that this particular application is not intended as a panacea for all ills, and that it is not expected to use this in those cases where we have fully developed mines, with fans requiring practically constant speed. There are conditions, as we all know, where we have to open a mine with very little air required. In a short time it will require a large fan operating at increasing speed. Mr. Beers suggests putting in a small fan and operating it at suitable speed, and later changing the fan. This is a waste. If we can install a motor which will do everything from the start to the finish of the operation, and get fair efficiency out of it, it would seem to me to be very desirable.

One of the greatest difficulties in the application of motors to mine fans is this requirement for variable speed, and engines, even though wasteful of power, are often installed as a matter of convenience. It is always desirable to have reserve speed and power in mine fan equipment, which ordinarily means inefficiency under normal conditions.

I believe the regulating system described by Mr. Crosby to be very desirable on fan motors of 300 h.p. and over, particularly for bituminous mines, with high and rapidly increasing water gage.

I am glad to note that a number of sets of this kind are being installed by the Pittsburgh Coal Co., both of the motor-generator style for large motors, and three-phase commutator type for small sizes. These will all use purchased power, and valuable data should soon be available.

H. C. Eddy: I ask Mr. Crosby as to the expense attached to the use of the installation—the cost of installation of such a device—as compared with the cost of the main motor, not necessarily in dollars and cents, but in percentages.

Graham Bright: I would like some practical mine ventilation man, to tell us why we cannot run a fan at a constant speed at all times, and throttle the outlet of the fan? They do this in hydraulic work, and why can it not be done in fan work? Let the fan run at the same speed all the time, and then apply some arrangement of putting a throttle at the inlet to the mine, so that as the mine develops we can open up a little more and allow more air to go through. There may be some objections to this scheme, and some of our practical ventilation men may tell us what they are. The efficiency may be somewhat lower than obtained by some of the variable speed methods but the simplicity is such that the continuity of service should be very high.

Mr. Crosby says that sometimes an ordinary wound-rotor motor is used and speed reduction obtained by cutting a resistance in the rotor circuit and that this scheme is objectionable on account of the speed changing with the load. As to a change in load, with a fixed speed, this cannot be, since with a fan running at practically constant speed, the load is constant, and you therefore will not get very much variation of speed with any resistance you put in the rotor circuit.

I would like to know if there is any difficulty in constructing a three-phase commutator motor for 60 cycles, as most of the mine circuits have 60 cycles instead of 25 cycles.

Mr. Crosby also remarks that changing pulleys is rather in the form of a makeshift. When we start a new mine, and the mine develops and requires more air, it is not a question of changing the pulley every day or two. The fan will probably run for six months or more, before it is necessary to change the pulley, and preparation can be made beforehand, so that the change of the pulley will not be a hardship and can be accomplished with little or no delay in the mine operation.

H. Meyer-Delius: I think there would be no question at all that we would use a normal squirrel cage constant speed motor if the mine operators did not want to change the speed, as the first speaker, Mr. Beers, said. I am not at all familiar with these ventilators or fans, and I am told that it is a very difficult problem to deal with. I spoke to several mine operators in Europe, and they came to conclusions contrary to Mr. Beers,' but I do not know whether that is due to different conditions in European mines or to different opinions about the same conditions. The usual conditions in Europe are such that at first when the mine is small they do not need much air, and since they are using a fan and a motor equipment of the full capacity they have either to throttle the pipe or to reduce the speed, and I am told that reducing the speed is much more economical than to throttle the pipe. They mostly run at first at 30 per cent less speed than at the end, and that means a very big difference for the air volume, and apparently they have found out that even at this low speed the fan operates at pretty good efficiency.

I had the opportunity to visit practically all of the installations equipped with such regulating sets, which amount to about thirty or more, and I have some figures which remained in my mind, which may be of interest to you. There are some five or six 1200-h.p. motors, 5000 volts, 50 cycles, with 33 per cent speed reduction, and a maximum speed of about 300 rev. per min., and four plants, of about 900 h.p., with the same speed, and 30 to 25 per cent speed reduction, some of 600 h.p. with the same percentage of regulation, and some smaller ones, among them one of 200 h.p.

It was mentioned in this discussion that the average operator abroad is a much more trained and intelligent man than in America. I really do not know if that is actually true, because the men I have found watching these plants could not be called intelligent men, they mostly were old miners, they would turn the hand wheels according to their printed instructions and had no conception of what happened, but they were apparently able to operate these fans. I must mention that all of these fans were so installed that the operator had nothing else to do than to start a normal induction motor, and the switching over from the starting resistance to the regulating set, was done automatically, so that the man had really nothing else to do than throw in the main switch and cut out the resistances of the rotor, and I think for this work really there could be employed rather unintelligent people.

Moreover I saw some of these fans running entirely alone, the doors locked, and I think once a day a man came over to see if all was right; so I think in Europe, these equipments with regulating sets are apparently developed to such a state that they are very reliable machines, and I have never heard of any complaint of their being unreliable. As far as I could see there, the commutation was perfectly correct, which is, of course, the main trouble with these commutator machines.

From some mine operators I got the figures that they had to pay about three-quarters of a cent per kw-hr. to the central station, from which they bought their power, and that in about one year, the additional investment for the regulating sets was paid by the saving of current, and since the speed of these fans has to be reduced, three, four, or five years, they believed that that equipment was a very good investment.

Mr. Sykes mentioned another scheme, using a frequency changer, a single auxiliary machine. With the frequency changer there were also tests made in Europe, and as far as I know two of them were installed; one was a 600-h.p. fan motor, with 33 per cent regulation, and as far as I am informed this scheme was abandoned because they could not get the frequency changer to commutate at the lower speeds and that is my experience also, that is, that these frequency changers have entirely different commutation conditions from normal commutator machines, and it seems to me so far very difficult to get satisfactory commutation at least, at larger outputs.

W. O. Oschmann: The statement has been made here that mine fans are usually put in operation and then not looked after at all, until something happens. I would not like that statement to be published in the PROCEEDINGS or go into the TRANSACTIONS, without making a defense, for the miners of Pennsylvania in particular. I have here a little book containing the Bituminous Mine Law of the State of Pennsylvania. It plainly states that each fan ventilating a mine must be provided with a recording gage that records the revolutions of the fan, or the pressure entering into the mine at all times, also that the fan must be inspected periodically. The provision is also made that in case anything goes wrong with this fan it must not be stopped, even for repairs, until the mine foreman has been notified, who, when he considers it safe, will allow them to stop that fan in order to repair it. In case the fan does stop, due to accident or otherwise, they are compelled to withdraw the miners from the mine until the fan has been made safe. For that reason, a great many companies have two fans, one in operation and one in reserve, so in case anything goes wrong with either fan they can immediately put another fan into operation, and there is quite a stiff penalty attached to any violation of this law.

B. M. Fast: In the territory I cover I find that the fan requirements vary from 25 to 50 h.p., and in no case have I found them larger. Up to the present time, most mines are operated single shift, but since they have been buying power the question has arisen of operating double shifts, and thereby saving in cost of operation. That time is coming, and even now the operators are talking of developing the mines and running double shift. During the night shift only 25 or less percentage of the men are underground, as in comparison with the day shift, and therefore it is not necessary to have as much air during the night shift. The question arises: can we not save the operator a certain amount of power cost by a change of speed of the fan? If it can be done, the central station man should have the credit of doing it, if it is possible, because the operator is only too glad to save in his cost.

In matters of this kind it seems to me there is only one of two things to do. First, as to the question of buying power for a new mine, I simply change the pulley speeds and it solves the problem, but for a mine that has been developed and wants to run cutters or pump-men under ground at night, I would rather have a split ring motor, with external resistance, or a two-speed motor, doubling the number of poles for the lower speed.

The next question the operator asks is, which of these it is more advisable to install and operate. If the two-speed motor is simpler and has fewer parts to get out of order, and these fans will run without attention, they naturally prefer that motor. The two-speed motor may be less complicated, and there may be a saving in power, as compared with the variable speed motor, yet the cost is very much higher.

The next question I submitted to manufacturers was with reference to variation in speed between full speed and half speed. The result I found was that there was not 5 per cent difference in half speed operation on the two-speed and variable speed fan motors. Whether or not the data submitted to me by these companies is reliable, I am not in position to say, but it showed less than 5 per cent difference in the amount of power used. So that from my experience the two-speed fan motor is preferable, rather than the variable speed, due to the fact that the simpler construction requires less attention. In this particular case it was a 40-h.p. motor, variable speed, requiring on half speed of the fan something like 15 h.p. The data on the two-speed fan showed that it required 15 h.p. on half speed. It is simply a question of which motor you want to buy in that case. It seems to me that will apply to the territory I covered.

H. L. Beach: I had occasion some four or five years ago to purchase a fan and motors for a mine where we were doing developing work to a considerable extent. The exact figures of the amount of air required have slipped my mind now, but I know that we needed a total of 200 h.p. to operate the fan at full capacity. At the time the fan was to be installed they needed possibly only one-third of that. My purchase consisted of two squirrel cage motors, manufactured at the same time, so as to get the characteristics identical, tested and guaranteed to divide the load equally at the same speed. When tested the motors were fastened together with a flange coupling, and the load driven by a belt using the coupling as a pulley, the load dividing within a per cent or so between the two motors. I then set one motor on some temporary work we had, and put the other motor on the one fan. That motor has been running something like three years, and they have not had occasion yet to change the pulley. The arrangement was that we would make a high pulley ratio to start with, and as the requirements for air increased they would decrease the pulley ratio and speed up the fan. The arrangement has been absolutely satisfactory, and has not given a moment's trouble or delay in any way, and so far they have all the air they want with one motor and the original pulley layout. It is ready at any time to put another pulley on. They can make one increase in speed with the present motor, and then by releasing the other motor they can increase the speed until the full capacity is obtained. I can, for my part, see no reason for the large expense and complication of variable speed outfits. The change in pulley is not a proposition which comes up every day, but a proposition which may come up only once in two or three years. Two or three fans are usually required for a large mine anyway and it is a simple proposition to shut down one or more at night and run all during the day.

F. B. Crosby: The discussion aroused by this paper would appear to be ample justification for its presentation. The particular point about which the discussion has chiefly centered, namely, the desirability of adjustable speed drives for mine fans, was taken for granted by the writer. As stated in the paper there is a somewhat limited field in which constant speed drives are satisfactory, but the very great percentage of requests for adjustable speed fan drives as compared with constant speed applications which have to come to the writer in his professional capacity, as well as the ready defence of the adjustable speed drive by many experienced mine operators and engineers this afternoon, can leave no doubt as to its desirability.

The method of obtaining speed control by dynamic regulation as described has been developed to meet an existing need. These speed regulating sets and brush shifting motors are the result, and not the cause, of the demand for adjustable speed drives. The entire range of horse power capacities and speeds ordinarily required can be simply and more efficiently met by one or the other of these two equipments than by any other commercially practicable drive yet devised.

For small drives, 100 h.p. or less, the extreme simplicity of the brush-shifting motor and control is especially desirable since the single motor involved is started, stopped, reversed and the speed controlled by merely shifting the brushes.

It has been suggested that the regulating set introduces a complexity of control equipment prohibitive in the hands of the average fan attendent. This feature has, I believe, been unduly emphasized.

Considering the results obtained nothing can be simpler than the operation of the set. The main motor is started in the usual manner by simply closing the primary oil switch and throwing the controller to the full-on position. The motor generator is started by means of a standard induction motor compensator. Electrical equilibrium between the main motor and motor generator is absolutely automatic. Any desired speed of the main motor within limits of design is obtained simply by adjusting the exciter field rheostat.

Referring to Mr. Eddy's request for information regarding costs, it is difficult to answer such a question, except in general terms, owing to the many variable factors involved. The cost is obviously more than for a single-speed motor and depends upon the frequency and range of speed required. For 25-cycle equipment designed to give 20 per cent speed regulation on the main motor, the complete equipment including main motor and control may cost from 15 per cent to 20 per cent more than a two-speed motor designed for the upper and lower speeds.

A paper presented at the Pittsburgh Meeting of the American Institute of Electrical Engineers, Pittsburgh, Pa., April 18, 1913.

CENTRAL STATION POWER FOR MINES

BY J. S. JENKS

The subject of this meeting covers such a broad field that it would be folly to undertake to cover more than a limited portion of any particular branch; hence I will deal only with the historical side of the question as it has to do with the development of central station service in connection with coal mining on the West Penn system.

Central station power for mine service has been greatly handicapped by the prejudice of some mining engineers and mine inspectors who have actually fought the installation of central station power, stating that central station service was not as reliable as an isolated plant, was more dangerous on account of the high voltage and more costly. They often eliminated all cost of plant labor and fuel when making comparisons between central station service and isolated plants, arguing that the plant labor would have to be around the mine at any rate, and that the cost of fuel was so small to the mining company that it should not be considered.

In order to overcome these objections it was necessary to prove the reliability of central station service and its advantages. The objection of the mine inspectors was the hardest to overcome, even after the mine operators were convinced that central station service was more economical and were in favor of installing it the mine inspectors frequently prohibited central station service for some uses in and about the mines, particularly for fan service.

They argued that transmission and distributing lines, of necessity, made central station service more liable to accidents and acts of God beyond the control of man, contending that mine ventilation was of such importance that no mining company should be dependent on another corporation for its power for fan operation. It was only after years of successful operation of all other classes of mine service that we were able to overcome this prejudice and succeed in getting the mine inspectors to approve central station service for fans, much less recommend it.

