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MURRAY, S. W.....Murray, Dougal & Co., Milton, Northumberland Co., Pa.

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NASON, CARLETON W..........Nason Mfg. Co., 71 Fulton Street, New York City.
NEWCOMB, CHAS. L..............Supt. Deane Steam Pump Co., Holyoke, Mass.
NEWTON, ISAAC............................20 Union Square, New York City.
NICHOLSON, STEPHEN..Providence, R. I.
NICHOLSON, W. T........................Nicholson File Co., Providence, R. I.
NICOLL, WILLIAM L....................Navy Department, Washington, D. C.
NORTON, HAROLD P....Asst. Eng. U. S. N., Bureau of Steam Engineering, Navy Dept., Washington, D. C.

ODELL, WM. H..Yonkers, N. Y.

PALMER, GEORGE E..........................64 S. Canal Street, Chicago, Ill.
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PARKHURST, E. G.......................Pratt & Whitney Co., Hartford, Conn.
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PARSONS, WILLARD P....Walter A. Wood M. & R. Machine Co., Hoosick Falls, N. Y.
PARTRIDGE, WM. E..National Car Builder, 140 Nassau Street, New York City.
PAYNE, DAVID W..Elmira, N. Y.
PATTON, WILLIAM H..Virginia City, Nev.
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SCHWEINITZ, DE P. B..........Colorado Coal and Iron Co., South Pueblo, Col.
SCOTT, OLIN...........Gunpowder Mills and Machinery, Bennington, Vermont.
SCOTT, IRVING M..San Francisco, Cal.
SCRANTON, W. H.............................Oxford Iron Co., Oxford, N. J.
SEE, HORACE....William Cramp & Sons' Ship and Engine Building Co., Philadelphia, Pa.
SEE, JAMES W..Hamilton, Ohio.
SELEY, CHARLES A........................53 Gilfillan Block, St. Paul, Minn.
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SELLERS, COLEMAN, JR...................412 N. 33d Street, Philadelphia, Pa.
SELLERS, WILLIAM........................1819 Vine Street, Philadelphia, Pa.
SHELDON, THOS. C..Boylston, Mass.
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SMITH, CHAS. D........................H. D. Smith & Co., Plantsville, Conn.
SMITH, GEORGE H................Brown & Sharpe Mfg. Co., Providence, R. I.
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Sweet, John E.....................Straight Line Engine Co., Syracuse, N. Y.
Sweetland, Wm. L......................126 Union Street, New Haven, Conn.

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Thomas, James.............................Davies & Thomas, Catasauqua, Pa.
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Wall, Edward B...........Supt. M. P. P. C. & St. L. R. R., Columbus, Ohio.
Wallis, John Mather.....Supt. Motive Power P. B. & W. and B. & P. R. R., Broad Street Depot, Philadelphia, Pa.
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Ward, W. E....................Russell, Birdsall & Ward, Port Chester, N. Y.
Warner, Worcester R..................Warner & Swasey, Cleveland, Ohio.
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Watts, George W.............................Dickson Mfg. Co., Scranton, Pa.
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Webber, William Oliver....Supt. Lawrence Machine Shop, Lawrence, Mass.
Webster, Ambrose............. Supt. Am. Watch Tool Co., Waltham, Mass.
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Wheeler, Norman W....................Goss Marine Iron Works, Bath, Me.
Wheelock, Jerome..Worcester, Mass.
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WILLSON, FREDERICK N..........Professor Princeton College, Princeton, N. J.
WITHEROW, JAMES P...Pittsburgh, Pa.
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WOODBURY, L. S...Calumet, Mich.
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WRIGHT, JOHN Q........Putnam Machine Co., 115 Liberty St., New York City.

ZANE, ABRAHAM V........Bureau Steam Eng., Navy Dept., Washington, D. C.
ZIMMERMANN, WM. F.....Insp. & Cons. Eng'r, 91 Fourth Ave., Pittsburgh, Pa.

Associates.

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BAILEY, W. H.................................20 Gold Street, New York City.
BROWN, DAVID N.......Asst. Ed. *Sanitary Engineer*, 140 William Street, New York City.
DELANO, THOS. H... Publisher *Electrical Review*, 23 Park Row, New York City.
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EVANS, EDWIN T............L. S. Transit Co., 189 North Street, Buffalo, N. Y.
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HAWKINS, GARDNER C...........................40 Oliver Street, Boston, Mass.
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ROBBINS, E. H...Pittsfield, Mass.
SHARPE, LUCIAN..................Brown & Sharpe Mfg. Co., Providence, R. I.
SPERRY, CHARLES..........................Port Washington, Queen's Co., N. Y.
STOCKLY, GEO. W...........V. P. & Man. Brush Electric Co., Cleveland, Ohio.
WOOD, WALTER......R. D. Wood & Co., 400 Chestnut Street, Philadelphia, Pa.
WORTHINGTON, GEO....Editor *Electrical Review*, 23 Park Row, New York City.

Juniors.

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Day, F. M...Milford, Mass.
Du Villard, Henry A............Corliss Steam Engine Co., Providence, R. I.
Greene, Isaac C........Columbian Iron Works & Dry Dock Co., Baltimore, Md.
Guthrie, Edward B....Buffalo, N. Y.
Higgins, Samuel...Susquehanna, Pa.
Hill, William..................................Collins Co., Collinsville, Conn.
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Smith, Albert W...............Kingsford Iron & Machine Co., Oswego, N. Y.
Smith, John Wm.........................Solvay Process Co., Syracuse, N. Y.
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Trump, Edward N........................Solvay Process Co., Syracuse, N. Y.
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Warrington, Jas. N...........................127 Park Avenue, Chicago, Ill.

Deceased.

Henry R. Worthington	Dec. 17, 1880.
Theodore R. Scowden	Dec. 31, 1881.
Alexander L. Holley	Jan. 29, 1882.
Erastus W. Smith	June 12, 1882.
Peter Cooper, Honorary Member	April 4, 1883.
James Park, Jr.	April 21, 1883.
W. K. Seaman	July 2, 1883.
Redmond J. Brough	July 21, 1883.
Siemens, C. W., Honorary Member	Nov. 20, 1883.
Henry F. Snyder	Nov. 25, 1883.
O. Hallauer, Honorary Member	Dec. 5, 1883.
William Atwood	Feb. 16, 1884.
Wilmer G. Cartwright	Feb. 23, 1884.

RULES

OF THE

AMERICAN SOCIETY OF MECHANICAL ENGINEERS.

OBJECTS.

ART. 1. The objects of the AMERICAN SOCIETY OF MECHANICAL ENGINEERS are to promote the Arts and Sciences connected with Engineering and Mechanical Construction, by means of meetings for social intercourse and the reading and discussion of professional papers, and to circulate, by means of publication among its members, the information thus obtained.

MEMBERSHIP.

ART 2. The Society shall consist of Members, Honorary Members, Associates and Juniors.

ART. 3. Mechanical, Civil, Military, Mining, Metallurgical and Naval Engineers and Architects may be candidates for membership in this Society.

ART. 4. To be eligible as a *Member*, the candidate must have been so connected with some of the above-specified professions as to be considered, in the opinion of the Council, competent to take charge of work in his department, either as a designer or constructor, or else have been connected with the same as a teacher.

ART. 5. *Honorary Members*, not exceeding twenty-five in number, may be elected. They must be persons of acknowledged professional eminence, who have virtually retired from practice.

ART. 6. To be eligible as an *Associate*, the candidate must have such a knowledge of, or connection with, applied science, as qualifies him, in the opinion of the Council, to co-operate with engineers in the advancement of professional knowledge.

ART. 7. To be eligible as a *Junior*, the candidate must have been in the practice of engineering for at least two years, or he must be a graduate of an engineering school.

The term "Junior" applies to the professional experience, and not to the age of the candidate. Juniors may become eligible to Membership.

ART. 8. All Members and Associates shall be equally entitled to the privileges of Membership, provided that Honorary Members, who are not also Members or Associates, and Juniors shall not be entitled to vote nor to be members of the Council.

ELECTION OF MEMBERS.

ART. 9. All candidates for admission to the Society, excepting candidates for honorary membership, must be proposed by at least three members, or members and associates, to whom they must be personally known, and be seconded by two others; the proposal to be accompanied by a statement in writing of the grounds of their application for election, including an account of their professional service.

ART. 10. All such applications and proposals received by the Council, up to thirty days before a regular meeting, shall, at least twenty days before such regular meeting, be passed upon by the Council. The Secretary shall, at least twenty days before such regular meeting, mail to each member and associate, in the form of a letter ballot, the names of candidates recommended by the Council for election.

ART. 11. Any member or associate entitled to vote may erase the name of any candidate and return to the Secretary such ballot inclosed in two envelopes, the inner one to be blank and the outer one to be indorsed by the voter.

ART. 12. The rejection of any candidate for admission as member, associate, or junior, by *seven* voters, shall defeat the election of said candidate. The rejection of any candidate for admission as honorary member by *three* voters shall defeat the election of said candidate.

ART. 13. The said blank envelopes shall be opened by the Council at any meeting thereof, and the names of the candidates elected shall be announced in the first ensuing meeting of the Society, and also in the first ensuing list of members. The names of candidates not elected shall neither be announced nor recorded in the proceedings.

ART. 14. Candidates for admission as honorary members shall not be required to present their claims; those making the nominations shall state the grounds therefor, and shall certify that the nominee will accept if elected. The method of election in other respects shall be the same as in case of other candidates.

ART. 15. All persons elected to the Society, excepting honorary members, must subscribe to the rules and pay to the Treasurer the entrance fee and yearly dues before they can receive certificates of membership. If this is not done within six months of notification of election, the election shall be void.

ART. 16. The proposers of any rejected candidate may, within three months after such rejection, lay before the Council written evidence that an error was then made, and if a reconsideration is granted, another ballot shall be ordered.

ART. 17. The Council shall pass upon applications of juniors for membership, and shall order ballots upon such recommendations, in the manner hereinbefore described.

FEES AND DUES.

ART. 18. The initiation fee of members and associates shall be $15, and their annual dues shall be $10, payable in advance at the annual meeting; *provided*, that the persons elected at the meeting following the annual meeting shall pay $8, and persons elected at the meeting preceding the annual meeting shall pay $4 as dues for the current year. The initiation fee of juniors shall be $10, and their annual dues shall be $5, payable in advance. Any member or associate may become, by the payment of $150 at any one time, a life member or associate, and shall not be liable thereafter to annual dues.

ART. 19. Any member, associate, or junior in arrears may, at the discretion of the Council, be deprived of the receipt of publications, or stricken from the list of members, when in arrears for one year; *provided*, that such person may be restored to membership by the Council on payment of all arrears, or by re-election after an interval of three years.

OFFICERS.

ART. 20. The affairs of the Society shall be managed by a Council, consisting of a President, six Vice-Presidents, nine Managers, and a Treasurer, who shall be elected from among the members and associates of the Society at the annual meetings, to hold office as follows:

ART. 21. The President and the Treasurer for one year; and no person shall be eligible for immediate re-election as President who shall have held that office, subsequent to the adoption of these rules, for two consecutive years; the Vice-Presidents for two years, and the Managers for three years; and no Vice-President or Manager shall be eligible for immediate re-election to the same office at the expiration of the term for which he was elected. *Provided*, that at the meeting for organization the entire board of elective officers and managers shall be chosen, of whom the President, the Treasurer, three Vice-Presidents, and three Managers shall serve until the first Thursday of November, 1881; three Vice-Presidents and three Managers shall serve until the first Thursday of November, 1882, and three Managers shall serve until the first Thursday of November, 1883. The holding for the several terms shall be determined by lot among them.

ART. 22. A Secretary, who may or may not be a member of the Society, shall be appointed for one year by a majority of the members of the Council at its first meeting after the annual election, or as soon thereafter as the votes of a majority of the members of the Council can be secured for a candidate. The Secretary may be removed by a vote of twelve members of the Council, at any time after one month's notice has been given him by a majority of its members to show cause why he should not be removed, and he has been heard to that effect. The Secretary may take part in any of the deliberations of the Council, but, if not a member of the Society, shall not have a vote therein. His salary shall be fixed for the time he is appointed by a majority vote of the Council.

ART. 23. At each annual meeting after the first, a President, three Vice-Presidents, three Managers, and a Treasurer shall be elected, and the term of office shall continue until the adjournment of the meeting at which their successors are elected.

ART. 24. The duties of all officers shall be such as usually pertain to their offices or may be delegated to them by the Council or the Society; and the Council may, in its discretion, require bonds to be given by the Treasurer. At each annual meeting the Council shall make a report of proceedings to the Society, together with a financial statement.

ART. 25. Vacancies in the Council may occur by death or resignation; or the Council may, by vote of a majority of all its members, declare the place of any officer vacant, on his failure for one year, from inability or otherwise, to attend the Council meetings or perform the duties of his office. All vacancies shall be filled by the appointment of the Council, and any person so appointed shall hold office for the remainder of the term for which his predecessor was elected or appointed; *provided*, that the said appointment shall not render him ineligible at the next annual meeting.

Art. 26. Five members of the Council shall constitute a quorum; but the Council may appoint an Executive Committee, or business may be transacted at a regularly called meeting of the Council, at which less than a quorum is present, subject to the approval of a majority of the Council, subsequently given in writing to the Secretary and recorded by him with the minutes.

Art. 27. No bill shall be paid for the Society, until it has been certified by the person authorized to contract it, and audited by the committee on finance.

ELECTION OF OFFICERS.

Art. 28. At the regular meeting preceding the annual meeting, a nominating committee of five members, not officers of the Society, shall be appointed, and this committee shall, at least thirty days before the annual meeting, send the names of nominees to the Secretary, who shall at once mail the said list of names to each member and associate, in the form of a letter ballot.

Art. 29. In the election of Vice-Presidents, each member and associate may cast as many votes as there are Vice-Presidents to be elected. He may give all these votes to one candidate, or distribute them among more, as he chooses. Managers shall be voted for in the same way.

Art. 30. Any member or associate entitled to vote may vote by retaining or changing the names on said list, leaving names not exceeding in number the officers to be elected, and returning the list to the Secretary—such ballot inclosed in two envelopes, the inner one to be blank and the outer one to be indorsed by the voter. *Provided*, that no member or associate in arrears since the last annual meeting shall be allowed to vote until said arrears shall have been paid.

Art. 31. The said blank envelopes shall be opened by tellers at the annual meeting, and the persons who shall have received the greatest number of votes for the several offices shall be declared elected.

MEETINGS.

Art. 32. The annual meeting of the Society shall be held in the City of New York, on the first Thursday in November, at which a report of the proceedings of the Society and an abstract of the accounts shall be furnished by the Council; the Council may change the time of the annual meeting, and shall, in that case, give six months' notice to members and associates.

Art. 33. Two other regular meetings of the Society shall be held in each year, at such times and places as the Council may appoint. At least thirty days' notice of such meetings shall be mailed by the Secretary to members, honorary members, associates and juniors.

Art. 34. Special meetings may be called whenever the Council may see fit; and the Secretary shall call a special meeting at the written request of twenty or more members. The notices for special meetings shall state the business to be transacted, and no other shall be entertained.

Art. 35. Any member, honorary member or associate may introduce a stranger to any meeting; but the latter shall not take part in the proceedings without the consent of the meeting.

Art. 36. Every question which shall come before the Society shall be decided, unless otherwise provided by these rules, by the votes of a majority of the members and associates present, provided there is a quorum.

Art. 37. At any regular meeting of the Society, thirteen or more members and associates shall constitute a quorum.

Art. 38. The Council shall have power to decide on the propriety of communicating to the Society any paper which may be received, or to refer it back to its author for revision or amendment; also, to decide which of the papers read before the Society shall be printed in the *Transactions*. Before such paper appears in the *Transactions* of the Society, a revised proof of the paper and discussion shall be sent by the Secretary tc the author, and, so far as practicable, to every member taking part in the discussion, with request that they call attention to any errors therein. When the Council shall so direct, printed copies of papers shall be distributed to the membership in advance of the meeting at which they are to be presented and discussed.

Art. 39. Intimation, when practicable, shall be given at a general meeting, of the subject of the paper or papers to be read, and of the questions for discussion at the next meeting.

Art. 40. The Society shall claim no exclusive copyright in papers read at its meetings, nor in reports of discussions, except in the matter of official publication with the Society's imprint, as its *Transactions*. The Secretary shall have sole possession of papers between the time of their acceptance by the Council and their reading, together with the drawings illustrating the same; and at the time of such reading, or as soon thereafter as practicable, he shall have printed copies of such papers "subject to revision," with such illustrations as are needed for the *Transactions*, for distribution to the members and for the use of technical newspapers, American and foreign, which may desire to reprint them in whole or in part. The policy of the Society in this matter shall be to give papers read before it the widest currency possible, with a view to making the work of the Society known, encouraging mechanical progress and extending the professional reputation of its members.

ART. 41. The author of each paper read before the Society shall be entitled to twelve copies, if printed, for his own use, and shall have the right to order any number of copies at the cost of paper and printing; *provided*, that said copies are not intended for sale.

ART. 42. The Society is not, as a body, responsible for the statements of fact or opinion, advanced in papers or discussions, at its meetings; and it is understood that papers and discussions should not include matters relating to politics or purely to trade.

AMENDMENTS.

ART. 43. These rules may be amended, at any annual meeting, by a two-thirds vote of the members present; *provided*, that written notice of the proposed amendment shall have been given at a previous meeting.

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PAPERS

OF THE

NEW YORK MEETING, 1883.

CXXIX.

PROCEEDINGS

OF THE

AMERICAN SOCIETY OF MECHANICAL ENGINEERS,

NEW YORK, OCTOBER 31st, 1883.

Standing Committee on Regular Meetings.—Washington Jones, Chas. E. Emery, Henry Morton, Coleman Sellers, Matthias N. Forney, J. F. Holloway, Wm. Lee Church.

Local Committee for New York Meeting.—Prof. Henry Morton, *Chairman;* Jas. C. Bayles, Chas. E. Emery, Matthias N. Forney, H. S. Hayward, F. R. Hutton, Chas. A. Moore, R. H. Soule, Henry R. Towne, F. M. Wheeler, W. H. Wiley, W. P. Trowbridge.

Executive Committee.—James C. Bayles, Chas. E. Emery, F. R. Hutton, Wm. H. Wiley.

THE opening session was held at the rooms of the American Society of Civil Engineers, at 127 E. 23d Street, New York, which had been generously tendered by their Board of Direction.

The President, Mr. E. D. Leavitt, Jr., after calling the meeting to order at 8.20 P.M., said:

GENTLEMEN: In the schedule which has been handed to me by the Secretary it is stated that the meeting will be called to order by Professor Morton, Chairman of the Local Committee. Unfortunately, Professor Morton has not appeared, and the President is obliged to call the meeting to order himself. The second item on the programme is "Opening Words," by the President. You will all bear me witness that my words have not been very many, and I do not propose to go back on my record in that regard. I will therefore proceed to the third article, which is the appointment of tellers to count the ballots for officers elected at this meeting, and I will appoint Messrs. Emery, Trowbridge and Porter for that service. They will report at the session to-morrow morning. The next matter is a paper by Mr. Pickering.

Mr. Pickering.—I rather carelessly committed myself, to our Secretary, to the undertaking of writing a little paper on American Machinery at International Exhibitions, to occupy in its reading some fifteen or twenty minutes. When I commenced the task I found it was greater than I had anticipated, and it is a pity almost to attempt to deal with the subject in such a small way as I have. This paper is more in the form of a few rambling notes that have occurred to me, which I have condensed into a compass suitable to the time that I supposed would be available.

Mr. Pickering then read his paper, and in the discussion which followed Messrs. Holloway, Strong, Emery, Grimshaw and Hobbs took part.

At ten o'clock the Society took a recess till ten the next day, and a conversazione and supper completed the evening.

SECOND DAY.

November 1st.

At ten A.M., the Society convened at the rooms of the Civil Engineers, at 127 East 23d Street, President Leavitt in the chair. The Secretary's register included the following names of members in attendance:

Alden, Geo. J.	Worcester, Mass.
Almond, T. R.	Brooklyn, N. Y.
Angstrom, Carl	Worcester, Mass.
Bailey, Jackson	New York City.
Bailey, E. B.	Windsor Locks, Conn.
Baker, W. S. G.	Baltimore, Md.
Baldwin, W. J.	New York City.
Baldwin, S. W.	" "
Barr, Wm. M.	Cleveland, O.
Bayles, J. C.	New York City.
Beggs, J.	" "
Bergner, T.	Philadelphia, Pa.
Betts, William	Wilmington, Del.
Billings, C. E.	Hartford, Conn.
Bond, Geo. M.	" "

Burnett, J. H	New York City.
Burdsall, E., Jr	Portchester, N. Y.
Caldwell, A. J	Brooklyn, N. Y.
Capen, T. W	Stamford, Conn.
Cartwright, W. G	Hoboken, N. J.
Christensen, A. C	Brooklyn, N. Y.
Coes, Z. B	Hyde Park, Mass.
Colwell, A. W	New York City.
Cotter, John	Norwalk, Conn.
Couch, Alfred B	Philadelphia, Pa.
Davis, D. P	New York City.
Davis, E. F. C	Pottsville, Pa.
Dean, F. W	Cambridge, Mass.
Deane, C. P	Springfield, Mass.
Denton, J. E	Hoboken, N. J.
Drummond, W. W	Louisville, Ky.
Durfee, W. F	Bridgeport, Conn.
Du Faur, A. F	New York City.
De Schweinitz, P. B	So. Pueblo, Col.
Du Villard, H. A	Providence, R. I.
Edson, J. B	North Adams, Mass.
Egleston, T	New York City.
Emery, C. E	" "
Fay, R. C	Milford, Mass.
Fritz, John	Bethlehem, Pa.
Forney, M. N	New York City.
Galloupe, F. E	Boston, Mass.
Gardner, E. L. B	Passaic, N. J.
Good, W. E	Reading, Pa.
Goubert, A. A	New York City.
Grant, J. J	Flushing, N. Y.
Grimshaw, R	New York City.
Green, H	Jeanesville, Pa.
Hall, A. F	Boston, Mass.
Halsey, F. A	New York City.
Hand, S. A	Toughkenamon, Pa.
Hayward, H. S	Jersey City, N. J.
Hemenway, F. F	New York City.
Hill, H. A	Boston, Mass.
Higgins, M. P	Worcester, Mass.
Hoadley, J. C	Boston, Mass.
Hobbs, A. C	Bridgeport, Conn.
Hollerith, H	Washington, D. C.
Holloway, J. F	Cleveland, O.
Hornig, J. L	Jersey City, N. J.
Hunt, R. W	Troy, N. Y.
Hutton, F. R	New York City.
Illingworth, J. J	Utica, N. Y.
Johnson, Wm	Lambertville, N. J.
Jones, H. C	Wilmington, Del.
Jones, Washington	Philadelphis, Pa.

Kent, Wm.	New York City.
Kirchhoff, C.	" "
Laureau, L. G	" "
Leavitt, E. D., Jr.	Cambridgeport, Mass.
Le Van, W. B.	Philadelphia, Pa.
Lipe, C. E.	Syracuse, N. Y.
Loring, C. H.	Brooklyn, N. Y.
McElroy, S.	New York City.
May, D. C.	Baltimore, Md.
Maynard, G. W	New York City.
Melvin, D. N.	Staten Island, N. Y.
Miller, Alex.	New York City.
Miller, H. B.	" "
Morgan, T. R., Sr.	Alliance, O.
Moore, Lycurgus B.	New York City.
Morris, H. G.	Philadelphia, Pa.
Morton, Henry	Hoboken, N. J.
Neftel, K.	New York City.
Odell, W. H.	Yonkers, N. Y.
Parks, E. H.	Providence, R. I.
Partridge, W. E.	New York City.
Pickering, T. R.	Portland, Conn.
Porter, H. F. G.	Trenton, N. J.
Porter, Charles T.	New York City.
Pratt, F. A.	Hartford, Conn.
Pusey, C. W.	Wilmington, Del.
Rae, T. W.	New York City.
Richards, C. B.	Philadelphia, Pa.
Root, J. B.	Greenpoint, N. Y.
Root, Wm. J.	Brooklyn, N. Y.
Rose, J.	New York City.
Rowland, T. F.	Greenpoint, N. Y.
Scheffler, F. A.	New York City.
See, Horace	Philadelphia, Pa.
Sellers, Coleman	" "
Shock, Wm. H.	Washington, D. C.
Sinclair, Angus	New York City.
Smith, Geo. H.	Providence, R. I.
Smith, Oberlin	Bridgeton, N. J.
Stearns, A.	Brooklyn, N. Y.
Stratton, E. P.	New York City.
Strong, Geo. S.	Philadelphia, Pa.
Suter, G. A.	New York City.
Sweet, John E.	Syracuse, N. Y.
Thompson, C. T.	Philadelphia, Pa.
Thurston, R. H.	Hoboken, N. J.
Towne, H. R.	Stamford, Conn.
Trowbridge, W. P.	New York City.
Vanderbilt, A.	" "
Ward, W. E.	Portchester, N. Y.
Webb, J. B.	Ithaca, N. Y.

Webber, S.	Lawrence, Mass.
Webber, S. S.	" "
Webster, H., Jr.	Brooklyn, N. Y.
Weeks, G. W.	Clinton, Mass.
Weightman, W. H.	New York City.
Wellman, S. T.	Cleveland, O.
Wheeler, F. M.	New York City.
White, M.	Bethlehem, Pa.
Whiting, L. B.	Pottsville, Pa.
Wiley, Wm. H.	New York City.
Wolff, A. R.	" "
Wood, Walter	Philadelphia, Pa.
Woodbury, C. J. H.	Boston, Mass.
Worthington, C. C.	New York City.

As guests of the Society were also present Messrs. Weston, Cartwright, Wetzler, Alexander, Paine, Croes, Randolph, Bogart, and others, together with Messrs. Platt, of Gloucester, and Fox, of Leeds, England, and Dr. Wolff, of Berlin.

The reading of the minutes of the Cleveland meeting was, on motion, dispensed with.

A report from the Council was presented as follows:

Since the report presented at Cleveland, the Council has held two sessions, August 8th and September 27th, which have been largely attended.

Beside the routine business and scrutiny of applications for membership, the Council has discussed the question of finance, and, in payment of past indebtedness, has ordered the sale of the $1,900 bond held as invested fund by the Society.

The Standing Committees have regularly reported, and their action has been approved.

A series of pamphlets relating to boiler and engine tests by Prof. Reuleaux, of Berlin, has been presented by him to the growing library of the Society, and an abstract from it will be duly offered as a paper to the Society.

The Treasurer's Annual Report was presented to the Society and read by the Secretary, as follows:

NEW YORK, November 1, 1883.

To the American Society of Mechanical Engineers:

GENTLEMEN: I have the honor to submit the third Annual Report of the finances of the Society, covering the period from November1, 1882, to this date.

The receipts and payments have been as follows, viz.:

RECEIPTS.

Balance on hand, November 1, 1882	$7 58
Initiation fees	1,235 00
Annual dues	3,405 12
Badges	197 06
Binding Transactions of the Society	132 85
Paper sales, Engraving, etc.	143 07
Sale U. S. Govt. Bond, face value $1,000, cost	1,138 75
Premium on do	71 25
Interest on do	40 00
Report of Cleveland Meeting	25 00
American Society Civil Engineers	20 00
" Institute Mining Engineers	20 00
Total	$6,435 68

PAYMENTS.

Engraving	$381 94
Traveling	63 25
General expenses	996 88
Salary	1,825 00
Printing and Stationery	2,704 11
Postage	161 32
Total	$6,132 50

—thus leaving a balance, in bank and cash, on hand of three hundred and three and $\frac{18}{100}$ dollars ($303.18.)

The U. S. Govt. Bond mentioned in my last Annual report of the face value of one thousand dollars, ($1,000), and held as an invested fund, was sold by direction of the Council to procure necessary funds to liquidate the accumulated indebtedness of the Society contracted previous to, at, and subsequent to the June meeting at Cleveland, Ohio. In so doing I have disposed of the last of the Government Bonds placed in my hands when first elected to the office of Treasurer of the Society.

There are no bills of any kind against the Society, in my hands, awaiting payment at this date. The only standing indebtedness of the Society at this time, so far as known to me, is for one month's salary to the Secretary and two months' clerk hire for the Treasurer, amounting in all to one hundred and seventy-five dollars ($175.00), and due this day.

There is due from the membership of the Society for initiation fees and annual dues, which were due on or previous to November

1st, 1882, the sum of five hundred and nine dollars ($509.00) divided as follows, viz.:

Initiation fees	$60 00
Annual dues	449 00
Total	$509 00

I would call your attention to the increased amount of indebtedness of the membership for these items over that given in my last annual report, where I stated: "It seems not to be well understood by some members of the Society that the annual dues are payable at the commencement of the Society's year, namely in November commencing the year. . . . If this was clearly understood by all the Membership, the dues would undoubtedly be more promptly paid." Article 18 of the Rules of the Society plainly sets forth at what time the annual dues are payable. A detailed statement of the amounts due by the delinquent membership of the Society has been submitted to the Finance Committee by their request.

It will be noticed that the payments for printing and stationery are in excess of the same item in last year's annual report by an amount of over eight hundred dollars ($800.00), but by referring to the latter report it will be seen that there were at that time two bills for printing unpaid, amounting to one thousand and forty-five dollars ($1,045.00), to be paid when the funds were available for their payment. This amount was paid from the funds of the Society during the present year, and in the estimate of the expenses of the current year, should properly be deducted from the amount of twenty-seven hundred and four and $\frac{11}{100}$ dollars ($2,704.11).

Respectfully submitted.

CHARLES W. COPELAND, *Treasurer.*

After the reading of the report:

Prof. Egleston.—I move that the report be referred to the Auditing Committee, and that so much of the report as requires other action be referred to the Council.

Agreed to.

The President.—The next report due is from the Committee on Tests.

The report of this committee was presented by Professor Egleston, as follows:

The Committee on Tests of the American Society of Mechanical Engineers beg respectfully to report that since the last meeting of the Society, they have been actively engaged in corresponding with persons in all parts of the United States, and that they have received a large number of enthusiastic letters which lead them to believe that if they can have the active co-operation of all the members of the Society, there is every hope of success in the next session of Congress. The demands for information have been so numerous that the copies of the transactions of the Philadelphia meeting have been exhausted.

The committee deem it important that these transactions shall be reprinted, and they beg respectfully to offer the opinion that no money which the Society could spend would be more profitably employed in the interests of mechanical engineering than in reprinting and circulating among members of Congress this pamphlet, if it should directly or indirectly influence the passage of the bill; and they therefore request that a sum sufficient to defray the expense of reprinting such parts of the discussion at Philadelphia as seem best adapted to secure the passage of the Test Commission bill, be appropriated.

Prof. Egleston.—As chairman of the committee, I wrote to the Council, requesting them to reprint the proceedings on tests at Philadelphia, and the Council, as I think, very properly declined to make an appropriation on account of there being no funds in the treasury. They have no right to incur debt without a formal vote of the Society. The committee, therefore, have come before the Society to bring the matter to its attention. It is the opinion of the committee that the Society should appropriate the funds, and that some means should be taken to secure them. The Society is at the present time in an unfortunate financial condition. I cannot see that there is any reason for it, and I believe that if the fact was known to the members, and proper means were taken, the Society could have sufficient funds to do all that it does now, and a great deal more besides. I have no hesitancy whatever—although as a matter of finance it is not apparently a good measure—to ask the Society to appropriate the funds, and I believe that every cent so appropriated will bring in three or four hundred per cent. interest.

Mr. Towne.—I would inquire if Prof. Egleston asks that an appropriation be made now?

Prof. Egleston.—Yes, sir; the committee desire to ask for an appropriation at the present time.

Mr. Towne.—And that it be made by the Society at this meeting, and not by the Council?

Prof. Egleston.—By the Society. The Council can only appropriate funds they have in hand. It will cost, as nearly as I can remember, about three dollars a page to set the proceedings up; there are about fifty pages, I think. There would have to be copies for members of Congress, and for the use of the members of the Society.