The mining engineer, and the electrical employees, opposed central station service for obvious reasons, one of which, frequently frankly admitted, was, that with central station service they would have no job. The truth of the matter has been that central station service has actually enlarged their field of labor, as more mines are being electrified every day on account of the many advantages of central station service, thus requiring the services of an engineer and electricians to most efficiently install and operate central station power. High grade engineering and labor pays such great returns on the investment that the demand for first class men is continually increasing. While on the other hand it is an admitted fact that almost any mine foreman or master mechanic could install and operate a steam drive or a d-c. isolated plant, this very frequently was a most uneconomical operation, often actually costing many times what it was supposed to. The lack of electrical engineering in the early days of central station service was a large factor in retarding its growth as will be shown by the initial installation in this territory.

This first installation of central station service in a mine on the West Penn system was made at the Larimer mine of the Westmoreland Coal Company in 1896. It consisted of a 120-h.p., 4,000-volt, single-phase, 133-cycle, induction type synchronous motor, belted to 100-kw., 500-volt, direct-current, multipolar generator. The motor was excited by a belt-driven exciter and started by a single-phase, 100-volt motor which actuated through shifting belts. The switchboard consisted of standard d-c. marble panel with ammeter, voltmeter, circuit breaker and switches. The a-c. board consisted of a wooden panel having mounted on it a small two-pole oil switch for controlling the large motor, a two-pole knife switch for the starting motor, pilot and synchronizing lamps. This apparatus was located in an underground substation as it was feared it might be damaged by employees during strike periods if it were above ground.

In order to supply this service there was installed at the power house of the Irwin Electric Light & Power Company at Manor, one 150-kw., single-phase, 2000-volt, 133-cycle alternator and 125-kw., 2200 to 4400-volt, 60-cycle transformer for raising the generator voltage to the transmission and motor voltage.

The line consisted of two No. 4 insulated wires carried by glass insulators on a two-pin crossarm over private right-of-way from Manor to the sub-station located near Circleville, except through the town of Irwin where it was on the street. The length of this line was about 5 miles (8 km.) A telephone line consisting of duplex insulated wire was strung on a ridge pin on the top of the pole. The wire entrance to the underground substation was made by sinking a bore hole through which lead cables were carried to an underground tunnel which led to the machine room. In order to protect the lead cable where it entered the ground, a high circular stone wall was built around the bore hole.

The apparatus supplied from this substation consisted of six mining machines, a 10 h.p. pump and a 60-h.p. haulage, but no fan service was supplied for reasons already explained.

The first trouble that developed was the falling in of the roof of the substation, which not only damaged the apparatus, but put the mine out of service for some time until the debris could be removed and a brick lining put in to prevent a recurrence of similar trouble. This brick lining sweated so that it made all the apparatus wet, which resulted in frequent burn-outs of the starting motor, which stood idle for long periods. In order to insure starting, duplicate starting motors were provided and at times it was a problem to keep one in condition for service. The difficulty of sweating was partially overcome by putting a wooden lining inside the brickwork.

The next difficulty arose from a breakdown in the lead cable caused by lightning. This had the effect of charging everything in the substation. It was overcome by removing the lead from the cable and supporting it on glass insulators, except where it passed through the bore hole.

The next weakness developed in the oil switch which consisted of eight ½ in. (1.27 cm.) brass rods working through small brass bushings, mounted on a wooden board submerged in oil. This two-pole switch had eight breaks of about ¾ in. (1.9 cm.) each and was contained in a tank 8 by 10 by 9 in. (20.3 by 25.4 by 22.8 cm.) and operated by hand-wheel and pinion, which worked on a rack pulling the rods out of the bushings. This made a very slow operating switch with which it was very difficult to synchronize. After numerous interruptions caused by failures of this switch a make-shift switch consisting of an ordinary two-pole, two-break, knife switch on a marble base was mounted on insulators in the bottom of a half barrel. The switch handle was removed and a broomstick tied to the cross-bar with a belt lace. Leads were brought over the edge of the barrel and connected to the switch. The barrel was filled with oil. The switch was operated by pulling or pushing on the broomstick. This switch was so much more easily operated and such improvement was made in the time of synchronizing that the consumer would not have it changed and it remained in service until the substation was finally abandoned on account of the mine being worked out.

The Irwin Electric Light & Power Company was acquired by the West Penn interests and in 1905 the 133-cycle power house at Manor was discontinued and service established from a 60-cycle turbine station at Connellsville through 28.52 miles (45.5 km.) of 22,000-volt transmission lines and a substation at Manor. This necessitated the reconstruction of the motors from 133 to 60 cycles. The work on the large motor was done in the field and the starting motors were sent to the factory one at a time. This reconstruction had the effect of reducing the capacity of the motors and resulted disastrously in the case of the starting motors, making it necessary to provide larger motors.

The next trouble to develop, was rather peculiar in that the large motor started to drop out of step without any apparent cause and would drop out when hauling practically no load. This was a very puzzling circumstance and no amount of adjusting by attendant seemed to remedy the trouble. It was found, however, when the supply from Connellsville was generated by a single unit that this trouble was most pronounced and later discovered that there was a splice in the belt between the motor and generator that caused little jerks which would get in step with the governor mechanism on the turbine, causing the turbine to hunt. This hunting was exaggerated in the motor, making it drop out of step. The remedy for this trouble consisted in direct-connecting the motor and generator, which happened to be the same speed. On account of the high voltage of the motor it was necessary to have the motor frame insulated and a satisfactory insulating coupling became the question. This was solved by turning the shafts end to end, setting the pulleys about six inches apart, drilling and tapering the rim of the pulleys for cap screws and laying a piece of belt around inside of both pulley rims and securing it in place with cap screws. This proved a very satisfactory flexible insulating coupling, which gave no trouble and operated for a number of years until the mine was worked out.

Notwithstanding these difficulties enumerated, this installation proved a very satisfactory one to the mining company, saving them a considerable amount of money, even though the rate charged was several times greater than that charged for similar service to-day, and it led to many other large installations.

The first mine where West Penn service was used for fan operation was at the Penn Gas Coal Company's mine near Penn Station, and consisted of a 100-h.p., 500-volt d-c. motor, belted to the fan. This was installed about 1901 when the mine was completely electrified with direct current. The installation consisted of numerous pumps, mine machinery and a large haulage. The fan operated until 1907 when this motor was replaced by a 200-h.p. a-c. two-phase, 60-cycle motor. This installation, which originally consisted of about 500 h.p., has been increased from time to time until it now has about 1250 h. p. in electric motors.

In 1907 our first high-tension installation was made at a coal mine. This was made at the Noami Mine of the United Coal Company near Fayette City. In this case the Coal Company built its own substation and bought current at 22,000 volts installing three 200-kw. 22,000 to 440-volt transformers, one 300- and one 150-kw. synchronous motor-generator sets, one 150-h.p. a-c. haulage, 100-h.p. chain lift, two 75-h.p. fan motors, a-c. pumps and numerous haulage locomotives and mining machines. This installation was of particular interest as it was the first one where engineering had very careful consideration and was our first a-c. haulage.

Another very prominent example of a coal mine installation where the most minute detail was worked out by the engineers in charge with the idea of producing the most efficient results is illustrated by the Keystone Coal Company's installation at its Crows Nest substation. Here 1500 h.p. is delivered at 2300 volts from 22,000-volt transformers installed in the substation, together with the necessary switches and lightning protection. This installation consists of 750 h.p., a-c. haulage, two 300-kw. synchronous motor-generator sets, two 150-h.p., a-c. pumps and innumerable locomotives, mining machines and small motors.

While the growth of central station service was very slow at first, it is now growing very fast, and from the original installation in 1896 of 120 h.p. we have steadily added all kinds of mine installations until at the present time we have in operation 76 coal mines consisting of 14,831 h.p. and have contracts with 10 companies which aggregate 5,701 h.p which is being installed as rapidly as possible. This will make a total of 20,532 h.p. In addition to this we are at the present time figuring with a number of coal companies and have every reason to believe that in a short time we will have under contract more than 10,000 h.p. additional, which will increase our total to over 30,000 h.p. in coal mine service only.

A paper presented at the Pittsburgh Meeting of the American Institute of Electrical Engineers, Pittsburgh, Pa., April 18, 1913.

CHARACTERISTICS OF SUBSTATION LOADS AT THE ANTHRACITE COLLIERIES OF THE LACKAWANNA R.R. CO.

BY H. M. WARREN AND A. S. BIESECKER

In view of the fact that practically no data of this nature are available and as the Lackawanna Railroad Company has a large number of substations to which the rated connected loads widely differ, the writers were led to conduct a series of tests, the result of which form the basis of this paper.

Tests were made on 15 substations ranging in size from 150 to 700 kw. The apparatus in these stations consists of 60-cycle, six-phase diametrically connected synchronous converters delivering direct current at 275 volts, step-down transformers of either the single or three phase type, and the necessary a-c. and d-c. switching apparatus. These substations are usually located on the surface at the colliery, and the three-phase high-tension power is furnished from central stations.

The power apparatus driven from the substations consists of locomotives, hoists, pumps, and under-cutting machines. The locomotives vary in size from 7 to 13 tons and are usually geared to operate at a speed of from six to eight miles (9.6 to 12.8 km.) per hour at full load. Although the locomotive weights vary, about 80 per cent of the total number weigh seven tons or less. All of the locomotives have double motor equipments with series-parallel controllers. The motor equipments average about 10 h.p. (railway rating) per ton of locomotive weight. The d-c. hoists operate on either slopes or planes and vary in size from 20 to 160 h.p. As most of the large pumps are driven by a-c. motors, the d-c. pumping sets are usually small in size and operate intermittently. The power required for undercutting machines is at present comparatively small. It is, therefore, important to note that about 75 per cent of all d-c. power supplied from these substations is used by locomotives and that 80 per cent of all locomotives are rated at 70 h.p. or less.

The tests on these substations were conducted as follows:

In order to obtain accurate readings, an a-c. single-phase watt-hour meter with high geared dials was obtained. This was connected in one phase of the a-c. end of the synchronous converter and readings taken every half hour. A record of the peaks was obtained by connecting a graphic ammeter which was geared to give a paper speed of 7½ in. (18.8 cm.) per minute in the d-c. side of the converter. A note was also made of the machines operating in the mine at the time of the tests.

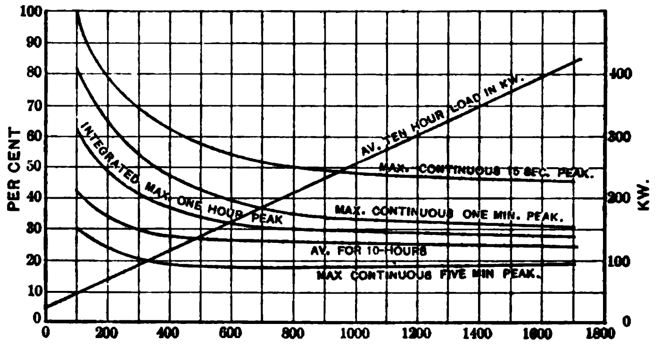


FIG. 1—SUBSTATION LOAD AND PEAKS BASED ON SYNCHRONOUS CONVERTER OUTPUT

The rated connected loads were based on the following motor ratings:

Electric locomotive—10 h.p. per ton.
Hoist motors (railway type) —one hour rating.
Pumps—Name plate—continuous rating.
Undercutting machines—one hour rating.

The total of these horse-power ratings was reduced to kilowatts in determining the kilowatts rating of the total connected load.

After the above test data were obtained, a tabulation was made showing the maximum peaks for 15 seconds, one minute, five minutes, and one hour; the average load for a ten-hour day; the kilowatt-hours per month, and the rated connected kilowatt load for each substation. In working up these data, the 15-second, and also the one-and five-minute peaks were measured on the graphic ammeter paper as block peaks, while the one hour peaks were taken from the watt-hour meter readings and are, therefore, the integrated peaks. However, after determining the d-c. peaks, an amount equivalent to the synchronous converter losses was added, so that all tabulations were made on the converter input basis.

From the tabulations, a set of curves was then plotted as shown in Fig. 2. On this chart the abscissa represents the rated connected load in kilowatts. The lower curve representing the kilowatt-hour per month is read on the right hand margin, while other curves are read in kilowatt on the left hand margin. From this chart other curves and factors were derived which will be discussed later.

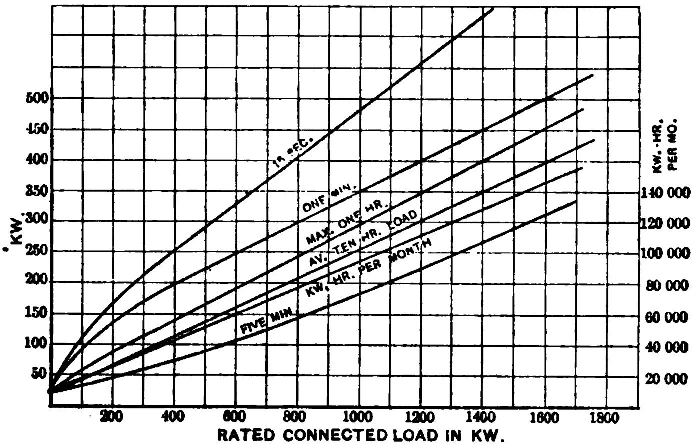


FIG. 2—SUBSTATION LOADS AND PEAKS

From the above, we have been able to make some interesting deductions regarding diversity factors and load factors. Diversity factor has been defined as the ratio of the sum of the maxima of the subdivisions of any part of the system to the coincident maximum demand observed at the point of supply. For the present we will, therefore, consider the subdivisions as loads taken by the individual locomotive, hoist, etc., and the point of supply as the a-c. side of the converters. In order to illustrate how the diversity factor increases from unity to higher values as the number of units and consequently the rated connected load is increased, we have shown in Figs. 3 and 4, sections of graphic ammeter charts taken at different substations. Fig. 3 shows the load on a small substation to which the rated connected load was only 465 kw., while Fig. 4 shows a similar curve for a substation to which the rated connected load was 1720 kw. From tests made on a seven-ton locomotive, rated at 70 h.p. or 52.5 kw., we find that the maximum continuous peaks in per cent of its rating for 15 seconds, one minute and five minutes, are 180, 100, and

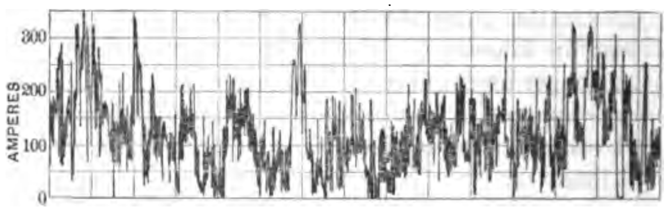


FIG. 3—D.C.AMMETER CHART (MULTIPLY BY 4). RATED CONNECTED LOAD, 465 KW. VOLTAGE, 275

60 respectively. Based on the above as peaks for a single unit the diversity factor for 15 seconds, one minute, and five minutes for various rated connected loads, are as shown in Fig. 5.