I wish to state that the bill in the last session of Congress did not fail on account of its merits. It was not introduced before February, and it was impossible to have it brought up in regular order. I was in Washington very often, and in personal contact with the members of the committee in charge of the bill, and the members of Congress, and they all assured me over and over again that if the bill could come up it would be passed without opposition.

The cost of printing the proceedings of the Philadelphia meeting would, I think, be from $200 to $250.

Mr. Towne.—An appropriation by the Society now would be a pledge of the coming year's income to that extent.

Prof. Egleston.—I have had a great deal of experience in other societies, and I think the appropriation a justifiable pledge of the income of the Society. I stated to the President just before we came into session that several societies who were bankrupt have been revived and now are some of the most prosperous in the country, and I think the same thing could be done for us.

Mr. Towne.—The best thing, therefore, would be to become bankrupt, and then we would prosper. I would ask, Mr. President, whether there will not be a discussion later on in the meeting as to the financial outlook for the coming year, which perhaps had better take precedence of any action on this motion of Prof. Egleston? Should not we hear from the Council as to the financial probabilities for the coming year, as to whether there will be any funds available for this work?

The President.—So far as the President is advised, there will not be. I may state that in the Council meeting we felt the necessity for retrenchment. We reluctantly acceded to the sale of the bond, not because we thought it was a good thing for the Society to have much of an invested fund, but simply for the reason that a bad impression would go forth that we were going behind. I think that we shall have to sail very close to the wind for perhaps two or three years to get on a good basis, and if we incur this expenditure we shall have to trust to luck.

Mr. Towne.—On the other hand, it is most directly in the line of the Society's work.

The President.—I should like to state, however, that there has been a joint committee of the three societies on this subject of an appropriation. I happened to be a member on the part of the Mining Engineers, and was at the Washington meeting of the Mining Engineers a year ago last February in company with Prof. Egleston and Mr. Ashbel Welch, and I would ask the Society to listen to Mr. Bogart, the Secretary of the Society of Civil Engineers, as to the progress then made.

Prof. Egleston.—I move that Mr. Bogart be asked to inform us of the action of the Society of Civil Engineers in the matter.

Agreed to.

Mr. Bogart.—The action of the Society of Civil Engineers, after the Washington meeting of the Mining Engineers, when there was a meeting of a joint committee, was substantially this. The committee of the Society of Civil Engineers, consisting of the President, Mr. Ashbel Welch, and several other members, including myself, found that, on account of the lateness of the introduction of the bill, as Dr. Egleston has said, there was a strong probability that it would not pass at that session of Congress. A number of us, on the invitation of the Chief of Ordnance, called at the Ordnance Office in Washington, and had a very interesting conversation with the Chief of Ordnance upon the general subject of the tests of materials. I think in that conversation a good deal of misapprehension which had existed previously was cleared away. Gen. Benét, the Chief of Ordnance, expressed himself as anxious to cooperate with the engineers of the country in securing the best results from the Watertown testing machine, and from the work of the officers of the Ordnance Department in every respect. He said, substantially, in regard to the bill which was before Congress, under the auspices of the joint societies, that on account of the lateness of the season it probably would not pass—it probably could not be got through the Houses. He also said that if the Ordnance Department could do anything to promote the progress of securing desirable information on the subject, he would aid in the work. He himself wrote a clause, which was introduced into the bill, making appropriations for the Ordnance Department which empowered him to consult with the engineers of the country and to take their suggestions with regard to tests. He also asked for a larger appropriation, the amount originally named in the bill being only enough for the mere maintenance and current work of the Ordnance Department with the Watertown machine. It was so late in the session that the additional appropriation was not secured. The feeling of the gentlemen who were there—I think there were members of the three societies at that interview—was, that if we could not pass the bill creating a commission, we should certainly not antagonize the very kindly suggestions of the Chief of Ordnance, but that we should try to act in co-operation with him.

Since that time the course of the American Society of Civil Engineers, through its Board of Direction, has been in co-operation with the Chief of Ordnance, and no action has been taken since that time with regard to the bill to create a commission. The Society is probably favorable to such a bill, but they have not appropriated any money to promote any further action, and I think that the hope of many members of the Society of Civil Engineers who have taken a special interest in this matter is that it is possible, by the co-operation of the Chief of Ordnance, and perhaps through the work of the officers of the army in connection with engineers, that the work of testing can be pushed successfully forward—at all events up to the time when a special bill may be finally passed. That is now about the position of many members of the Society of Civil Engineers upon the subject, as I am advised. We shall have the annual meeting of the Society here in January, and what action may be taken then, I, of course, do not know; but at the present time I do not think that the Board of Direction of the American Society of Civil Engineers feels authorized officially to take any further action. I think that about defines the position.

Prof. Egleston.—I worked constantly and very earnestly with the old commission, and since the constitution of a new one has been discussed, I have worked at it with the same zeal. I look upon this as the most important measure ever brought up in the United States. There is a great deal of feeling which, I think, is entirely unnecessary and which rests wholly on misapprehension. I have had a number of interviews with officers of the Engineer and Ordnance corps and they have expressed themselves sometimes rather severely, having the idea that the engineers of the country were somehow or other, opposing the War Department, and that there was a want of harmony among them. What is necessary in a matter of this kind for the Society is prompt action. If we wait until January, until the meeting of the Society of Civil Engineers, we shall undoubtedly have their active co-operation; but if we wait until then it will be too late. The reason the bill did not pass before was that it was introduced so late in the session that it was impossible to have it pass. What your committee hope to do is to have the bill introduced on the first day of the session, or as near the first day as practicable, certainly within the first week. If that is done and the bill is allowed to rest on its merits, we have no doubt it will pass. There is more prospect of the bill passing than there ever was before. I do not wish to do anything or to commit the Society to doing anything that would seem like a want of concert between the engineers of the country; but I am very certain as an engineer and as a member of the Society that the action required is *prompt* action. Several persons have written anxiously to know whether they might not introduce the bill. I think, therefore, there will be no difficulty in getting the bill passed if we are intrusted further with this matter.

Mr. Oberlin Smith.—I move that the Society appropriate this amount—a sum not exceeding $200. It seems to me very important, as we have commenced this thing, that we should carry it through, taking into consideration the great importance of the work itself. If the other societies are not yet ready to co-operate, we must not lose the time. The amount of money is a small matter; although I do not believe in running the Society in debt, yet we have some funds in the treasury and it is the only work, outside the publication of our transactions, that we are doing. We certainly could not do a more important work for the benefit of the country than this.

The President.—The motion is that $200 be expended for the republication of the report of the Committee on Tests in the Philadelphia transactions.

Agreed to.

Prof. Egleston.—I should like to make another suggestion, as the action of the Society need not be hurried in this matter, and that is, that the Committee on Ways and Means of the Society—if there is such a committee—be requested to call before them persons familiar with the organization and reorganization of other societies with a view to eliciting opinions as to how it is best to conduct the Society so as to have a more active Society and a larger income. I do not put this in the form of a motion, but if it meets the views of the other members of the Society it seems to me it would bring about the desired result. If it comes simply as a suggestion to the

Committee on Ways and Means, perhaps that of itself would be sufficient without the formal action of the Society; I have no doubt that the means could be increased.

Mr. Towne.—I understand that we are to have a report from the Committee on the Revision of Rules during the morning, and perhaps that would cover the point Professor Egleston raises and even the discussion following it would bring it out.

Prof. Egleston.—I did not know that.

The President.—The report of the Publication Committee is next in order.

The Secretary.—I have a telegram from the Chairman saying that he cannot be present. In his absence I will say that the Publication Committee would report that, had it not been for the strike of the printers who print the transactions, Volume IV., which includes the transactions of New York in 1882 and Cleveland in 1883, would have been out during October. I hope to get the volumes during the month of November and send them to the members. Postal cards will be sent out asking in what form members desire to have the transactions sent to them. If any members want all their volumes bound hereafter, they will be kind enough to let me know, and the office will bind them all without asking this question at every issue.

The President.—The report of the Committee on Gauges and Standards would be the next thing in order. Since it has no report to make, we will hear the report of the Committee on the Holley Memorial.

Mr. Bayles.—I will state that the treasurer of the fund advises me that the amount necessary to erect the kind of monument contemplated by the joint committees of the three societies has been practically raised. Perhaps a little more money will be needed, but there will be no trouble about getting it. Work is in progress upon the design for the base, Mr. Laureau having kindly consented to submit suggestions and drawings. There is, however, no haste in the matter, as we must wait until at least five years after Holley's death to know certainly that we can obtain permission to erect the monument in Central Park. There is in connection with this matter another which I wish to bring before the Society, with a view to receiving instructions. It relates to the expected co-operation of the Society of Mechanical Engineers with the Institute of Mining Engineers, and possibly the Society of Civil Engineers, in the publication of a memorial volume, which was talked of at the time of our memorial session. Concerning this I have a letter in my hand from Dr. Raymond, who undertook the preparation of that volume, in which he acknowledges the receipt of a letter from the Secretary of this Society, and asks for definite information as to what the Society of Mechanical Engineers proposes to do in the matter. He says in conclusion that "it will not be a very expensive undertaking, and the Institute of Mining Engineers will be quite able to do it alone; but the general understanding among us has been that the three societies would like to take part and receive copies for distribution to their members." This letter has been in my hands since August 21; but of course I was unable to reply to it in the absence of instructions from the Society. The matter was certainly talked of; in a certain vague way it was understood by Dr. Raymond that the three societies would co-operate in the publication of that volume, and his work has been done in the expectation of such co-operation. As he says, however, if the other societies do not co-operate, it is probable that the Institute of Mining Engineers will print the book, and I would ask that the committee be instructed as to what reply should be made to Dr. Raymond in regard to this publication. I think possibly the best shape in which that could come would be a motion from some member that the Council be authorized to instruct the committee.

Prof. Egleston.—I make that motion.

Agreed to.

The President.—The report of the Committee on Revision of the Rules is next in order.

Prof. Sweet.—Professor Trowbridge left the report in my hands; but he understood that it was to be given to the Council, and not to the meeting.

The President.—It would seem that the Society ought to be advised of such a matter; but it can perhaps be done later, after the Council have seen it.

Prof. Sweet.—I think it would be better to let it lie over until Professor Trowbridge comes himself.

The President.—The introduction of new resolutions comes next.

Mr. Bayles.—I have a letter which I bring to the notice of the Society. The facts are briefly these: During the past summer two gentlemen, well known to many of our members, were in England and had conversations with a good many members of the British Iron and Steel Institute, and one of them, Mr. Carnegie, had a formal interview with the Council of the Institute. The general opinion expressed by those gentlemen was that it would be greatly to their pleasure to come to this country in 1884 and here hold a meeting, if the proper recognition could be received, and if it would be made pleasant for them. The method they proposed to adopt in bringing this about was to attend, as members, the meeting of the British Association in Montreal and then come over the border and hold their fall meeting here, if it could be so arranged. This matter has been somewhat canvassed before. The Institute of Mining Engineers once took formal action, and feel rather sore at the way it was received. It is not probable, therefore, that from that Institute, or from any organized society in this country, will a formal invitation to the British Iron and Steel Institute to hold a meeting here—an invitation involving responsibility for their entertainment—be extended. But that matter is being approached in a different way, and it has been deemed expedient to communicate with them to the effect that if it is their pleasure to come to Montreal and attend the meetings of the British Association, it will be our pleasure to have them come across the border and we will try to make their visit pleasant. Such a letter to the Secretary of the British Iron and Steel Institute has been prepared, is being circulated, and has received a considerable number of signatures of iron masters. I will, therefore, offer this resolution:

Resolved, That the Council be requested to appoint a committee of three members to co-operate with committees representing the Institute of Mining Engineers and the Society of Civil Engineers, to consider and report what action should be taken in the event of an expression by the British Iron and Steel Institute of a desire to hold a meeting in this country in 1884.

The resolution was adopted.

Mr. Towne.—I had understood that the Committee on Revision of Rules would make a report, and I looked with interest to see what the nature of their recommendations might be. As it seems that we are not to have that report before us, I would like to ask a few questions of the officers of the Society and suggest to the meeting the expediency of considering the question of ways and means. It seems to me that if we are not bankrupt we are at least without funds for carrying on the work, and without much prospect of having the funds needed for the work of the coming year. And the question comes to me in this shape: Is it not expedient that our annual dues should be increased? At present they are ten dollars a year. An increase of two dollars beyond that would be twenty per cent. and would give us eight or nine hundred dollars more to expend than we now have. It is a question that perhaps some of the officers and older members of the Society can answer better as to whether that increase in the rate would be onerous upon any of the members or upon any considerable number. If not, I consider it would be a wise thing to make a change of that kind. Before offering a definite resolution to that effect, I hope that other members of the meeting will express their views.

Mr. Porter.—I am impressed with the conviction that the Society is not in a desperate condition. On the contrary, I believe that it is in an exceedingly good condition; that its future financial ability rests upon that steady and substantial growth of membership and an increase of interest which is now being witnessed. Our membership is increasing. The interest of our meetings is increasing. The character of our papers is improving. The finances of the Society are being husbanded; and I am satisfied that we have only to go on in this steady, healthy, conservative manner, and that the end will be satisfactory to all the members, and that no action of any kind whatever looking toward any change of a radical nature is desirable.

Mr. Samuel Webber.—I am decidedly opposed to any proposition to increase the fees of membership. I think it is more easy by individual effort to double the membership, which ought to be done. (Applause). I have two or three candidates myself to present and I have asked the Secretary for blank applications. I think the annual fees are ample.

There is one thing more which Professor Egleston referred to. I must confess, as one of the original members, that I have been somewhat disappointed in some of the precedents of the Society. The papers presented to the Society have not been of a sufficiently *practical* character to interest members. (Applause). It is to that that the lack of interest in the Society is to be attributed. If we could have practical common sense, matter-of-fact opinions that mechanics can understand and be interested in, the interest in the Society would increase and its finances would be placed on a satisfactory basis.

Mr. Weightman.—In regard to that motion of last year to amend the rules, I happen to be the one that made it. Last year the motion was made on account of two conflicting amendments. The motion was made in the Society that a committee should be appointed by the Chair to report and attend to the revision of conflicting parts of the by-laws. That committee was appointed under the auspices of the Society, and not under the auspices of the Council; so I can hardly see why that report should not come direct to the Society at once rather than go to the Council. And then also in regard to this additional two dollars: that of course would have to come up as an amendment and would be out of order at the present time. An amendment to the by-laws calls for a motion given at one meeting and action at the next annual meeting. The revision of the by-laws referred to in my motion was an arrangement of those by-laws so that no one part should conflict with the others, and that report was to be made to the Society itself, as I understood it.

Prof. Sweet.—It is not too late to have that done. The committee could report to the Council meeting and then it can be brought up before the afternoon meeting. If it can be sent out in the form of a ballot, probably it would be more satisfactory all around; then the members can vote for such changes as they wish to.

Mr. Weightman.—Then, Mr. President, all that committee has to do is to give notice that they propose a general amendment to the by-laws; then they can pass that around and it can be decided at the next annual meeting.

Prof. Sweet.—Prof. Trowbridge wished to put it before the Council first and have some resolution to authorize the committee to do this, so that we would not have to wait a year as we have to now. As the rules are now a change could not be made this year.

Mr. Weightman.—Even that, Mr. Chairman, it strikes me it has to come before the Society. The Society is the only body that dispenses with its rules temporarily.

Prof. Sweet.—Can it not be done at the afternoon session as well as this forenoon session?

Mr. Weightman.—Certainly.

Mr. Holloway.—As a retiring member of the Council, whose sessions I have very rarely attended, I would like to say that the Council want to get at the sense of the members in regard to a great many of these points and that is brought out by these very discussions. I think it would aid them very much, in a great many others that come before them, if they had the expressions of the members themselves on those various points. It is perhaps a mistake to refer so many things to the Council, but it would seem to me that in many of these points a simple word or so from the various members would give the Council a clue as to the sense of the Society.

With regard to the raising of the annual dues, I would say that I think it would be improper to do that; and in order that we might understand something as to the progress of the Society and something as to the probability of its future and revenue—I think very few of us know how very rapidly the Society has increased in numbers—I would request that the Secretary give us very briefly something as to its progress, so that we may have some idea of what the future is going to be.

Mr. Oberlin Smith.—I move that the Secretary give us a brief resumé of the *prospective* finances of the Society.

The Secretary.—The growth of the Society this year since November, 1882, has been 97; and I have good reason to infer that during the coming year the growth will be more than in the year that is past. The present membership is 454, including those that are coming in now and with one or two exceptions of members who are in arrears, and whose names will probably be withdrawn from the list. Perhaps the actual year during which the canvass for new members has been made, is the year from March, 1883, rather than from November, 1882, to November, 1883; so that I have every reason to think that the growth next year will be even more rapid than this. Of course that means an increase in the income this year of $1,500 from initiations (one hundred members) and of a thousand dollars from dues. This is an increase in our income this year of $2,500 over what it was in 1882. There will be no increased expenditure beyond the comparatively small expense resulting from the increase in bulk of the transactions.

Mr. Oberlin Smith.—Will there be enough, if we have that increase of membership, to cover all expenses?

The Secretary.—So far as the estimates of the Finance Committee throw any light upon it, there ought to be a surplus of $500, as a minimum. Of course that is with the supposition that all the members will pay up with the exception of an estimated fifteen per cent.

Mr. Towne.—I am very happy indeed, Mr. President, to hear so favorable a prognostication, and in view of that it seems to me that no immediate action in this matter is required. But we cannot count on permanently and indefinitely increasing our membership and depend on the initiation fees of new members to meet current expenses. The time will come when that will be a small portion of the receipts. I am happy that the discussion has brought out so satisfactory a statement of the prospects for next year.

The President.—In regard to the functions of the Council, the Council have felt most decidedly that they are the servants of the Society. They have been very guarded in their action in matters which they thought properly belonged to the Society; and as to the matter of the revision of the rules, that was a heritage which was left to the President from the preceding Council, and he acted as promptly as was judicious in appointing a committee. We feel that the Society should understand thoroughly what is going on, and that they should have the privilege, as they have the right, of acting entirely for their own interest, and I have no reason to believe that any persons whom you may elect to office will feel otherwise.

I will call on Mr. Porter for the report of the tellers.

REPORT OF TELLERS APPOINTED TO COUNT THE BALLOTS FOR OFFICERS.

The tellers appointed to count the ballots for officers respectfully report that the result of the ballot for officers of the Society to fill the places falling vacant under the rules is as follows:

For President—There were returned 199 ballots, of which Professor John E. Sweet received 197, and Messrs. Leavitt and Trowbridge each one.

For Vice-Presidents—A. B. Couch received 196, W. R. Eckart 197, and J. V. Merrick 198, and there were three scattering.

For Managers—W. F. Durfee received 194, Oberlin Smith 187, C. C. Worthington 198, T. W. Rae 9, and 8 were scattering.

For Treasurer—Charles W. Copeland received 198 votes, and T. W. Rae one vote.

Respectfully submitted,

CHAS. E. EMERY,
W. P. TROWBRIDGE, } *Tellers.*
CHAS. T. PORTER,

Mr. Wolff.—It seems to me that if the Council are so desirous of carrying out the intentions of the members, as I think they are, they have unwittingly erred in regard to counting the ballots. The rule says the blank envelopes shall be opened by tellers at a regular meeting and the names of candidates elected shall be then announced. That has been heretofore the custom. The tellers have been appointed at this meeting and the ballots were counted here and the vote taken here and the announcement made. Now, it seems this time it has been done previously to the meeting. It is possible that the President has ordered this without asking the members about it; but certainly nothing has transpired in the meetings to give the idea that tellers were appointed.

The Secretary.—Tellers were appointed last evening.

Mr. Wolff.—The reason I made this remark was that I understood that the ballots had been opened at a previous date to this meeting, and I think that that was in violation of the by-laws, though of course I do not think that any such violation was intended.

The President.—The Chair appointed members of the Council as tellers to count the ballots for new members. These tellers supposed that under the resolution introduced by Mr. Holley they were authorized to count the ballots for officers also, which they proceeded to do, and I did not discover the mistake until it was too late to correct it. The Chair having the undoubted right to appoint tellers to count the ballots for officers, appointed the same tellers and they reported to the Society; it was only a matter of misapprehension.

Mr. Wolff.—I would not have you believe that I looked upon it in any other way. It was simply that I wished to call attention to the fact that it was a deviation from the by-laws.

The Secretary then read the

REPORT OF THE TELLERS

APPOINTED TO COUNT THE BALLOTS FOR MEMBERS.

The Tellers appointed by the Council under Rule XIII., as amended at Hartford, 1881, would report as follows:

They have scrutinized and counted the ballots for members. Two hundred and fifteen (215) votes were received. There were not enough negative votes to defeat any of the candidate proposed, and the following gentlemen have been elected to the Society in their respective grades:

MEMBERS.

BALL, FRANK H., Ball Engine Co., Erie, Pa.

BARRY, AUGUSTUS B., Copp Bros. & Barry, Hamilton, Ont., Canada.

BEARDSLEY, ARTHUR, Prof. of Mechanics and Engineering, Swarthmore College, Swarthmore, Del. Co., Pa.

BEGGS, JAMES, 9 Dey Street, New York City.

BLODGETT, GEORGE W., Electrician, Engineer's Office, Boston & Albany, R.R., Boston, Mass.

BUTTERFIELD, FREDERIC E., Designer and Draughtsman, Deane Steam Pump Co., Holyoke, Mass.

CAMPBELL, GEORGE W., Engineer, U. S. Illuminating Co., New York City.

CARPENTER, R. C., Prof. of Engineering, State Agricultural College, Lansing, Mich.

CHENEY, WALTER L., Asst. Supt. Hancock Inspirator Co., 34 Beach Street, Boston, Mass.
COLE, WENDELL J., Detroit Emery Wheel Co., P. O. Box 84, Columbus, Ohio.
DONOVAN, WILLIAM F., Yale & Towne Manufacturing Co., 64 Lake Street, Chicago, Ill.
DUNCAN, JOHN, Asst. Supt., Calumet and Hecla Mine, Calumet, Houghton Co., Mich.
DURAND, WILLIAM F., Asst. Engineer, U. S. Navy, Birmingham, Conn.
ELMES, CHARLES F., Engineer and Designer, Fulton and Jefferson Streets, Chicago, Ill.
FORSYTH, WILLIAM, Mechanical Engineer, C., B. & Q. R. R., Aurora, Ill.
HIBBARD, HENRY D., Springfield Iron Co., Springfield, Ill.
MACKINNEY, WILLIAM CHILDS, H. W. Butterworth & Sons, 719 East York St., Philadelphia, Pa.
MATLACK, DAVID J., I. P. Morris & Co., 1057 Richmond St., Philadelphia.
MCFARLAND, WALTER M., Engineer Corps, U. S. Navy, Cornell University, Ithaca, N. Y.
MESSIMER, HILLARY, Engineer and Designer, Coxe Bros. & Co., Drifton, Luzerne Co., Pa.
MILLHOLLAND, WILLIAM K., Supt. Falls Rivet Co., Cuyahoga Falls, Ohio.
MOFFAT, EDWARD S., Asst. General Manager Lackawanna Iron & Coal Co., Scranton, Pa.
NEWCOMB, CHARLES LEONARD, Supt. Deane Steam Pump Co., Holyoke, Mass.
PECHIN, EDMUND C., Consulting Engineer, Cleveland, Ohio.
PORTER, JOHN B., Metallurgist and Chemist, 17 W. Third St., Cincinnati, O., or Glendale, Hamilton Co., Ohio.
ROGERS, CHARLES L., Designer and Draughtsman, Bethlehem, Pa.
SWAIN, GEORGE F., Asst. Prof. of Engineering, Mass. Inst. of Technology, Boston, Mass.
TATNALL, JAMES E., Metallurgist and Engineer, Bethlehem, Pa.
THOMAS, JOHN, Supt. Thomas Iron Co., Hokendauqua, Pa.
TUDOR, FREDERIC, Heating Engineer, 202 E. Twelfth Street, New York City.
UEHLING, EDWARD A., Bethlehem Iron Co., Bethlehem, Pa.
WHITHAM, JAY M., Asst. Engineer, U. S. Navy, Bureau of Steam Engineering, Navy Dept., Washington, D. C.

ASSOCIATES.

HALL, JOHN H., T. R. Pickering & Co., Portland, Conn.
STOCKLEY, GEORGE W., V. P. & Manager Brush Electric Co., Cleveland, Ohio.

JUNIORS.

HILL, WILLIAM, Collins Company, Collinsville, Conn.
SUTER, GEORGE A., 272 East Houston Street, New York City.

PROMOTIONS TO MEMBERSHIP.

WALLIS, MATHER J., Supt. Motive Power, P. B. & W. & B. & P. R. R., and Junior A. S. M. E.

At the close of the business session, the President called for a paper by Mr. Hoadley on "A Tumbling Water Meter for Experimental Purposes."

Mr. Hoadley.—It may be appropriate to say a few words with relation to the occasion that called out this little invention, more in detail than it would be becoming to put on our record. In a case in which I was engaged it became necessary to substantiate certain well-known principles of physics too clear, almost too axiomatic, for proof. The statement seemed to be the highest possible proof of which they were susceptible. The whole theory of the other side depended on the denial of those axiomatic principles, so that opinions did not seem to be of any use. In order to meet opinions more weighty and more authoritative than mine, it was necessary to have facts based on quantitative experiments, so that after a series of public experiments in actual wells, I devised a series of physical experiments which might be called laboratory experiments. It became necessary to measure under certain definite conditions actual quantities of water, and to know precisely both the quantity of water and the time in which these waters moved. Attempting to use the piston water meters and valves, I found they were frequently obstructed by fine sand; and if I screened out the fine sand the remaining sand was too porous. Consequently I was led to devise this little apparatus, which is not liable to such obstruction.

Mr. Hoadley then read his paper, and Messrs. Grimshaw, Oberlin Smith, Emery, Odell, Partridge, Porter, Strong, Wolff and Leavitt took part in the discussion which followed. At the close of the discussion Prof. Egleston's paper was called for on "A Machine for Observing the Physical Properties of Metals." A discussion followed, led by Messrs. Oberlin Smith, Durfee, Grant, Towne, Grimshaw, Leavitt, Webber and Hobbs.

The hour for recess having been reached, the Secretary read an invitation from Prof. G. W. Maynard to visit the Hecla Iron Works in Brooklyn, where the Bower-Barff process for rustless iron is carried on, and a recess was then taken until half-past two.

AFTERNOON SESSION, 2.30 P. M.

Mr. Holloway.—In the absence of the President, I am requested to call the meeting to order. I believe Mr. Towne wishes to make a remark if you will give your attention for a moment.

Mr. Towne.—I understood there were some business matters to be cleared away before going on with the papers, and I wish to take the opportunity of saying that we desire to have the presence of all the members on the excursion to Stamford. The preparations for receiving them are complete, and I think I can promise you all an interesting day. There are a good many novelties there, and in addition to the works generally, machines will be operated which are novel, and I think the performance will interest you all.

Mr. Holloway.—I think there is nothing to prevent us all from attending the excursion generously tendered to us to-morrow.

Mr. Durfee read a paper on a Power Crane, and Messrs. Towne and Barnes took part in the discussion of it.

By request of Mr. Angstrom, the Secretary read his paper on "A New Valve Motion," which was illustrated by a model showing the parts. At its close, Mr. Angstrom, in answer to questions, showed the working of the gear as compared with those of Marshall and Joy. Prof. Thurston, and Messrs. Barr and Strong took part in the discussion.

The Chairman then called for Prof. Thurston's paper: "A Note on the Pressures attained under the Drop Press." Messrs. Couch, Towne, Higgins, Smith, Morgan, Grant, Bond and Holloway took part in the discussion.

After Mr. Oberlin Smith's paper on "Machine-shop Algebra," the Secretary read some letters of invitation from Prof. G. W. Maynard, of the Bower-Barff Process Co., from Col. Paine, of the East River Bridge, and from the Controlled Combustion Co., extending hospitable courtesies to members of the Society while in this city.

Mr. Holloway.—I move that the thanks of the Society be tendered to Colonel Paine, Professor George W. Maynard, and to Dr. Mott, of the Controlled Combustion Company, for their courtesies tendered to the Society.

Agreed to.

The Secretary.—There has also been handed to me an amendment to our rules respecting the nomination of officers. It was introduced at the last meeting, and the constitutional time has passed. The present system is that at the meeting preceding the annual meeting, the President has authority to appoint a committee of five members who nominate a straight ticket, and that ticket is forwarded by the Secretary to the members who have the power to erase and substitute other names. The method proposed, as you will see, is somewhat different from the one we now have. The amendment is as follows:

> ARTICLE 28. At the regular meeting preceding the annual meeting of the Society, a Nominating Committee consisting of not less than five members, and they not officers of the Society, shall be appointed by the President. This Committee shall request from each member his choice, in writing, of a candidate for each vacancy, such list of candidates being communicated to the Nominating Committee two months previous to the annual meeting. From this list of preferences, the Committee shall select such three of the candidates for each office as shall have received the highest number of indorsements for the specified office; and these shall constitute the nominees. A printed list of the three nominees for each office shall be mailed by the Secretary to each member and associate, in the form of a letter ballot, at least twenty days before the annual meeting, to be voted upon.

Prof. Sweet.—If I understand the resolution, it requires the Nominating Committee to write a letter to each and every one of the members of the Society. That of course might be done by the Secretary by sending out a printed request or something of that kind. It ought to be arranged so that it can be done by the Secretary and not by the Nominating Committee. Another point about it is that there is no guaranty that the party selected will accept the nomination. I would like to state a few facts of which I am in possession, and of which I believe no other member of the Society is in possession. First, there have been more members chosen for office in the Society that have refused than have ever been elected. Secondly, there has never been in the Society up to this year—and I presume the same is true of this year—there has never been nominated or elected any member who sought the office in any way, directly and indirectly. Thirdly, I wish to say there have been men seeking office who were not nominated. These three points had better be borne in mind before you change the rule. We have all been mechanics long enough to know that there are a good many things we think will work all right, but when we come to try them they do not work at all; and we know of a good many things that are theoretically wrong but work very well in practice. The Mining Engineers had a rule very much like the one which is proposed, but they found in practice it did not work. It was not exactly like this perhaps, for they had no Nominating Committee, and they found it necessary to have a Nominating Committee. Who have been presidents of the Mining Engineers? Dr. Raymond, Mr. Holley, Mr. Hewitt, Mr. Eckley B. Coxe. Do you want a better set of men than the Mining Engineers have had for officers?

The President.—I hope this matter will be thoroughly discussed, because I think it is a very serious one.

Prof. Thurston.—I should think it best that a time be set for the debate on this motion, and I would move that this motion be called up as the first in the regular order of business to-morrow evening.

Agreed to.

The meeting then adjourned to the following day at 8 o'clock, P.M. Several members visited the point where a steam boiler was operating under the methods of the Controlled Combustion Co. In the evening a large deputation visited the Fair of the American Institute, whose Board of Managers had courteously furnished tickets to the attending members.

SESSION OF FRIDAY, NOVEMBER 2, 8 P.M.

The President.—The first matter to come before us to-night is the amendment to Article XXVIII.

The Secretary read the amendment, as presented at the previous session.

Prof. Thurston.—I made the motion that this come up to-night as the first in the order of business, not only in order that there should be time for discussion, but also that there should be time for the members to think about it. The method of election now adopted by the Society is, I suppose, known by every member to have been, substantially, that previously adopted by the Society of Mining Engineers, and that they were driven into that method by the lack of interest of their own members; the difficulty of securing nominations in that Society led to the appointment of a Nominating Committee and that Nominating Committee is still instructed by their rules to present a list of candidates. In the making up of the rules of this Society, the experience of the Mining Engineers has been a matter of consideration in the settlement of this point. It was determined to give this provision the form which it has here taken, and to adopt the method now familiar to all as that which the Society has practiced from the beginning. There is undoubtedly an objection to that method of nomination in the fact that the members do not have very much opportunity of choice. That objection is probably not as great as it might seem to be, from the fact that, if a nomination is made which is in any way objectionable, members enough can always be found to get out a slate which may be presented in competition with the Nominating Committee's ticket. How far the objection to the present method of nomination may prove to hold good, ultimately, of course no one can say. My own impression is that our present method is a tolerably safe one. I, myself, should like to see two or three or more names, if necessary, presented, if it were practicable, in order that I might have a choice in voting; but it is a matter about which I, as yet, have no decided preference. The fact that the rules have taken their present form partly through consultation with me has, in some measure, committed me to the present method; but I am not strongly tied to it, and, if a better one is found, I shall be glad to give my vote for it. A complete change, such as is now proposed, would perhaps do no harm, if the method can be practically carried into effect. The method now adopted has, as yet, done no harm, and it *has* been carried out successfully. A compromise method has been proposed, which may perhaps meet the views of members better than either of these methods; that is, to instruct the Nominating Committee to present several names which may be presented to the members and from which they may choose. This is a matter for present discussion, and I hope the discussion will be free.

Mr. Weightman.—Yesterday afternoon, in the absence of Mr. Wiley, I was requested to move the adoption of the amendment, so that a debate upon it would be in order. But on the recommendation of Prof. Thurston it was postponed until to-night. I see Mr. Wiley is not here, and I hardly know exactly what to do. It would perhaps be as well, instead of making a motion for the adoption of the amendment while Mr. Wiley is absent, to make one that it be referred to the present Committee on Revision of Rules. Then it will come up with the balance of the amendments and come before the Society as a whole in a year from now. I would make that motion.

Agreed to.

The President.—The Chair desires to express his satisfaction as an individual member at this, because the committee is very desirous of having the views of individual members of the Society in regard to the revision, and I think it would be very wise if each member who desires to see any amendment of any rule would write to Prof. Trowbridge as chairman of that committee. I can see that there is a difficulty in our present method, and that it might appear that there possibly would be a ring, and that the President, perhaps, might so constitute a Nominating Committee that if he wanted a renomination he could get it. I hope any man who has definite views on this subject will communicate them to this committee.

I will call upon Mr. C. J. H. Woodbury to read a paper on "Experiments upon Non-conducting Coverings for Steam Pipes."

Mr. Woodbury read a paper with this title by Prof. Ordway, of Boston, and Messrs. Thurston, Grimshaw, Emery, Barr and Leavitt took part in the discussion.

The Secretary.—There are three papers to be read by title. Mr. A.W. Robinson, of Montreal, Canada, presents a paper on "Motion Curves for Slide Valves." I have that paper in print, and the diagrams which illustrate it. There is also a paper by Prof. Thurston on the "Theory of the Turbine," which is also in print; and there is a paper by Mr. Harris Tabor, of Pittsburgh, on "Compression as a Method of Governing Steam Engines."

At the request of several members, Prof. Thurston gave an abstract of his paper on Turbines as affected by the whirl of the water.

Prof. Thurston.—Before leaving the stand I will call the attention of the Society to a matter which I was asked to bring before it. The American Association for the Advancement of Science has formed a section of "Mechanics" which has been in operation for about two years, and in that section it is hoped that members interested in mechanical matters will group themselves. The meetings of the Association take place annually, usually about the end of August. The section of Mechanics contains very few members, and it is proposed by the gentlemen who are now in that section to send out circulars to the members of this Society inviting them to come in and take part in the formation of a section in that Society which shall devote itself to the applications of science in the arts. That Association, so far, has been a purely scientific association, although there has been an applied side in its work. The fees of the Association are three dollars a year. The opportunity there offered to make the acquaintance of others who have the same interests is one should not be slighted. Prof. Webb is now Secretary of the section, and would be very glad to give any information respecting it to gentlemen who may talk to him or write to him about it.

Prof. Webb.—The next meeting of the Association is purposely placed a little later than usual. It is supposed that it will occur about the third or fourth of September, and will be in Philadelphia. It is arranged in this way for the British Association who meet in Montreal the week previous, and it is thought a large number of the members of the British Association will come to Philadelphia to attend the meeting there.

Mr. Oberlin Smith.—I understand that it is to be the policy of this Society hereafter not to publish in the transactions all the papers read, as they have done heretofore, nor a very large proportion of them. I do not know what proportion, but a large percentage are to be omitted entirely, I understand. It seems to me that that is a wrong policy. Of course, objectionable papers are offered. Such should be refused at first. If a paper is good enough to be read before the Society and discussed, it seems to me it is good enough to go into the transactions, and the very object of those transactions is to form a record of the doings of the Society. I understand in the Civil Engineers' Society, nearly all the papers offered are accepted, a very small percentage only being refused, and that nearly all are put into the transactions, some few being left out on account of mistakes being made in hastily reading them over when first offered. Their rules allow abstracts of papers to go into the transactions. If it is a mere matter of economy, it seems to me that it would be a better plan to delay the publication for a time than to omit altogether.

The Secretary.—I would reply that if such an impression was derived from what the Secretary said, it certainly was not the impression intended to be conveyed. Hitherto, when about twenty papers have been presented to the Society, it has been the case sometimes that perhaps two of those papers have not been published. The reason for omitting them has been one of finance. What the policy of the Society will be in the future is entirely an open question. I agree with the speaker that a paper which should not be published should not be read. But no precedent in any sense has been established hitherto, nor do the Publication Committee propose to be held by any precedent. If circumstances will permit, all papers will be published which are read before the Society.

Prof. Egleston.—As there seems to be some doubt about the future financial ability of this Society, I would move that the Society resolve itself into a Committee of the Whole, and every member of it into a committee of one, to bring members into the Society, so that we shall have plenty of funds, and have no anxiety about this question of finance.

Mr. Oberlin Smith.—I am very glad that I misunderstood the Secretary, and that it is not necessary to make any motion on the subject.

Mr. Le Van.—A great many papers have been read here that ought not to be read. If any one should try to use this Society as an advertising medium, I think there ought to be some way to prevent such papers from being read. I think there are some papers published in our transactions which some day we shall wish we could erase.

The Secretary read a letter from Mr. R. H. Soule, Superintendent of Motive Power of the West Shore Road, in reference to the excursion November 3d, on that road.

Mr. Oberlin Smith.—I beg leave to offer the following resolution:

Resolved, That we tender our thanks to the American Society of Civil Engineers for their cordial co-operation in arranging for the present meeting, and for the use of their rooms for our sessions.

Adopted.

Mr. Durfee.—Mr. President, I desire to offer the following resolution:

Resolved, That the thanks of this body are due to the local committee for the New York meeting, and to the executive committee, for their very satisfactory arrangements for the entertainment of the Society while in the city.

Adopted.

Mr. Holloway.—I offer the following resolution:

Resolved, That the thanks of this Society are extended to Mr. Soule, Superintendent of Motive Power of the New York, West Shore and Buffalo Railway, and to the New York, West Shore and Buffalo Railway Company, its Manager and Assistant Manager, for the excursion to Kingston, tendered to the Society, and for the other courtesies which the Society expects to receive at their hands to-morrow.

Adopted.

Prof. Webb.—I am sure that all will concur in adopting the following:

Resolved, That the thanks of this Society are due to Mr. Towne, of the Yale & Towne Manufacturing Company, and his associates, for the very enjoyable visit to his works at Stamford, and for the admirable manner in which the excursion and entertainment were conducted.

Adopted.

Mr. Weightman.—A year ago a certain resolution was neglected, though it was spoken of afterward. It occurred to me at the time, though I did not attempt to call it up. On the retiring of Prof. Thurston and his associate officers, it was unfortunately neglected to offer a resolution of thanks for the close attention that Prof. Thurston had paid to the organization and building up of this Society during two years, and we are about to adjourn again before we have at least thanked our present retiring officers for their services. Therefore, I would move that a resolution be adopted thanking Prof. Thurston and his corps of officers as well as the present retiring officers for their services, and for the close attention they have given to the affairs of the Society.

Mr. Holloway.—In seconding that motion, which I do with the greatest pleasure, I would like to add that we make it somewhat cumulative. We all are certainly very much indebted to our retired presidents for what they have done for the Society. I take the liberty of putting the motion.

The resolution was adopted.

Mr. Leavitt.—As Professor Thurston was President for two years I think he ought to respond.

Prof. Thurston.—I have forgotten my speech. It was prepared a year ago. (Laughter).

Mr. Leavitt.—I must thank you for your kindness to me during my tenure of the office of President, and I bespeak your kindness for my successor, whom I am sure you will delight to honor.

The meeting then adjourned.

EXCURSION DAYS.

FRIDAY, November 2d, by invitation of Mr. Henry R. Towne, President of the Yale & Towne Manufacturing Company, the Society spent the day at their works at Stamford, Conn. Two special cars were attached to a regular train on the N. Y., N. H. & H. R. R., and were switched into the Company's yard. An escort of gentlemen of the works piloted the visitors, leading them through the foundry, pattern shop, "post-office" building, pulley-block shop and the machine shop for building the Emery Testing Machines. A lunch was served in this, as yet unfinished, shop. After luncheon the engine and boiler house were visited, and the new crane shop was inspected. The hammer shop, the lock department, including the profiling, plating and grinding rooms, led the way to the open room where the 75-ton Emery Testing Machine was erected. After a brief description of the special features of the machine, a piece of iron was broken by tensile strain, another by compressive strain, and a piece of timber by compressive and another by transverse strain. The party boarded their special cars after these experiments, and returned to the city.

Saturday, November 3d, by the courtesy of the New York, West Shore & Buffalo R. R., an excursion to Kingston and return was tendered to the Society. A special train awaited the party at the new Weehawken terminus, to which they had been conveyed by tug and by the new ferry-boat Newburgh. The train carried the special manager's car for the use of the ladies, and the seats had been removed from another car that it might be used as a dining-car. The beauty of the ride was very thoroughly enjoyed, and the special features of the line were admired. Lunch was served at noon and while the train was at rest at Kingston. Returning, a stop of an hour was made at West Point, and the party reached Jersey City after dark.

CXXX.

MOTION CURVES OF CUT-OFF VALVES.

BY A. WELLS ROBINSON, M.E., MONTREAL, CANADA.

AMONG the many forms of diagram illustrating geometrically the movement of slide-valves by eccentrics, none have come under the writer's observation which will satisfactorily represent the distribution of steam effected by that class of valve gear in which a main slide-valve, actuated by link motion, constitutes a moving seat for double cut-off valves.

Looking at the slide-valve in its simplest form, operated directly by a single eccentric, the effects produced by certain proportions of lap, lead and travel are comparatively well known and easily under-

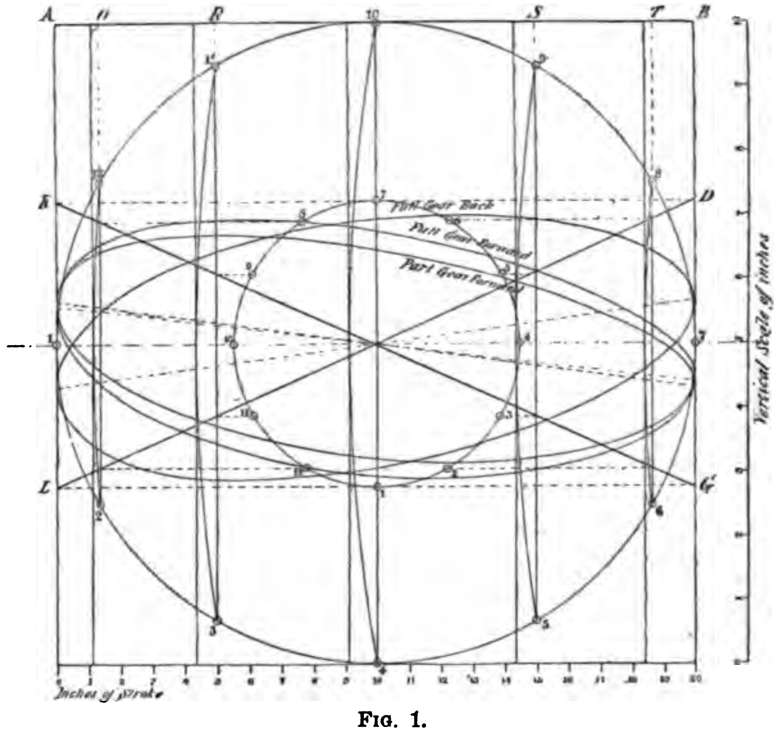

FIG. 1.

stood; but when a link motion is introduced for effecting expansion within certain limits, as well as reversing the direction of motion, the action becomes extraordinarily complicated. Put, now, on the back of this motion double variable expansion valves, and it becomes a subject affording an unlimited field to engine designers and mathematicians for study and investigation.

The form of diagram about to be described is a modification of one already known to some engineers, and the idea of adapting the principle to link motions and variable cut-off valves, so as to show the extent and duration of the port opening, occurred to the writer while designing a small condensing marine engine recently, the dimensions of which are selected for the present illustration.

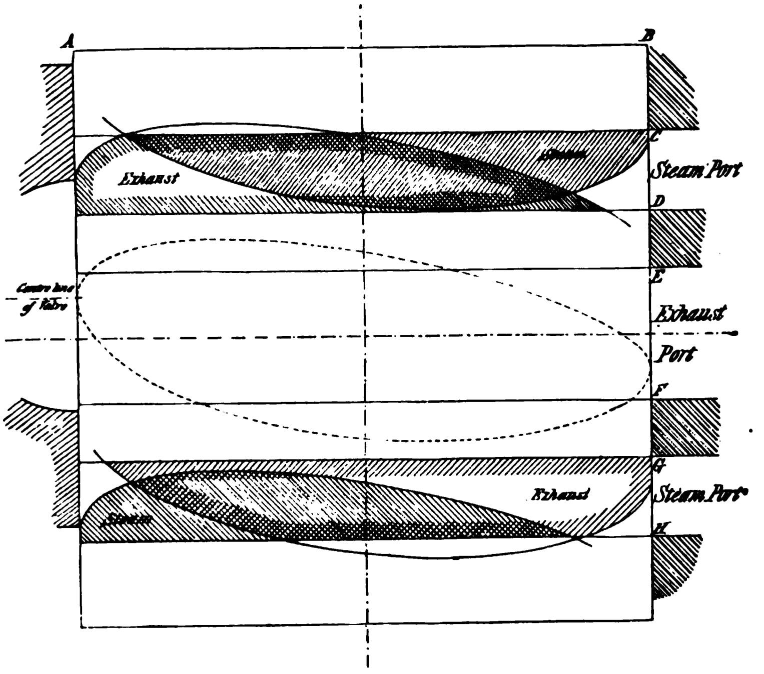

FIG. 2.

In the diagram No. 1, the large circle representing the path of the crank-pin is divided into a number of equal parts—12 in this case. These points of division are projected up to the line A B, which represents the stroke of piston, the points on which thus obtained are the piston positions corresponding to the numbered divisions of the crank-pin circle. The motion of the valves must now be considered to take place at right angles to the line *A B*, so that distance horizontally represents piston movement, and distance vertically represents valve movement. Now, suppose the diagram to be moved horizontally a distance equal to and corresponding with *A B*, or the stroke of piston, in a similar manner to the movement of an indicator diagram, while the valve, receiving its relative motion from the link, moves vertically on it; then a point in the centre of the valve would trace a curved line of an elliptic form. This motion curve may be taken to represent the path of the centre of the main valve, and it may be drawn for various positions of the link, those shown in the diagram, Fig. 1, being full gear forward, second notch forward and full gear back. They are laid down by ordinates derived from diagrams of link positions similar to Fig. 4, which shows the journeyings of the link through its successive positions corresponding to the before-mentioned divisions of the crank-pin circle. A curve showing the exact movement of the main valve being drawn in this way, we can now draw parallel curves to represent the movement of its edges over the ports in the valve seat, as shown in diagram, Fig. 2. The ports are projected across the diagram from *C D*, etc., and the extent to which they are opened during the stroke for steam or exhaust is shown by the curves of the outer and inner edges of the valve respectively. Referring to Fig. 2, it will be seen that the steam opening commences with $\frac{3}{16}''$ lead at *C*; then, widening rapidly, it reaches its maximum at about $\frac{33}{100}''$ of the stroke, after which it gradually closes, cutting off at $\frac{9}{10}$ of the stroke.

Proceeding now to consider the movement of the cut-off valves, it will be seen that, if we draw parallel curves representing the moving parts in the main valve, we may lay down the movement of the cut-off valves over them in a similar manner, and thus trace the events between them. The position of the cut-off eccentric being diametrically opposite the crank-pin, the movement of the cut-off valves, if we suppose it to be traced in a similar manner to the main valve, will be represented on the diagram by straight lines *K G* and *L D*, Fig. 3, and variable positions of their edges to effect any desired cut-off will be straight lines parallel to *K G* and *L D*. In the example the shaded areas of the part terminated by these lines represent the port opening up to their respective points of suppression by the cut-off valves. The various proportions of the cut-off valves for any desired range of expansion may now be determined by direct measurement from the diagram. For example, if the range of cut-off is to be from ⅕ to ¾, the distance, *J M*, between these two positions, is 2 inches, which is the limit of adjustability for each valve. In like manner the necessary width in order to cover at extreme travel will be equal to *N P*, plus a small amount for cover; and if at the latest limit of cut-off the inner edges of the valves are shown to overlap each other, the ports in the back

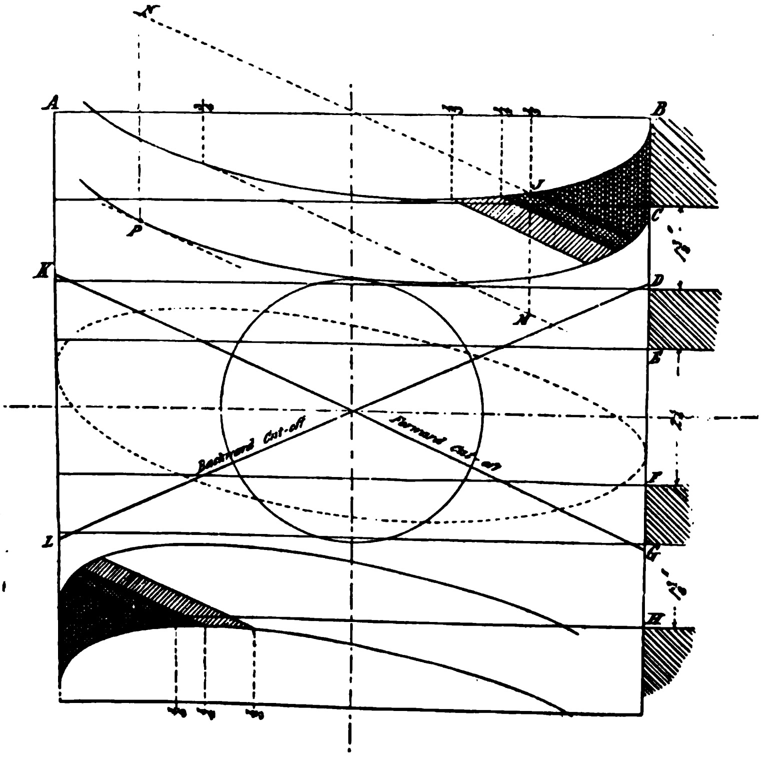

FIG. 8.

of the main valve must be separated by that amount in order to allow space for the adjustable movement.

As before stated, the piston positions *O R S T*, Fig. 1, are projected by straight ordinates from the divisional points of the crank circle. The positions thus obtained, however, would only be correct were the connecting-rod of infinite length. The effect of the obliquity of the connecting-rod is always to draw the piston nearer to the crank-shaft than it would be if the connecting-rod were infinitely long. In order, therefore, to apply a correction to this effect,

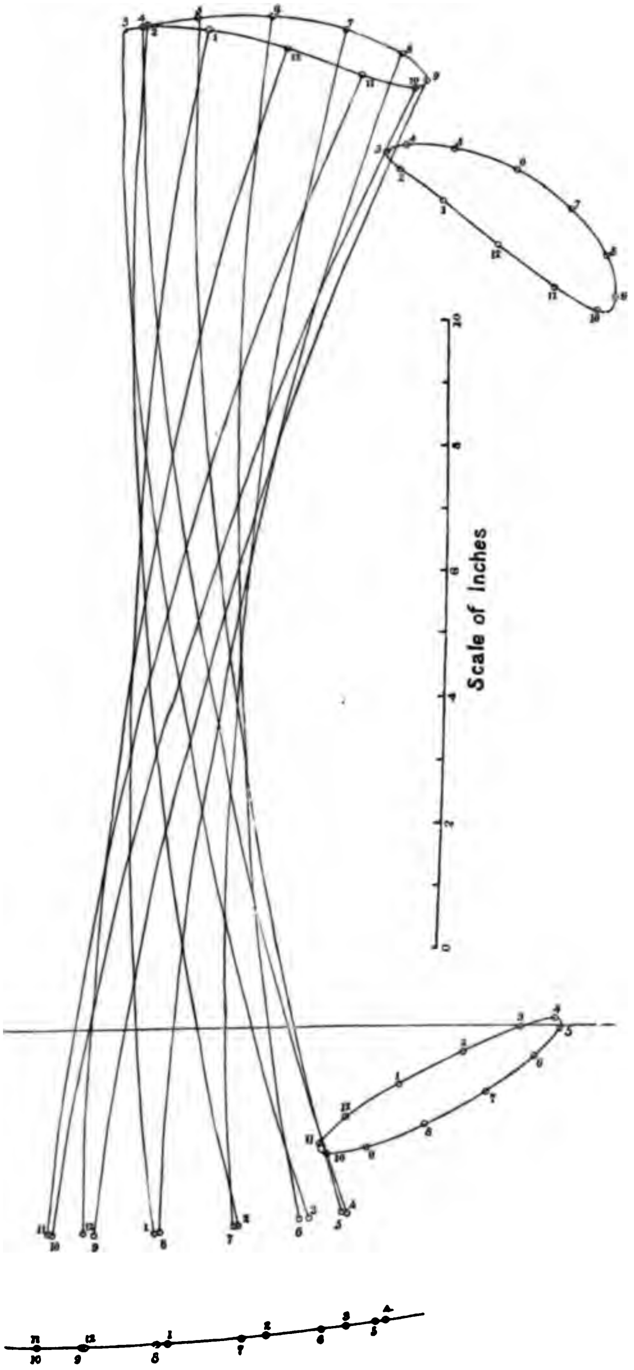

FIG. 4.

we join the divisional points of the crank circle by circular arcs, the radius of which is equal to the length of the connecting-rod. The ordinates of the motive curve are then laid off tangent to these arcs, the result being that the port openings measured at any point on this curve correspond to the fractional parts of the stroke at which they were taken. As before mentioned, the usual position of the cut-off eccentric on the shaft is diametrically opposite the crank-pin, in which case, if it be a reversible engine, and the two eccentrics of the link-motion have an equal angular advance, the admission will be equal both for forward and backward motions. Under some conditions, however, it may be desirable to give a greater admission during backward motion than during forward, in which case the position of the cut-off eccentric may be shifted toward the backward eccentric, giving it a later movement, and thus prolonging the admission, while for the forward movement its angular distance in advance of the forward eccentric is increased, thus producing an earlier movement and a greater relative travel. Its movement in this case, instead of being a straight line, *K G* (the valve going and returning upon that line), would be an open curve, similar to, but flatter than, those of the main valve, according to the position of the eccentric.

The movement of the cut-off valves (curve or straight line, as the case may be) may be plotted direct from the eccentric circle, as shown in Fig. 1. The eccentric circle is divided to correspond with the crank-pin circle, and the points of division of the former are projected horizontally and those of the latter perpendicularly, as before; the intersections of like numbered lines are then points in the curve (or straight lines) *K G* and *L D*.

It will be borne in mind that while the absolute travel of the cut-off valves is constant, we change the relative travel, for a given position of the link, by shifting the position of the cut-off eccentric in the manner before described, for it is manifest that the relative travel of the two valves would be zero if the position of the eccentrics imparting motion to them coincided (or equal to the difference of their throw, if any), and greatest when diametrically opposite. In these illustrations the diagram has been divided into three parts, for the sake of clearness, and many lines have been introduced which would be dispensed with in practice. The desired results may be easily obtained by sliding a tracing of the central curves over the diagram of steam ports.

It is not supposed that this form of diagram will displace or supersede the ordinary methods of design in vogue in the drawing office, but it is thought that the method employed of drawing the ports and graphically representing the extent of their opening, at once appeals to the eye, and a clearer idea of the valve movement is conveyed to the mind than if certain geometrical constructions bearing a conventional, but not apparent, relation to the movement were employed. It also admits of every variation of its elements, and furnishes the means of comparison and of judging the effects of such variation, while at the same time its strict accuracy in neglecting no disturbing influence, such as obliquity of the connecting-rod or varying positions of the link, will perhaps render it valuable for purposes of investigation.

CXXXI.

A NEW VALVE MOTION.

BY CARL ANGSTROM, WORCESTER, MASS.

This valve motion belongs to the same category as those of Brown, Marshall and Joy, and known by the name of "radial" valve motions.* In a radial valve motion the motion is generally accomplished by an arm, two points of which move in different curves. One point moves in a closed curve, such as a circle or an ellipse, this motion being derived from an eccentric, crank or from the connecting-rod. The other point again moves either in an open or a closed curve, and this motion is accomplished either by levers or slides, or both combined.

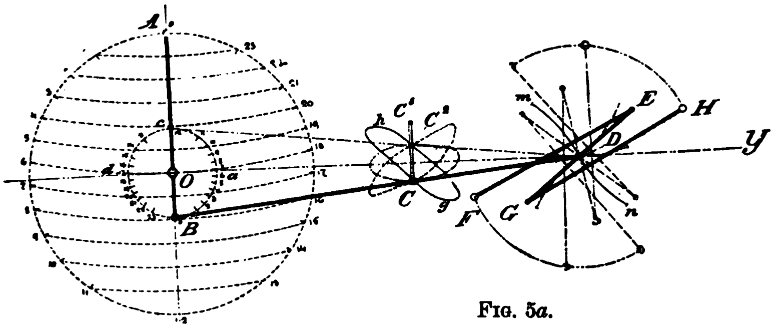

FIG. 5a.

Ratio of Connecting rod to Crank = 5:1

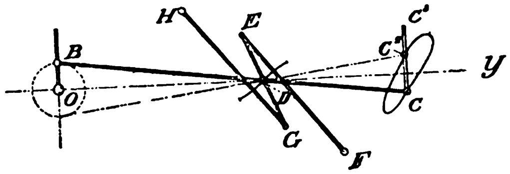

FIG. 5b.

In the valve motion to be described the difference from those previously mentioned consists chiefly in the mechanism for giving motion to the last-mentioned point of the valve actuating arm.

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* "On Radial Valve Gears," by R. H. Graham. *The Engineer*, London, Feb. 23, 1883.

The outlines of the valve motion are shown in Fig. 5a, in which *O* is the centre of the main shaft, *O A* the crank and *O B* the eccentric radius. The valve-rod $C\,C^3$ is attached to the arm *B D* at the point *C*. The arm *B D* is connected at *D* by means of a cross piece *E G* to the two radial arms *E F* and *G H*. These radial arms are pivoted at *E* and *H* to a frame *J* (see Figs. 6 and 7), this

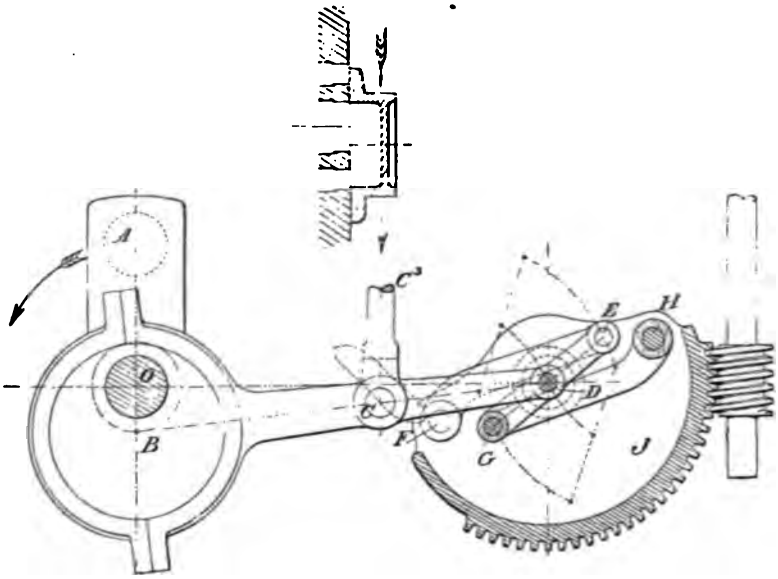


FIG. 6.

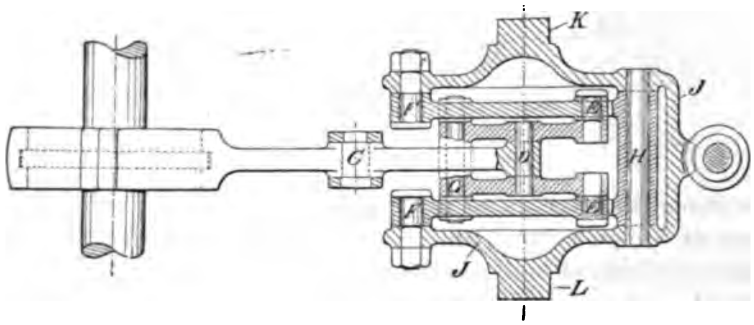
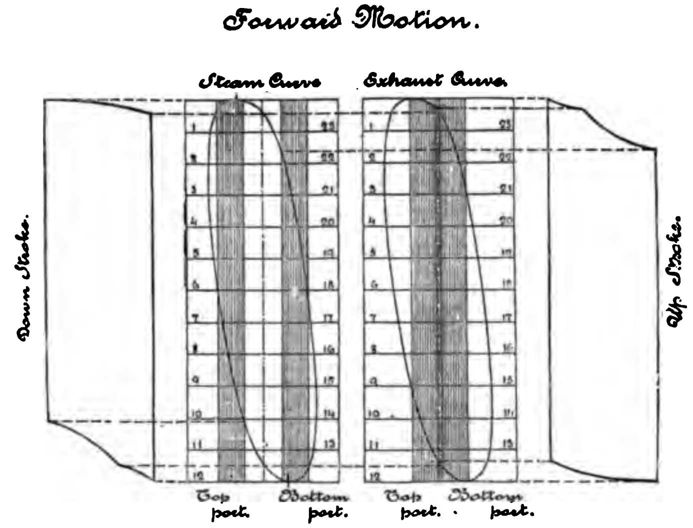


FIG. 7.

frame being free to turn on trunnions K and L, the centre line of these trunnions being coincident with the point D on the rod $B\ D$



Ratio of Connecting rod to Crank = 5:1

FIG. 8.

at the end positions A and A^2 of the stroke $A\ A^2$ of the crank. The combination of the radial arms $E\ F$, $G\ H$ and the cross-piece

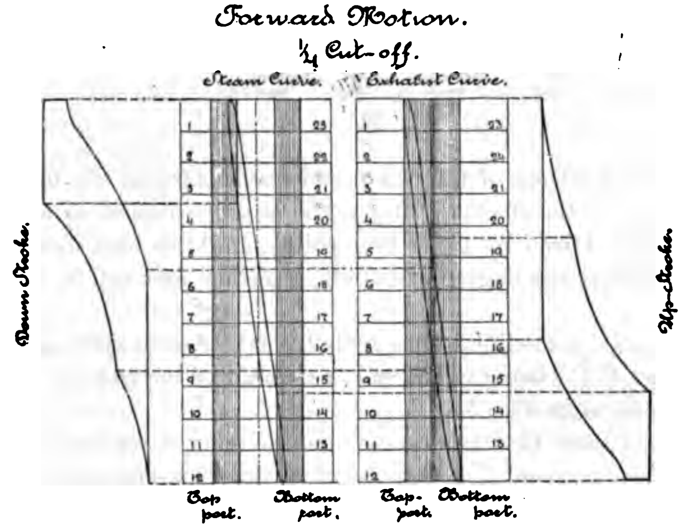


FIG. 9.