Load factor is usually defined as the ratio of the average load for a certain period to the rating of the substation. However, as the load factors on the substations are not considered in this

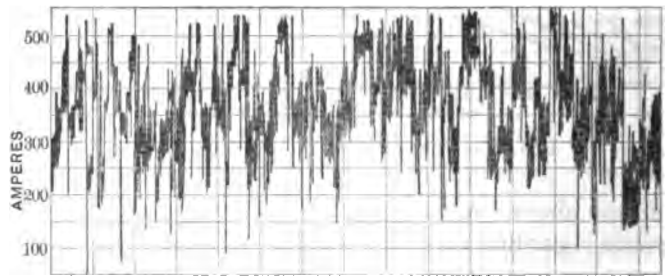


FIG. 4—D.C. AMMETER CHART (MULTIPLY BY 4). RATED CONNECTED LOAD, 1720 KW. VOLTAGE, 275

paper, we have expressed all load factors as the ratio of the average loads on the substations to the rated connected loads. By so doing, any data contained in the paper become applicable to other similar installations.

Fig. 6 shows a 10-hour load curve taken on one of the largest substations. This curve was plotted from watt-hour meter readings taken every half hour. It will be noted that it has about the same characteristics as are generally found in shops or factories where the consumption of power depends on the activity of the employees operating the machines. However, there is a low point in this curve which occurs about nine o'clock. This is due

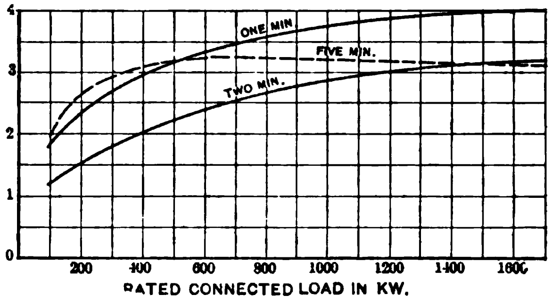


FIG. 5—DIVERSITY FACTORS

to the fact that there is a certain amount of coal mined and loaded during the night which is ready for the locomotive crews at seven o'clock in the morning. After this night coal is pulled out, the crews ease up for a while and take a morning lunch. During this

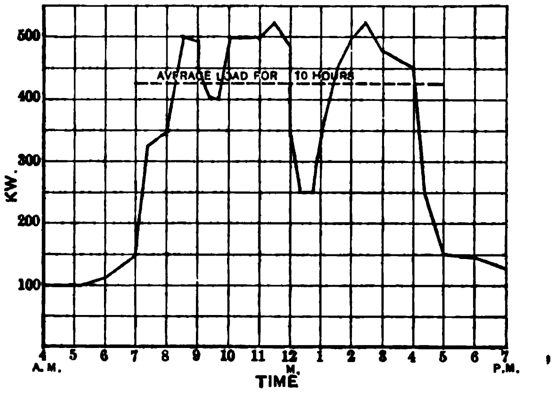


FIG. 6—A TYPICAL SUBSTATION LOAD

time, the day miners have been loading coal which keeps the locomotives busy until about 11:30 when the load begins to drop again. The substation on which the above curve was taken had a rated connected load of 1720 kw. which consisted of eight 10-ton, and 20 7-ton locomotives, and a small hoist. The average ten hour load factor was 24.6 per cent whereas the average for all substations was 24 per cent. The following tabulations give the load factors for various lengths of time for this largest station and the average of fifteen stations tested:

Station	Largest %	Average %
1 Hour	29	32
8 "	26	25.5
10 "	24.6	24
24 "	14	12.5
Annual 250 days	9.6	8.6
(Night) 14 hours	6.6	6.2

The 24-hour load factors give the average loads during a twenty-four hour working day and this might be considered as the annual load factor. However, as the average mine is not operated over 250 days during the year, the load factor on this basis for all stations tested is 8.6 per cent. It is interesting to note that the ratio of the power used during the ten hour working day to the total used during the 24 hours was 73 per cent on the largest station and 79 per cent was the average for all stations. The average 14-hour night load is, therefore, 6.2 per cent of the rated connected load.

In the application of data obtained from these tests, it will be noted that the peaks call for substation apparatus designed to carry and commutate high overloads and that the annual load factors are very low. Regardless of the question of taking care of the peaks, we find that the load increases so rapidly, due to new apparatus being installed, that it does not always pay to figure closely when deciding on the size of a substation. Some of the first stations installed for this work had 150-kw. and 200-kw. converters. These have been transferred until now it is difficult to find a mine where the load is not too heavy for them.

We would, therefore, not recommend a converter smaller than 200 kw. and in case there is a probability of the load growing rapidly, a 300 kw. unit will be cheaper eventually. There are certain conditions where motor-generator sets work out more advantageously even though the initial cost may be much higher. Let us assume a condition where the substation is to be located near an a-c. motor-driven coal breaker, both of which are to be fed from a central station over a transmission line. The breaker motors will pull a load on which the power factor will be poor and this load together with the hoisting and pumping load will be so large in comparison to the substation load that the small amount of leading current obtainable from a converter, will not compensate for the lagging current taken by the induction motors. The above will result in poor voltage regulation on transformers and transmission line, and the attending bad effects due to poor power factors on a central station. If instead of using a converter we install a synchronous motor-generator set with interpoles on the generator and a motor having a rating 30 to 40 per cent in excess of the generator, we are in a position to correct the power factor, and better both the d-c. and a-c. voltage regulation. Where it is necessary to transform to a lower voltage for the synchronous motor, it will often be possible to wind the breaker motors and synchronous motor on the set for the same voltage and feed them from one bank of transformers. By combining the leading current with the lagging on the low-tension side of one bank, the kilowatt-ampere rating is very much reduced from that required for separate transformers. There is also a decrease in the cost per kilowatt-ampere due to the units being larger.

While the data obtained from this investigation will probably not be of any particular value except for use in connection with electrical installations in anthracite mines, it is believed that on account of the number of stations tested that the data will, at least, be of great assistance in determining power requirements for anthracite mining installations.

DISCUSSION ON "CENTRAL STATION POWER FOR MINES" (JENKS) AND "CHARACTERISTICS OF SUBSTATION LOADS AT THE ANTHRACITE COLLIERIES OF THE D.L. & W. RY. CO." (WARREN AND BIESECKER), PITTSBURGH, PA., APRIL 18, 1913.

Graham Bright: Mr. Jenks has given us some very interesting historical matter. In the first part of his paper he mentions the early prejudice of the operators in regard to the purchase of central station power. I think this early prejudice was justifiable, when we consider that central stations had not made any records for themselves, and the operator had no way of telling what the continuity of service was going to be. As we know, in any new industry the pioneer usually stands the cost of development while those who follow reap the benefits, and you can hardly blame those early operators for having a prejudice against the use of central station power.

I think a great deal of credit is due to the operating forces of the central stations in bringing the continuity of service to such a high degree of efficiency. We have records of long periods of operation with little or no delays, and records made by such companies as the West Penn Railways Co. have given to operators confidence in central station power, and have made the sale of such power a much easier matter than formerly.

There is one point I would like to bring out in connection with the paper by Messrs. Warren and Biesecker which has given us a great deal of valuable information on actual mine conditions, and that is, referring to Figs. 1 and 2, you will notice that the five-minute peak is considerably smaller than the integrated average for 10 hours. That brings up the point of just what kind of a peak to use on which to determine the fixed charge. This illustrates the fact that the block peak is not the proper one on which to base the fixed charge, because here is a case in which the block peak is considerably less than the average for 10 hours. You can see it is not a square deal to the central station to use a peak load which is less than the average 10-hour load as a basis for the fixed charge. It should be the integrated peak rather than block peak, and these two curves illustrate that particular point very strongly. In fact a power circuit can be so manipulated that there will be little or no block peaks at all.

W. A. Thomas: Various statements of load factor, and particularly those brought out in the paper by Messrs. Warren and Biesecker, show the necessity of coming to a common basis in determining the expression of load factor. This is perfectly correct as explained by Mr. Warren, but unless you give a good deal of thought in transposing from one statement to another, we are led to confusion. In this case we have not only the load factor stated in terms of the average demand to the connected load, but we have the connected load rated on both constant and intermittent basis, and while, as I said before, it is perfectly legitimate and correct, we ought, in using it, to get to a common basis of understanding. Of course, one statement of load factor is the ratio of the average consumption to the capacity of the substation. Another one is that which Mr. Bright touched on, the ratio of the average load to a given peak, five minutes integrated peak, or 15 minutes integrated peak. The point I wish to lay particular stress upon is the desirability of coming to a common basis for determining load factor and reducing the amount of labor necessary to transpose from one to another. I do not mean this as a criticism of the paper in any way. I consider that the data which is submitted in this paper is a most valuable contribution to the art, and one which will be of extreme value in this very active campaign on the part of the operators of central power stations in the coal mining regions.

J. Paul Clayton: The characteristics of coal mine loads as shown by this paper are extremely low annual load factors. The annual load factor controls almost directly the cost of producing power in any plant, such as the installation under discussion, and further to illustrate this point I have recomputed the cost presented in Mr. Beers' paper presented this morning, on the basis of lower annual load factors as they actually occur in these mines. In Mr. Beers' paper of this morning, the total cost of producing power in a station of 1000-kw. capacity, operating at 50 per cent load factor, was given as $35,000 on an annual output of 4,380,000 kw-hr. or a total cost of eight mills per kw-hr. Reducing this cost to the basis of a 20 per cent. annual load factor, which is rather high for mines with which I am familiar in Illinois, we have a total cost of $31,200 (about the only difference in the cost being the item of coal) and the total cost works out at 1.8 cents per kw-hr. Reducing this load factor further to 15 per cent, the cost of operation is reduced from $35,000 at 50 per cent load factor to $30,630, at 15 per cent load factor, or a total cost per kw-hr. of 2.4 cents.

In the operation of such a station for Illinois mine conditions, serving only one mine, on one shift per day operation, 200 days per year, you could not secure an annual load factor of 20 per cent. The load factor as I am using it, is the actual energy consumption in a year divided by the energy consumption which would have taken place had the actual 15-minute annual maximum demand basis been carried on throughout the year. This analysis shows that the probable cost of energy for the installation of the plant cited, when used under the conditions obtaining in the paper or under conditions obtaining in the bituminous mines of Illinois, would be in excess of two cents per kw-hr. instead of 8 mills per kw-hr. It would be perfectly possible to make power in that same station for about 6 mills per kw-hr. if you could obtain a 100 per cent load factor, but I believe the publication of such costs without adequate explanation of the effect of load factor on them gives a wrong impression as to the cost of producing power under the conditions obtaining in the vast majority of all coal mines.

George R. Wood: I think it important that we come to a better understanding of what we mean by the term load factor. In the course of this meeting we have had three or four varieties of this factor, based on connected load, station capacity, instantaneous, one minute and 15 minute integrated peak, etc. In other words, every central station figures customers' load factor as the ratio of average load to maximum demand, as defined by contract. It seems to me probable that an integrated peak over some such period as five, ten or fifteen minutes will ultimately become standard for determining maximum demand, but in the meantime a definition should accompany reference to "load factor."

H. M. Warren: If the gentleman who just spoke, contemplates using the figures I quote, as a basis of the kw-hr. cost, he should bear in mind that the information in the paper refers only to the direct current used in the mines in question, and that the large amount of alternating current power used, has not been considered. The load factor of our central station is 65 per cent, and is figured on the average yearly load in kilowatts compared to the rating of the station.

George H. Morse: It would be interesting to know how the gentlemen who made the calculations on their load factors proceeded in making these calculations. I think there are various ways in which individuals would proceed, and if Mr. Clayton would care to state how he made his calculations, I think we would be much interested in hearing from him.

J. Paul Clayton: As to the basis of the costs of power, I took the cost as given on page 1038 of Mr. Beers' paper and assumed that, for practical purposes, the fixed charges remained exactly as given, and I think that is approximately true and that the item of coal, easily the largest single item, varied with the load factor. If we reduce the load factor from 50 per cent to 25 per cent, the coal consumption will be something more than half the coal consumption at the high load factor, as it does not fall off in proportion because the efficiency of boilers and turbines would be less.

In computing the cost given in my previous discussion I assumed that the coal consumption at 20 per cent load factor was half that at 50 per cent load factor and in obtaining the load factor at 15 per cent I have assumed that the coal consumed at the 20 per cent load factor was reduced by 15 per cent, and that the other items in the operating cost would be approximately the same.