$E\ G$ with the arm $B\ D$ makes the point D move in a reversed curve, $m\ D\ n$. The point B again moves in a circle, $B\ a\ c\ d$, and the point C, to which the valve-rod is connected, will thus have the combined movement $C\ g\ C^2\ h$ derived from the circle and curve motions. The degree of expansion and the reversing of the motion is accomplished by changing the angularity of the reversed curve $m\ D\ n$ with reference to the line $O\ Y$. This change in angularity is done by simply turning the frame J on the trunnions K and L. As shown in Fig. 5*a*, all the different curves representing the travel of the valve-rod pin C will intersect at two common points C and C^2, each at equal distances from the centre line $O\ Y$, the

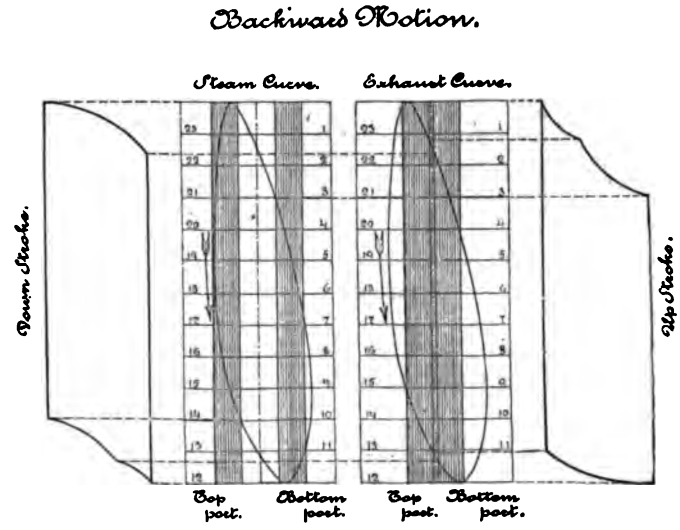


FIG. 10.

corresponding positions of the piston and crank being at the upper and lower end of the stroke. The valve is proportioned so as to give the desired lead at these two points, and this lead then remains constant at any degree of cut-off at either forward or back motion.

Fig. 5*b* shows a modified arrangement of this same valve gear, the valve-rod $C\ C^3$ being connected to the end of the rod $B\ C$, instead of inside, as in Fig. 5*a*.

Figs. 8, 9, 10 and 11 illustrate the motion curves for full gear and for quarter cut-off for both forward and back motions. On each side of the motion curves, as shown in the figures, are diagrams similar to indicator cards, showing the steam distribution for each end of the cylinder. These diagrams are simply intended to assist in locating the various points on the motion curves. The curves for forward motion, Figs. 8 and 9, indicate an exceedingly quick opening or steam admission, while the closing or cut-off is more like a common link motion.

It is thought that this valve motion can be used with advantage on engines running chiefly in one direction. The radial arms and connections could then be proportioned so as to give a good steam distribution and equal cut-off at each end of the stroke. The cut-off could be changed automatically by allowing the governor to act on the movable frame *J*.

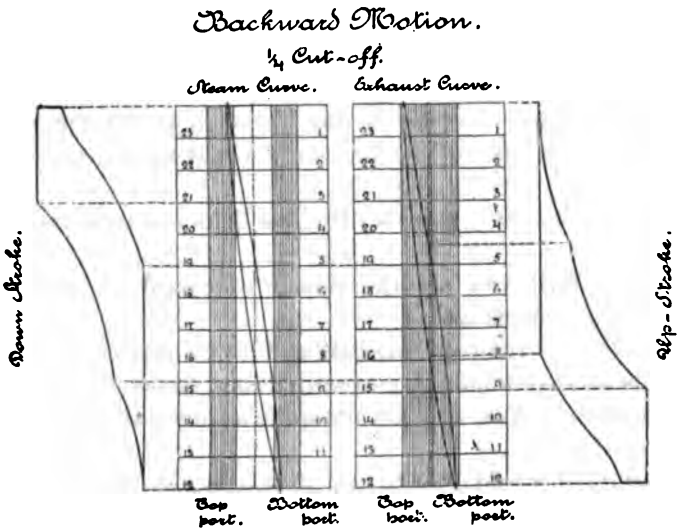


FIG. 11.

DISCUSSION.

Prof. Thurston.—Can you show us the difference between this and the Marshall gear?

Mr. Angstrom.—The Marshall gear consists of just one arm and that is simply swung round. With that motion the lower curve and the upper differ considerably, so that the valve has less motion on the upper end than on the lower. He has, therefore, to use a valve double ported on the upper end, and besides it takes much more room on one side of the centre line.

Prof. Thurston.—If I understand you, this motion is sym cal because you hang this end of the rod from a pair of radial arms so that it swings on the same arc this side as it does on that.

Mr. Angstrom.—Yes, sir; and being symmetrical, the pivoted frame takes less power to move, which is of importance in connection with an automatic governer.

Prof. Thurston.—You have the advantage over the Marshall gear in that you have symmetry of action on both sides of the centre?

Mr. Angstrom.—Yes.

Mr. Barr.—As I understand it, the lead is always the same.

Mr. Angstrom.—Yes, sir.

Mr. Barr.—Then, will you be kind enough to set the model used to cut-off at a quarter? Now, where would the exhaust begin to close?

Mr. Angstrom.—At a point a little less than a quarter from the other end.

Mr. Barr.—Will you set the model to cut-off at one-fifth? Where will the exhaust close?

Mr. Angstrom.—Between one-half and three-quarters.

Mr. Barr.—That would give excessive compression?

Mr. Angstrom.—Yes, sir; but not quite as much as with a common link motion.

Mr. Strong.—I would like to ask if this is not very similar to what Brown uses in his engine of 1878.

Mr. Angstrom.—Brown uses, I think, two arms and one of these sliding in a sleeve. I do not remember exactly how it is, but I know that he has a sliding motion.

Mr. Strong.—Would there be a change in the curve with regard to the radius of the valve rod itself? Is the curve any more than what the angularity of the valve rod is?

Mr. Angstrom.—It will approximate a straight line very nearly, I think. Brown's motion on a curve of the model is in one direction, instead of being reversed, as in mine.

Mr. Strong.—The curve on the slotted link is always struck from the centre where the rod couples on to the valve stem and the object of the curve is to compensate for the angularity on this. Now, on Joy's link he could get a straight slot there.

Prof. Thurston.—I presume I am partly responsible for the presentation of this paper. I became very much interested in this valve gear last year, and looked it over pretty carefully, and persuaded Mr. Angstrom to present a description of it at the Fall meeting. It seemed to me then that it possessed some very decided advantages over most of the forms of valve gear of this class now in use, and would very easily adapt itself to a form of engine in which such a valve gear is desirable. He avoids the sliding which occurs in the Joy arc by swinging about these centres; The centres can then be adjusted so as to get exactly the same motion on both sides of the median line. Then he gets a perfect symmetry by his power of adjusting those arcs by determining the length of these radial bars to the proportions of his rod. Then he secures exactly the same lead, or if he does not choose to do that, he can so make his connections that the lead will be slightly varied. In a vertical engine, for example, he can get a larger lead on.the lower end than on the upper. In fact, the thing has such a form that the engineer can accommodate it to his own ideas of steam distribution, and he gets perfect symmetry of action on both sides. He has a compact system which is easily kept in repair. The surfaces are easily lubricated, and it seems to me that from what has been brought up by the paper, and by the later descriptions by Mr. Angstrom, it possesses advantages over other forms of gear that make it well worthy of consideration. I should think the man that would take that up and introduce it would probably find that he had a very excellent thing.

CXXXII.

COMPRESSION AS A METHOD OF GOVERNING.

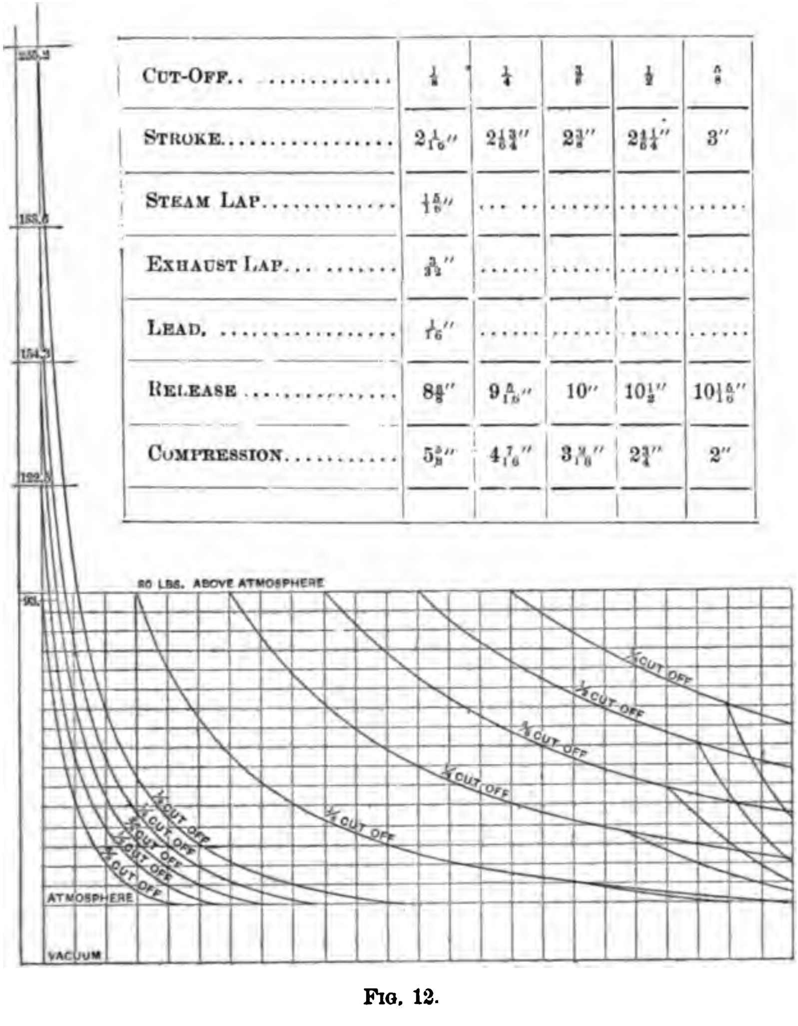
BY HARRIS TABOR, PITTSBURGH, PA.

THE discussion brought up in the Altoona meeting by Prof. Thurston's remarks on compression as a method of governing, etc., was dropped too soon. It is a question which is of great importance to the engineering profession, and points to one of the most direct roads to a better steam economy. Where the engineer can be sure of constant load, and has full control of all conditions, the problem takes its simplest form, and any of the existing types of engines may be designed to give the best of economy. But, unfortunately, these conditions are never found. We are constantly confronted with large engines, lightly loaded, or what is equally bad, with conditions where the resistance is intermittent. The engine at times works to the best advantage with proper load, but much of the time it carries so low a mean effective pressure as to be very wasteful. These are the conditions most frequently met, and what is needed to insure a better steam economy is some method or system of valve gear which shall make such distribution of the steam that when the engine is called upon for its least duty a resistance may be opposed to the piston, independent of the load. This, of course, must be amenable to the governor, and consequently will be a factor in regulation. Compression seems to be the only apparent agent which we may call to our aid.

It is conceded that Mr. J. C. Hoadley was the first engineer in this country to realize fully the importance of this question, and for a number of years he applied it to practice in an engine bearing his name. Since then a number of manufacturers have introduced the same type of engine under various names, all of which have been more or less successful. I refer to the single-valve automatics, in which one valve is made to do the duty of induction and eduction.

To one in search of the highest attainable economy, there is an element lacking in this type of engine, viz.: small clearance. There seems to be no way to introduce it without change of construction.

The writer has constructed various diagrams based upon 3% clearance, to illustrate his meaning more clearly. The curves are all those of Mariotte, but they serve the purpose of comparison. It will be noticed that release and compression vary with the point of cut-off—the lead only remaining constant for all diagrams, which, by the way, is one of the best features of the single-valve system.



CUT-OFF..	1/8	1/4	3/8	1/2	5/8
STROKE	2 1/16"	2 13/64"	2 3/8"	2 41/64"	3"
STEAM LAP	1 5/16"	......	......	......	......
EXHAUST LAP	3/32"	......	......	......	......
LEAD.	1/16"	......	......	......	......
RELEASE	8 3/8"	9 5/16"	10"	10 1/2"	10 15/16"
COMPRESSION	5 5/8"	4 7/16"	3 5/16"	2 3/4"	2"

FIG. 12.

It will be assumed that the engine has a stroke of 12"—just equal in length to the diagrams which are shown one-third size in Fig. 12. The compression due to ½ cut-off will be first considered. The exhaust valve will close at 9¼″, when compression commences, and run through the remaining 2¾″ of the stroke, rising, at the end of the stroke, to 122.5 lbs. absolute—about 27 lbs. above initial pressure. When the cut-off is earlier—at ⅜ stroke—the compression is increased, and terminates at 154.3 lbs. At ¼ cut-off 188.6 lbs. is reached, and when cutting ⅛ of the stroke the compression rises to the enormous pressure of 235.2 lbs. There is no certainty that the highest compression has yet been reached, for the diagram shows a mean effective pressure of 11.46 lbs.—ample, in many cases, to furnish all the power the engine is called upon to deliver. A lighter load, which is always at times probable, would run the compression still higher. The diagrams thus show the obstacle to small clearance in the type of engine in question.

At first thought, one would naturally look to the steam lead for a remedy, by compelling that to take place when compression reached initial pressure. Unfortunately this cannot be done. The edge of the valve that gives lead to the steam also performs the duty of cut-off. Excessive lead would retard the closing so much that an early cut-off would be impossible.

We cannot stand the enormous pressures that compression will give in an engine of this type, with small clearance. There must be some relief, or the clearance space must be enlarged. There is certainly economy enough in the lesser clearance to warrant some additional cost to the engine.

Relief or equilibrium valves, in each end of the cylinder, seem to offer assistance, so arranged that steam-chest or steam-pipe pressures should always be opposed to the pressure in the cylinder. These valves might be placed in the cylinder heads in such a way as to assist in steam jacketing, or might be made to connect directly to the steam-chest. Their area should be sufficiently large to insure small lift, to prevent hammering.

This construction would seem to be a move in the direction of steam economy. Aside from the noise of the relief valve, the problem is very simple. The steam valve must be so arranged that it cannot leave its seat if the pressure in the cylinder should exceed that in the steam-chest—for the same valve does the duty of exhaust and the loss would be serious from lifting, or leaving its seat. The resistance due to the early compression would call for a heavier fly wheel. The crank pressures would be far from uniform, but in this respect there would not be a very great departure from the present results in compression engines.

The gain from this construction when compared with engines of the Corliss and similar types would apply only to a cut-off earlier than ¼ stroke, just where we most need the better economy. It will do much to check the excessive internal condensation due to low terminal pressure. A glance at the diagram will show that with ⅛ cut-off the cylinder would be exposed to an open exhaust port only 6⅜″ in a stroke of 12″, whereas in a Corliss, or similar engine, the exhaust would remain open much longer.

The following table of mean effective pressures due to given cut-off, and their theoretical consumption, shows that we have little to fear from light loads, if we can reduce clearance and control compression in some practical manner:

POINT OF CUT-OFF.	MEAN EFFECTIVE PRESSURE LBS.	STEAM PER H. P. PER HOUR.
⅛	13.82	19.61
¼	35.33	20.35
⅜	50.53	22.52

It is fair to assume that an engine using such liberal compression would fulfill the theoretical conditions more nearly than the ordinary type. Prof. Thurston says: "We must look in the direction of reduced internal condensation for greater economy." It is an insatiable robber whose depredations require attention.

Super-heat offers advantages, but only in part, for it offers no objection to the foolishly low terminal pressures we meet in every day practice. We must have some means of securing a degree of economy when conditions compel us to ignore engineering principles and build engines to run under a mean effective pressure of 12 or 15 pounds. We cannot add to the belt resistance in such cases, but we can stuff the idle end of the cylinder full of steam at a low tension, and compress it to a higher, thus making work for an under-loaded piston at our will.

If one is inclined to strain after the highest possible attainment without regard to cost or complications, he would find it in the four-valve system, where separate valves are used for eduction. The exhaust valves could then be made to have a constant release and a varying compression, and the steam valves a constant lead and varying cut-off, giving the same results as in the case of the single-valve system, but without the slight loss that comes from the early release when the engine is running under a light load. This, however, seems to me to be so slight that it is not worth the cost or complication necessary to secure the gain.

When separate valves are used, the steam valves might be allowed to lift, and thus serve as reliefs, but I fear the constant attrition, when running under a uniform load, would reduce the valve seat, at the point of closing, to such an extent that leakage would ensue, if the travel of the valve were changed through change of load.

Evidently, compression is to be the strongest ally if better economy is hoped for with our present steam pressures. Certainly there is nothing so promising which comes with so little cost.

CXXXIII.

PRESSURE ATTAINABLE BY THE USE OF THE "DROP-PRESS."

BY ROBERT H. THURSTON, HOBOKEN, N. J.

THE writer has recently taken occasion to determine the magnitude of the pressures attainable and not unfrequently utilized in the use of the "drop-press," now so extensively employed in the process of "drop-forging" and in the manufacture of small parts of sewing-machines, firearms and light machinery.

The opportunity was afforded to make this determination in the course of an investigation of the efficiency of drop-presses lately made by the Mechanical Laboratory of the Stevens Institute of Technology. It was found that the most efficient presses experimented with had an "efficiency," as the term is technically used, of 90 per cent.—*i.e.*, the work done by the drop was 90 per cent. of that which was due to the weight falling through the measured height. The table which follows is based upon the assumption that this efficiency can be reached, and exhibits the mean pressure attained when the piece attacked is crushed to the amount of $\frac{1}{4}$, $\frac{1}{8}$, inch respectively. The maximum pressures must exceed those given. The latter are calculated by determining the amount of energy of the falling drop at the instant before stopping—*i.e.*, of the quantity of work done upon it by gravity and stored in it, and dividing that measure in foot-pounds by the distance through which the crushing of the "work" takes place. These figures are seen to be simply enormous, and the power of this form of press is evidently limited only by the rigidity of its parts and their strength.

The figures given for the pressures reached when the compression is $\frac{1}{16}$ inch can only be obtained when the anvil is so set and of such material that the yielding there occurring cannot absorb more than the allowed 10 per cent. of the total work of the falling mass. The same remark applies to the table generally, but the loss may always be expected to fall within the assumed figure for the smaller weights and lesser heights fallen through; if the machine is well built, and the anvil and foundation are of ample size and rigidity for good work, it is not improbable that the higher figures can be readily obtained, also, if proper precautions are taken in the setting of the press.

PRESSURES OF THE DROP-PRESS.—(EFFICIENCY 90 PER CENT.)

FALL OF DROP IN FEET AND INCHES.	COMPRESSION IN INCHES.	WEIGHTS OF DROP.								
		POUNDS. 50.	POUNDS. 100.	POUNDS. 200.	POUNDS. 400.	POUNDS. 600.	POUNDS. 800.	POUNDS. 1,000.	POUNDS. 1,500.	POUNDS. 2,000.
0′ 3″	$\frac{1}{16}$″	2.160	4,320	8,640	17,281	25,921	34,562	43,202	64,803	86,405
	$\frac{1}{8}$″	1,080	2.160	4,320	8.640	12,960	17,281	21,601	32,401	43,202
	$\frac{1}{4}$″	540	1,080	2,160	4,320	6,480	8,640	10,800	16,200	21,601
0′ 6″	$\frac{1}{16}$″	4,320	8,640	17,281	34,562	51,843	69,124	86,405	129,607	172,811
	$\frac{1}{8}$″	2,160	4,320	8,640	17,281	25,921	34,562	43,202	64,803	86,405
	$\frac{1}{4}$″	1,080	2,160	4,320	8,640	12,960	17,281	21,601	32,402	43,202
0′ 9″	$\frac{1}{16}$″	6,480	12,960	25,921	51,843	77,764	103,686	129,608	194,412	259,216
	$\frac{1}{8}$″	3,240	6,480	12,960	25,921	38,882	51,843	64,804	87,206	129,608
	$\frac{1}{4}$″	1,620	3,240	6,480	12,960	19,441	25,921	32,402	43,603	64,804
1′ 0″	$\frac{1}{16}$″	8,640	17,281	34,562	69,124	103,687	138,249	172,811	259,216	345,622
	$\frac{1}{8}$″	4,320	8,640	17,281	34,562	51,843	69,124	86,405	129,608	172,811
	$\frac{1}{4}$″	2,160	4,320	8,640	17,281	25,922	34,562	43,202	64,804	86,405
1′ 6″	$\frac{1}{16}$″	12,960	25,921	51,843	103,686	155,529	207,373	259,216	388,824	518,432
	$\frac{1}{8}$″	6,480	12,960	25,921	51,843	77,764	103,686	139,608	194,412	259,216
	$\frac{1}{4}$″	3,240	6,480	12,960	25,921	38,882	51,843	64,804	87,206	129,608
2′ 0″	$\frac{1}{16}$″	17,281	34,562	69,124	138,249	207,373	276,498	345,622	518,433	691,244
	$\frac{1}{8}$″	8,640	17,281	34,562	69,124	103,686	138,249	172,811	259,216	345,622
	$\frac{1}{4}$″	4,320	8,640	17,281	34,562	51,843	69,124	86,405	129,608	172,811
2′ 6″	$\frac{1}{16}$″	21,601	43,202	86,405	172,811	259,216	345,622	432,028	648,042	864,056
	$\frac{1}{8}$″	10,800	21,601	43,202	86,405	129,608	172,811	216,014	324,021	432,028
	$\frac{1}{4}$″	5,400	10,800	21,601	43,202	64,804	86,405	180,007	162,010	216,014
3′ 0″	$\frac{1}{16}$″	......	51,843	103,686	207,373	311,059	414,747	518,433	777,649	1,036,866
	$\frac{1}{8}$″	......	25,921	51,843	103,686	155,529	207,373	259,216	388,824	518,433
	$\frac{1}{4}$″	......	12,960	25,921	51,843	77,764	103,686	129,608	194,412	259,216
3′ 6″	$\frac{1}{16}$″	......	......	120,967	241,935	362,902	483,871	604,838	916,257	1,209,677
	$\frac{1}{8}$″	......	......	60,483	120,967	181,951	241,935	302,419	458,128	604,838
	$\frac{1}{4}$″	......	......	30,241	60,483	90,975	120,967	151,209	229,064	302,419
4′ 0″	$\frac{1}{16}$″	......	......	......	276,498	414,747	552,996	691,244	1,036,866	1,382,488
	$\frac{1}{8}$″	......	......	......	138,249	207,373	276,498	345,622	518,433	691,244
	$\frac{1}{4}$″	......	......	......	69,124	103,686	138,249	172,811	259,216	345,622
5′ 0″	$\frac{1}{16}$″	......	......	......	.....	518,434	691,245	864,056	1,296,084	1,728,112
	$\frac{1}{8}$″	......	......	......	......	259,217	345,622	432,028	648,042	864,056
	$\frac{1}{4}$″	......	......	......	.....	129,609	172,811	216,014	324,021	432,028

The intensity of pressure attainable is evidently determined by the area of the surface exposed to the action of the drop, and this in turn determines the distance through which crushing may occur. The figures given in the table are total pressures, and the mean intensity of pressure corresponding to these amounts is to be obtained by dividing the total pressure as shown in the table by the area of section of the crushed piece, or by the mean area opposed to the crushing action during the operation. The proper comparison is that of the energy of the falling weight with the "resilience," elastic or total, or both, of the mass on the anvil or in the dies.

The limit to the resistance of any mass on the anvil is found at the pressure at which the metal will "flow" continuously. This pressure varies with not only the kind of metal, but with every variation in the chemical composition, the physical structure or the form and method of support of the piece. For general use the value of this "modulus" may be taken at about the value of the shearing resistance of the material. For soft wrought iron, for example, it may be taken at about 50,000 pounds per square inch (3515 kg. per sq. cm.) for moderately hard iron at a figure 20 per cent. higher, and for pure copper at about one-half the latter figure. There is, however, a great difference in the behavior of the two metals under pressure. The former has a distinct elastic limit in its original state which becomes "exalted," as was shown by the writer some ten years ago, when the piece is distorted, and becomes approximately equal to the maximum force, producing change of form, remaining permanently altered. The metal thus transformed does not yield subsequently to any less pressure. It will not flow under a pressure much less than that which is required to produce distortion immediately upon its application. Copper, however, has no true and measurable elastic limit in its original condition as found in the market, and it does flow under the continued action of forces far less than those required to produce rapid and continuous distortion by steady pressure. A load which produces no visible effect when first applied will, after a time, be found to have caused a very decided, and often a very extensive, alteration of the form of the mass. This is also a now well-known property of some kinds of brass and of many other metals belonging to what the writer has called the tin class, to distinguish them from the metals of the iron and steel class, which do not exhibit this treacherous behavior. This difference is of some importance, not only as indicating the best method of working them, but also as showing that the first of these two classes is a safer class to deal with, where the metal is to be used in the carrying of heavy and unintermitted stress, than is the second-class.

Another important distinction between these two classes is, as indicated by the results of investigations made by the writer, that the "iron class," which includes all the irons and all the steels, offers more resistance as the rupturing action is slower, while the "tin class," which includes nearly all the other metals and very nearly all the alloys that the writer has ever tested, yields the more readily the more slowly the distortion goes on. The second class is thus subject to that singular kind of change of form under heavy, continuous stress which is illustrated in the movement of all viscous solids—ice, for example, as seen in its flow in the glacier. This, it seems probable, may often occur under pressures far within those which are required to cause change of form in the testing machine in the ordinary methods of test. Iron has been found by Vicat, and later by the writer, to exhibit something such a phenomena, but only when the pressures are considerably above one-half those usually found for the moduli of rupture, and this action is only seen in serious degree when the iron has been annealed and thus softened. Common merchant iron, so far as the writer is aware, does not show any tendency to such slow and imperceptible yielding under moderate loads.

The bearing of these facts upon the value of the drop-press as a means of working iron and other metals into shape is obvious. Change of form can only begin when the elastic limit of the material is passed, and flow can only progress steadily and uninterruptedly when the pressure applied is in excess of the maximum resistance of the metal to shearing. The soft metals which belong to the "tin class" are best attacked by processes which cause a comparatively slow motion of their particles in changing form; iron and steel, on the contrary, being less resistant at high than at low velocities of flow, are best worked by methods which produce rapid distortion. Professor Kick, of Prague, has shown very plainly that this difference in the amount of work demanded by the soft metals under the two kinds of treatment may amount to a very important quantity. He finds that the distortion of bodies by the action of the hydraulic press, and by the action of a hammer dealing a succession of blows to produce the same change of form, consume power in the ratio, in some cases, of one to ten. It is thus evident that the hammer or the drop is to be used for those special cases in which the pressures desired cannot be reached by ordinary methods, and that it is better adapted to the working of iron and steel. The hydraulic press and automatic machinery are to be preferred where they can be conveniently and cheaply used. For much of the work that is now done in our smaller kinds of manufacturing, the drop has been shown by experience to be the only machine which will give the required enormous pressures and do the work rapidly and cheaply.

The maximum area of surface exposed of pressure which will be allowable for any given amount of compression can be determined approximately by dividing the total mean pressure due to the action of the drop with the given fall and the proposed compression, by the maximum resistance of the material. The maximum area which will permit action upon that surface is to be ascertained by dividing the same maximum pressure due to the fall of the drop by the elastic limit of the metal in compression.

The total work absorbed, or the resilience of the mass, up to the elastic limit is to be measured by multiplying the elastic resistance by one-half the percentage of compression which marks the elastic limit; the result measures the resilience in inch-pounds when the unit of measure is the inch, and in centimeter-kilograms when the units are metric. The total work done in any permanent change of shape is proportional to the volume affected and to the maximum resistance of the material to such deformation.

What figures shall be adopted for the resistance to be calculated upon in the production of flow in metals subjected to the action of the drop-press is a question which the writer is unable to answer definitely. It would seem probable that the effect of the blow may be, in the case of cold metal, somewhat similar to that of cold-rolling, and, this being the case, the initial resistance to flow must be taken as at least 70,000 pounds per square inch (4,921 kg. per sq. cm.) and the resilience during flow at as high as 70,000 inch-pounds per cubic inch (4,921 kg. m. per cubic centimeter) for good common wrought iron. It may be safe to take the figure for hot iron, as usually worked, at less than one-half this amount. For copper, the writer would, in the absence of exact data, take the work of deformation to be two-thirds that of iron for pieces of small section, and would expect a great increase of resistance with either metal when the surface acted upon by the drop becomes large in proportion to its thickness. Probably no very reliable figures can yet be given. Whatever the resistance may be, the drop will be very certain to overcome it, and the variation in its amount will simply determine how many blows must be struck to obtain a given amount of change of form.

DISCUSSION.

Prof. Thurston.—Another point which occurs to me, and which did not occur to me when I wrote this paper, is this: Prof. Kick, who has made a long series of such investigations, has, I think, always insisted that the efficiency of a drop-press, or of any blow-delivering machine, is exceedingly low, and generally comes down to 20 per cent., or something like that. His papers give an efficiency of five to one in favor of slow action. The experiments with these presses showed that they utilize at least 90 per cent. of the energy due to the blow, and therefore Kick's figures must be obtained from experiments with some peculiar form, or with some very different apparatus. It seems to me a very interesting fact that such efficiency as 90 per cent. can be obtained.

Mr. Towne.—Is the loss through the resilience of the machine and the foundation?

Prof. Thurston.—Through the spring of the parts and friction.

Mr. Towne.—Chiefly the friction of the moving parts, I suppose?

Prof. Thurston.—Probably. These data give one no idea of the efficiency of the press as a whole. It might be possible that 35 per cent. of the power used in the work in the steam engine might be wasted in hoisting that drop.

Prof. Webb.—How was the efficiency of 90 per cent. ascertained?

Prof. Thurston.—That was ascertained in this way. A set of copper cylinders was prepared of pure Lake Superior copper; they were subjected to the action of presses of different weights and of different heights of fall. Companion specimens of copper were compressed to exactly the same amount, and measures were obtained of the loads producing compression, and of the amount of work done in producing the compression by the drop. Comparing one with the other, we found that the work done with the hammer was 90 per cent. of the work which should have been done with perfect efficiency. That is to say, 90 per cent. of the work done in the testing machine was equal to that due the weight of the drop falling the given distance.

Prof. Webb.—In a quick blow, is the same amount of work performed as in a slow blow? Was that ascertained?

Prof. Thurston.—I presume it could be determined. In these experiments we did not determine it. I have no doubt the efficiency of the blow would vary with its rapidity.

Prof. Webb.—Would there be a difference between the compression due to slow pressure and the work required for it, and the compression of the drop-press?

Prof. Thurston.—So far as I can judge, from what I have been able to do in that line of investigation, there is a difference, although it is probably not great enough to affect those figures seriously. With quick motions, the resistance of the copper is rather greater; that is to say, the work demanded of the drop-press is rather higher than the work demanded of the testing machine.

Mr. Towne.—There has been one point touched upon in the questions asked that comes home to any one who has had practical experience in the use of drop-presses,—and that is, the difference between a light weight falling a considerable distance, or a heavy weight falling a smaller distance. In my own experience, I have found that there are some kinds of work that need one, and some another. I attempted once to do work with a light press falling a considerable distance, and found afterward that the same work would be better done by a somewhat heavier press falling a very much smaller distance, and, speaking roughly, and, from memory only, I think that the energy developed in the second case was less than in the first. The metal was brass and German silver. I think the element of time comes in. Some pressing operations can be performed with light pressure and slow motion. Others cannot be performed in that way, and require the drop-press. I think, also, that the total energy developed in the two cases may be equal, but the result is very different. I think the question of the flow of material is one of the elements. It takes time for the particles to conform themselves to the shape desired. In some cases we want a slow pressure; in others, a quick, sharp blow.

Mr. Oberlin Smith.—We have had considerable experience in drawing thin sheet metals from flat disks into cylindrical forms, where the flow of the metal is very great. Near the edge, the disk is compressed circumferentially, and drawn out radially. The metal, after being formed into a cylinder, is of about the same thickness as when it started in the disk, and there has been a violent flow. I have found that high speeds in some cases tend to break the metal, when, if we put on a slower speed, it will draw nicely. It seems to me that in this matter of drop-press work, there are two principles coming in; one is the *speed* at which the molecules of any metal can flow readily among each other (which is different with different materials), and the other principle is that of *inertia* in the particles themselves. I suppose with comparatively low speeds and short distances in which the particles have to move when struck in the drop-press, the inertia is hardly worth counting. Whether it is partly the inertia and partly the resistance to flow that makes the thing struck most quickly "upset" for the shortest distance down, I do not know. I think, however, it is a subject that ought to be investigated. There is a field here for a good deal of interesting investigation, and it is a thing we certainly ought to know more about.

Mr. M. P. Higgins.—I would ask Prof. Thurston if he did not observe a great difference in the contour of the two pieces compressed, when one was treated in the testing machine and the other under the drop-press?

Prof. Thurston.—Not sufficient to attract my attention. The general appearance of the two pieces was the same; both were bulged in the middle and had the barrel form.

Mr. Morgan.—It seems to me that we are running away from the question as I understand it. It is not a question of metals at all, but a question of drop-hammers. In my opinion, a light hammer falling from a great distance would certainly not give as effective a blow as a heavy hammer falling from a short distance; because the light hammer would be much more affected by friction in falling than the heavy hammer. The light hammer would require to be much better guided than the heavy hammer to do the same class of work. I mean, for instance, that if you have a heavy hammer, its own weight will guide it much better in falling vertically than a light hammer. A light hammer would require to be guided very nicely to do good work and to keep nicely in line. With a heavy hammer, I think, the proportion of friction would be much less, and the effectiveness of the blow would certainly be very much more with a heavy hammer than with a light hammer. If it has been found that 10 per cent. of the effectiveness of a light hammer, falling a great distance, has been lost, I think it would reduce itself down to 5 per cent. at least on the heavy short-fall hammer, as a kind of a comparative guess. We always find that steam hammers when they are light require to be much better guided than when they are heavy. I believe that the effectiveness of blows or fall of hammer is also very much changed by increasing the weights of anvils. One great trouble builders meet, in trying to get the most effective work from such machines, is caused by the action of the commercial law of competition which is always staring us in the face. There are certain arbitrary laws laid down to us and we are compelled to keep within those laws. I think that you will find generally that our weights for anvils for drop-hammers are about ten to one. Krupp runs up in some cases, I believe, to twenty to one. When we talk about the effectiveness of blows, we must bear in mind that we lose the effectiveness of blows simply because the anvil is not heavy enough in our general practice. The blow is often transmitted through the ground and through the buildings, and is not absorbed in the anvil and the work. At Pittsburgh it is surprising how men can work in the offices attached to some of the works. The blows of some of their hammers go all through their buildings and shake them badly. What does this mean? It means that the anvils are not heavy enough. Were the anvils made heavier the effects would be received in the work. I think these are some of the reasons why we lose, at any rate, some of that ten per cent., and I believe that percentage can be much reduced.

Mr. Couch.—I do not want to say much, but I would like to express my appreciation of what Mr. Morgan has said, and to remark that he knows as well as I do that there are two ways of designing machinery; one is the ideal way and the other is the commercial way. Unfortunately, the former is very little followed in the construction of machinery.

I should like to ask Prof. Thurston if the figure representing the energy expended in the testing-machine in compression would be a rectangle.

[Prof. Thurston made a sketch of an apparently semi-parabolic curve in answer to Mr. Couch's question, and said :]

In this curve, the horizontal dimensions measure the amount of compression produced, and the ordinates of the curve represent the force producing that compression. The height of any point above the base will measure the load the piece was carrying. We always find that copper, tin and zinc, and that class of metals, will give a curve of that character. In cases like those which are considered here, in which a piece is confined between two heads, it spreads as the compression progresses, and finally becomes a slab, and in such cases we find a different form of curve from that obtained from comparatively long pieces in which the compression is a comparatively small percentage of their total length. But the form obtained in these experiments is what I have shown here, both in compression and in tension, and the amount of work done on a piece is measured by the area of that curve. That was made from work on the testing-machine by putting on a small load, measuring the compression and the load, putting on a heavier load, observing the compression, and so on, until a curve was obtained for that case. If any error, small or great, entered one figure, striking the curve through the series of determinations would always give the correct position of the uncertain point.

In closing the subject—I suppose most of the members think that it has been discussed sufficiently for the present—I would say that I have no doubt in my own mind that very great differences will be found, in the action of presses, in the direction indicated by Mr. Towne. But I presume that, *in general*, it will be found that the quick blow is usually the best, for cutting and breaking, and the slow compression is best used for work in dies. I was once talking with a distinguished officer of the army on this subject, who told me that, during the war, he had occasion sometimes to knock the trunnions off guns—not in retreating; he never retreated; they were captured guns—and found that with a blacksmith's sledge he could always knock the trunnions off by a few quick rhythmically repeated blows.

Then, again, the change of form due to flow is, I have no doubt, as I said a few minutes ago, very much such a change as occurs when ice or resin is altered in form. There seems to be something like what might be called a molecular friction, occurring there, and, perhaps, the resistance will, as in all fluid motion, vary as the square of the velocity. In the Vienna report of 1873, by Blake, is given a very complete account of Haswell's method of forging by the hydraulic press.

Mr. Grant.—At the Benjamin Atha Tool Works in Newark, New Jersey, they forge all their hammers under pressure, and have found that to be a great deal better than forging with a blow. The product was better and the flow into the smaller spaces was better.

Mr. Oberlin Smith.—I quite agree with Mr. Morgan in regard to the necessity of putting more "anvil" into the anvil. With regard to what he said about friction, I do not think it is of as much importance as has been inferred here. I think when a weight drops vertically between vertical guides the friction amounts to almost nothing. I should say, if I were going to guess, that it would be a fraction of one per cent. The heavy drop requires less guiding than the light one because its inertia keeps it from vacillating so much.

Mr. Bond.—As to using the press for forging by this Newark firm, they are bound to use drops for that work. I understand that they have ordered two drop-presses.

Mr. Grant.—It is only for a very slight portion, though. I was over there the other day myself, and know all about it.

CXXXIV.

A TILTING WATER METER FOR PURPOSES OF EXPERIMENT.

BY J. C. HOADLEY, BOSTON, MASS.

HAVING had to make, in the course of the year, a great number of experiments in pumping water filtered through sand under various prearranged conditions, in quantities ranging from little more than one gallon in an hour to eight or ten gallons in a minute, the writer soon found that all commercial water meters were wholly unsuitable for his purposes. They were liable to obstruction by sand, inaccurate at best, wildly inaccurate at very low speeds, and difficult to read at high speeds. In addition to this, the readings were at all times insusceptible of verification and of permanent record.

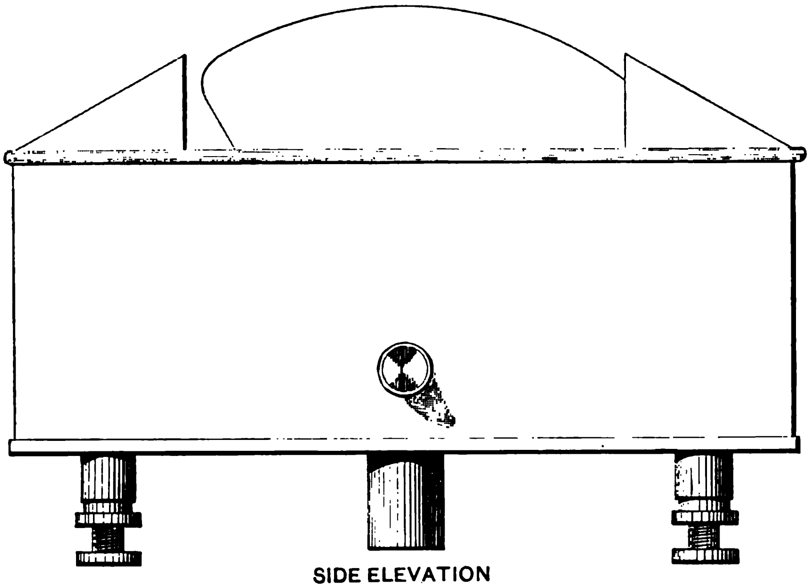


FIG. 13.

Not having found, after search made, any account of a suitable instrument, it became necessary to devise one, and without presuming that this is wholly new or unknown to all the members of this Society, drawings and a description of it are presented as a matter of record for what it is worth.

There was required an instrument which would measure and record with all possible accuracy, and without liability to important error, the quantity of water flowing in a continuous, but pulsating and sometimes variable, stream, in accurately ascertained intervals of time. As constructed and used, the instrument is very simple and inexpensive, and is clearly shown in the accompanying drawings.

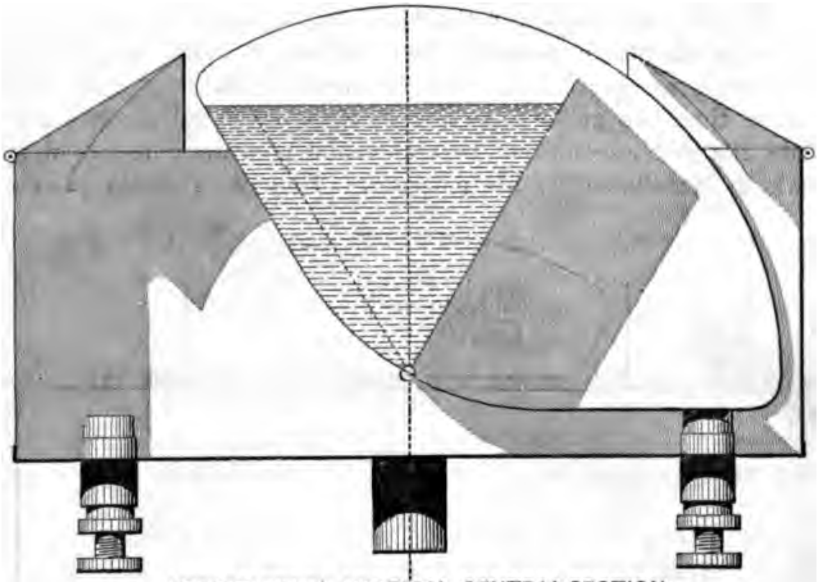
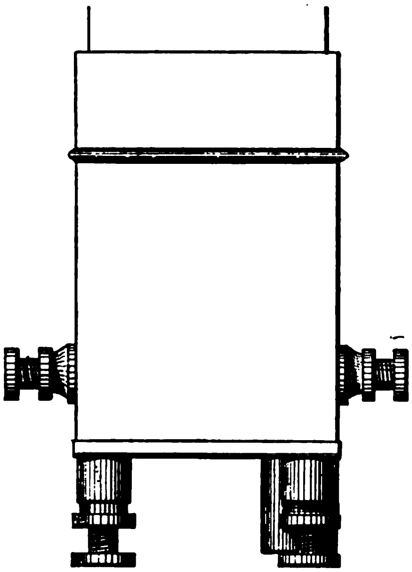
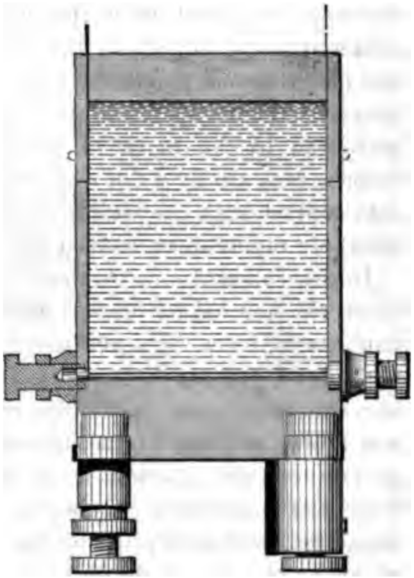


FIG. 14.

Two V-shaped cups (Fig. 14), each embracing an angle of 60°, are joined together by a common side, which is, in fact, a mere partition between them, so that the two cups together embrace an angle of 120°. This double cup is supported in a case upon pivots directly under the partition, turning in hollow, adjustable screws in nuts attached to the case, one on each side. When one of the outside plates of the double-V cup is in a horizontal position, supported in that position by two cork stops on which it rests, the partition between the two V-cups makes an angle of 30° with the vertical, and the outside plate of the upright cup makes an angle also of 30° with the vertical, but at a little greater distance horizontally from a vertical plane passing through the axis of the pivots, on account of the curve by which the outside plate is joined to the partition and to the outside plate of the prostrate cup at its lower end. But for this greater horizontal width on the outer side there would be no tendency to tip—the upright cup would simply fill up and overflow, and there an end. But as the water rises in the upright cup, the prism of water outside of a plane passing through the axis of the pivots and making an angle of 30° with the vertical, acquires constantly increasing preponderance over the equipoise of the wedge-shaped body of water bounded by planes, each making an angle of 30° with the vertical, and intersecting in the axis of the pivots. When this preponderance becomes sufficient to overcome the mechanical advantage of the prostrate cup itself over the upright cup—an advantage due to its greater leverage with equal weight—the cup will tilt, the water in the upright cup, nearly filling it, will be poured out into the case to run



END ELEVATION
Fig. 15.



CENTRAL CROSS SECTION
Fig. 16.

away through its spout, and the now empty cup, lying prone on its cork stops, will become the prostrate cup, with the greater leverage —its centre of gravity being at the greater horizontal distance from the axis of the pivot. The other cup, now upright, will be filled in its turn and repeat the tilting process, and so on alternately as long as the stream flows into the cups.

The tip is very sudden, and is made with considerable force. A light spring of sheet brass attached to the case in the middle of its length by a piece of wood which insulates it, is connected by a binding post with one pole of a battery, and the case itself is in like manner connected with the other pole. A bit of sheet brass soldered to the outside of the tilting double cup, directly opposite the partition and above the case, forming a sort of cam, comes into contact with the spring in passing. This completes the circuit interrupted by the block of wood which supports the spring, and records each tip by a dot or short dash. The cam does not come into contact with the spring until the tilting cup has acquired considerable momentum, so that the tilting is not sensibly retarded.

In designing this instrument, the weight per square inch of the sheet tin selected for it was first ascertained. The whole of the double cup, ends, sides and partition lying on each side of a vertical plane, passing through the axis of the pivots, was then divided into simple geometrical figures, the centre of gravity of each figure was found, and the horizontal distance of the common centre of gravity of each portion from the vertical was computed. The weight of each portion lying on each side of the vertical was also computed, and multiplied by the corresponding horizontal distance of each centre of gravity, which gave, of course, the static moment of that portion of the double cup on each side of a vertical plane passing through the axis of the pivots.

The difference of these static moments was the preponderance of the prostrate over the upright cup—both empty—to be overcome by the water in the upright cup when full to the tilting point, and this was computed in a similar manner. A slight adjustment of the cork stops sufficed to compensate for the inaccuracy of these calculations.

This leads me to speak of the matter of adjustment by means of the cork stops. I actually used a mere socket, like a short candlestick, to hold each cork, entirely inside of the case, which was plain on the bottom. In the drawings will be seen four long sockets, projecting below as well as above the bottom of the case, with adjusting screws and check nuts. The adjustable corks are held in short sockets, with thick bottoms to rest on the adjusting screws, heavy enough to keep the corks from floating away. (Fig. 17.) This device is untried, but I think it quite practicable, and no less desirable.

I had two of these meters, substantially alike, and adjusted them by flowing water through them into a tub placed on scales, counting the tips, and taking the weight and temperature of the water. I then placed one of the meters directly over the other, supported upon the tub by a suitable frame, and flowed a stream through both, the electric register recording the tips, and the scales accounting for the quantity of water. The first tip of the upper meter did not fill the lower meter to the tipping point by about $\frac{1}{16}$ inch, a little water adhering to the cup and case of the first meter; but the second tip of the first was instantly followed by the first tip of the second, and so on until the tub was filled. A repetition of this experiment gave substantially the same result—61 tips in $202\frac{7}{8}$ pounds = 3.33 pounds per tip, and at 80° F., 0.4 gallon per tip—231 cubic inches per gallon.

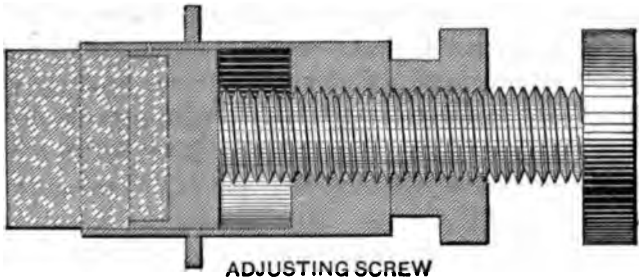


FIG. 17.

The electric register also recorded upon the same $\frac{1}{4}$-inch strip of paper the beats of a seconds pendulum, and, when two pumps were used, the strokes of the pumps and the tips of each meter. It was also easy to note the tips by the ear while watching the second-hand of a horse-timer, by which means the interval of time between tips could easily be observed to quarters of a second. This is not an instrument of precision. It is, perhaps, a little more accurate than a gallon measure, at all practicable speeds; at slow speeds probably decidedly more accurate, and, with proper care, no considerable error is likely to occur. When operated very rapidly, the swaying of the surface tends to accelerate or retard in a small degree the tipping of a cup nearly full, but there is no tendency to accumulation in such errors, which, therefore, may be presumed to balance each other, at least in some degree. The sloping covers at the ends of the case were an afterthought to prevent spattering, and were kept a sufficient distance apart to admit of taking out and replacing the tilting double cups. The curved wings under these sloping cups were a second afterthought for the same purpose, and, as it stands, the case arrests all water—there is no spattering. Of course, a suitable funnel is generally desirable to collect the water from a pump, and to convey it into the cup in a stream as steady as possible, and vertically over the axis of the pivots. It is also necessary that the meter should be level both when adjusted and when in use.

This instrument substantially weighs the water of a flowing stream, and may possibly prove useful, if strongly and delicately made, nicely adjusted and suitably proportioned to the quantity to be recorded, for keeping the record of water used during tests of steam engines and boilers. It is respectfully submitted to the profession for what it is worth.

DISCUSSION.

Dr. Grimshaw.—I should like to say a word in favor of the water meter—not the ordinary meter, but such as this might be made. Very often, evaporative tests in which water meters are used are decried by reason of the errors of water meters. But in very many cases where muddy water is used there is an error with the scales much greater than with the meters. The boiler is credited with the weight of mud pumped in and baked on.

Mr. Oberlin Smith.—I would like to ask Mr. Hoadley how much error comes from the water going on running through the pipe and striking the partition as it passes. As the stream of water flows into the cup it is still running after the cup commences to tip. Does the partition pass this stream ?

Mr. Hoadley.—The partition certainly passes the stream while it is flowing. Of course after the cup begins to tip, a little water will flow into it for an instant. That is weighed. The time of tipping will be constant, but the flow during that time will vary with the rapidity of the stream. It is not an instrument of precision, but it is much more accurate than any ordinary meter.

There is no mystery about it. There is no secondary effect to watch. You have not to observe two things by the eye at the same moment—the second hand of a watch and the pointer of a water meter.

Mr. C. E. Emery.—There is one point in regard to this method of construction which may not be evident to all at the moment. The meters of this kind I have seen before were not of this exact shape and depended on a counterbalance to retard the movement up to a certain point. In this case it will be noticed that the design is such that the water itself forms the counterbalance. It is, in my opinion, a very valuable contribution to our means of measurement for the purposes indicated.

Mr. Odell.—I always use the Worthington Meter, but I always correct the meter, when making tests.

I have a memorandum here which contains notes that may be of interest.

The first case is a 2″ meter; 20 feet of water at 205° passed through under the same pressure and speed at which it was fed to the boiler. It weighed 1,292½ lbs., thus giving a factor of 64.67 lbs., for each cubic foot registered by the meter.

In the second case the same amount of water under the some ditions passed through a 1″ meter weighed 1,339½ lbs., thus giving a factor of 66.87 lbs. In another case, water at 120° gave a factor of 68.5 lbs. This last case was also a 1″ meter.

I have always found that when I once get a factor for a meter, there is very little error afterward, but there is a necessity to get the actual amount of water that passes through a meter by weight. I have also had occasion to make a test in a case where a 1″ Worthington meter has been in use eight years. As it will be necessary for the proprietor of the works to refer to this meter frequently, I passed 102.35 ft. through it at the speed at which it is usually used. The total weight of water was 6,840 lbs., thus giving a factor of 66.73 lbs., water for each cubic foot registered by the meter.

Dr. Grimshaw.—After you have got the correction of the meter, the correction is not correct. We will say that the piston and the barrel of the meter expand in a certain way. Suppose a meter with cast-iron shell and brass piston and the working parts expanding in different degrees in different directions. If the boiler is pumped up spasmodically those checkings will not be found to register correctly. There is another thing which comes in. Very often, in comparing test records made by two different persons, you will find that there are in use about four different standards for the weight of a cubic foot of water at given temperatures; and I think it is worthy of the attention of the Society to establish which one of those different corrections for temperature shall be adopted. I understand that one of our members also finds that sometimes he gets results with regard to distilled water that are contrary to general experience; and I should like to call on Mr. Partridge to tell us what he has found.

Mr. Partridge.—My experiments on the weight of water were not very extensive, but they were exceedingly definite. I went to Fairbanks, and used one of the large but very accurate beam scales which they have for their own use; used sealed standard weights and also sealed measures and drew the water which I was experimenting with from the Croton service pipes. I was comparing the sizes of various measures used by druggists and I found that invariably Croton water failed to weigh as much as the books said distilled water should weigh, by a very considerable percentage. I forget the figures at the present moment. The weighing was done by filling a sealed measure; sliding a plate of glass over it, excluding air, and being sure that it was full; wiping outside of measure and glass perfectly dry and putting the measure on a scale that had already been balanced with both measure and with glass. Then the weight was taken and it failed to come up to the weight of the distilled water by a very considerable amount. The only theory that I could find at the time that seemed applicable to the case was that dissolved air, caused it occupy an increased volume. Whether that is anything more than a hypothesis I don't know. The experiments are certainly well worth repeating by any one who has access to fine scales and gallon and quart measures which have been sealed.

Dr. Grimshaw.—I would like to ask Mr. Partridge whether he also weighed the distilled water, or whether he took the weight given by some book or other.

Mr. Partridge.—I will say that I did not weigh distilled water, but I had at that time got, as far as possible, figures from every original authority that gave the weight of a given volume of distilled water. I think, gentlemen, if you will give a little attention to what the authorities say a cubic foot of water ought to weigh and then will weigh a gallon or two of water as you find it, that you will not be very particular in your calculations whether you use 62 or 62½ or 62¾ pounds.

Dr. Grimshaw.—I would like to inquire the experience of some of the members as to the method of ascertaining these temperatures. It is customary in using the meter to take the temperature at one place or another. It is usual to stick a thermometer in the pipe and it is very common to find that the pressure of the pump will squeeze the glass of the thermometer and make a difference of two or three degrees in the reading of the thermometer. I would like to inquire if any one knows a good way of getting the thermometer to the feed pipe so that the thermometer will register as hot as, but no hotter than, it ought.

Mr. Porter.—The use of the mercury well, I suppose, answers that purpose. It is screwed into a pipe so that the mercury in the bottom of the chamber is in the centre of the current. No pressure can come on the glass bulb. The mercury is a perfect conductor and the thermometer introduced into it registers correctly the temperature of the water in which it is immersed.

Mr. Strong.—There is another matter, which may be of interest in regard to weighing the water from locomotive tenders. That may be done by putting a glass gauge on the outside of the tender running the full height of it and weighing the tender empty. The tender is then filled with water and run on a track scales. On uncoupling the tender from the locomotive and running the water out, two hundred pounds at a time, a scale can be constructed on the glass one graduation of which represents two hundred pounds. The distance between the graduations will not always be the same because of inequalities in the tender due to stays and variations in shape. By reading this scale every time water is taken, the engineman can have an accurate record of water evaporated on his locomotive. If the coal is weighed into the tender as it should be, he will know what the evaporation per pound of coal is, which should be an every-day practice, and not one instituted on special occasions by an expert who makes it appear as a great feat altogether beyond the comprehension of the ordinary engineer.

The President.—I would state for the information of the Society that I have for several years used specially constructed Worthington meters to measure feed water in regular service—water that approaches 200 degrees temperature, and when we have clear water the meters are sufficiently accurate for practical purposes. I think that, in passing, we might say that it is not a matter of very great practical consequence whether a pound of water weighs 62 or $62\frac{1}{10}$ pounds in the evaporation of the boiler. The greatest difficulty in the use of meters arises where grit is present in the water. No manufacturer can provide against a difficulty of that kind, and for this reason I think the meter proposed by Mr. Hoadley is a very valuable one. At the same time it does not admit of being used under pressure, whereas I have used the Worthington meters I have spoken of under pressures varying from 90 to 150 pounds. In our practice we have found that the meters do very well when not cut out by gritty water.

Mr. Wolff.—Following the remarks of Mr. Leavitt, I should like to say that, even admitting that there may be this variation of one-quarter of a pound in the weight of a cubic foot of water—which I doubt the variation would amount to—this would only equal about four-tenths of one per cent., and I doubt that a boiler test was ever made where the errors of observation alone would not far exceed this variation.

Mr. C. E. Emery.—This form of meter has been used under pressure. There is one in the Patent Office in which the tilting cups are arranged in a closed vessel and an air chamber is maintained for them to work in by taking in a small quantity of air at each stroke through a little slide-valve at the bottom which receives motion from the cups. The water which escapes from the cavity of the valve at each stroke is collected in a tank placed in a convenient position for utilizing its contents—as, for instance, over a kitchen sink. I have never seen any of them at work. The pistons of the Worthington meters strike the buffers quite hard at a high velocity, depressing them, so that the stroke is longer than at the slow rate when they do not hit the buffers at all. At some public experiments at one time they were running so regularly that I had no doubt the meter rate was exactly correct, because they were determined for the exact velocity with which the engine was running.

Dr. Grimshaw.—In reference to the mercury well thermometer, I found it had two faults; first of all, it uniformly registers low; secondly, it does not indicate the temperature of the water at the time you are taking the measurement.

Mr. Porter.—I know an instance in which a thermometer in a mercury well showed 211 degrees. I do not think it fell much short of the reality.

CXXXV.

EXPERIMENTS UPON NON-CONDUCTING COVERINGS FOR STEAM PIPES.

BY PROF. JOHN M. ORDWAY, BOSTON, MASS. PRESENTED BY C. J. H. WOODBURY, BOSTON, MASS.

INTRODUCTION.

In addition to the usual number of fires caused by steam-heating pipes, there have been several fires during the past year from the coverings of steam pipes.

An examination of the matter showed that neither dye-stuffs nor oils were present in these coverings, so the fires could not be ascribed to spontaneous combustion.

There seemed to be very little accurate knowledge respecting the efficiency of steam-pipe coverings, although their general importance is universally acknowledged.

There was so much at stake in this matter that the underwriters in interest considered that it would be desirable to investigate the question of these non-conductors, both in respect to any possible dangers of combustion and also to the measure of their economic efficiency. The question was submitted to Professor Ordway, and, by the courtesy of Mr. Edward Atkinson, President of the Boston Manufacturers' Mutual Fire Insurance Company, I have the opportunity of presenting to you that portion of Professor Ordway's report treating of the value of the coverings, with a description of the methods employed, and the results obtained.

C. J. H. W.

In undertaking an investigation of steam pipe coverings, it was necessary, in the first place, to decide what method, or methods, should be used for determining their efficiency as non-conductors of heat. I have met with no recorded experiments of which the details are given in full, but it seems that, in general, two modes have been employed heretofore. In one—the air chamber method—a portion of the covered pipe, while in use, is inclosed in a small box so as to form a close chamber into which the bulb of a thermometer is inserted. The inverse ratio of the temperatures indicated by the thermometer in different trials is supposed to show the relative excellence of the different coverings.

In the second, or condensation method, the steam is allowed to pass from the main pipe out into a side branch covered with the substance in question, and so arranged that whatever water is formed in an observed number of minutes may be drawn off from time to time and weighed or measured. The water is reckoned as having parted with as much latent heat as is contained in that weight of dry steam.

As to the air-chamber method, it might be thought easy to carry out, but it is difficult to fit a box of any kind so closely to the covering that there will be no circulation of air into and out of the inclosed space. Of course, a lack of tightness will fatally vitiate the experiment. Again, a box surrounding the pipe and covering, presents a large radiating and cooling surface as compared with the covering itself; and there is no ready way of determining the amount of this continual radiation which increases with the temperature of the air within the chamber. There is no perfect non-conductor wherewith we can surround the air chamber, so as to confine therein all the heat received. If we could prevent all outward radiation, the cavity would, sooner or later, acquire the temperature of the steam in the pipe, and all coverings would finally give the same result. The only way to make useful observations would be to start with everything cold, and find the time required to raise the air in the chamber to a given temperature. This is, however, hardly practicable.

We cannot obtain absolute or quantitative results by this method and the comparative figures require indeterminable corrections. Still it was thought advisable to try this plan among others, and see how far the results would correspond to those found by more definitive modes. Accordingly, the apparatus shown in Figs. 18 and 19 was devised and used for this purpose. Fig. 18 shows a transverse section of the whole apparatus as it was mounted, together with the covering and pipe. Fig. 19 represents one-half as seen from the side next the covering. In making the apparatus, pieces of white pine plank, *e*, are squared and rabbeted at the ends to receive the wooden braces, *f f*, which are firmly screwed on. A two-inch hole is bored from the inner side, two-thirds through, to form the cylindrial chamber *c*. The inner side is then planed out so that the concavity *s*, when properly lined, may exactly fit the convexity of the covering to be tried. A hole, *x*, is bored from the top edge down into the chamber *c*. The concavity *s* and the chamber *c* are lined with thick woolen blanket, the large piece *d* being held in place by tacks *n n*.

The halves are clamped on the pipe-covering with the four iron rods *g*, and tightened to a close fit with the thumb nuts *h*. Thermometers, *t t*, are let down into the chambers, a little cotton wool being crowded in around the stems at *x*, to prevent the ingress of cold air.

The two chambers so applied serve to check each other: for if the thermometers differ much, there is a defect in the adjustment.

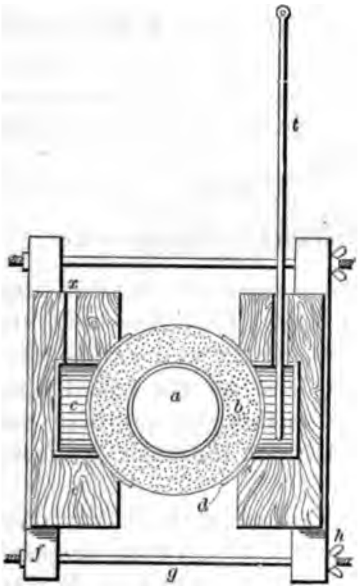


FIG. 18.

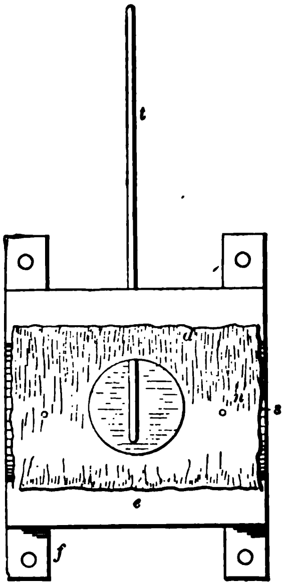


FIG. 19.

A difference of two or three degrees, however, may occur, on account of an inequality in the two sides of the covering itself.

The condensation method is indirect, and therefore a little uncertain. It necessarily assumes that the pipe is all the time filled with dry steam. But we can hardly expect to have pure steam when its generation is going on rapidly, and the vapor passes off through a long pipe which is all the time radiating heat. We can justly expect only a mixture of real steam with more or less mist. And if this mist, which has already lost its 500° C. [932° F.] of latent heat, is reckoned as invisible steam, our figures will not give the exact truth.

And then again, besides the two or three feet of pipe with the covering to be tried, there are necessarily the cap and other fittings which will lose heat in spite of any wrapping that we may put around them. The gross results, therefore, need to be corrected by the amount due to the condensation by that portion of the branch which is not protected by the covering. It is quite possible to find the amount of this correction, if one has the material of the covering so that he can apply it himself and make it uniform. Thus we may cover, say, three feet of pipe, wrap the fittings with a good non-conductor, and make trials enough to get a fair average. Then we may cut off one foot of the pipe and covering, keeping the other parts and their wrappings exactly the same as before, and make a second series of trials. Now, let x be the amount of condensed water due to the three feet of coating, and y that due to the fittings. Having found for one hour $x \times y = a$, by the first trials, and by the other set $\frac{2x}{3} \times y = b$; by combining these equations we get $x = 3\ (a\text{-}b)$; and $\frac{x}{3} = a\text{-}b =$ condensation per foot per hour.