C. W. PenDell: The item of load factor, as the Chair has said, is one that we should have a definite idea upon. It is something that should be settled and defined, and not a thing that one engineer should have a rule for talking on, in one line, and another engineer have another rule for talking on, in another line. If I design a plant to operate a mine and put in 1000 kw., and another man puts in 500 kw., that should not affect the load factor. The consumption of the two plants will be the same. The load factor on the 1,000-kw. plant would be one-half what the load factor would be on the 500-kw. plant, if you base your load factor upon the size of the plant. Load factor is something definite, not something ethereal. It is the relation between the average load 24 hours a day and the maximum demand. You may take the maximum demand for five minutes, or you may take it for an hour, according to the way your contract reads. Whether you take a five-minute peak or a 30-minute peak, will probably make about 15 per cent difference in your load factor; that is, a load factor of 40 per cent on a 30-minute basis would be equivalent to 0.85 times 40 per cent on a five-minute basis. Load factor on a five-minute basis will be lower than on the 30-minute basis, because the maximum for 30 minutes will not be as high as the maximum for five minutes. Many base load factor on the operating time for the mine. According to the central station idea, and I am a central station man, the load factor should go over the whole 24-hour period. The reason for that is; supposing we have a plant which has 100 h.p. maximum demand, the average running load is 50 h.p. and the running time is 10 hours per day. If you base the load factor upon the hours during which the plant is running, you get a 50 per cent load factor. If the plant runs 20 hours per day, with an average load of 50 h.p., and you base your load factor upon the running hours, you still have a 50 per cent load factor. Under this method you have nothing on which to base a comparison of one plant as against another; so that in the central station field we have adopted the general ruling that load factor is the ratio of the kw-hr. consumption divided by the total number of hours in the period under consideration, and that result divided by the maximum demand as determined by the contract. To base the load factor upon the size of the installation is, according to my idea, erroneous. I may come along and install a plant with a 1000-h.p. unit—I do not know the business perhaps—I put 1000 h.p. in the plant to be safe; another man comes along who knows the industry, and puts in a 500-h.p. plant—why should the load factor of that plant, which is a concrete item, be dependent upon whether I know that line of industry, or whether I do not know it? Load factor is something which we can see if we stop to consider the curve. You have a certain curve running along with certain peaks in it; there is your load factor. It is the relation of the average line across the chart to the high point that constitutes the peak.

Graham Bright: I ask Mr. PenDell whether he has reference to the integrated peak or the block peak.

C. W. PenDell: The integrated peak is the only proper peak. Some contracts on railway lines, where the load fluctuates seriously, are based upon the highest instantaneous demand. It all depends upon the capacity of the station furnishing the load and the class of business you are serving.

I was talking with Mr. Jenks a few minutes ago relative to mine hoists. He showed us a picture of a 750-h.p. haulage system. In Illinois we have mine hoisting outfits from 400 h.p. to 1800 h.p. We have not felt that we could take these hoists directly onto our lines, and have asked the customers to interpose flywheel motor-generator sets between the hoists and our lines. Now, with the flywheel motor-generator set we would integrate our peak. If we felt, perhaps, that we could stand the load directly on the line, we might give these parties contracts stipulating that the peak should be the highest swing of the needle on the chart. The integrated peak has been adopted in probably 90 per cent of all maximum demand contracts for central station companies. There are four common durations of peak, 5 minutes, 15 minutes, 30 minutes and one hour. Five-minute peaks have been adopted through the central states for coal mines and large stone quarries. Fifteen-minute peaks have been adopted by some companies for that class of business. General power is now being taken on over a great part of the country on 30-minute peaks. Railway contracts, large interurban systems, are going on commonly with 60-minute peaks. In addition to having the single 5-minute, 15-minute, 30-minute or 60-minute peaks, contracts are made wherein the maximum is based upon three 5-minute, 15-minute, 30-minute or 60-minute periods, no two of which periods shall be taken on the same day. That will, of course, give a lower maximum demand than a single peak.

George R. Wood: Perhaps we have no right to be surprised when the coal operator doubts our figures showing what he will get under these various rate schemes, and I cannot much blame the operators for refusing to buy power except at so many cents per ton of coal produced, and, indeed, power has been contracted for on that basis.

P. M. Lincoln: Reference has been made to 5-minute, 15-minute, 30-minute and 60-minute peaks. On the lines of the Niagara, Lockport and Ontario Power Company, which distributes power from Niagara Falls through Central New York, they have adopted the one-minute peak. I am inclined to think that for certain kinds of service measuring the current on a one-minute peak is more equitable than measuring it on any of the longer periods.

I want to say something about this matter of load factor. The writer of the last paper definitely told us exactly what he means by load factor, so that when we study his paper we have right before us what he means by the term "load factor," as he uses it. He is much more considerate in that regard than many other writers, because many writers use the term "load factor," and do not give data by which one can tell what they are talking about. Load factor, I believe, should be taken as the function of the load, and as having nothing to do with the size of the plant; in other words, I believe the definition of load factor as given us in the paper by Warren and Biesecker is not the proper basis for true load factor. I believe the proper basis for load factor is the ratio of the average kilowatts during a given time to the kilowatts integrated through some definite shorter period; that may be one minute, 5 minutes, 10 minutes, 15 minutes, or any other period of time. It is the average for the whole length of time to the integrated value for the shorter length of time that should constitute the proper definition of load factor.

Then, again, the duration of the whole period is not necessarily restricted to 24 hours. We may have daily, weekly, monthly or yearly load factors and in general the longer the period the lower will be the load factor.

C. W. PenDell: This one-minute peak that Mr. Lincoln speaks of is more or less in the nature of an instantaneous peak. I threshed over in our company the question of how long a period we should take for the peak, and on our general power we adopted thirty minutes, for this reason: In studying a customer's load we say it will take 500 kw. to handle his entire requirements, that is, 500 kw. for the transformers. These transformers will stand considerable overload for a few minutes without damage. Our lines will stand the same overload. The generators will stand it. If we take a short duration peak period, the starting up of the factory in the morning, especially if there are a number of large motors, will have a tendency to boost the customer's peak beyond where we want it to go. We do not want the rates on paper to look excessively low. We would rather keep the rates so that they look reasonable on paper, and yet have the customer earn a low rate by having a maximum demand which is low, rather than to give him a one-minute peak which will make his maximum demand quite high, and charge him a relatively low rate for the maximum.

As the chairman has said, we can hardly blame the operators of the mines for not buying power when they find that we have all these different rates to offer. There is one thing that the power salesman must get the first thing when he goes out—that is the confidence of his prospect. The salesman must have faith in the commodity he is trying to sell the prospect, and he must know that when he figures out a rate for his prospect that the rate is right. On the other hand, we must also imbue the prospect with faith in the company, that he will secure equitable and just treatment.

The central station should not, according to my theory, sell at so much a unit output of the customer, because in so doing we are taking all the risk of inefficiency in the customer's operation. You go to one man and say "I will give you current for so much a ton of coal mined," and you to go the next man and try to figure his cost and say "I will give you current for so much per ton of coal mined", these two men get together, and then the second fellow says, "Here, you are charging me 30 per cent more than you are charging the other fellow, what right have you to do that?" We try to explain to him that his method of operation is not the same as that of the other man, but you cannot explain that to him. He has you, because he knows more about the coal mining business than you do, certainly more than I do, because I have not been in the business long enough, but if I tell them both "I am giving you the same rate per kw-hr.," and it is up to them to earn the same rate per ton mined if they can, they cannot accuse us of injustice, as they are liable to do if we try to sell them on the unit basis of output.

Sidney G. Vigo: The question of load factor is not only of extreme interest to the central station company, but also to the customer as well. The formation of rate schedules is for the purpose of giving to the customer a lower rate, corresponding to the increased hours of use of his maximum demand over any month. A customer operating twenty four hours a day naturally should receive a better rate than an eight-hour user. The schedules of rates that are designed, therefore, are inherently based on this idea of load factor.

It can be readily seen that if load factor were defined as the percentage of the actual consumption divided by the total horse power in motors installed, over 720 hours per month, that the customer would be given a higher rate than if the load factor were the percentage of the actual consumption divided by the maximum demand, over a period of 720 hours per month.

This is made all the more evident in some central stations in the Northwest, where the rate per kilowatt hour is each month based on the load factor existing during that month. At the end of each month, the maximum demand, having been measured, and the consumption being obtained, the load factor for that month is determined, and the corresponding rate for that load factor is applied, and with each decrease in load factor, the rate is correspondingly increased. Therefore, with this schedule, if instead of measuring the customers' maximum demand, the total motors installed were considered, the customer's load factor would show a decrease, and he would pay correspondingly higher for his service.

This idea of actually measuring the highest demand of any customer is urgent oftentimes, because it is found that installations in large numbers of cases are made with the idea of future development, which we all admit is bad policy, owing to the inefficiency of the operation of large units underloaded, and consequently, a customer would be paying an unfair rate for service if his load factor were based on the size of his installation, rather than on his actual conditions. There are numbers of such cases that have come to the speaker's personal attention, but there is no doubt that many of you have experienced the same difficulties.

C. I. Weaver: There has been considerable discussion about the duration of the maximum demand; whether the integrated peak should be for a period of one minute, 5 minutes, 15 minutes or 30 minutes. This diversity of opinion led to the remark that the rate for power to the coal mine should be based on tonnage. The tonnage basis of rates is, in my opinion, very unsatisfactory. A comparison of the rates on the one-minute period, the 5-minute period, the 15-minute period, and the 30-minute period, would show the net power bills to be approximately the same for any given load.

Different lengths of periods for maximum demand readings are the results of different conditions in the central stations and widely varying opinions of rate makers.

It is not difficult, however, to estimate the cost per ton of coal from a rate having a demand charge and an energy charge. It would be discriminatory to base power bills on tonnage since the kilowatt hour per ton varies widely in different mines.

Our organization has for a few years been marketing power to mines with a rate based on 15-minute demand with additional charge for energy. It has proved fair to the station and satisfactory to the mine operator. The field of our activities has extended into Illinois, Indiana, and Michigan.

T. E. Tynes: I wish to supplement Mr. Lincoln's remarks and the gentleman preceding, in regard to the load factor. I agree with them that the load factor is a function of the load and not of the installed capacity. I know of a certain plant in which, if the load factor was based on the ratio of the average installed capacity, it would be somewhere around 15 per cent, but based on the ratio of the average to the maximum demand, it is around 70 per cent.

I ask Mr. PenDell what method they use for integrating their 5-minute, 15-minute, 30-minute, and one-hour peak. In the company we take power from we used to measure the peak on a graphic instrument, a clear one-minute peak, and they had been working on that system for two or three years before going on the regular maximum demand meter, but finally did it, and that integrates the entire peak. We find that it raised our maximum demand from 8 to 14 per cent, depending on the nature of the load. We had to get busy to counteract that, and by generating our own power we brought our peak up to 93.1 per cent.

C. W. PenDell: In connection with the matter of measuring the peak, there has been developed in Chicago a meter for stamping the registrations of an integrating wattmeter. The train of gears on any standard wattmeter is replaced with another set of dials which have numbers on them. There is a tape which runs over the numbered dials, and a typewriter ribbon is placed in between. There is a clock which is set for 5-minute, 15-minute or 30-minute intervals, and as the contact maker goes around, it stamps the registration of the wattmeter dials, the same as if a man were standing in front of the meter and reading the integrating meter at stated intervals, only the clock does it automatically.

We used to have graphic wattmeters on certain loads. I found there was liable to be a discussion whenever it came time to decide on what the maximum demand had been. Every tenth of an inch that the customer's engineer could screw me down on the curve meant anywhere from $100 to $1,000 to him, and I did not like to have him perhaps accuse me of trying to slide up a tenth of an inch, to make $1,000 for the company, so that I took them out and put in the other meter.

George H. Morse: Recently in connection with some properties in Minneapolis, I had occasion to study the conditions with reference to load factor as interpreted by the company. There we were using the actual maximum demands of consumers as registered on the Wright demand meter, the ratio of the average load being taken for the monthly load factor—that was all right in cases where we had non-inductive loads. Where we had an inductive load, as induction motors, in order to get the maximum demand in kilowatts, we were sending out men to make actual measurements on the customers' premises with wattmeters at such times as we thought we would strike the maximum for the month, and in this connection I will say that the man in charge of the meter department has a tradition that there is a certain company in the city there, a manufacturing company, that pays one man to sit at the door throughout the month and look for the man who comes to take that maximum demand—it is his conviction that, when he sees the meter man coming in the distance, he runs through the factory and gives a general alarm to the men at work, and the machines are drawn off throughout the factory.

That company was experimenting with the maxicator or printometer the gentleman refers to, an instrument for registering on tape the wattmeter readings. That instrument is the only instrument we can apply today to inductive loads to get the maximum demand kilowatt. The Wright demand meter is not of assistance in this case, because that registers the maximum current. That is what I want to emphasize and the reason that I rose. To my thinking it is not the maximum kilowatts that we ought to base our rates on, but the maximum current, after all, even in the case of inductive load, because we have to hold in our power station capacity for that maximum current, not maximum kilowatts.

C. W. PenDell: I am having carried on now some experiments on the measurement of maximum demand of inductive loads with the Wright demand meter. I believe there is some general ratio between the power factor and the maximum load of average size commercial customers. I want to put in the Wright demand meter, if possible, on a lot of these customers, to see if I can find a ratio between the power factor and the maximum.

Relative to charging customers for maximum current rather than maximum kilowatt, in our company all generators are purchased on a 75 per cent power factor basis. We put a 750-kv-a. generator—taking that as a unit—on a 500-kw. turbine, a 7500-kv-a. generator on a 5000-kw. turbine, taking fair care of the wattless current in that way.