If cutting the pipe is not feasible, the deduction to be made may presumably be ascertained in another way. First, determine the whole condensation by the covered pipe and the well wrapped fittings. Secondly, strip off the covering and try the naked pipe with the wrapped fittings. Thirdly, wrap the pipe just like the fittings, and make more trials. Lastly, strip off the wrappings from pipe and fittings and try all naked.

Now let x = the amount of condensation by the naked pipe alone; y = that by the naked fittings; z = that by the wrapped pipe alone; w = that by the wrapped fittings; and u = that by the covering in question.

By the last determination above mentioned we have found $x + y = a$; by the second, $x + w = b$; by the first, $u + z = d$; and by the third, $z + w = c$. We may fairly assume that $x : y :: z : w$, or $xw = yz$. Then by the various eliminations and substitutions, we have:

$$x = \frac{a\,(b - c)}{a - c};\ y = \frac{a\,(a - b)}{a - c};\ w = \frac{(a - b)\,c}{a - c};\ z = \frac{(b - c)\,c}{a - c};$$

$$u = d - \frac{(b - c)\,c}{a - c}$$

In an actual trial, the result of which is given in No. 25 of the appended table, the slag wool inclosed in straw board, supported by plaster rings at the ends, made altogether a covering 21 inches long. The extra pipe and the fittings were well wrapped with cotton wool, and the condensation was found $d = 108.6$ grams per hour. Removing the slag wool covering, it was found that $b = 344$ grams. Replacing the covering by cotton wool wrapping, it appeared that $c = 105.3$ grams. With all wrappings stripped off, it proved that $a = 428$ grams.

Hence, by substituting these values in the above formulas, we find $x = 316.7$; $y = 111.3$; $w = 27.3$; $z = 77.7$; $u = 81.3$. Then $\frac{12}{21} \times 81.3 = 46.5$ grams per foot per hour.

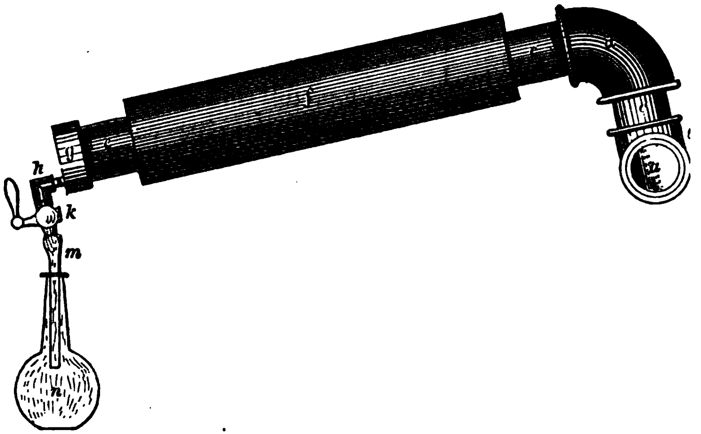


FIG. 20.

For making trials by condensation, I used the arrangement shown in side view in Fig. 20: *a*, the main steam pipe; *b*, a T by means of which the steam may pass freely through the nipple *c*, and the elbow *d* into the branch pipe *e* with its covering *f*. A bit of India-rubber tube *m*, attached to the stop cock *k*, connected with the cap *g*, allows the condensed water—when the cock is opened a little—to pass into the glass flask *n*, without direct exposure to air currents; *d*, *g*, *h*, and the uncovered parts of *e* are wrapped with cotton wool. The angle of inclination is such that whatever condenses in *d* and *c* runs back into the main pipe *a*, unless it remains suspended as mist, and is swept forward.

As to the question of mist, I see no way of settling it except by combining the condensation method, including the correction given above, with the calorimetric method now to be described.

The calorimetric method, besides being direct and absolute, seemed to me to promise a closer approximation to the truth than any plan used hitherto. To carry it out, the contrivance represented in Fig. 21 A and B, and Fig. 22 A and B, was provided of different sizes to suit different coverings.

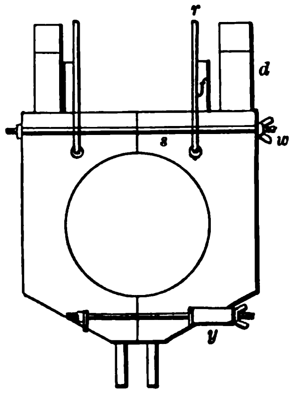


FIG. 21 A.

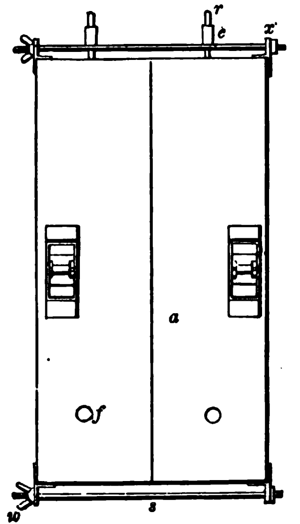
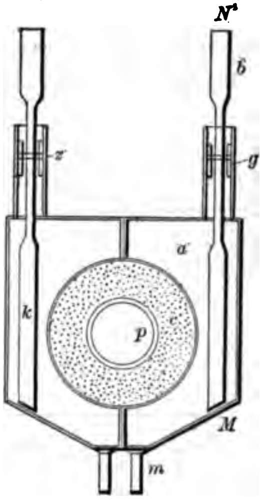


FIG. 21 B.

Fig. 21 A shows a transverse section of the pipe, covering and a pair of calorimeters. Fig. 21 B gives a longitudinal section through the line M N of 22 A. Fig. 22 A is an end view of the calorimeter. Fig. 22 B represents the same as seen from above.

The calorimeters are made of sheet brass No. 29, and are so shaped that when clamped together they may completely and closely include a portion of the pipe covering *c*. The tube *f* serves for the introduction of water, and the subsequent insertion of the thermometer, which is retained in place by the perforated cork *h*. Another pipe in the bottom *m* serves for drawing off the water by removing the cork *n*. The glass tube *r* attached by a caoutchouc connector to a small brass tube in the end, shows the height of the water. The top piece *d*, to the inner sides of which are soldered slotted brass plates *g*, allows the wooden paddle *v* to be swung back and forth on the brass pin *z*, to equalize the temperature of the water. The perforated brass ears *x* and the binding rods *s*, with the thumb nuts *w*, furnish the means of clamping the two halves over the pipe covering. Thick wooden washers *y* give a chance to turn the thumb nuts past the edge of the slanting bottom.

A different mode of clamping is shown in Figs. 23 and 24, the first being a view from above, and the other a side view. Here the pine wood braces *h*, held together by the bolts *r*, are distinct from the calorimeters. This mode of clamping was actually used in most of the experiments, but the wooden braces were not so easy to manage, and they were much in the way of the wrapping. For in use the whole apparatus was covered with cotton batting put on thick and held on by cotton twine wound around in various directions. Cotton wool makes a good and cheap covering, but it takes much time to apply it. Latterly, it seemed best to try other wrappers, and for the sake of greater compactness the form shown in Figs. 25, 26, 27 was constructed. Here the long sheaths *z* receive the wooden rods *x*, which are held by pins at one end and the wooden wedges *w w* at the other. A calorimeter of this form was surrounded by a box of thin wood made much larger, so as to leave a space about 1¼ inches thick all around the brass boxes—the pipes *c* and *f* and the top piece *p*, as well as the gauge *d*, projecting outside the wooden box. The space was filled with nearly two pounds of live geese feathers. The feathers make an excellent non-conductor, but they are rather expensive, and by no means easy to handle.



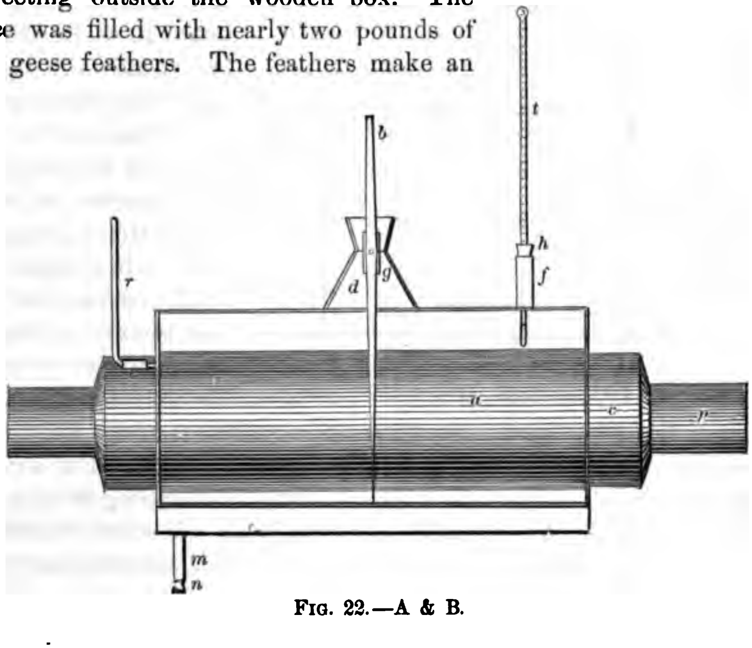


FIG. 22.—A & B.

Then the clamping arrangement of Figs. 21 and 22 was tried, and a cover was made of three thicknesses of very soft woolen blanket sewed on; this is rather costly, but it is, perhaps, the best wrapper to use. A wrapper of hair-felt was tried twice, but it was too tender to be used many times, and it did not admit of sewing on, but had to be held on with twine wound around.

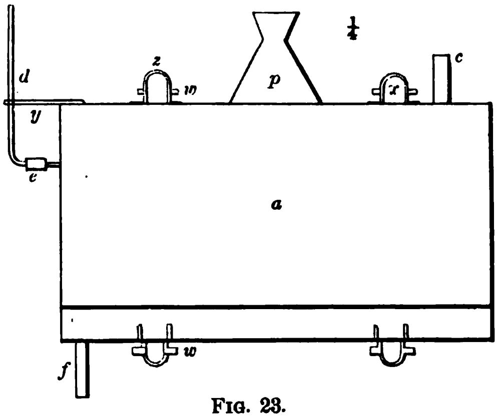


FIG. 23.

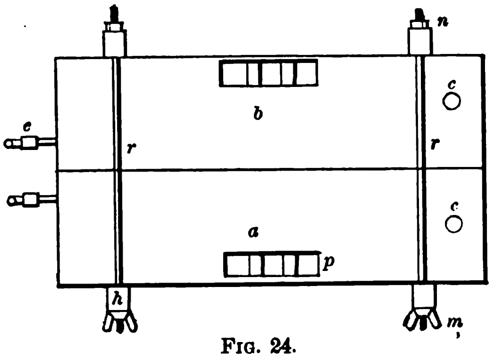


FIG. 24.

In making trials, the calorimeters are filled up with water 10° [50°F.], or 12° C. [53.6 F.] colder than the air of the room, the thermometers are inserted, and the water is well agitated with the paddles. The temperature of the water, the steam pipe, and the air of the room, and the time are noted down. Observations are made every half hour, or oftener, till the water stands 10° C. [50°F.], or 12° [53.6° F.] higher than the surrounding air. The water is then drawn off and weighed. The experiment is repeated times enough to give a fair average.

Of course, all the heat transmitted by the length of pipe covering inclosed by the apparatus is taken up by the water, and could be exactly determined were there no radiation from the calorimeter itself. But wrap as we may, there will still be a loss when the surrounding air is colder than the water. To neutralize the error from this source we should use only that part of the experiment which lies between two observations, in one of which the water is about as many degrees colder than the air as it is hotter in the other; thus the absorption of heat from without in the first part of the time is balanced by the radiation from within in the latter part.

The calorimeter itself takes up heat as well as the contained water, and we must therefore add to the weight of the water as much as corresponds to the weight of brass and immediate surroundings, the specific heat being taken into account. For every calorimeter, this is a constant quantity which may be determined practically by mounting the apparatus on an unheated pipe, wrapping it as usual. Cold water is run in and allowed to stand some time, the temperature being noted. Then the water is as quickly as possible run out and replaced by warm water of known temperature. After a thorough agitation, the temperature is observed, and the warm water is drawn off and weighed.

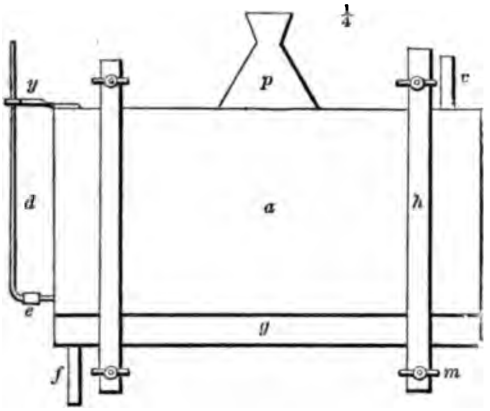


FIG. 25.

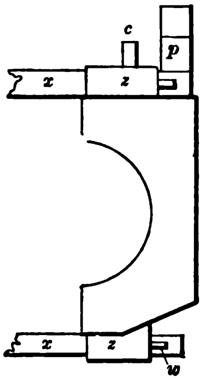


FIG. 26.

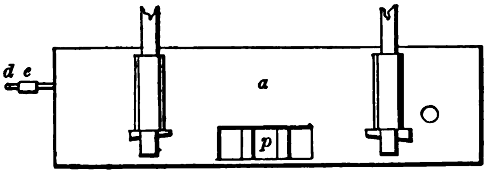


FIG. 27.

Let t = the temperature of the cold calorimeter.

t' = the temperature of the warm water at first.

T = the temperature of the warm water after it is run in, and a = the quantity of warm water drawn out and weighed.

If x = heat units taken up by the calorimeter, reckoned either in grains of water heated 1° C. or in pounds of water heated 1° F.; then

$$T = \frac{a\,t' + t\,x}{a + x}\text{; hence } x = \frac{a\,(t'-T)}{T-t}$$

In an actual trial, the water equivalent of the calorimeters *A I*, *A II* was found to be 194 grams for each.

In an experiment with covering No. 34 of the table hereto appended:

At 9h. 15m., *A I* stood at 12.63° C. [54.73° F.], and *A II* at 12.64° C. [54.75° F.] *

At 3h. 25m., *A I* stood at 42.73° C. [108.91° F.,] and *A II* at 42.18° C. [107.92° F.].

Mean temperature of the air 27.7° C. [81.86° F.]

Interval, 370 minutes.

From *A I* were drawn off 3,260 grams of water; from *A II* 3,320 grams.

Calculating from these data:

$$(42.73 - 12.63)\ (3260+194)\times\frac{60}{370}=16.859^\circ \text{ C.}$$

$$(42.18 - 12.64)\ (3320+194)\times\frac{60}{370}=16.833^\circ \text{ C.}$$

The average of these and trials made on two other days, was one kilogram of water heated 16.671° C. per hour in each calorimeter. But the two brass boxes include 14 inches in length of the covering.

Hence $\frac{12}{14}\times2\times16.671 = 28.579$ kilogram-centigrade heat units, or one kilogram of water heated 28.579° C. per hour by each linear foot of the covering. To reduce this to pound-Fahrenheit heat units, we multiply by $\frac{9}{5}\times2.205$, which gives 113.43° per foot per hour.

Thus we have an absolute measure of all the heat which is trans-

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* Though throughout this report many temperatures are expressed in degrees with two decimal places, it should be understood that these are not actual readings, but in most cases the observed numbers have been corrected according to the calibration table of each thermometer; and in calibrating, it was thought as well to carry out the calculations to hundredths of a degree.

mitted by the covering. But it may, with some reason, be objected that the rapidity of transmission, and therefore the amount of heat passing off from a constant source in a given time, is influenced by the temperature and nature of surrounding bodies; and hence that the communication of heat to a fixed quantity of water is not necessarily the same as that actually given off to air in free circulation. Further experiments are needed to determine exactly how the heat imparted to the water calorimeters compares with that given out to air by the freely exposed covering. We should naturally expect that as water has a higher specific heat than air it would induce a more rapid cooling, and that therefore the water calorimeter would give higher results than the condensation method. But we have a limited quantity of water allowed to get pretty warm as compared with an unlimited supply of cold air. In fact, the coverings No. 24 and No. 25 of the appended table were intended to be alike, and were very nearly so. As the temperature of the steam averaged 150° C. [302° F.] its latent heat was 500° C. [932° F.] Now the quantity of water condensed per foot per hour in No. 25 was 46.5 grams. And $46.5 \times 500 \times \frac{1}{1000} = 23.250$ kilogram-centigrade heat units, while the calorimeter trial of No. 24 gave 22.807°. The difference is not large, and this tends to show that air-cooling and calorimeter cooling are not very unlike.

Any uncertainty as to whether water calorimeters show the actual loss of heat by pipe coverings does not affect their comparative indications respecting different coverings. A more important matter, perhaps, is the not unfrequent impossibility of exact fitting. Coverings that are plastered on are never of uniform thickness, nor are they exactly cylindrical. In such cases the contact of the calorimeters will be more or less imperfect, and radiation through confined air will be partly substituted for direct conduction. On the other hand, yielding coats, like hair felt, are somewhat compressed by clamping on the brass boxes, and yet more by the weight of the filled apparatus; and the more closely fibrous matter is compressed, the greater its transmitting power. So the results of the trials are likely to be somewhat too favorable to the hard and inelastic coverings.

In carrying out the examination of pipe coverings, it seemed best to get samples such that each one could be used for the three methods in succession. Accordingly, circulars were sent out requesting manufacturers and others interested in the subject to furnish whatever specimens they wished to have submitted to competitive trial. The directions called for pieces of ordinary two-inch steam pipe two feet long, cut with a right-hand thread at each end, and then covered, in the usual way, for a length of eighteen inches between the threaded ends. In response to this invitation, thirty-one samples were sent in by various makers, and eight kinds were brought and applied directly to our hot steam pipe in place. In only one or two instances have prices been given.

The room available for the experiments is an iron-turning shop, through the upper part of which runs thirty-six feet of two-inch pipe, conveying to an engine steam of sixty pounds pressure. The engine is run, in term time, from 8.45 A.M. to 12 M., and from 1.30 to 4.30 P.M. During the noon hour the pipe is full of hot but not moving steam. Before entering the room, the pipe runs about 110 feet from the boiler. Two lengths of the pipe in the room were taken out and replaced by as many as possible of the two-foot sample pipes coupled together. Near the middle of a set was inserted a T with a three-quarter inch side connection turned upward. Into this was screwed a bushing furnished with a long thimble reaching nearly to the bottom of the T inside, as is shown in Fig. 28, in longitudinal section. *a* the T; *p* the plug; *n* the thimble, made of a piece of three-eighths in. gas pipe capped with *c* and filed thin. A thermometer *t* suspended in the thimble by means of the perforated cork *s* gives the temperature of the passing steam.

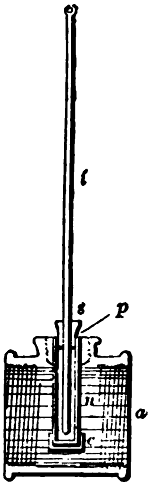

FIG. 28.

Calorimeter and air-chamber trials were made with each covering two, three, or sometimes four successive days. When one set was gone through with, another set was mounted in their place. But several of the samples had been so covered as to leave too little space for a good grip of the pipe wrench, and therefore could not be dismounted in fit condition for connecting again as side branches. Moreover, the number of specimens sent in was unexpectedly large, some makers furnishing many pieces differing more in size than in kind. Hence it was necessary to be content with setting up again for the condensation trials only such uninjured pieces as might represent the different types of coverings.

When the experiments with each piece were finished, the cover, while still at its maximum of dryness, was stripped off, dissected, and weighed; for, of course, the non-conducting power is not the only thing to be considered. We must take into account the cost, weight, bulk, necessary thickness, durability, ease of application, ease of removal, repair and renewal, simplicity, appearance, freedom from smell, temptation to insects or mice, hardness, resistance to moisture, combustibility, liability to crack, and the possible chemical effect on the pipe.

Pipe coverings may be divided into four general classes:

1. Those consisting essentially of light fibrous matter, as hair, slag wool, or paper, applied immediately to the pipe.
2. Those composed of a paste or mortar, which is plastered directly on the pipe, in one or several coats.
3. Those having an air space next the pipe.
4. Complex combinations of different layers.

It will be seen that of all the coverings tried, as shown by the annexed table, the most efficient was simple hair felt with a cheap cover of burlap. It appears also that of the whole number, seventeen owe their efficacy to hair.

Slag wool came third in rank; but it should be noticed that this was a most remarkable covering. The slag wool was two inches thick and was surrounded by wooden slats one inch thick, these being covered with three thicknesses of cloth. So the whole was enormously and absurdly bulky. On the other hand, this wool was not of commendable quality, for it parted with 38 per cent. of heavy globules when it was thrown on a sieve, and this superfluous portion had increased the weight without doing any good. A more feasible covering was tried in Nos. 24 and 25, with the very same fiber after shifting out the shotted slag. This one-inch coating showed a fair result, though of course, by long heating and sifting and handling, the fibre had become much broken, and could not therefore be as efficient as new wool. It was desirable to try new slag wool of the best quality, but the dealers in the article were unwilling to sell a small quantity. No doubt the best kind would give a more favorable result than that shown in No. 24, and would prove really more economical than the cheap sort. I suppose this latter kind is the same substance that is known in England under the misleading name of "silicated cotton."

Spongy paper, as in No. 16, proves to be a tolerably good non-conductor. In a condensation experiment, not given in the table, Reed's covering gave a net result of forty-six grams per foot per hour, which almost coincides with that of slag wool in No. 25.

Straw covered with cotton cloth, as in No. 28, does not show an encouraging degree of excellence.

The otherwise useless rice chaff of No. 18, moistened with water-glass to make it less inflammable and somewhat coherent, proved much more efficient than straw rope.

It should be remembered, fibrous or porous matter acts mainly by virtue of entrapped air, and hence the looser it is the better. Thus everybody knows that hard-spun woolen stuffs do not make warm clothing. Asbestos is commonly supposed to have wonderful virtue in resisting heat, but there is really no magic power in the mineral fiber. It is a non-conductor only when it is in a light, downy condition and full of air. The figures given in No. 50 show that hard-pressed asbestos paper conducts heat very readily. And it was observed that in those cases in which asbestos paper is put between the pipe and hair felt, the asbestos fails to prevent the scorching of the hair. Incombustibility should not be confounded with non-conducting power.

As to the second class, the plastered coverings, none seems to be worth much except the diatomaceous earth or "Fossil Meal," of Nos. 21, 26 and 27. Of only one or two of them was the exact composition known, but there is not one of such excellence that the secret of its composition is worth keeping. Most of the pastes have an admixture of hair, vegetable fiber, or asbestos, to make them tougher and keep them from cracking. The more organic fibrous stuff which can be worked in the better, for it makes the covering lighter and looser and hence less capable of transmitting heat. When such fibers are surrounded by clay, plaster, or other mineral matter, it makes little difference whether they are of themselves combustible or not; they cannot char or burn unless used in connection with steam of extremely high pressure, or superheated steam. So here again, as compared with animal or vegetable fibers, asbestos, which is really a heavy mineral, has more plausibility than positive virtue. Most of the makers of plastered coverings appear to have been experimenting with materials that are too dense.

To the third class, those with greater or less air space, belong Nos. 9, 12, 19, 20, 22, 23, 34 and 37. .

With regard to the efficiency of coverings with an air space, the experiments so far are not decisive, because in no two trials was it certain that the material was otherwise of precisely the same quality and thickness. In Nos. 34 and 36, which were apparently the same, with the exception of an additional wire gauze support in No. 34, the air space showed but a very slight advantage. The comparison of Nos. 16 and 19 is even unfavorable to the narrow air space.

But when there is no visible covering at all, as in Nos. 47, 48 and 51, it makes a wonderful difference whether the calorimeter comes in direct contact with the pipe, or a thin stratum of air intervenes. It seems, too, that a quarter of an inch of air is as good as an inch. This calls to mind the well-known fact that one may safely stay a few moments in the air of a room heated to a point much above the boiling-point of water, as in the old "hot room" of calico print works; but if the skin touches a metallic body or a liquid of the same temperature, burning or scalding ensues.

So it was also observed that when hair or paper remained for a considerable length of time in contact with the hot steam pipe the organic matter became browned or scorched, while the hair felt in No. 9 remained, to all appearances, entirely unchanged, except at the ends where it was gathered in and touched the pipe. It might be thought that the bright tin plate case, as such, had something to do with preventing the scorching; for, from the tradition of Leslie's old experiments on heat, a surface of bright tin is reputed to be a poor radiant and recipient. But when the mere tin case of No. 9 and the straw-board case of No. 20 were put on the pipe, side by side, the tin box soon became hotter than the hand could bear, while the straw-board could be handled.

An air space, then, may prove very useful in obviating one of the great objections to coverings of organic fibrous matter, though it is not specially beneficial in other respects. Woolly asbestos, or asbestos paper, which the makers of some of the specimens appear to have relied on for this purpose, does not accomplish the object, for in all those samples in which a wrapping of asbestos came between hair and the pipe, the hair, after the trials, was found to be discolored by the heat. And then again, experiments Nos. 47 and 50 show that a wrapping of asbestos paper does not insulate so well as the same thickness of mere air. The popular confidence in asbestos partakes of the character of a superstition.

Coverings of the fourth class, those made up of many layers of different kinds, have not proved better or more efficient than the simpler ones; and we may justly set down much of the ingenuity shown in devising coverings of this class as fruitless Of course, complexity enhances the cost, and there should be some corresponding advantage.

But of the actual prices charged, I have received statements in only one or two instances. It is evident, however, from the labor necessarily required to produce some of the specimens, that cheapness has not been kept sufficiently in mind. The question as to whether a covering shall be used or not is one mainly of dollars and cents, and the inquirer must be satisfied that the saving of heat will soon make up for the outlay.

From No. 51, it appears that a naked two-inch pipe, carrying sixty pounds steam, may condense 181 grams per foot per hour, and No. 25 shows that a cheap covering may reduce this to 46.5 grams, making a saving of 134.5 grams per hour, or 1.345 kilos.= 2.96 lbs. of steam in a day of ten hours. So the covering of one hundred feet of pipe would save in a year of 300 working days, coal enough to convert 88,800 lbs. of water into steam. If we consider one pound of coal as capable of making 8.88 lbs. of steam, we shall have a saving of five tons of coal per year for one hundred feet of the covering. So, where coal is worth $5 per ton, it would certainly be worth the while to use a covering costing not more than twelve cents per foot, but we might wish to think twice before taking one worth twenty-five cents per foot.

In some cases it may be worth the while to add a little to the expense for the sake of securing a good appearance and having a covering that can be easily kept clean. An encasement of cotton duck or canvas looks well, whether the cloth is drawn together by the edges and stitched, or is torn into narrow strips and wound around spirally. Except the costliness of this closely woven stuff, the only objection to such a jacket or bandage is its combustibility, and this ought to be obviated by painting the canvas with water-glass. Some of the plastered coverings sent in have a hard, smooth exterior finishing coat, which gives a pretty appearance, but adds too much to the already excessive weight.

The weight and bulk of a covering are of some consequence, for if we add to the pipe three or four times its weight or size of other matter, we make it troublesome to support. A coating over five inches in diameter for a two-inch pipe seems absurdly disproportionate; and as the pipe itself weighs fifty-six ounces per foot, an additional weight of sixty ounces or more is altogether beyond reason. The weights given in the table show that some makers have sinned grievously in this matter. In the large and heavy specimens tried, excepting No. 3, there appears to be a lack of efficiency, and there is little else to commend them.

Of course, for every kind of covering there is an optimum of thickness beyond which the cost and bulk of any addition is not compensated by any further gain in efficiency, and this best size can be approximately determined only by a series of careful experiments with each particular substance or composition. As most of my trials have been of ready-made coverings furnished by others, there are few data for reasoning about the matter of thickness. In comparing Nos. 1 and 2, we see that an increase of hair beyond an inch of thickness, or thirteen ounces of weight per foot, does very little good.

Nos. 27 and 35 were made with the same fossil meal paste, and put on by the same person; and here we see that a much less thickness than one inch of fossil meal is insufficient.

Though Nos. 3 and 24 are not strictly comparable, the two taken together go to show that when poor slag wool is used it will pay to have it considerably more than an inch thick.

As to ease of application, repair or renewal, Reed's covering, Nos. 16 and 19, and the Chalmer Spence Co.'s complex tubes, Nos. 6, 10, 12 and 7 stand foremost. These are molded into form and partially bisected lengthwise—Reed's so as to leave merely a thickness of paper for a hinge, and the Chalmer-Spence through one side of the hollow cylinder—so the tube has only to be opened or sprung apart somewhat, clasped over the pipe, and fastened together at the meeting edges with double-pointed tacks. The covering can be taken off at any time by taking out the tacks and prying the joints apart. Next to these comes hair felt which can be cut of suitable width, clasped around the pipe, and held on by winding string or fine wire around spirally. It may be left so, or cloth can be sewed on around it.

Straw rope can be wound around spirally at a pretty rapid rate, but in time it becomes so brittle that it is worthless when unwound again.

In No. 37, the tin plate cylinders are made in halves which lock together and are more easily put on than taken off. The inner case is held off from the steam pipe, to make an air space, by means of short corrugated tin plate rings.

The tin plate case of No. 9 is made in one foot lengths, with two opposite longitudinal ribs projecting inward. Each length is made in halves, and the ribs are formed by turning in the edges so that they come double when the two halves are put together and fastened with solder.

The cylinders are so joined end to end that their ribs lie in planes at right angles to each other. Both this covering and No. 37 are lacking in simplicity and ease of adjustment.

The air space in No. 19 is made in a ready way by winding around the pipe narrow strips of asbestos paper, some distance apart, before the covering is clamped on. In No. 12 the complex cylinder of hair and pasteboard is held off from the pipe by short, thick paper cylinders.

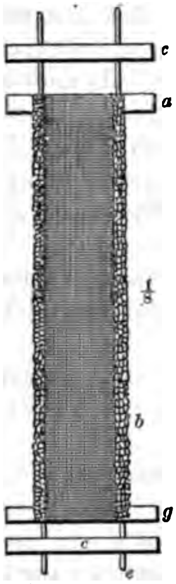

FIG. 29.

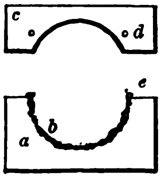

FIG. 30.

In No. 20, the air space was made in a cheap and easy way with rings of plaster of Paris placed a foot apart, and a cylinder of straw board sprung on over them. This straw board had been shaped by rolling it in the machine with which tinkers form stove pipe, and was made large enough to have one edge lap over the other a little. The plaster rings were made in halves, with a groove around the outside to receive the string with which they were tied together on the pipe. Such rings can be cast with little trouble, and they should be well dried before using. They could be made of porous terra-cotta at trifling cost, and it would be better to fasten them on with small wire. The half rings in No. 22 were cut out of thin pine boards with a scroll saw, and the straw board was tacked to them; but pine rings shrink and become scorched, while those of plaster or burned clay are hard, incombustible, and poor conductors of heat. The case of No. 24 was made in the same way, but with an incomplete cylinder of straw board so that there was left, along the whole length of the upper side, a narrow aperture through which the slag wool was crowded in. The long aperture was closed over with a somewhat wider strip of straw board, the whole being finally held together by winding twine around.