Our operating department is trying to get the contract department to get our customers to put in synchronous machinery, so as to cut down the wattless current. We have taken the stand that we will not needlessly complicate customers' installations to correct the company's power factor, for two reasons: Whenever you get the customer to install expensive synchronous machinery to correct the company's power factor, they want a special rate, and special rates are something that we fight shy of. Another thing about the correction is, when you want the correction for power factor on your line the customer will have some reason for shutting down the synchronous machinery. You go to figuring on it, and the first thing you know he balls you up by not running it. I have told the operating department if they want power factor correction to put it in their substations.

H. M. Warren: While the tests which we ran on these substations were primarily for our own particular benefit, the results of these tests were urgently sought by a number of our companies, and I feel that with the data which were obtained and are available the central station man has all the information which is necessary, and a great deal more than he would have if he simply knew what the load factor was as outlined by the gentleman who has just spoken. One of the gentlemen stated that load factor was a definite term, comprising the average load, but from the discussion which followed I do not think it is. I have not been able to determine whether it should be based on instantaneous peak or on any peak up to thirty minutes. Therefore, it would seem to me that when the term "load factor" is used, it is necessary to qualify it by a statement explaining just exactly what is meant, in a manner similar to that which has been done in this particular paper.

A paper presented at the Pittsburgh Meeting of the American Institute of Electrical Engineers, Pittsburgh, Pa., April 19, 1913.

MINING LOADS FOR CENTRAL STATIONS

BY WILFRED SYKES AND GRAHAM BRIGHT

The most desirable type of load that a central station can obtain is one which has both a high load factor and high power factor. Load factor is, generally, defined as the ratio of the average load of a machine, or system, to the rated capacity. Capacity is, sometimes, based on the name plate rating, but, for the purpose of obtaining load factor, it should be based on some integrated time peak, that represents the capacity which the central station must provide, and hold in readiness for the use of the customer.

The load of a mine operation, taking power from a central station, consists, in general, of the following: Haulage, Hoisting, Ventilation, Coal Cutting, Pumping, Tipple or Breaker Power, Machine Shop and Blacksmith Shop, Lights.

Haulage. The load due to a haulage system, is, as a rule, very ragged; the variation depending upon the number of locomotives operating and the grade conditions. Figs. 1 and 2 are typical, and show the wide variation in load that takes place in short intervals of time. Unless the power system has a rather small capacity, this variable load will not seriously affect the regulation for power loads, but may give unsatisfactory regulation for lighting.

In practically all cases in this country, the power used for mine haulage is direct current, either 250 or 500 volts. With purchased power, the current is obtained either from synchronous converters or motor-generator sets. The motor-generator sets may be either induction or synchronous. When there are very few locomotives in use the d-c. generator must be able to stand very heavy overloads, for short periods, so that the actual capacity is often determined by the ability to carry heavy momentary overloads, rather than the continuous heating capacity. When this peak load is the determining factor, it is possible to supply a driving motor smaller than the generator, provided the motor has ample pull-out torque. The efficiency and first cost would be improved, when using such a motor, and also the power factor, if the motor is of the induction type. This scheme is particularly applicable to the case when old types of generators are at present driven by steam engines and it is desirable to change to motor drive. In many cases, these old generators will stand very little overload, due to poor commutation, and seldom receive overloads, due to the inability of the engines to stand much more than full load without becoming stalled. Instances have occurred when a 200-h.p. motor was ample to drive a 200-kw. generator. Fig. 2 illustrates a load of this

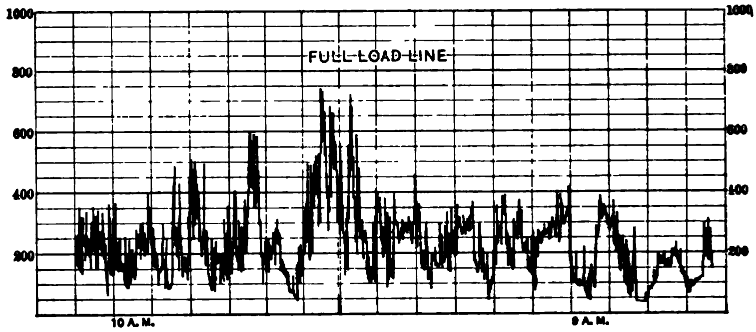


FIG. 1—DAY LOAD ON 800-AMPERE MINE PLANT

character. Where the capacity is not determined by the peak load, it should be determined by the r. m. s. rather than the average kw., for both motor and generator, since the heating for such a variable load will be considerably greater than that due to the average load. This point is well illustrated by Figs. 1 and 2.

The load factor of the haulage system will not, as a rule, be high, but can be improved in some cases by a careful study of the schedules on which the trips are brought to the surface.

The effect of the haulage load on the power factor of the system depends upon whether synchronous converters, induction motor-generator sets, or synchronous motor-generator sets are used.

With the induction motor-generator set the power factor will depend on the load, and will not average very high, due to the fact that the average mine load is low. The synchronous converter will have 100 per cent power factor at full load, and can be made to give a slightly leading power factor at lighter loads. The synchronous motor-generator set has somewhat better characteristics than the synchronous converter, and is better adapted for mine service, due to the superior compounding characteristics of the generator. Heavy compounding is very desirable for mine service, especially when the voltage is 250. The high power factor of the converter or synchronous motor-generator set will, of course, tend to compensate for the lower power factor of the fan, hoist, and tipple motors.

From a standpoint of cost, the desirability of the various types of apparatus for converting a-c. power to direct current is in the following order:

1. Synchronous converter

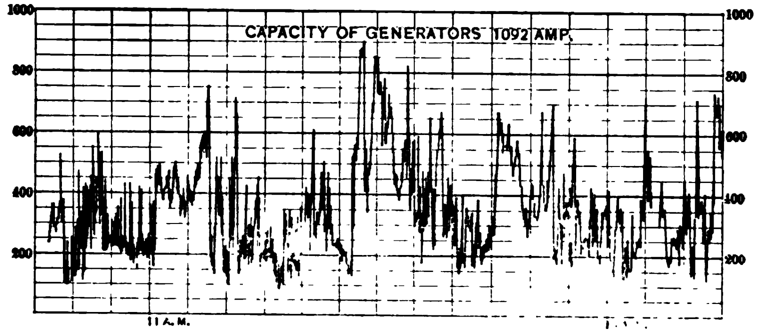


Fig. 2—Day Load on 1092-Ampere Mine Plant

2. Induction motor-generator set
3. Synchronous " " "

From a standpoint of best operating conditions, the desirability is as follows:

1. Induction motor-generator set
2. Synchronous " " "
3. Synchronous converter.

From a standpoint of power factor correction, the desirability will be as follows:

1. Synchronous motor-generator set
2. Synchronous converter
3. Induction motor-generator set.

Hoisting. In mines where hoisting is necessary, the load factor depends upon the nature of the hoist. Where the shaft is vertical, and high speed and frequent hoisting is required, the load curve covers a wide range in a few seconds of time. The momentary peaks are very high, while any integrated time peak of one minute, or more, will be fairly low.

This type of load will often cause poor regulation on a power company's system, and is not, as a rule, a desirable load. However, if there is considerable haulage, fan, pump, and cutting load at the same time, the high peaks will be somewhat smoothed out.

Where the peaks are excessive, and cause bad regulation, some method of equalizing the load is used. The best known of these systems is the Ilgner, which employs a separately excited hoist motor, receiving power from a separately excited generator driven by an induction motor. By means of a flywheel, and slip regulator, the load on the power system will be practically constant. The selection of the type of hoisting equipment will depend upon the depth, output, and central station rate for power. For long slope hoists the variation in power is not so great as for vertical hoists, and the a-c. wound-rotor motor has desirable characteristics. The power factor and efficiency are both low for the average hoisting conditions where a-c. hoist motors are used, and the synchronous apparatus must be depended upon to improve the power factor.

Ventilation. Fans for mine ventilation are in most cases of too low speed for direct connection to the motor. The type of motor used to drive a fan depends upon the conditions under which the fan is to operate.

When the fan operates at the same speed 24 hours per day, and is changed only at intervals of a few months, to take care of the mine development, a simple arrangement is to belt a constant-speed motor to the fan, and change pulleys when a change in speed is desired. The simplest type that can be used for this application is the a-c. polyphase squirrel cage motor. When this motor is to run at low load for long periods the power factor can be improved by a special winding. In some cases it is desired to operate a fan at a certain speed most of the time, and, occasionally, at a somewhat higher speed for emergency conditions. A simple method of accomplishing this is to supply a double pulley, and change the speed by sliding the belt from one pulley to the other. Since the motor is to be operated at a reduced capacity a large percentage of the time, it would be desirable to have a high power factor and high efficiency at light load. Fig. 3

illustrates what can be accomplished by supplying a special winding to a standard motor, to improve the power factor at light loads. The efficiency may suffer at full load, but this makes little difference, as the proportion of time that the motor operates at full load is small. The same effect can be accomplished by supplying reduced voltage taps so that the motor can be run at a lower voltage when lightly loaded.

When two definite speeds are required, these can be best obtained by the two-speed squirrel cage motor, if a-c. power is available. If d-c. power is to be adopted, the commutating pole d-c. motor can be used to give high economy at a large range in speed, by field control. Where variable speed is required for an a-c.

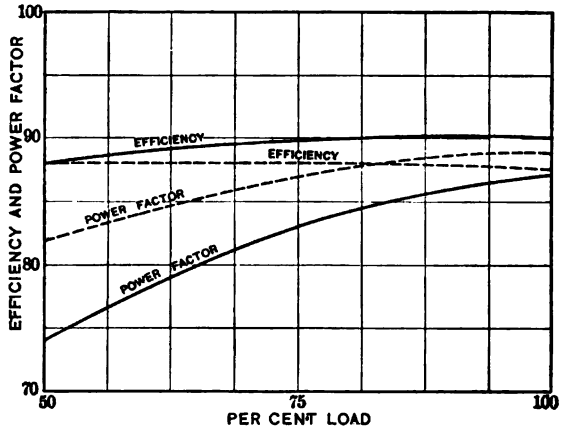


FIG. 3—PERFORMANCE CURVES OF SQUIRREL CAGE TYPE MOTOR
Full line curves are for standard windings
Dotted line curves are for special windings designed for high power factor at light loads

motor, this is generally accomplished by using a wound rotor and putting resistance in the rotor circuit. The economy is of course low at any but full speed. The fan load, being very steady, will greatly improve the load factor and regulation. The power factor will not be high, since a fan motor is seldom run at full load.

Several schemes are being tried out in Europe to give economical ranges in speed when using a-c. power, by the use of the three-phase commutator motor, and also a combination of wound-rotor induction motor with low-frequency synchronous converter and d-c. motor. While these schemes may be worked out with a fair degree of success in Europe, their success in this country remains to be seen.

Coal Cutting. The power used for coal cutting is generally direct current, and is often taken directly from the trolley system. Much better voltage regulation can be obtained, as a rule, where separate feeders are run for the cutting machines. The load factor of a single cutting machine is rather low, as it operates but 10 to 15 per cent of the time. Each operation lasts several minutes, so that the effect on regulation is not bad, and with several machines in operation, the load factor will be fairly high.

A practise which seems to be gaining favor is to do most of the cutting at night. This greatly improves the low load factor at night, and in many cases relieves the generators, which are over-loaded during the day.

The air puncher is still used, to a large extent, for under-cutting, but is being rapidly replaced by electric mining machines, wherever it is possible to do so. Where compressors must be used, they can, of course, be driven by motors. The load factor due to this load is high, owing to the fact that with no cutter in operation considerable power is required to keep up pressure in the long pipe lines, in which there are, generally, many leaks. The induction motor is, as a rule, used for operating compressors, with a power factor ranging from 75 to 90 per cent. Synchronous motors are becoming very popular for this service, but must be started with the load relieved by unloading valves, or by-passes. The compressed air system is very uneconomical, and in many cases 50 to 75 per cent of the power used can be saved by changing to electric drive.

Pumping. When the pumps are some distance inside the mines, direct-current motors, direct-connected to centrifugal, or geared to triplex pumps, are generally used. Power is taken from the haulage, or coal cutting lines. For large pumps, a-c. motors are used to advantage. These are, in most cases, induction motors. Where compressed air is used for punchers, it is also used, sometimes, to operate pumps and fans. Often the old steam engines are simply connected to the air system, and with the usual low air pressure and absence of pre-heating, the efficiency will, of course, be very low.

The load factor due to pumping will be very high, as a pump is usually run on constant load for hours at a time. In some mines the 24-hour load factor is very materially increased by using small pumps during the day to pump to a common reservoir or sump, and then using a large pump to raise the water out of the mine during the night. For these small pumps the self-starting d-c. commutating-pole motor has been very favorably received by the mine operators, as it cuts down the pump attendance to a minimum.

Any scheme by which a day load can be shifted to the night turn will not only improve the load factor, but will cut down the capacity of the generating apparatus. Fig. 4 shows a typical night load, consisting mostly of cutting machines, and indicates the need of improvement of load factor.

Tipple or Breaker. Motors used on the tipple, or breaker, are generally of the induction type, squirrel cage or wound rotor. Direct-current motors are frequently used where the operation has an isolated power plant. The load factor is fairly high, but the power factor will not average high, due to the fact that many of the machines are working under-loaded at times. The load factor can only be improved by regularity of

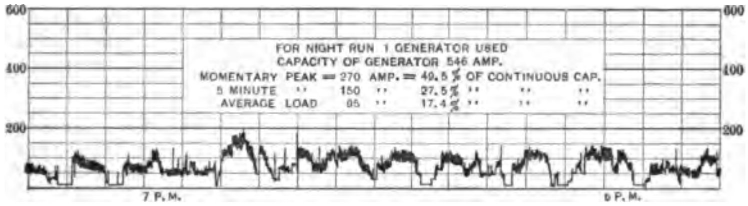


FIG. 4—EVENING LOAD ON 600-AMPERE MINE PLANT

output, while the power factor can be best improved by a close study of the power requirements, to see that each motor is of the proper capacity for the work. In some cases, by the addition of a small flywheel the capacity of the motor can be reduced, and the power factor and efficiency increased. It is for such a load as a tipple, or breaker, that power factor correction is desired, by the use of synchronous apparatus.

While making investigations, at different mines, of the power requirements, one of the authors has frequently come across steam power applications where the engine is far larger than necessary. The general reason for this is that the particular engine was the only one available at the time, and the advantage obtained was that the engine would operate for long periods with little or no attention. The difficulty of making tests on an engine is sometimes responsible for these misapplications. On the other hand, the ease with which electrical tests can be made makes it a comparatively simple matter to select a motor of the proper capacity.

Machine and Blacksmith Shop. The machine and blacksmith load is, generally, too small to have much influence on either load factor or power factor. Direct-current or squirrel cage induction motors are used.

Lights. The lighting load is a small percentage of the total, where the lighting is confined to the tipple and mine proper. This lighting is, generally, taken from the haulage system, to save wiring. With purchased power, all outside lighting should be alternating-current, since the regulation on the haulage system is not suitable for economical lighting.

The following table shows a summary of the various load factors and power factors, and ways in which they can be improved:

Load	Load factor	Method of improvement	Power factor	Method of improvement
	Per cent		Per cent	
Haulage.........	15 to 30	Proper arrangement of schedule	70 to 100	Use synchronous motor-generator sets or synchronous converters
Hoisting.........	10 to 30	Equalizing system	60 to 90	Equalizing system
Ventilation......	100		70 to 90	Use special winding to give high power factor at light loads
Coal cutting.....	20 to 50	Do cutting at night to improve load factor of system.	70 to 100	Use synchronous motor-generator sets or synchronous converters
Pumping........	70 to 100	If possible change part of pumping to night turn	70 to 100	Use synchronous motor-generator sets or synchronous converters
Tipple or breaker	20 to 60	Keep output steady	50 to 70	Use motors of proper capacity—not too large
Machine shop Blacksmith shop	40 to 70	Load too small to affect load factor seriously		Use motors of proper capacity—not too large
Lights...........	60 to 90	Load too small to affect load factor seriously		If a-c., use transformers of proper capacity—not too large

Power Rates

The larger purchasers of power generally have a fairly good idea of how their power costs are made up, although very often they have no definite information as to the actual costs. Most operators appreciate that their costs may be roughly divided into two groups, the first including the fixed charges, such as interest, depreciation, and amortization, and the second the various charges such as fuel, stores, labor, etc.

In a given station some of the items included under the second heading may be considered as fixed charges, as they are, practically, independent of the load. This is the case with such items as labor, line maintenance, etc.

Keeping in view the fact that the cost of power is made up of a fixed element and a variable element, it is clear to most large consumers, who have given the matter attention, that the most equitable way of charging for power is one that is based on a system that takes this into consideration.

Furthermore, they are alive to the fact that their total power cost is likely to be lower with such an arrangement than would be the case with a flat rate, as the central stations must arrange their rates so that a profit can be made, which leads to setting flat rates at such a figure that any load factor of the system would be profitable.

It is immaterial whether the power is generated in a central station, and distributed to a number of customers, or whether each of the consumers has his own generating station; the power cost will be made up of the same items, which can be very easily demonstrated to a prospective customer. The difficulty always occurs in persuading the customer that the particular rate proposed is a fair one.

Although the question of power rates is, mainly, a local one, a short discussion of various power rates, and how they may be influenced by the customer's load, may not be out of place.

Flat Rate. Very little power is sold to large users on a flat rate. On account of the simplicity of this system, it is very suitable for small customers, where it is impossible to obtain an intelligent appreciation of the proper basis for charging for power. Flat rates are usually very high, and necessarily leave a considerable margin for variation of load factor.

When power is to be purchased in large quantities, a flat rate does not enable the customer to obtain the advantage that should be derived from high load factor, or, if it is so arranged as to do this, it does not adequately protect the central station. This system is therefore quite unsuitable for large customers, and can only be defended on account of its simplicity, and the difficulty that is encountered in properly determining the basis of an equitable power charge.

Maximum Demand System. The late Dr. John Hopkinson suggested this system over twenty years ago, the idea being to take the load factor into consideration when determining the power costs. This system, as worked out by various companies, principally in England, of recording the maximum instantaneous demand, and basing the power costs upon one rate for a certain number of kw-hr. per month per kw. demand, and upon another rate for the remainder of the power, has been generally known as the Wright system, from its principal advocate, Mr. Arthur Wright, who introduced it many years ago.

For instance, for the first 50 kw-hr. per month per kw. demand the rate might be five cents, and for all power in excess the rate of, say, one cent might be charged. Thus, if the maximum demand is 100 kw., and the monthly power consumed is 10,000 kw-hr., the first 5000 kw-hr. would be charged at five cents, and the second 5000 kw-hr. at one cent, making a total of $300, or an average rate of three cents. This system has been used in various forms, and is still used by a number of concerns.

For industrial power it has a great many disadvantages, not the least being the difficulty of determining the maximum demand. The system is not on the correct basis, as it does not take into consideration the equipment that the power station must have available for the customer. It is, however, a decided improvement upon the flat rate system, but a poor system to use with small customers, in view of the difficulty of explaining the great variation in the cost of power that is liable to occur at different seasons of the year. This difficulty is increased in proportion to the number of customers on the system. With a few large customers there is usually very little trouble in adjusting the accounts, and explaining the variations.

Due to the fact that it does not clearly recognize the basis on which the cost of power is founded, it is not an easy rate to explain to a customer. There are a number of variations which attempt to avoid the defects of this system. For instance, a certain percentage, only, of the maximum peak is sometimes taken as a basis of charging, but the system is fundamentally wrong.

Flat Rate with Fixed Charge. In view of what has been said as to the cost of power, it is obvious that the only correct basis is one that takes into consideration the fixed charges on the equipment that must be kept in reserve for the customer, and a variable charge depending on the amount of power consumed.

The only way that the amount of power station equipment that is required by each customer can be determined is by keeping some record of the customer's load conditions.

There are certain differences of opinion as to what is a fair basis on which to determine fixed charge. Some operators are of the opinion that any load that may be sustained for a period of, say, five minutes, is the proper basis, and others take into consideration the average load for some definite period. The latter is, in our opinion, the correct system, especially for mining work, or for any service where the load fluctuates very rapidly. For instance, if the five-minute peak load is to be taken as the basis for fixed charges, it is quite possible to have such conditions that there never will be any peak that will last this time. It has been our experience that there is quite a number of cases where a strict reading of the contract would mean that the power company could not collect any sum for fixed charges. Then, again, within certain limits, the instantaneous peak loads do not concern the central station, as the generators have sufficient overload capacity to take care of such peaks.

It is the load that must be carried for, say, fifteen minutes, or half an hour, that really determines the capacity of the generating plant, and therefore the proper basis for charging is one that takes this point into consideration. In other words, the average, or integrated, peak load, for a reasonable period, must be used. The period taken is, generally, fifteen minutes to half an hour; the larger power stations adopting a longer period, as the variety of their load enables them to take a more liberal attitude towards the customer than is possible with a smaller station.

To determine, to some extent, the effect of the length of the integrated peak upon the basis of charging, a number of investigations have been made, and the results of one case are shown in Fig. 5. This figure shows the peak load on a basis of a 5-minute, 10-minute and 15-minute average peak. The results may be summarized as follows. Taking the 15-minute peak as a basis, the 5-minute and 10-minute peaks are given as a percentage:

15-minute peak	100 per cent
10- " "	104 per cent
5- " "	106½ per cent

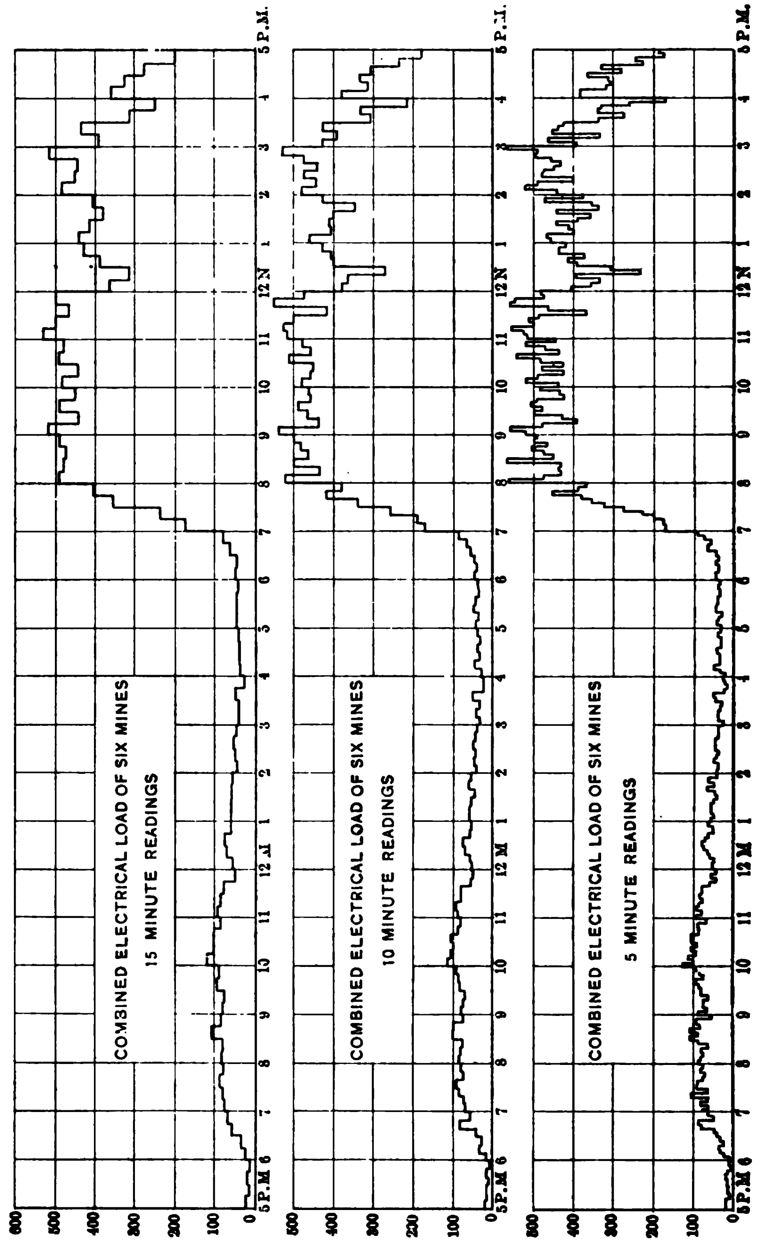

COMBINED ELECTRICAL LOAD OF SIX MINES
15 MINUTE READINGS
COMBINED ELECTRICAL LOAD OF SIX MINES
10 MINUTE READINGS
COMBINED ELECTRICAL LOAD OF SIX MINES
5 MINUTE READINGS

Fig. 5

The difference is comparatively small, and it is generally easier to persuade a customer that a 15-minute peak is an equitable basis, which is not always the case with a 5-minute peak. The results shown in Fig. 5 are made up of simultaneous tests, made on six different mines belonging to one company.

It is usually not possible to arrange the fixed charge that a large customer pays so that it will cover the total fixed charges of the station that would be represented by the capacity measured on, say, a 15-minute integrated peak, as the charge would be so high that it would be difficult to secure business.

It is usually very easy, however, to obtain a fixed charge that would be equal to the corresponding figure if the customer installed his own station, which he could do, generally, at a lower cost per kw. capacity than the central station, as he would have considerably shorter lines, and the standard of equipment probably would not be so high.

It is, therefore, generally necessary to take a figure somewhat lower than the full amount, and from the standpoint of the central station this is justified, because the central station obtains the benefit of the diversity factor, which under certain conditions may be considerable.

This may be illustrated in the case of the diagram shown in Fig. 5. The total 5-minute peak of the six mines is only 68 per cent of the sum of the individual 5-minute peaks of the different mines. The diversity factor, in this case, is 1.46 to 1. Where many mines are being operated from one station a much greater diversity would be expected.

The additional flat rate per kw-hr. is a very much more flexible charge, and the average customer is usually prepared to stand for a flat rate that represents a fair profit, whereas the fixed charge must be reduced below what is really an equitable figure. A great deal of this difficulty is due to the fact that customers have, as a rule, very little idea as to the proportion of fixed and variable charges in their power accounts, and therefore have not been educated to see these charges in the proper relation.

In the case of a large system, supplying power to both large and small consumers, the question naturally arises, should the large user pay the same rate as the smaller one? In general, it is reasonable for the large customer to expect a lower rate, as he looks on the matter from the standpoint of what he could do with his own generating plant, and this is a condition that must be met by the central station.

The cost to a station decreases, somewhat, as the amount of power used increases, as the expenditure for lines, labor, etc., for the equipment required by any one customer is not very different whether the average load is 100 kw. or 1000 kw. A system of giving a discount on the rate per kw-hr., depending upon the consumption, would be a fair and equitable method of meeting this condition.

Effect of Power Factor on Rates. This is a question that is of considerable interest to the central station. Usually, no difficulty is encountered in demonstrating to a consumer that a reasonable power factor should be maintained.

Some central stations are attempting to meet this condition by regulations designed to control the type of apparatus used by the customer, but there are limits to this arrangement that reduce the value of the scheme. It is customary to require synchronous motor-generator sets to be used for direct-current work, and, as a great deal of the power required by mines is for haulage, coal cutters, etc., fairly good results can be obtained by this simple expedient, but there are a great many cases where induction load predominates, and a more definite scheme is required. It appears to the authors that, if it is found necessary seriously to consider the power factor, a definite limit should be set, and for lower values an increased rate charged. This can be arranged for by determining the kv-a. and kw. consumption of the consumer. To make it desirable for the customer to maintain his power factor as high as possible, a bonus might be given if it averages above the limit set.