The rice chaff of No. 18, the sphagnum of No. 22, and the charcoal of No. 29 were put on with the help of a wire cage specially contrived for the purpose. This is represented in Figs. 29 and 30. The wire gauze *b* is turned at the edges around the long wires *e*, and is tacked to the wooden supports *a*, *g*. The boards *c*, perforated with the holes *d*, are placed on the top of the pipe, the wire cradle is brought under, and the loose wires *e* are slipped through the holes *d*. A sufficiently wide piece of cotton cloth is laid in the cradle, and the hangers *c* are raised up with wedges till the cylindrical part of the gauze is parallel with the lower half of the circumference of the pipe. The filling is now crowded in around the bottom and sides of the pipe, and heaped over the top; the edges of the cloth are drawn together, basted, and then tightly sewed, the hangers are finally slipped off the ends of the wires, and the cradle is taken away to be moved on for making another length. With a little care the cloth edges may be drawn over so as to make the upper half of the covering cylindrical. The cotton cloth used was of the cheapest kind, costing about one cent for a foot of the covering. Of course, a cradle of sheet-iron or of wood could be used, but the wire gauze allows the free escape of any vapor that may be formed during the application of a moist filling to the hot pipe.

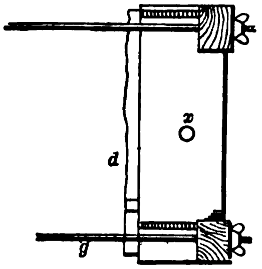

Fig. 81.

It requires some practice to put on paste coverings with a trowel, and it is by no means easy to get them uniform and round. With the exception of the fossil meal, the plastered coverings are worthless when they are taken off.

I have observed no chemical action by any of the coverings, except such as contain plaster of Paris, which, while wet, rusts iron rapidly. The corrosion of pipe, which is said to have occurred sometimes with slag wool that had become damp, must have been caused by the sulphate of lime formed by the oxidation of a trace of sulphide of calcium in the slag.

Respecting durability, little can be learned by trials lasting only a few weeks. But it is well known that animal and vegetable substances undergo a change by long-continued heating, and this sometimes becomes obvious even after a few days' exposure. Wool, hair, cotton and paper in contact with a pipe at 150° C. [302° F.]

soon turn brown, and have their elasticity much impaired. To be sure, it is only a moderate thickness that becomes so affected, and samples of old coverings that have been sent me show that it takes years to scorch any considerable portion of the whole depth.

Straw suffers farther out than the poorer conductors. Specimen No. 39, which was said to have been in use nine years, was still bright and straw-like outside, but the steam pressure had been under fifteen pounds. The straw alone in this sample weighed 4.2 ounces per foot, while the new straw of No. 28 weighed 10.6 ounces. If No. 28 really represents the original dimensions and character of No. 39, as it was intended to, the impairment of efficiency by the shrinkage bears a strikingly small proportion to the loss of weight.

The change of organic matters by a steam heat is too slow to produce any sensible odor, but if by any chance hair-felt gets wet while on the pipe, it gives out an unpleasant smell for a long time. I have known instances in which this proved so great an annoyance that the covering had to be stripped off; and the possibility of such an occurrence is no slight objection to the use of hair in immediate contact with the hot pipe. The intervention of an air space offers a possible prevention of this trouble as well as of the crisping of the hair.

As to the chances for spontaneous combustion of any covering consisting of vegetable fibre, it is difficult to pronounce with certainty. There is a report in circulation that a certain paper covering has taken fire of itself; but I believe this is rather a matter of interested surmise than of positive proof. I put two pieces of the indicted covering on a pipe near the boiler, where the temperature was very high outside and at least 150° C. [302° F.] within the pipe—one of the pieces as it came from the maker, the other charged with cotton-seed oil (this oil readily induces the combustion of cotton waste), and yet both the paper tubes remained so exposed to heat for six months without showing the sligthest inclination to take fire.

Of course, coverings made of organic substances become excessively dry and tinder-like when they are constantly exposed for a long time to steam heat, and then they very readily catch fire when a spark or a flame touches them. Therefore, though there is little danger of fire from within, it is well to guard against fire from without. The impregnation of cloth wrappings with borax, tungstate of soda, or water-glass is calculated to lessen very much the danger from fire.

In connection with the testing of what were offered for fire-proof window shutters some years ago, I was led to believe that one of the best and cheapest non-conductors could be made of water-glass and wood charcoal, since by charring, all gas-forming material is eliminated from the wood, and carbon does not oxidize rapidly when covered with the varnish-like and fusible silicate. It was this mixture that I tried in No. 29; but as there was no light pine charcoal at hand I was obliged, by want of time for making some, to take a rather too dense substitute. Still the result is encouraging, and I hope to follow up the matter farther, for this concreted uninflammable coal is capable of many useful applications.

The rice chaff in No. 18 was also mixed with enough water-glass to render it somewhat coherent when dry, and as the chaff is itself rather silicious, we thus get a covering so charged with mineral matter as to be hard to set on fire, and at the same time quite light and efficient as a non-conductor. Doubtless chopped straw might be used in the same way. But sawdust soaks up so much water-glass as to make a paste that dries too dense.

Coverings that contain flour or meal are liable to be troubled somewhat by mice. Even silicated rice chaff is not altogether proof against them. These animals also gnawed the interior of specimen No. 12.

When it is desirable to have a covering water-proof outside, this can be effected best by putting on a wrapper of sized cloth and applying to it one or two coats of oil paint. Of course, this should be done only after the covering has become perfectly dry. But trouble is sometimes caused from within, by leaking joints, and in such a case a water-proof coat only occasions a spreading of concealed mischief inside. On the other hand, a very porous coating allows the vaporized water to escape, and, if the leak is slight, no harm is done. It is well to use a pretty loose material for covering the joints, to separate those parts from the rest by impervious diaphragms of tin plate or plaster, and to make them so that they can be easily removed without disturbing the other portions.

The following table of specimens tried, Table I., is arranged in the order of their transmitting power as shown by the calorimetric method. The first column gives the source from which each of the samples was obtained, together with a concise description of the make-up, beginning with the coating next the pipe. Those marked "J. M. O." were home productions.

The maximum diameter is given in the second column, few being quite cylindrical.

The weight in the third column includes the average of the whole of the covering, but in many cases the essential part constitutes only a moderate portion of the whole weight. Fuller details of the structure are given in the second table.

The fourth column gives the highest temperature observed in the air chambers during the trials. In one or two instances, the covering was so irregular that the air chambers could not be made to fit closely enough for a fair trial, and so no figure is given.

The numbers in the fifth column show the condensation by each foot of the covered pipe in one hour. "Gross" signifies that the condensation by the fittings and extra pipe is not allowed for, and the figures given are therefore really from one-fourth to one-third too high. The method given above for eliminating this error, was not invented till most of the trials had been made. In the trials made latterly, the word "net" shows that the proper deduction has been made. It takes many days to get the data for the requisite correction, and it is hardly worth the while to spend the time for this, with many samples, till further careful experiments shall show whether the matter of mist really vitiates the results of the condensation method as much as we may suppose it can.

The sixth column shows how many heat units are actually transmitted in an hour by one foot in length of the pipe covering—that is, how many degrees centigrade one kilogram of water may be heated by it, or how many kilograms of water may be raised 1° C.

In the last column the same loss of heat is expressed in degrees Fahrenheit which one pound avoirdupois of water may be heated.

As all the samples beyond No. 30 allow more than twice as much heat to pass through as is transmitted by No. 1, it would seem that in No. 31, and all after it, there is much room for improvement.

The average of the 46 coverings—No. 50 being left out—is 24.623 kilogram-centigrade heat units transmitted.

The average weight is 49 ounces, or a little over three pounds per foot.

TABLE I.

	DIAMETER OF COVERING.	WEIGHT PER FEET IN OZ. AV.	MAXIMUM HEAT IN AIR CHAMBERS.	CONDENSED 1 FT., 1 HOUR. IN GRAMS.	KILO. CENT. HEAT UNITS 1 FT., 1 HOUR.	POUND-FAHR. HEAT UNITS 1 FOOT, 1 HOUR.
No. 1. From Lowell Felting Mill—I. Hair felt, with single cover of burlap................	5⅜ in.	21.4	53.98° C.	40.67 gross	12.842°	50.966°
No. 2. From Lowell Felting Mill—II. Hair felt, with single cover of burlap................	4½	18.2	58.0	....	12.999	51.590
No. 3. Chalk & Lawton, Pawtucket, R. I.—II. Slag wool, wooden cage, burlap, cotton cloth, double....	8	117.8	39.5	48.00 gross	14.465	57.408
No. 4. H. W. Johns' Non-Cond. Covering—II. Asbestos fiber, asbestos paper, hair felt, asbestos paper, hair felt, asbestos paper, canvas................	5⅝	29.3	47.88	....	14.498	57.589
No. 5. Greenwoods Co., New Hartford, Conn. Asbestos paper, hair felt, canvas....................	4½	17.8	61.8	....	15.074	59.705
No. 6. Chalmers-Spence Co., New York—II. Asbestos paper, hair and pasteboard coiled together....	4⅜	30.1	55.65	....	15.718	62.361
No. 7. Asbestos Packing Co.—III. Asbestos paper, double, hair felt, paper canvas.........	4½	19.9	51.61	....	15.761	62.551
No. 8. Asbestos Packing Co.—II. Asbestos paper, hair felt, paper, canvas....	5	18.4	53.46	41.00 gross	16.078	63.809
No. 9. Chalmers-Spence Co.—V. Air space, tin plate case, hair felt, canvas.......	4¾	20.8	57.0	....	17.122	67.952
No. 10. Chalmers-Spence Co.—I. Asbestos paper, hair, and pasteboard coiled together....	4⅝	21.7	62.0	51.00 gross	17.551	69.255
No. 11. H. W. Johns' Non-conducting Covering—VI. Asbestos paper, hair felt, paper, canvas................	4	17.2	61.9	....	17.801	70.647
No. 12. Chalmers-Spence Co.—IV. Air space, asbestos board, hair felt, asbestos paper, hair felt, pasteboard................................	6½	58.1	59.7	....	18.588	73.717

TABLE I.—(*Continued*).

	DIAMETER OF COVERING.	WEIGHT PER FOOT IN OZ. AV.	MAXIMUM HEAT IN AIR CHAMBERS.	CONDENSED 1 FT., 1 HOUR. IN GRAMS.	KILO-CENT HEAT UNITS 1 FT., 1 HOUR.	POUND-FAHR. HEAT UNITS 1 FT., 1 HOUR.
No. 13. H. W. JOHNS' Non-Conducting Covering—I. Asbestos paper, asbestos paste, hair felt, asbestos board, hair felt, asbestos board, canvas	7¼ in.	105.3	48.22° C.	....	19.291°	76.385°
No. 14. J. H. GRAHAM & SON, Boston—III. Clay, paper, hair felt, laths, plaster	5	52.9	....	....	19.423	77.085
No. 15. J. H. GRAHAM & SON—V. Asbestos paper, hair felt, paper, canvas	4½	16.1	65.0°	....	19.632	77.913
No. 16. REED'S Covering—I. Paper cylinder, joint covered with paper	4⅜	80.5	61.0	....	19.670	78.064
No. 17. CHALMERS SPENCE CO.—III. Asbestos paper, hair and asbestos paper coiled together.	4⅜	30.8	53.5	....	20.129	79.886
No. 8. J. M. O. Silicated rice chaff, cotton cloth cover	4¾	22.7	60.0	....	20.208	80.181
No. 19. REED'S Covering—II. Air space, asbestos paper, paper cylinder	4¾	29.3	....	....	20.439	81.117
No. 20. J. M. O. Air space, straw board, hair felt, no cover	4¾	12.0	....	....	20.693	82.124
No. 21. S. C. NIGHTINGALE & CHILDS—IV. Fossil meal and hair plastered on	4¾	60.7	....	58.00 gross	21.151	83.940
No. 22. J. M. O. Air space, straw board, peat moss, cotton cloth	4¾	10.6	....	....	21.631	85.848
No. 23. J. H. GRAHAM & SON—I. Air space, hair felt, laths, plaster	5⅝	63.1	54.4	....	21.820	86.596
No. 24. J. M. O. Slag wool, straw board	4¼	24.1	....	....	22.807	90.510
No. 25. J. M. O. Slag wool, straw board	4⅝	24.8	....	46.50 net	....	....

No. 26. S. C. NIGHTINGALE & CHILDS—I. Fossil meal and hair plastered on	4¼	31.9	64.5	....	22.942	91.050
No. 27. S. C. NIGHTINGALE & CHILDS—II. Fossil meal and hair plastered on	4⅛	26.9	....	....	23.462	93.113
No. 28. W. E. PARKER, Pacific Mills—I. Rye straw rope wound around, cotton cloth 4 ple	4⅛	20.2	64.8	55.00 gross	24.424	96.933
No. 29. J. M. O. Silicated hard wood, charcoal, cotton cloth cover	5	41.9	....	47.00 gross	24.650	97.830
No. 30. J. M. O. Carbon, plaster of Paris, flour, and hair plastered on	4½	33.0	....	....	26.909	106.800
No. 31. ASBESTOS PACKING CO.—IV. Asbestos paste, clay and flax, paper pulp, mortar	5½	100.1	63.0	....	27.411	108.78
No. 32. J. M. O. Dry rice chaff, straw board	3¾	8.4	....	....	27.607	109.56
No. 33. J. H. GRAHAM & SON—IV. Asbestos and clay, laths, paper, mortar	5	58.0	66.0	....	28.159	111.75
No. 34. CHALMERS-SPENCE CO., "Pat. Air Space." Half-inch air space, wire netting, asbestos paste	5	41.0	72.8	74.00 gross	28.579	113.43
No. 35. S. C. NIGHTINGALE & CHILDS—III. Fossil meal and hair	3¼	17.1	....	....	28.882	114.62
No. 36. CHALMERS-SPENCE CO., "Solid Covering." Asbestos paste	4⅛	34.4	74.2	61.00 gross	29.599	117.47
No. 37. CHALK & LAWTON—II. Air space, tin-plate case, asbestos paper, tin-plate case	4	29.0	52.14	....	29.660	117.71
No. 38. EUREKA COVERING CO., Fitchburg, Mass. Meal, clay and hair, meal, clay, sawdust, flax fiber	5¼	81.9	70.0	79.00 gross	30.171	119.74
No. 39. W. E. PARKER, Pacific Mills—II. Rye straw-rope, cotton cloth, 6 ple. (after nine years' use)	4¼	9.8	70.8	....	30.286	120.20
No. 40. ASBESTOS PACKING CO—I. Asbestos paper, plaster and flax fiber	5⅞	111.5	73.8	....	31.267	124.09
No. 41. J. H. GRAHAM & SON—II. Plaster paste	5	67.5	79.8	....	33.477	132.86
No. 42. SAMUEL TAYLOR'S Non-conducting Composition. Clay and short fibrous matter	5¼	94.1	77.3	....	36.782	145.98
No. 43. H. W. JOHNS' Non-conducting Covering—IV. Asbestos paper, asbestos paste	6¼	201.8	69.2	....	37.951	150.61
No. 44. J. M. O. Anthracite ashes, plaster of Paris, flour, hair	4¾	79.2	....	....	39.159	155.41

TABLE I.—(*Continued.*)

	DIAMETER OF COVERING.	WEIGHT PER FOOT IN OZ. AV.	MAXIMUM HEAT IN AIR CHAMBERS.	CONDENSED 1 FT., 1 HOUR, IN GRAMS.	KILO.-CENT. HEAT UNITS 1 FT., 1 HOUR.	POUND-FAHR. HEAT UNITS 1 FT., 1 HOUR.
No. 45. H. W. JOHNS' Non-conducting Covering—V. Asbestos paper, asbestos paste	6½	171.2	70.0°	....	41.079°	163.08°
No. 46. H. W. JOHNS' Non-conducting Covering—III. Asbestos paper, asbestos paper	5¼	99.8	77.8	87.00 gross	43.097	171.04
No. 47. J. M. O. Mere air space	2 15/16	....	....	....	49.241	195.42
No. 48. J. M. O. Mere air space	4¾	....	....	....	50.405	200.04
No. 49. FALL RIVER STEAM PIPE COVERING CO. Clay and refuse of vegetable fiber	4¼	65.2	91.8	....	51.727	205.29
No. 50. J. M. O. Asbestos paper wound round four times	2⅞	....	....	....	56.371	223.72
No. 51. J. M. O. Naked pipe	2 11/32	....	....	181.00 net	391.830	1555.10

TABLE II.

No. 1. Hair felt, 826 grams; burlap and twine, 85g. Length 18 in.

No. 2. Hair felt, 493g.; burlap, 50g.; twine, 17g. Length, 18 in.

No. 3. Slag wool, 2 in. thick, 3,860g.; wooden slats, ¾ in. thick, and nails, 1,815g. wooden rings at ends, 1⅝ in. thick, 540g.; tin-plate rings between wooden rings and pipe, 51g.; burlap, 127g.; cotton cloth two thicknesses, and paint, 270g.; tacks, 13g. Length, 24 in.

No. 4. Asbestos paper faced with loose asbestos fiber, 16½ × 25 in., 205g.; ¾ in. hair felt, 18 × 10½ in., 282g.; twine, 3g.; asbestos paper, 17½ × 18 in.; 112g.; ¾ in. hair felt, 17½ × 14 in., 407g.; twine, asbestos paper, 17½ × 16, 133g.; canvas, 19 × 18 in., 80g. Length, 17½ in.

No. 5. Asbestos paper, 119g.; twine, 4g.; hair felt, 458g.; canvas, 94g. Length, 16½ in.

No. 6. Hair and pasteboard, not easily separated. Whole weight, 1,280g.; Length, 18 in.

No. 7. Asbestos paper doubled, 110g.; hair felt, 486g.; paper, 157g.; canvas and string, 93g. Length, 18 in.

No. 8. Asbestos paper, two thicknesses, 98g.; hair felt, 1¼ in. thick, 16½ in. wide, 16½ × 12 in., 456g.; twine, 6g.; paper, 17¾ × 31½ in., 156g.; canvas, 87g. Length, 17½ in.

No. 9. Tin-plate case and ribs, 580g.; 1 in. hair felt, 494g.; canvas and twine, 94g. Length, 23 in.

No. 10. Hair and pasteboard cemented together. Whole weight, 910g. Length, 17¾ in.

No. 11. Asbestos paper, three thicknesses, 172g.; twine, 6g.; 1 in. hair felt, 552g.; twine, 23g.; paper, 123g.; canvas, 81g. Length, 23½ in.

No. 12. End pieces of paper, 3 in. long, lined with asbestos paper, 575g.; asbestos paper, hair felt and pasteboard cemented together, 1,860g. Length, 17¾ in.

No. 13. Asbestos paper, two thicknesses, 167g.; asbestos paste, 1 in. thick, 2,910g.; ⅝ in. hair felt, 385g.; twine, 5g.: asbestos board, 220g.; ⅝ in. hair felt, 505g.; twine, 6g.; asbestos board, 301g.; canvas, 107g. Length, 18½ in.

No. 14. Clay, 760g.; paper, 115g.; hair felt, 280g.; laths, 430g.; iron wire and plaster, 2,270g. Length, 29 in.

No. 15. Asbestos paper, several thicknesses, 299g.; hair felt, 425g.; twine, 2g.; paper, 198g.; canvas, 137g. Length, 28 in.

No. 16. Alike throughout, 1,280g. Length, 17¾ in.

No. 17. Hair and asbestos cemented together, 1,290g. Length, 18 in.

No. 18. Silicated rice chaff, 1,060g.; wooden rings and cloth wrapper, 120g. Length, 22 in.

No. 19. Asbestos paper rings, paper tube, whole weight, 1,230g. Length, 17¾ in.

No. 20. Wooden rings and straw board, 249g.; hair felt and twine, 443g. Length, 24½ in.

No. 21. Fossil meal and hair, alike throughout, 2,940g. Length, 20½ in.

No. 22. Wooden rings, 97g.; straw board, 192g.; tacks, 5g.; outer rings of paper, 1¼ in. wide, 113g.; sphagnum, 174g.; cloth, 40g. Length, 24¼ in.

No. 23. Two iron rings, 1 in. wide, and tacks, 440g.; ¾ in. hair felt, 218g.; laths, 351g.; wire and plaster, 2350g. Length, 21¼ in.

No. 24. Gypsum rings, 256g; straw board cover, 201g.; slag-wool filling, 940g. Length, 24¼ in.

No. 25. Plaster ends, 253g.; straw board, 175g.; slag-wool filling, 802g. Length, 21 in.

No. 26. Hair and fossil meal, uniform throughout, 980g. Length, 13 in.

No. 27. Fossil meal and hair, uniform throughout, 1,270g. Length, 20 in.

No. 28. Straw rope, 1 in. thick, 400g.; cotton cloth, four thicknesses, 400g; iron rings at the ends, not included in the weight given in Table I. Length, 16 in.

No. 29. Silicated charcoal, 1,760g.; wooden rings and cloth wrapper, 120g. Length, 19 in.

No. 30. Paste of plaster, carbon, flour and hair, 1,560g. Length, 20 in.

No. 31. Asbestos paste, 820g.; clay and fiber, 960g.; paper pulp, 1,560g.; twine, 23g.; mortar, 3,260g. Length, 42 in.

No. 32. Wooden rings, 74g.; straw board, 166g.; tacks, 5g.; rice chaff filling, 240g. Length, 24¼ in.

No. 33. Asbestos and clay, 580g.; wood and wire, 325g.; paper, 332g.; plastering, 2,052g. Length, 24 in.

No. 34. Wire netting and sheet-iron props, 210g.; asbestos paste, 1,340g. Length, 16 in.

No. 35. Fossil meal and hair, uniform throughout, 770g. Length, 19 in.

No. 36. Asbestos paste, uniform throughout, 1,300g. Length, 16 in.

No. 37. Corrugated rings of tin plate, 97g.; tin-plate cylinder, 345g.; asbestos paper, 204g.; tin-plate cylinder, 445g.; tin-plate ends, 30g. Length, 16¾ in.

No. 38. Two kinds of paste not separated, 3,290g. Length, 17 in.

No. 39. Straw rope, 160g.; six thicknesses cotton cloth, 212g.; iron rings at ends not reckoned. Length, 16 in.

No. 40. Asbestos paper, 179g.; three coats plaster with fiber, 4,430g. Length, 17¼ in.

No. 41. Paste, uniform throughout, 5,740g. Length, 36 in.

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DISCUSSION.

Prof. Thurston.—I want to ask the gentleman who read the paper if he can give us any further information in relation to the matter to which reference is made in his second paragraph. He says there: "An examination of the matter showed that neither dye stuffs nor oils were present in these coverings, so the fires could not be ascribed to spontaneous combustion." We had some correspondence in regard to that some time ago. It happened that a number of years ago I was endeavoring to get some information on that subject. I could not find much, and resorted, at last, to the old works on the preparation of charcoal. I prepared a curve, showing the relation of temperatures of preparation to the temperatures of ignition of charcoal. Carrying that curve out, I found that preparation of charcoal at the temperature met with in steam pipes could not give rise to ignition, unless there was a change of law. But, later, Mr. Woodbury has sent me, or induced the author to send me, Mr. Moore's little work on fire insurance and causes of fires, which contains some very valuable matter, especially with reference to spontaneous combustion. I would like to ask if he has any further facts to offer in that connection?

Mr. Woodbury.—I have not with me any special matter with reference to the fire risk from steam pipes, because I did not consider the subject germane to the objects of this Society, but we have had four fires in the last year from steam pipes covered with a mixture of wool waste and wood pulp made into a paper and put around the pipes. There has been one fire this last year arising from the contact of pipes with wood, which was very curious in its nature. It was in a new mill, where the proprietor had taken extreme care that the steam pipes should be at all times, and in all places, away from contact with combustible materials; and in one place there was a steam pipe which extended 100 feet horizontally, and then turned in a vertical direction near a partition, but was wholly free from the partition. When steam was admitted to the pipe it was expanded enough by the heat to press firmly against the wood partition at the end, and ignited a fire which injured the mill to the extent of $75,000. With reference to the experiments which Professor Thurston has cited, and which are quoted in detail in his latest and very interesting work, wherein he has undertaken to prove that steam pipes cannot start a fire, because the ignition point of charcoal, produced by a temperature equal to that of steam at low pressures, is greater than the temperature of steam at any pressure. I agree with the Professor in his statement of the fact that the ignition point of a charcoal exceeds in some ratio the temperature at which the charcoal was formed, but we do not know the temperature at which charcoal is formed by the steam pipe, because there is more or less air circulating around between the pipe and the combustible matter. The actual contact of a pipe with wood is a tangential line, and the wood on each side of that line is merely heated by radiation from the pipe, and its temperature must be appreciably less than that of the steam. The long continued application of the heat chars the wood, and ultimately some slight increment of heat, or possibly some sudden absorption of the oxygen of the air produces a combustion. There is a great deal that is mysterious about it. It is impossible in the laboratory, as a matter of experiment, to repeat it over at will, but in that respect it does not differ from a great many other facts which are forced upon us by our every-day experience.

Dr. Grimshaw.—So far as I know, the number of experiments which have been published in reference to the non-conductive power of material, is, up to the presentation of this paper, very small. There have been some few published on the other side with reference to the radiation from plates. Mr. Charles E. Emery had a very interesting series some years ago. About a year or so ago, I had the pleasure of publishing a test of about a dozen of the ordinary non-conducting materials. I would like to get at a way of comparing these, one with the other. I would like to say that my own tests differ squarely from the views expressed on page 87, where the author speaks of the air space having no effect on the value of the covering. I made a number of tests of different non-conductors, varying the thickness of the air space from one-quarter of an inch up to an inch, and I found (particularly with the mineral non-conductors), that there was more in the air space than in the other non-conductors. I made an experiment about a year and a half ago with various coverings, taking the amount of water condensed both in vertical and horizontal pipes, and after a year I found that some of them exchanged places in the order of non-conductibility. I do not know whether these tests have been carried on with old covering as well as new. I think they have been new. A good many of the plaster coverings are very poor when first started, and improve with age. Then, again, some of the coverings which char decrease in non-conductibility, and others increase. I would like to ask Mr. Emery's experience in that line, which has been rather large and varied.

Mr. Emery.—Mr. Chairman and Gentlemen: These experiments show an earnest effort to get at certain truths in reference to this subject of non-conductors, and it is to be regretted that they were not carried on on a somewhat larger scale, and by methods approximating more nearly those of average practice. I have no doubt of the substantial accuracy of the general results, but methods were available and were suggested to the gentlemen who conducted the experiments, which would have required far less effort, and have given results which could be repeated by any person here present in simple, practical ways.

All of the tests stated in the paper are subject to criticism. The practical question is how much heat is lost by the conduction of steam through pipes, covered or uncovered. As the heat passes away the volume of the steam is reduced, and a portion becomes water. The amount of water is an exact measure of the quantity of heat that passes off. If the experiments be made on a sufficiently large scale, there is no method so simple as to measure the water directly. There is no difficulty whatever in putting up—with the plenty of capital these mutual insurance companies have—inclined pipes 50 or 100 feet long, covered throughout their entire lengths, arranged to receive steam at their higher ends, from the same source of supply through vertical risers, and to discharge the water of condensation separately at their lower ends.

The apparatus shown in Fig. 20 is on the right general principle, but the pipe needs to be long enough so that the ends have no governing influence on the result. Several such pipes covered with different materials should be tried at the same time, and a receptacle under pressure provided at the lower end of each to receive the water of condensation. Each receptacle should be of the same size, and covered with the *same* material, so that the steam condensed in each may be the same, and readily separated from that condensed opposite the covering under trial. The correction for the influence of the ends shown by the formulæ of the writer should be very small and could readily be made so by simply lengthening the pipes, as has been suggested.

The errors due to operating on a small scale also affect the calorimeter and air chamber methods. It cannot be supposed that the calorimeter fitted the various coverings, so the results are partially due to direct conduction from the covering to the metal of the calorimeter and partially due to radiation from one to the other through a more or less thin stratum of air. Again, a correction is due to the fact that the covering extends beyond the ends of the calorimeter. Some correction would be necessary even if the exterior covering of the whole apparatus, including ends of pipe, were absolutely non-conducting, for the reason that the influence of the calorimeter extends over a larger area than it covers. In the middle of the calorimeter the heat passes radially outward in right planes, and in due time a regular gradation of temperature from that of the steam to that of the calorimeter is established throughout the thickness of the covering. Just beyond the calorimeter, however, this gradation in right planes must cease, for there is nothing to absorb the heat (or it is absorbed at a different rate). The effect is that the heat establishes gradients in diagonal lines through the covering from points on the steam pipe beyond the calorimeter to the outer limits of the latter. Exact quantitative results are therefore impossible and may err widely for so short an apparatus. The comparative results of trials with different coverings are also affected, for the conductor with lowest resistance or the poorest non-conductor will absorb heat the farthest away from the limits of the calorimeter. The same reasoning applies to the limits of the air chambers in Figs. 18 and 19.

I may add that Prof. Ordway wrote me asking if I would send him some mineral wool fitted on a pipe two feet long. I did not at that time know anybody who was using mineral wool on pipes in that way. The New York Steam Company used it only in bulk. I wrote the professor to ascertain the proposed methods of testing, made some suggestions and turned his circular over to the agent of the Mineral Wool Company. Whether anything was ever done with it I do not know, but the evidences appear here to be that there never was; that the company never sent on any samples or troubled themselves about tests on such a scale; but it appears that some firm in Pawtucket furnished some wool which was very bad with a large amount of what they call "shot" in it. In regard to the experiments which I myself conducted, I may say that they were for an entirely different object. There some material which could be used in bulk was to be put in conduits about pipes under ground, and it was desirable to find the material best suited for that purpose. The experiments necessarily were to be short; therefore they were conducted on an unusually large scale and with an actual refrigerating apparatus. The results were strictly comparable. The whole apparatus was covered with hair felt, so the condensation at the ends was inconsiderable compared to the actual refrigerating influence of running water about the covering undergoing test. Hair felt is the best material readily obtainable when non-conducting properties simply are considered, and mineral wool comes next. At the present time there is in the market a covering of mineral wool which is enclosed in paper and readily applicable so as to give a reasonable thickness about the pipe. It would be very interesting to know what the result of a test of it would be. The fact that no mineral wool was furnished by the company perhaps caused the experimenter to neglect to state some of its advantages. He notes the necessity of durability and non-combustibility; but I observe that he discusses everything but the mineral wool with reference to these subjects. Certainly when mineral wool is kept dry it maintains its condition permanently, and it is absolutely non-combustible. Mention is made of corrosion in the paper. What is said is true of hair felt, mineral wool or any porous covering when the same becomes moist.

As to the question of mist, which has been raised here, it would never have clouded the mind of the experimenter had the experiments been made on a large scale under practical conditions. In experiments on a large scale the comparisons are made when the conditions become uniform. Everything is to be heated up at first and there will be more condensation the first half hour than there is the second, and so on; and as soon as the condensations become uniform, equal amounts in equal times, it is proper to take the results of the last interval, because the rate of condensation is then established under the conditions of actual practice. Under such conditions, there would be just as much mist in the pipe at the beginning as at the end of a trial.

Again, these experiments were intended to ascertain the liability to fire from coverings of steam pipes, and yet it does not appear that there were any tests made to determine that question. Fires have occurred in my experience where the steam pipes pass through partitions in neglected places and where return pipes come through holes in the floor or holes in the partition and dust collects on them. When the latter becomes highly heated, certain changes take place which cause a fire to occur in that particular location. I have found that when there is a slight steam leak the wood becomes charred very rapidly. That is particularly the case under ground where the temperature can rise very high. Wood when exposed to a steam bath that way becomes rapidly charred, whereas it will stand a very great heat without charring where there is no steam leak.

I have recently made an experiment to determine the condensation in four miles of mains. They run from 16 inches in diameter to 6 inches. Most of them are large. Out of the four miles I should suppose that two-thirds were over 15 inches in diameter. It requires 180 horse power for mains which will carry in due time 8,000. It shows how perfectly insulation can be procured if proper means are taken to secure it. The mineral wool, with a minimum thickness of 4 inches, is put about the pipe in trenches which are generally of brick, for the large pipes, with a board covering, over which tarred paper is placed to exclude moisture. The smaller pipes are covered with a wooden casing. The space between the pipe and casing, about two and a half inches all around, is filled with mineral wool. This plan gives the highest resistance because the wood is a very good non-conductor.