Methods of Measurement. Although some system based upon the peak loads is generally recognized as being correct, it is not at all easy to determine a rate with a basis that can be accurately measured, due to the fact that in the past, apparatus has not been available for the satisfactory recording of the peak loads. Graphic recording meters of the usual type are not at all satisfactory, as they do not show the maximum average for a definite period, without a considerable amount of investigation, and they are not accurate if used to record the instantaneous peaks.

When rates are based upon the instantaneous peak, a certain percentage of which is taken to represent the station capacity required by a particular customer, there is, usually, room for considerable discussion as to the methods of determining these peaks.

During some investigations made by one of the authors on the high cost of power for a particular installation, it was found that the meters used, under certain conditions, recorded peaks approximately 60 per cent above the actual value, due to the meters overshooting. This discovery led to a further investigation of a number of other installations, and it was discovered that a similar condition existed, which caused considerable dissatisfaction on the part of the customers.

The graphic meter has the advantage of giving a chart that is a permanent record, which can be used in case of disputes, and it shows extraordinary peak loads, due to short circuits, etc., which can be left out of consideration in determining charges.

The type of meter that simply indicates the maximum instantaneous peaks, has the disadvantage that it does not discriminate between peaks due to short circuits, and normal operating peaks, so that such instruments are practically useless, under modern conditions, for industrial plants.

Undoubtedly the proper instrument to use is a meter that will integrate the peak load for a certain definite period, and record this in such a way that the values may be referred to, at any time, in case of dispute. Such instruments have been devised, and we believe that the first were designed about six years ago for the Victoria Falls Power Company.

Due to the fact that the question of power rates has not been given the consideration that it deserves, such devices have not been developed to the extent that would be desirable, through a lack of demand on the part of the operators. During the last few years, however, the increased effort on the part of central stations to secure industrial loads has increased the number of large consumers, and has brought this question of charging to such a position that these devices are receiving more attention. In designing such apparatus provision must be made so that the meter will pick out the highest peak. For instance, if a 15-minute peak is the basis of charging, it would not be satisfactory to simply have a meter divide the hour into four parts, as it may happen that a high peak might be then divided into two parts, part occurring during one of the periods, and part during another. If a peak load of 15 minutes' duration occurs, and it should happen that it starts in the middle of one of the periods, the maximum recorded by the meter would be only half the correct value. If two integrating devices are used, both operating on the same recording device, and they are staggered, then there is no chance of such a condition occurring. It may happen that the record will not show the absolute maximum, but the difference will be very small.

Power Factor Measurement. The power factor must be considered in conjunction with the power requirements, and any scheme using a graphic power factor meter is unsatisfactory, as it might easily happen that for fairly long periods a leading component is provided by the motor-generator, which would be of little value to the station, as it would not exist during the time when the equipment is loaded to its capacity. A much more satisfactory system is to consider the average power factor, but it is not possible to obtain values direct, as no kilovolt-ampere meter is available. We can, however, integrate the wattless component by a suitable meter, and thereby obtain the average value of power factor.

Suggestions for Power Contracts. In view of what has been said, we would suggest that an ideal power contract for mines should be based upon the following:

(a) Fixed charge, depending upon the integrated peak load for a reasonable period, so that it will represent, approximately, the equipment required to carry consumer's load.

(b) In addition to the above, a flat rate per kw-hr., based on operating costs, taking into consideration the amount of power used, and allowing a graduated discount to give large consumers a lower rate.

(c) If power factor is a consideration, a reasonable limit should be set, lower power factors being penalized by increasing the rate per kw-hr., and a reduction in rate granted if high power factor is obtained.

We believe that if this plan were generally followed, it would be possible to meet all the conditions arising in mining work, and to fix rates that can be given to all consumers that will represent profitable business.

In conclusion, it must be pointed out that to secure this load the central station should adopt a generous policy towards its customers, and when there is doubt as to the effect of operating conditions upon the station's costs, it should be prepared to accept the risk, rather than attempt to place a burden upon the consumer which may not be justified by results. The central station has the advantage of the diversity of loads, and a part of this benefit should be given to the consumer.

DISCUSSION ON "MINING LOADS FOR CENTRAL STATIONS" (SYKES AND BRIGHT), PITTSBURGH, PA., APRIL 19, 1913.

P. M. Lincoln: I do not think in our industry there is anything on which the members of the fraternity have more divergent opinions than they have on this question of power rate. It is somewhat difficult in the first place to obtain a proper conception of the items to include, to arrive at a proper power rate. Possibly the best way to get at it would be to consider the things which go to make up the ideal power rate. In starting out on this task it is very possible, in fact it is almost certain, that what may be an ideal power rate in my opinion may not be an ideal power rate in the opinion of some one else.

In my opinion the ideal power rate should recognize load factor, it should recognize the power factor, it should recognize the quantity of power, and it should recognize the time of day at which the peak load occurs. In addition to these requirements, it should be easily measured, and measured by standard instruments one can get on the market, and by instruments which do not have an excessive cost. In addition to all that, it should be a rate which is easily explained to the customer.

When you have got that far, you can see that there are some inconsistencies, among the items that go to make up the ideal rate, because a rate which takes into consideration load factor, power factor, quantity of power consumed, and the time of day at which the peak occurs, is not easily measurable and also is not one which will be easily explained to the power customer.

We are, therefore, forced to the proposition of selecting a rate which will give the best compromise, and just what that compromise shall consist of is the point on which most us will differ.

The rate which has been suggested in the paper is, I think, an equitable one. It depends upon the maximum demand plus a kw-hr. rate, and a suggestion is made that the power factor should be taken into consideration, but just how that is going to be accomplished is not disclosed. I believe that a rate that is based upon the suggested method of measuring is an equitable one. However, the question of measurement is a difficult one. There are instruments on the market which will measure maximum demand, but they are not cheap, and as yet they have not had a very wide application, so that here again is a serious difficulty that confronts us when we come to this question of power rates. The demand for some instrument which will give us the maximum demand and possibly also take into consideration the power factor, will eventually result in the production of a meter which will be cheap and accurate, and will be standardized, but as yet I do not believe that we can now say that we have such an instrument.

Sidney G. Vigo: In the paper by Messrs. Sykes and Bright, on page 1136, under (b) the statement is made: "In addition to the above, a flat rate per kw-hr., based on operating costs, taking into consideration the amounts of power used, and allowing a graduated discount to give large consumers a lower rate." I believe that this basis of charge is considered very carefully in Mr. Hopkinson's design of his wholesale rate of charge, of which he speaks in the early part of his paper, that is, based on a kilowatt demand charged for fixed investment, and a sliding scale for the operating charges or the kilowatt-hours consumed per month.

Along these lines various central stations in the country have based a secondary charge, as it is called, per kilowatt-hour consumed, on such a sliding scale as ranges down. If a mine consumes, say 50,000 kw-hr., it gets a rate which runs down until it strikes about 6 or 7 mills per kw-hr. and below this sliding scale rate there is a still further discount given for prompt payment of bills.

Now, the central station, in addition to offering a rate of this kind, should take into consideration, as Mr. Lincoln suggested, the time at which the peak comes during the month. Where the central station is located in a large community, where it has considerable general business, and the business of the mine is only incidental to its general business, it may be considered that the mining operation is primarily off-peak business. They consequently should rectify their primary charge, or fixed investment charge, to conform to the investment required for the mine, which keeps off its peak. This should involve a different rate from that which is offered to the ordinary consumers, and the central station should be in a position to offer a very flattering rate to mine operators.

The matter of peak we know is of considerable importance to a central station company, not only during the summer months when the mine operates, but speaking of it in a general way. It might be shown that in one of the largest central stations, probably the largest, in the middle west, there are investments of $80,000,000 in equipment, transmission lines, etc., and during the summer months $25,000,000 of that investment is lying idle, and this large amount of equipment is installed to take care of the peak in the winter months; consequently, it has arranged very flattering off-peak rates to all classes of industries in order to improve its yearly load factor.

There is one point which Mr. Bright touched on in reading the paper, in the early part of it, which did not deal with the rate situation, but was a question of operating steam engines in mines. The fact that the engine was considerably larger than was required was due to the fact that there was no other engine available. That is quite a common occurrence in mines, as well as a matter of fact, in all classes of business, and it brings out very forcibly one of the strongest points that central station companies could use as arguments in favor of their service, and that is the flexibility of central station drive. As the mine will increase in capacity, or other machines are added to its equipment, it is not necessary to put in a larger unit—simply add a larger motor; and in view of the contemplated growth of a mine, it is sometimes customary to put in a considerably larger engine than is required at the outset, and you can see that in this way the inefficiency at which a plant was operated during the earlier period might be quite considerable.

The central station company, although it is not its inclination to curtail the activity of the consulting engineers, maintains men who are thoroughly familiar with all classes of industries. The central station will have a man for instance, who will be an expert on mining, and he will give every attention possible not only to the securing of the business but also to the designing of the best equipment for the mine, and one of the prime slogans, you might say, of central station activity is the fact that after the piece of business is secured the work of the power man has only just begun. He must necessarily follow this piece of business and see that it continues to operate to the satisfaction of the consumer, and he should always be ready to offer any suggestion that will improve the economy of the operation.

Theodore Swann: I am connected with a company in West Virginia which is installing a plant in the heart of the coal fields, with the express idea of serving the coal mine load only. We tried to determine that ideal rate, which I know we have not done so far, but we have given the customers a certain benefit of the diversity. If a customer's demand is between 50 kw. and 300 kw., we determine his demand on the basis of a five-minute integrated peak, and set his circuit-breaker at 100 per cent overload, thus giving the individual mine the benefit of a 100 per cent overload, which will take care of any drop. If the demand is between 300 and 500 kw., if there is more than one line being operated by this customer, because there are very few individual mines that require over 300 kw., we lengthen the time out to 10 minutes, and only give him 75 per cent overload, instead of 100 per cent overload. In the case of a customer using 500 kw. and over, we stretch the time to 15 minutes, and cut the overload down to 50 per cent. In that way we believe we have every class of customer paying us the same interest charge. By actual investigation of more than one hundred plants in the field, we find the average investment per kw. of station capacity by the customer to be $80. If we charge six per cent interest on that, and 7.5 per cent for obsolescence, as we prefer to call it, and 1.5 per cent for insurance and taxes, we have a total of 15 per cent fixed charges per kw. of rated capacity, which makes a rate of $12 per year for fixed charges, independent of operating expenses, and we determine our rate on the same basis at $12 per kw. of demand per year, to cover the carrying of reserved capacity for the operator. On tests made on fifty-six plants, which will average 200 kw. each, we found the integrated peak to be 60 per cent of the installed capacity, and that means the customers would be paying themselves on the basis of $12 per kw. per year, but on the same number of kilowatts, only paying us 60 per cent of that. They would have the 40 per cent, which means that is the reserve carried over the average of the fifty-six plants that we have tested on this basis.

As to the load factor, we found the minimum load factor based on the integrated peak to be 5.3 per cent, and the maximum load factor to be 49.7, and the average of the entire field to be 26.4, but the load factor based on demand to be only 15.8 per cent. These figures were determined by the printometer which we used on all the plants, conducting tests which covered from one day to ten days, in securing the average. We determined the average tonnage by the production for 1912, and during the time of all these tests we were "off" less than one per cent of the average normal production, so for that reason the figures may be taken as representing actual operating conditions to a degree which is measurably correct.

As to the diversity, by plotting the load curves of twelve plants we found the diversity to be 14.25, and taking twenty-two plants we found a diversity of 1.52. We figured originally we would have a diversity of 1.5, and instead of receiving $12 per kw. per year of demand we would receive $18 per kw. of our central station demand, thus enabling us to put more money than $80 per kw. including transmission, into our service.

From the present test, it would seem that our 1.5 diversity is going to be low. The 1.52 was based on the average of twenty-two plants, that aggregated 3,000 kw. of demand, and we expect to have on our line something like 20,000 or 25,000 kw. so that it would indicate, while the ratio is very much less as you increase the number of plants, that it will be possible to obtain a diversity, an hourly mining load, of at least 1.75. That is on bituminous coal, the majority of the mines being drift or slope mines, only about 10 per cent of our total output being shaft mines, and in that particular instance we measured the current to the customer at one point, and he carried it out to 18 points, so they get the benefit of the diversity before it goes on our line. If we had more of the hoisting loads, as you have in Pennsylvania, I do not think we could get the same results we had on that basis.

One of the most advantageous features to the mine operator in purchasing power, in my opinion, is to have a definite method of determining his cost. I have made the statement to some operators that we might charge them 25 per cent more per kilowatt-hour for current, and even at that we would save them money at the end of the year. At first that sounds more or less like a fish story, but relocation is our one hobby. The average power plant will lose from 30 to 50 volts in transmission from the power house to the drift mouth. In no case have we recommended underground substations. We want to simplify the operation in every way possible. This relocation means additional voltage on the machines; and in the case of one mine, which represented an average condition, which we tested, we found that by improving the voltage, due to the bonding, they could haul twenty cars per trip in lieu of twelve cars. This condition in that mine was more of an average condition, rather than an abnormal condition. We have made the statement, and we have proved it out in actual test, that two locomotives or two mining machines will haul more coal and cut more coal than three locomotives and three mining machines, if you give the two locomotives and two mining machines good voltage and give the three locomotives and three mining machines ordinary voltage, not taking the worst condition. We have found some mines attempting to haul coal with as low as 40 volts. It is needless to say what the results were. It appears that the average power plant was installed somewhere near water, and as the mines grew and opened up their workings it was natural that the scene of actual operation in the mines became more removed from the site of the power plant, and our idea, in going into the field of supplying power for mining operations, is that the generation of current for mine use is becoming an alternating current proposition, though its use inside the mine may be in the form of direct current.

As to a uniform contract, the first thing we decided on was that we would give an absolute uniform contract and not vary from it in the slightest particular. I am glad to say we have over half of our plants loaded, and we have not made a variation of our rate in a single contract. We at first determined the form of contract which we believed was fair and equitable, and we have stood on that contract. It is based on the $12 per kw. of demand, and in addition to that a base rate for current supplied of 1.5 cents per kw., with a discount starting at 10 per cent and going in steps of 2.5 per cent up to 55 per cent. Our current charge runs from 1.35 cents down to 6 mills, based on the quantity used. The lowest discount is given on 10,000 kw-hr. per month, and the highest on a million kw-hr. per month. We have been able on these rates to obtain three of the largest operations in the field, and a great many smaller ones, and in practically every case we were able to show that, even on our rate, which we were very frank to state to the operator is high enough, we cannot give them strictly first-class service, and it has been my experience that the operator would rather pay a reasonable rate and be guaranteed good service than to buy something cheap; in other words, we say we are selling them service, and incidentally furnishing them with power. To carry that out, I may say that all of our construction will be steel towers, the poles all on loop circuits, and there will be no mine but what we can serve from two different points, unless it is a small branch off the main line; we will use 44,000 volts for our secondary, and when we develop a waterpower plant we may use something higher.

I have found that the operators in West Virginia are not after something cheap, they are after the most reliable service which they can obtain; they are installing alternating current and properly applying it. In nearly every case that we canvassed, the operators of the mines were more than anxious to get the facts about their equipment, and many of them said " we want you to please tell us frankly what you think of our conditions." Sometimes they have been so bad that we did not dare tell them frankly what we did think of them. I have dealt with the coal operators for about four years, and of all the different classes of people, I believe they are a class you cannot " put anything over on" and get away with it. You must have a fair, square rate, you may tell them you are charging them enough, but you must give every one of them exactly the same rate. I have known of cases where a salesman would sell one operator something at one price, and another operator the same thing at a different price, and it is only a question of days until that man's usefulness is done with in that particular field. I think that is true of the coal operators more so than it is with any other class of people. The inter-relationship and ownership between the companies makes it absolutely necessary to have a uniform contract. We have a new Public Service Commission in West Virginia, which, to a certain extent, has been the reason why we cannot make exceptions. On the other hand, when a coal operator believes he is getting the same contract as every other man, the tendency is for him not to read the contract, even. He will say, " Is this the same contract that so and so has?" And I will say, " Yes," and in some cases show the signature of the other man, and he will sign the contract and say, " If I am getting what he is getting, it is all right."

H. C. Eddy: I think that Messrs. Sykes and Bright are to be congratulated upon the many good things that appear in their paper. There is one paragraph, which I think most mine operators ought to cut out and paste somewhere where they can refer to it frequently and become thoroughly familiar with it. That paragraph is this:

" It is immaterial whether the power is generated in a central station, and distributed to a number of customers, or whether each of the customers has his own generating station; the power cost will be made up of the same items, which can be very easily demonstrated to a prospective customer."

The only exception I would make to that is the last clause—it is not always so easy to demonstrate to the possible customer that he has any fixed charges. So many of them are apt to consider that the cost of the labor, the cost of the coal that they use, and the other incidental operating expenses are the only expenses that enter into the cost, and when you call their attention to interest on investment and depreciation, and some of these other items which are absolutely a part of the cost, they are apt to get back at you with the statement that the plant has long ago paid for itself, consequently no interest charge should apply, and they maintain the plant in good operating condition, and it is, therefore, not a part of their bookkeeping to make any charge for depreciation. I think that if the average owner and operator of a power generating plant would consider these points a little more carefully than they have done in the past, that perhaps their ideas as to the cost of generating would be materially altered.

There is another point which I had intended to speak about, but which has been covered very fully by Mr. Swann, and that is the question of the primary charge based, say, upon a kilowatt of demand. The paper states: "It is usually not possible to arrange the fixed charge that a large customer pays so that it will cover the total fixed charges of the station." I do not consider that that is altogether necessary, that it should cover, because of the fact of the diversity factor that enters into a large system supplying power to a number of different customers, whose demands on that station come at varying periods, so that there is no great likelihood of one peak being superimposed upon another, and thus making unusual demands upon the plant and requiring it to carry a very high reserve capacity.

So far as I am able, I should like to endorse absolutely the remarks of Mr. Swann with regard to uniformity of treatment of various customers. That is the only way that it is possible to sell either service or a commodity. One man's money is as good as another man's, and so long as one man makes the same use of the facilities which the central station offers him as another one, these two men should be treated exactly alike.

There is another point I should like to bring out, and that is that it seems to me there has been an unnecessary discussion of the rate per kilowatt-hour. I do not believe that that is of as much real interest to the purchaser of power as the amount of his bill. I do not believe that there is an operator who cares very much what his rate per kilowatt-hour may be, provided the total cost of power purchased from the central station is less than he can produce the same quality service for himself, and whether your rate be 15 or 20 cents per kw-hr., and you can save him money in the course of a year, he is your customer for all time and your friend as well. So that while there is a great difference of opinion as to what constitutes the proper rate, and as Mr. Lincoln has pointed out, a proper rate, an ideal rate, from the central station standpoint, is usually almost impossible of explanation to the man who is not familiar with the problems of the central station, yet I think it is possible to incorporate all or nearly all of the elements of an ideal rate in such a way as to at least appear reasonable to the man you are attempting to deal with, even though he does not understand all of it, and that lends even greater force to what Mr. Swann has said in regard to uniformity of treatment of various customers. If they feel that you are dealing fairly with them, and that each man gets the same rate as his neighbor, and they will soon find it out if he is not, the question as to whether the rate is properly constructed and whether it is readily understandable, becomes a secondary matter.

S. B. Storer: The question of rates is one I have been considerably interested in for the last ten years, and I am glad to see that so many of the expressions of sentiment here this morning indicate a tendency to one system—that of a maximum demand charge plus a kilowatt-hour charge. If there is one thing that upholds the belief that that is the only fair system, it is a continual attempt to sell power for a number of years to all classes of consumers. Furthermore, all seem to realize the growing necessity of having an absolutely uniform contract. The feature of how the maximum demand is to be measured is one that every customer tries to settle to meet his own conditions, so that his longest peak will not be subject to that measurement. In all of the contracts which I have negotiated personally, the duration of the peak has been limited to 60 seconds. That may sound pretty short, but it might better be 30 seconds than 60 seconds, and it would still be equitable alike to the consumer and the power company.

To show, as an extreme case, that that is not too short a time, I can refer to one particular factory that I have in mind, a sheet steel rolling mill located near Buffalo where power is purchased on the basis of their load factor, the demand being on a one-minute basis. While the contract itself provided that the demand might be measured either by the average load for one minute or by a block peak for one minute, as obtained by a line drawing wattmeter, the measurement at first was made by a meter of the latter type. For a considerable period of time, the load factor month by month was determined at 125 per cent, or in other words, they never had a peak that lasted over 10 to 15 seconds, it being caused by a quick shoot of the metal through the rolls. The line-drawing wattmeter would make a momentary stab and drop down again. The result was that the sustained one-minute demand was simply the friction load—coupled with a small supply of energy to the flywheel, which, of course, smooths out the peaks a trifle. As a load factor of 125 per cent is an absurdity, the only thing that could be done was to call it 100 per cent and charge it on a kilowatt-hour basis.

The fair way to get at a demand charge is to arrive at a time element so that if it is in use over a very large system, the demand will be shown in the operation or action of the governor controlling the prime mover, whether a waterwheel, steam engine, or gas engine. If it is a momentary fluctuation, the stored energy in the revolving elements operating in connection with the transmission system will take care of it. A waterpower plant having a capacity of 100,000 h.p. will take a momentary demand on a railway system of 500 or 1000 h.p. and the governor may not show it, but if that demand lasts for 30 or 60 seconds the entire system is slowed down in speed sufficiently so that the waterwheel governor opens the gate to compensate for the extra load. To my mind, therefore, any system of charging to be equitable alike to the power company and the consumer should take that feature into consideration. A steam plant can stand a peak of much longer time with safety, due to the stored energy in the boilers, than can any water plant which usually has no overload capacity in the wheel.

The fixed charge, as a proportion of the total cost, may in general be made considerably less with a steam plant than with a water plant, on account of the lower investment, but the kilowatt-hour charge must be correspondingly higher, due to higher operating costs. Twelve dollars per kilowatt-year as a fixed charge on a steam plant is fair to customers of almost all sizes, but on a water-power system, you must take into consideration the diversity factor to a much greater extent, and a user of 5000 h.p. should pay a great deal higher rate per horse power year or kilowatt-year, on the maximum demand, than the little fellow does. To my notion, the ideal system makes use of a low service charge per kilowatt-year for little consumers, perhaps of 5, 10, 15, 20 h.p., or even up to 50 h.p., and that may be $12 per kilowatt-year on their installed capacity or their contract amount of power, as they ordinarily require it, but if you put on a 5000-h.p. customer, your diversity factor is so cut down, that, as a general proposition, the rate per kilowatt-year should be 50 per cent more than it is to the small customer. Most of the contracts which I have negotiated take that feature into consideration and have a continually increasing kilowatt or capacity charge, from the smallest to the largest. To compensate for the increasing service charge rate, and also to meet the commercial conditions arising under actual operating systems, the kilowatt-hour rate for the small consumer is made perhaps 1.5 or 3 cents, while for the very large consumer it is only 3 or 4 mills, the service charge making up about one-half or even two-thirds of the total cost of power to the consumer.

I quite agree with the last gentleman who spoke that the average consumer does not care anything about the kilowatt-hour rate, provided his monthly bill is reasonable. While it is a statement of a somewhat retrogressive character, I am going to say that I think most of the tests to determine what the average manufacturing plant or any other power consumer uses, and what the bill is going to be, is pretty much time wasted. A good power salesman, with considerable experience, having a line of consumers with whose plants he is familiar, can tell what each plant ought to operate at, and if he has the courage of his convictions and believes it, and his power company has its nerve with it, they will go in and tell that customer— "We will equip your plant, supply it with power at our standard rates and you will run the plant with this power for a year. If at the end of that time the results do not appeal to you or you are not satisfied with the bills or service, you can discontinue its use and we will take the apparatus off your hands at what it cost you." I have used that same method repeatedly when it was impossible to convince certain manufacturers that their costs for electric power would be within a reasonable limit. They did not care about that. They knew that it cost them very much less for power than the figures we mentioned, but they were willing that we should put in an equipment on the basis which I have outlined, and we have never yet been called upon to take back the apparatus. Service is the meat of the whole power discussion, and whether it is one basis of charging, or another, does not materially affect the consumer, so long as the result is satisfactory; but it is necessary, because of public service commissions, and because of swapping of stories back and forth between consumers, to have a power contract which is absolutely uniform for all of them, and you cannot get that by giving a five-minute peak to one customer, a fifteen-minute peak to another and half-hour peaks to others.

Graham Bright: I have not very much to say in closing the discussion on this paper. Mr. Lincoln made the remark that there was not an instrument at a reasonable price, available at present for making a proper record as mentioned in the paper, but it probably will not be very long until we can get instruments of this type. It is simply a question of time, because when there is a demand for a certain instrument it will be developed, and in my opinion it will not be long until we have instruments of the proper kind, at a reasonable price, to accomplish this service.

Mr. Vigo mentioned the difficulty of moving a steam engine around, as the plant grows, after the engine becomes too small and must be replaced by a larger one. It is quite a difficult matter to take the engine out and put a larger one in its place, but when the power supply is received from a central station, and the motor which the customer has becomes too small, it is a comparatively simple matter to slip a larger motor in its place, and use the smaller motor in some other application.

The question came up yesterday in regard to using the name-plate rating as the basis for fixed charge. There is a point in connection with this scheme which makes it rather a hardship for the customer. Very frequently in opening up a mine it is desirable to put in apparatus large enough to take care of the conditions in the future, and if the name plate ratings are taken as a basis for the fixed charge, it means that the customer is burdened with a heavy fixed charge until his apparatus becomes loaded. The result is he might buy apparatus entirely too small for his future needs, which will soon become overloaded, causing general dissatisfaction.

There is one question which has not been brought up in this discussion—Mr. Eddy mentioned it in his paper yesterday—that one of the first advantages to the operator from central station power was the decreased cost. Under certain conditions this is not always the first advantage and it is not always necessary to show an operator for a mine that his costs will decrease. In fact, I believe that in many cases an operator can afford to pay at least 10 per cent more for power secured from the central station than it would cost him to produce it himself, due to the absence of *worry* and *care* and absence of investment, so that it is not always necessary to show the customer a decreased cost.

In regard to the policy of the central station with its customer, there has been too much secretiveness in the past, and the general opinion of the customer has been that the central station is charging all the traffic will bear. It is only by an open and liberal policy toward the customer that the central station can obtain the confidence of the public, and the central station should stand ready to make concessions to a customer who is in trouble, or has a long period of shut-down.

INDEX.

PAPERS AND DISCUSSIONS.

INDEX OF AUTHORS

TRANSACTIONS

OF THE

AMERICAN INSTITUTE

OF

ELECTRICAL ENGINEERS

JANUARY TO MAY 1913

VOL. XXXII, PART I

PUBLISHED BY THE
AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS
33 WEST THIRTY-NINTH STREET
NEW YORK, N. Y., U. S. A.
1913