I would further call attention that the paper is written in units of the metric system, and shows at once the annoyances which occur by a *partial* change of system. I don't know anybody who has given the matter thought but that would like to have a decimal system. But until a change is made by law, and possibly it will have to be ade by military force, there is very little comfort in reading a aper in the metric system. I tried to compromise the matter with he Civil Engineers by suggesting that both systems should be given, ut that the regular reading of the text be given in the English easures, and the metric measures in parenthesis. I would sug-est that some of the observations noted in this paper be given in he English measures as well as the final results.*

Mr. Barr.—I should like to ask Mr. Emery a question. Among he other things which would be taken into account is the possible hemical effect on the pipe. I think it was at the Philadelphia eeting something was said with reference to the chemical action f mineral wool upon wrought iron pipes. Now, I should like to now whether in his experience he has found that to be enough to mount to anything practically ?

Mr. Emery.—There is no action whatever of mineral wool on he pipe except when the wool becomes moist and is allowed to lie hile in that condition in contact with the pipe. I have seen ex-ctly the same effect, however, with hair felt under the same con-itions when there was a leak. The hair felting lay in a mat and ust collected on the pipe under the felt. I have seen the same esult with plaster covering. It is not the covering which is the ause of the damage ; it is the fact that moisture is there and it ab-orbs carbonic acid from the air. In the case of mineral wool there ay be a formation of acid compounds which hasten the action. t is perhaps in that respect very much like the difference between ater gas and coal gas. Water gas will kill in eleven minutes and oal gas in fifteen minutes, or something in that proportion. The uestion of corrosion does not enter into our steam system in this ity in any sense. The steam is on continuously night and day and ever has been shut off since it was started, nearly two years ago. he pipes are continually dry and there is no action seen, and mere xposure to steam will not cause corrosion. I would not advise nybody to lay steam pipes under ground for winter work only and over them with anything whatever. The true way would be to ave a tunnel which could be kept dry. This matter was brought p at Columbia College where they had return pipes. The pipes aid in the ground without any covering did not deteriorate as apidly as those which were covered. The covering was protected y an outer casing which kept in the moisture, due to leaks and ab-

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* The temperatures by Fahrenheit scale have been added in the body of the aper.

sorption from the soil. When the soil is porous the water ru away, and there is nothing to keep it in contact with the pipe.

The President.—When I became connected with the Calum & Hecla in 1874, the custom in carrying steam pipes was to mak a trench in the ground, put the steam pipe in and shovel the di back. There were pipes to be found there of several hundred fe in length which were laid in this way, to carry steam to run th works. In an 8-inch pipe 500 feet long, which was laid in a tunne the condensation was several hundred gallons per hour. There wa a separator placed near the engine and the water was taken awa through a trap. The condensation was enormous. A covering of plaster of Paris and sawdust was applied; it cost about 12¼ cents per square foot of pipe covered. Before the covering got dry there was a good deal more condensation than on the naked pipe. But subsequently when the covering was thoroughly dry, the condensation was less than one per cent. of what it was before the pipe was covered.

This calls to mind some experiments made by Mr. Isherwood in 1863, at the Vulcan Iron Works in Baltimore. He had an apparatus made of boiler plate which was very much in the form of a "Gold" radiator. It was composed of boiler plate with ten feet of surface, I should say, on each side. This was put into a place where it could be in perfectly still air, or where a current of air could not strike it. Means were provided for drawing off the condensed steam, and steam at about sixty pounds pressure was put through it. The practical result of all the experiments was that one inch of hair felt applied would prevent ninety-five per cent. of the condensation and that any additional thickness could only, save a total of five per cent. Now, we cover our large Calumet boilers, which expose hundreds of feet of surface, with about two inches and a half of plaster and sawdust, and one inch of hair felt outside that, and we find that our condensation does not go up higher in the winter than in the summer, notwithstanding the fact that the temperature goes down to nineteen degrees below zero. And I have actually seen within six feet of a nest of boilers, burning at the rate of twenty tons of coal in twenty-four hours, the thermometer standing at zero. I think that the criticism made by Mr. Emery on this method of trial is perhaps a very just one. We want experiments on a large scale. A man does not often make a steam pipe to connect his works a foot and a half long. It takes large experiments to get at true results.

Mr. Oberlin Smith.—Did you say the proportion of the mixture?

The President.—About one part of plaster and two parts of sawdust. The plaster and the sawdust are mixed up like mortar. Before using this covering our pipe lines could be located in winter by the melted snow line above them.

Mr. Emery.—Are those works going night and day?

The President.—Yes.

Mr. Oberlin Smith.—I think that is a very interesting fact with regard to the sawdust and plaster being so cheap. Was the plaster mixed in a fluid state first?

The President.—It was mixed up as you would mix ordinary mortar. The plaster and sawdust were first put in together dry, and then wet and mixed up. This covering was used at the Franklin Institute Fair in 1865, being applied to a boiler which was used to run the machinery. I obtained the idea there, and having had some experience at Lynn with asbestos, which was very expensive, it was very gratifying to get hold of something that was cheap. We usually put on the sawdust and plaster from one and a half to two and a half inches thick.

Mr. Holloway.—I was shown this covering not long since. It was divided by wooden segments.

Mr. Leavitt.—That was for boilers. What I have previously described was for pipes. If the Society would like, I will describe the method exactly. We take wood battens three-quarters by two and a half inches wide. We put between the edge of the batten and the boiler, about half an inch of this compound. These are fastened all around the boiler. We then take a band of hoop iron, putting it around, and fill between the battens with plaster. I think the mine people have adopted the practice of putting it on in little blocks about a foot square. Outside of that the specifications call for an inch of hair felt and canvas. We keep very accurate records of the daily evaporation, and I find that it is very uniform.

Mr. Oberlin Smith.—You wrap them with wire, I suppose, when you put them on pipe?

The President.—Yes, with wire or hoop iron.

Mr. Emery.—There is one point I left untouched here. The paper attempts to discuss how much we can afford to pay for a square foot for covering. That is not entirely dependent upon the actual cost; for with a comparatively small amount of water in the steam, we who have to do with steam know that the cost of power in an engine is greatly increased. In actual practice, I have found that by the methods adopted here we get steam half a mile away just as good as it is at the boiler. Of course there is a little water running along the bottom of the pipe, and if in any way that becomes mixed with the steam, it has to be separated again. Evidently there was considerable loss, after all, in the case mentioned by the Chairman. It is not to be expected that there should be very much difference between winter and summer. The differences of absolute temperature are not very great; but that there was a very considerable loss in the particular case mentioned by the President is shown by the fact that the snow was melted off the ground above the pipes. In our steam system here it is not possible to tell where the pipes are, excepting at man-holes. The ice and snow will remain above the pipes just as long as in any other part of the street. In covering boilers with the sawdust compound they put on an inch of felt besides the sawdust, and in Mr. Isherwood's experiments the felt was found to be sufficient without the sawdust.

The President.—We put this felt on because we wanted to save the additional five per cent. We have a large number of boilers which have never had felt put on them at all. Mr. Holloway will bear me witness that he saw a number of such boilers there, and we do not find practically any difference. But we did find, when we had boilers set in masonry, that our consumption of coal in the winter would go up from thirty-three to fifty per cent. over the summer consumption, showing what the advantages of protection amount to.

Mr. Woodbury.—In way of replying to certain matters which have been introduced, I would say, first, that if the condensation method had been attempted on long steam pipes, simultaneous calorimetric measures would have been required at either end of the pipe, because *the steam* would in all probability not be saturated and in exact conformity to the steam tables, but would be either superheated or wet, and therefore the water condensed in the pipes would not truly represent the amount of heat lost by radiation, except when the results were computed in connection with observations upon the thermal value of the steam fed into the pipe.

The question of size and length of pipe received due consideration before this series of experiments were begun. With the apparatus at Professor Ordway's command, I have not the least doubt that his measurements would have been made correctly and precisely even on smaller lengths of pipe. In regard to the effect of plaster upon pipe, I would like to call your attention to the fact that mortar (hydrate of lime) does not corrode iron, but plaster of Paris (sulphate of lime) will corrode very readily in the presence of moisture.

The President.—The fact may be of interest that in the English Navy they whitewash the boilers. Formerly locomotive boilers were whitewashed. Where we have had leaks with this plaster of Paris covering, we have not found any trouble from corrosion. If there is a leak we cut it (the plaster) out and caulk the boiler.

Prof. Egleston.—I would mention the fact about the bridge at Grenoble, which was built in the year 1626, and was destroyed by fire in 1837, that all the iron surrounded by mortar was found to be intact and just as bright when taken out as it was when the bridge was built. On the contrary, that which was not so surrounded, or where there was an air space, was very much rusted.

Mr. Kent.—A series of experiments might be made with a two-inch pipe with a stop cock. Fill the pipe with steam of known quality and then shut it up. Have a pressure gauge on that pipe and note how much time it took to go down to zero; or, at a given time, open the pipe and determine by actual weight how much condensation there is. The idea is to take a pipe full of steam of known pressure and temperature and of known condition. Several experiments can be carried on at once. I think that you could have then a measure of the conductive power of various substances by noticing how long it took the steam to get from one hundred pounds down to zero in a pipe filled with it and covered with a non-conducting material.

Mr. Emery.—I attempted that the other day on a pipe several miles long. We supposed that somebody was stealing steam, and therefore tried the fall of pressure with steam shut off. We found that the variations caused by the masses of material in the pipe and surroundings, the temperature of which had to be changed, caused more difference in the pressure than the mere quantity of steam in the pipe. The steam weighs only, say, a fifth of a pound to the cubic foot, so that the quantity contained in the pipes is very small compared to that generated in drying out the water when the pressure is lowered.

Mr. Levan.—What is the difference between work on week days and Sundays? Would not that make a difference?

Mr. Emery.—The tests were made on Sunday, when the return water showed the amount of condensation.

The President.—This is a very interesting subject, and the paper is very valuable from the fact that it has brought out a discussion of valuable results. I suppose, as a matter of fact, that in ordinary temperatures the greatest loss that will occur without covering the pipes at all will not be over fifteen or twenty per cent. The locomotive people run their locomotives with simply a wooden jacket. They will not put on any other non-conductor, because they say that if anything has to be done to the boilers they want to strip them quick. Taking the amount of boiler surface that they have, it is clear that they get pretty fair results.

Mr. Kennedy.—I would like to have some of the gentlemen give their views on the amount of heat lost with boilers with large grate surface. My experience, so far as it has gone, is that there has been a very large loss there.

CXXXVI.

AMERICAN MACHINERY AT INTERNATIONAL EXHIBITIONS.

BY THOMAS R. PICKERING, PORTLAND, CONN.

THAT International Exhibitions have offered exceptional facilities for the introduction of American machinery into other countries is evident from the fact that many of our manufacturers prefer not to give statistics regarding their export trade. A large proportion of this business is due to the fact that their productions have been shown at one or more of these exhibitions—which of late years have come to be considered affairs of necessity with the principal nations. It is sadly true that the demonstrations which have been made by the United States abroad have been but relative, if measured by the standard of what we might have done were it not for the apathy which Congress has always shown regarding the nation's participation in these affairs.

I consider the great success which attended the Centennial Exhibition was due to the fact that, while it had the sanction of the government, it was strictly the inception of the people, planned by the people, and managed by the people.

The success of our exhibitors at foreign exhibitions has invariably been positive, substantial and remarkable, and a no less important benefit has accrued to the country at large, in that we have been taught additional self-reliance,—and, I think I may add, self-respect. The works entered for competition at such times are judged—not by their standing at home—but in comparison with similar products from all parts of the world.

The judges appointed on these occasions are experts from various countries, and those who have distinguished themselves at one great exhibition, have in many cases been selected to act in the same capacity at others, so that some men come to each new contest with enlarged experience gathered at many world's fairs.

From the exhibition at Paris in 1867, the United States exhibitors retired in honor. The machine-tool exhibits of such representative American firms as took part received the highest commendations from the juries and from engineering experts generally.

The splendid exhibits of wood-working machines of New England and Western firms also redounded to the credit of the country; the strict mechanical simplicity of the designs of one exhibitor being especially commended, while the friendly criticisms on the ornamental scroll designs and fancy style of painting on other wood-working machines from this country, have been received in a proper spirit, and have resulted in the production of more correct designs, and in a much more appropriate style of painting.

The American locomotive was especially admired by railway engineers, and the challenge made by its builders to the builders of locomotives in Europe was not accepted. It brought away the gold medal.

I think it was in August, 1866, that Mr. Corliss, after considerable hesitation, promised to send an engine to operate the machinery in the American section, at the Paris Exhibition of the following year. Word was immediately sent to the Commissioner General for the U. S., then in Paris, who returned answer that the American proposition for furnishing power for the American section could not be accepted, as it contained a clause requiring the Imperial Commission to furnish the building and chimney for use of the boiler (which had been promised by Mr. Harrison, of Philadelphia.)

The proposition was at once renewed, offering to put up the necessary building and chimney. This second proposition was declared "too late, the contract for motive power in the American section having been awarded to a French firm of engineers."

Twelve days after receipt of the letter containing this information I interviewed the commissioner in Paris, and was told by him that it would be impossible to have the contract changed. I then waited on the American Minister, supposing he might have influence enough to bring about the desired result. There, however, I was still more disappointed when he expressed his opinion that "we could live through it, and in fact he thought it would be better to have the power *properly* furnished by a French engine than to have it *improperly* furnished by an American engine." My reply that "the superiority of French engineering should be shown in the French section and not in the American" closed our interview. Not yet satisfied, I visited the office of the Imperial Commission, where regret was expressed at this state of affairs, since it was the especial desire of the commission that each nation should make an exhibit of its methods of transmission by furnishing power for its wn section, and that the contract referred to had been given only fter receiving from the American Commissioner a letter to the ffect that "as he could not find an American firm of engineers in hom he had sufficient confidence, he would request the Imperial ommission to furnish power for the American Section." Now, ark the results. The Harrison boiler was not sent, but the Corliss ngine *was*, and it ran during the continuance of the exhibition—ut without a belt on its wheel, and partly hidden by the French ngine, which was placed prominently in front of it, and which urnished power for the American section in such a very unsatisfactory manner that a protest was drawn up by the American exhibitors.

Notwithstanding these difficulties and disadvantages, the "American Engine" attracted more attention than any other motor in the exhibition, and it was sold after having received the Gold Medal, while its French rival carried off all it deserved in a Silver Medal.

The exhibition of this engine at Paris in 1867 gave such an impetus to the demand for this class of steam motors, that when the exhibition of 1878 opened, the display of engines of the Corliss system built in Europe was so great in those sections, that the entire space allotted to the United States in the Machinery Hall would hardly have sufficed to exhibit them to advantage.

The engine referred to as having been exhibited in 1867, occupies to-day one of the most magnificently fitted-up engine rooms in Europe.

Heretofore English portable engines have been almost without competition at international exhibitions. At Melbourne, however, an American portable engine was exhibited, which, although very plain in appearance, and having little or no "finish" about it, and being of ordinary stock, successfully challenged competition with the best English portables in those essential points of design, material and workmanship, which are so necessary in a good portable engine. This engine was awarded the Gold Medal.

While it is a fact that watches do not strictly come under the head of machinery, it is nevertheless also a fact that the superiority of the American watch (which was so fully demonstrated at Philadelphia in 1876, at Paris in 1878, at Sydney in 1880, and again at Melbourne in 1881) is largely due to the fact that they are produced by the successful application of the system of duplication of parts by special machinery which has been brought to such per-

[illegible] [illegible] of the American mechanical engineer. In proof that the [illegible] of watches produced by this mechanical system is appreciated abroad, I would state that the 500 gold watches, 300 silver watches and 500 movements without cases, sent as an exhibit by the American Watch Co. to Paris in 1878, were, within the first week of the exhibition, all sold to dealers in Great Britain and on the Continent, and that the British Government ordered from this company nearly 400 watches for the use of guards and enginemen on the State Railways of India.

The success which American cutlery is carving for itself is also largely due to the employment of special machinery and therefore [illegible] [illegible] [illegible]. A sample card of some 800 specimens of pocket cutlery was sent to the last exhibition at Melbourne by a Connecticut firm. Some half-dozen of these knives were sent to the jury room, accompanied with a half-dozen ten-penny cut nails and a small package of fine iron chips. The chips had been cut from the nails by the knives. The test was repeated in presence of the jury, and a request made that the best English cutlery be subjected to a similar test. This the exhibitors of English cutlery declined to allow.

Now, it must not be supposed that the British cutlery interest would quietly submit to have American cutlery carry off the highest award. A re-examination was ordered; magnifying glasses and microscopes were brought into requisition. These were of English make, and through them it was discovered that the pearl, ivory and other fine mountings on the English cutlery were better fitted to the metal trimmings than was the case with the American mountings. At the next exhibition the mountings will be in keeping with the metal, or there will be American glasses through which to examine them.

Many manufacturers object to exhibiting on the ground that their best designs will be copied, and while this is often true, it is a compliment to which many foreign manufacturers would willingly submit. Examples of "copying" were quite numerous at Vienna in 1873, at Paris in 1878, as also at Melbourne in 1880-81. The Universal Milling Machines and other special tools exhibited by a Rhode Island firm at Paris in 1867, were purchased by continental tool makers for this purpose, and reproductions were shown at the later exhibitions in the British, the German and the French sections.

A very interesting case of copying occurred at Paris in 1878 where Brown & Sharpe had their usual exhibit of fine machine tools, duplicates of which were shown by a prominent firm in the British section. Now, most of you are aware that Brown & Sharpe do not make the little Universal Chuck which they furnish with their milling machine. If you look on the face of this chuck you may see plainly stamped on it, " E. Horton & Son, Windsor Locks, Conn." *The chuck on the English milling machine bore the same impression.*

When the awards were published the English firm made protest that " whereas the small exhibit of Brown & Sharpe in the American section had been awarded a gold medal, their more extensive exhibit of equally fine tools was to receive but a silver medal." The reply of the chairman of the jury was conclusive: " We cannot give the same award to *copies* that we do to originals."

Another interesting case occurred near the close of the same exhibition. A young man requested that the Surface Grinding Machine, shown by Brown & Sharpe, should be taken apart that he might make drawings of those internal details which could not otherwise be seen and measured. He stated that he had been sent by one of the departments of the Government to get drawings of that machine, and had completed an external view, and did not doubt that I would find pleasure in affording him facilities for completing the drawing " as it was for the Government." I told him as politely as I could that if it was for the Government I would afford him every facility possible, asking him at the same time how nearly he had completed his drawing. He at once handed me his sketch, which I examined, saying to him that I would keep it very carefully until he brought me the request from the department of the Government which desired the drawing, when the machine should be taken apart for him and his sketch returned.

I felt fully justified in this course, because, first, the French Government Commission had promised that no photographing or drawing should be allowed except by permission of exhibitors. Secondly, that if the French Government wanted such a machine it would be much better for it to have an " original " than a " copy " or drawing. As the young man has not yet called for his sketch I feel at liberty to show it to the Society (Mr. Pickering exhibited the paper), and I would also state that before I left Paris the French Government had bought and paid for that machine.

Next to the apathy which Congress has always shown regarding our representation at foreign exhibitions, the most discouraging feature has been the indifference with which this country's exhibit has been viewed by a class of Americans, who, by residence abroad, had lost not only much of their self-respect—but what is much worse—had lost what little respect they ever had for anything American. For this disease, I have so far found but one name; I call it *Toadyism.* Two remedies, however, have, in a measure, proved efficacious; one, the exhibition of American machinery at foreign exhibitions, and, the other, the discovery that to true nobility toadyism is very distasteful.

This disrespect for American productions, as shown by our Commissioner at Paris, in 1867, was the result of a long residence abroad, and that shown by the American minister on the same occasion could only be attributed to the disease mentioned. Another sad case of this occurred at Vienna, in 1873, where, during the Commission troubles, the engineer of our machine section applied to the American minister for advice, which he declined; and the latter seemed to be imbued with the idea that it was beneath his dignity to visit our department. The trouble here proved to be contagious. An intimate friend of the minister referred to, who had been sent out as Commissioner by the State of Massachusetts, reported that "the entire American exhibit—including that of machinery * * * would reflect no credit whatever on a Worcester County fair."

Now, when this minister came to see by the public prints that the Emperor of Austria had remarked—after visiting the entire machinery department of the Exhibition—that *America stood unrivalled in her exhibit of new and practicable inventions*—and when he heard that the Grand Duke Constantine of Russia had spent nearly two hours in our section, and that more than $20,000 worth of our machinery was going to St. Petersburgh; and when he heard of the visit of that most acute observer, Prince Bismarck, who, on entering our section, said he could remain but ten minutes, but who, at the end of forty-five minutes, expressed sincere regrets that he could not remain the balance of the day, finding, as he did, so much more of interest than he thought could possibly be contained in so limited a space; then, but not till then, was it considered to be the proper thing for a certain few Americans in Vienna to visit our department of machinery.

When it came to the publication of awards it was found that our exhibits carried off a larger proportionate number than those of any other nation.

That these lessons have resulted in much good there can be no doubt. The minister, who, in 1867, thought it would be better to have the motive power for the American section properly furnished by French engines, than to have it improperly furnished by American engines, was replaced before the close of that exhibition by a more truly representative man; and the minister who, in 1873, could not recognize the value of American productions until their superiority was publicly pronounced by foreign princes and potentates, was also soon after allowed to retire to private life; and I claim that the present policy of our Government in selecting for its ministers abroad, truly representative men irrespective of their political influence, has been very largely due to the results which have accrued from fair representations abroad of American productions, the most successful of which has been our machinery.

That I am not unduly proud of the enviable distinction which has been accorded to American machinery at the five International Exhibitions at which I have "assisted," I will offer additional evidence to that already given.

Professor Reuleaux, of Berlin, in his report to the German Government, referring to the machinery at Vienna, says that "Upon the field of inventions and inventive genius, there were but few highly remarkable achievements present, and among these America held the highest rank. Her machine exhibition bore almost exclusively the character of originality, * * * and it contained examples of the highest order of constructive ability and perfect workmanship.

"Newly-devised motors, forming part of complete machines and models of distinct parts, exhibited as novelties or inventions, were numerous. In the first direction, one firm of America has accomplished the most. The constructions of this firm—some of which have very rapidly made their way through Germany—bear, in regard to invention, the peculiar, unique stamp of American genius. They are distinguished from us by more direct and rapid conception. The American aims straightway for the needed construction, using the means that appear to him the simplest and most effective, *whether new or old*, while our historically heaped-up material and the cautious character of the German, are so inseparably interwoven, that among the number of known means, we often forget to ask whether they are the simplest, or whether new ones might not be better. The American really constructs in accordance with the severest theoretical abstractions, observing on the one side a distinctly marked-out aim, weighing, on the other, the already available means or creating new ones, and then proceeding, regardless of precedents, as straight as possible for the object. This spirit is manifest in lathes, shafting and bearings, in planing machines, with diagonal screw-shaft, in screw cutting machines, and it is strikingly prominent in that system of screw-threads which has boldly been placed alongside of the old venerated Whitworth system, in spite of the terror of its numerous adherents, after actual deficiencies have been discovered. A proper valuation of this proceeding contains the most instructive hints for our higher technical institutions."

So much for German evidence. The following is from a French authority. "The Chronique de l'Exposition, July 2, 1873, but referring to an American machine exhibit of 1867, says:

"The complete and varied exhibit of American machines at Paris in 1867 commanded naturally the attention of all the earnest visitors, and one could say, without fear of being charged with exaggeration, that the most competent men to judge about the merit of new mechanical inventions have found more matter for study, more original construction, and more real novelties in this American machine group, than in all the groups of similar machines in the entire Exposition." Even that most reluctant of all witnesses (a certain class of Americans already referred to excepted), the London *Times*, said, referring to our exhibit in 1878, that "The preeminence of the mechanical genius of the citizens of the States may be admitted, and it is illustrated, not for the first time, in the exhibition at Paris, and it may almost certainly be predicted of any modern mechanical congress that the Americans will carry off the palm for novel and ingenious application of force to practical purposes."

That this evidence, as given by Prof. Reuleaux and other witnesses, was considered trustworthy is shown by the wording of the recommendation of the group jury, which was endorsed by the council of Presidents, giving to the representative American firm the Diploma of Honor "*For pre-eminent achievement in the invention and construction of machine tools, many of which have been adopted as patterns by the constructors of tools in all countries.*

As the value of a gift rises in our estimation by reason of the eminent qualifications of the donor, and his honor and uprightness as a man, so the value of this award is enhanced by the knowledge that it was worded by a man whom mechanical engineers the world over delight to honor—Dr. John Anderson.

There can be no question that the common intelligence, which is the result of a thorough and general education, together with the efficiency and moderate cost of patent right protection in the United States, should have a great share of the credit of this "pre-eminence of American mechanical genius." The action of our Patent laws is so secure and equitable that the investment in brain labor is a safe one.

Let us cherish and appreciate these advantages, and while we take the lesson to ourselves, let us show to the world at large that, in the wise economy of nations, ideas are better than blows, and brains are better than blood; and that while we believe in what we consider judicious protection for our manufacturers, we also believe in free trade in genius.

DISCUSSION.

The President.—I am sure, gentlemen, we have all been very much interested in Mr. Pickering's paper; it is a paper perhaps that it would be difficult to discuss. I would like, however, to relate a little anecdote which bears on this matter and which was recently told me by Mr. Corliss. You will remember that at the Centennial there was a very handsome pair of horizontal Corliss engines built by a Belgium firm. I remember them especially because of a criticism made by Mr. Porter. The cranks had very thick hubs, and there was about two inches of collar between the hub and the bearing. Mr. Vanderkerchove, on viewing Mr. Corliss' engine, made an arrangement by which Mr. Corliss was to furnish him with plans of Corliss engines as built by himself and also to furnish plans of all subsequent improvements that he might make. Consequently it was Mr. Corliss' practice to send him drawings of everything new that was brought out in his own establishment. Mr. Vanderkerchove was employed by one of the large manufacturers of Belgium to construct an engine which is in all respects similar to a single one of the Centennial engines. This engine on being started gave such admirable results that the attention of the King of Belgium was brought to it, and he appointed a committee to investigate the matter and report upon it, which they did. The result was that the king knighted the manufacturer who bought the engine and Mr. Vanderkerchove who built it, with the statement that this dignity was conferred because of his introduction into Belgium of the invention of "one Corliss, an American." (Applause.)

Mr. Holloway.—I want to say in regard to this paper of Mr. Pickering's, although it applies more especially to international exhibitions, I am pleased to say that on a recent hasty visit I paid to Louisville, where the Southern Exposition is being held, which by the way is a very fine one, I noticed that the suggestion he makes of having the engines in motion is carried out very fully. There can be seen there some of the best engines of some half a dozen makers all driving machinery. The exhibition, as a whole, is a very fine one indeed.

Mr. Strong.—I was in Vienna the year following the exhibition and I met an engineer who was one of the commissioners at that exhibition, and he remarked on the extreme ingenuity displayed in the design and in the carrying out of the mechanical ideas in all the American machines he had seen, or the greater portion of them. Another remark he made was in regard to the difference between American mechanics—that is, laboring mechanics—and their mechanics. He wanted to know why it was that American mechanics in doing their work used their heads as well as their hands. He said an exhibit was made there of a barrel-making machine, and the man in charge of the work was a one-armed man. "Now," he said, "that one-armed man would do more work than any four mechanics I could pick up here."

Mr. Charles E. Emery.—Mr. Holloway's remarks remind me of the method adopted at the first fair of the American Institute in 1868. I was the General Superintendent at that time, but my attention was turned particularly to the machinery. The arrangement made there fulfilled, I think, the conditions suggested by the writer of the paper. All the engines were belted to pulleys on the main shaft, and there was a clutch provided for each of the pulleys. Only one clutch was engaged at one time, though the other engines ran their pulleys as usual. A card was hung over the particular engine which was doing the work, and the load was changed daily from one engine to the other. The system worked very well there. The reminiscences of exhibitions given by our friend Mr. Pickering are very interesting and instructive. There are a number of special features which could be added that might not be pertinent here. The little bickerings that occur at all exhibitions are interesting as showing the competition between one country and another, and even between one exhibitor and another, and accounts of the amusing and instructive incidents would, in themselves, make a large book.

Dr. Grimshaw.—I remember calling attention to several cases of very flat piracy during the Paris Exposition in 1878. In one case there were two agricultural machines, one from Canada and one from Sweden, both of which had the same pattern marks on the castings as the American machines with which they were in competition. Later on, my attention was called by an exhibitor to some locks which were exhibited by an Austrian firm in the Austrian Department, in competition with that exhibitor's American locks. A careful inspection revealed beneath the enamel—the very sheet marks of the American firm—the paint marks on the iron sheets. In other words, they were American locks, exhibited in competition with our own locks. I will speak of another case in which a piece of wood-working machinery from Cincinnati was purchased by a Manchester firm, almost with the avowed purpose of copying it. It was copied part for part. It is known that American castings are not usually much heavier than the law allows. The fact is, complaint is sometimes made that we do not put quite enough metal in. This wood-working machine, which was copied square inch for square inch of section, went to pieces in about three months' use. They had not calculated for the difference in the quality of the iron on the other side, and on the inexperience of the hands on the other side in making tough light castings. It was an awful warning which may be of use in the future.

Mr. Hobbs.—I happened to be at the exposition in London, in 1851. Whitworth & Company had some very nice shaping machines there and some very nice planing machines, and I have seen the very same patterns made by Mr. Sellers in Philadelphia. Mr. Sellers began by copying some very nice machines, and he has made better machines since. So I think that Mr. Sellers and Mr. Whitworth are about even in that particular. As to locks—I don't know much about locks; but I will tell a story, too. I went to the exhibition in 1851. I went out in an American ship. I had a little box with me which was full of various instruments, all honest, but they looked like burglar's tools, and when I landed at Southampton the Custom House people did not want to pass it. I went to the American Consul and told him what it was and he got me through. When I got through, I told the Consul what I intended doing, but I said, "Do not say anything

The splendid exhibits of wood-working machines of New England and Western firms also redounded to the credit of the country; the strict mechanical simplicity of the designs of one exhibitor being especially commended, while the friendly criticisms on the ornamental scroll designs and fancy style of painting on other wood-working machines from this country, have been received in a proper spirit, and have resulted in the production of more correct designs, and in a much more appropriate style of painting.

The American locomotive was especially admired by railway engineers, and the challenge made by its builders to the builders of locomotives in Europe was not accepted. It brought away the gold medal.

I think it was in August, 1866, that Mr. Corliss, after considerable hesitation, promised to send an engine to operate the machinery in the American section, at the Paris Exhibition of the following year. Word was immediately sent to the Commissioner General for the U. S., then in Paris, who returned answer that the American proposition for furnishing power for the American section could not be accepted, as it contained a clause requiring the Imperial Commission to furnish the building and chimney for use of the boiler (which had been promised by Mr. Harrison, of Philadelphia.)

The proposition was at once renewed, offering to put up the necessary building and chimney. This second proposition was declared "too late, the contract for motive power in the American section having been awarded to a French firm of engineers."

Twelve days after receipt of the letter containing this information I interviewed the commissioner in Paris, and was told by him that it would be impossible to have the contract changed. I then waited on the American Minister, supposing he might have influence enough to bring about the desired result. There, however, I was still more disappointed when he expressed his opinion that "we could live through it, and in fact he thought it would be better to have the power *properly* furnished by a French engine than to have it *improperly* furnished by an American engine." My reply that "the superiority of French engineering should be shown in the French section and not in the American" closed our interview. Not yet satisfied, I visited the office of the Imperial Commission, where regret was expressed at this state of affairs, since it was the especial desire of the commission that each nation should make an exhibit of its methods of transmission by furnishing power for its own section, and that the contract referred to had been given only after receiving from the American Commissioner a letter to the effect that "as he could not find an American firm of engineers in whom he had sufficient confidence, he would request the Imperial Commission to furnish power for the American Section." Now, mark the results. The Harrison boiler was not sent, but the Corliss engine *was*, and it ran during the continuance of the exhibition—but without a belt on its wheel, and partly hidden by the French engine, which was placed prominently in front of it, and which furnished power for the American section in such a very unsatisfactory manner that a protest was drawn up by the American exhibitors.

Notwithstanding these difficulties and disadvantages, the "American Engine" attracted more attention than any other motor in the exhibition, and it was sold after having received the Gold Medal, while its French rival carried off all it deserved in a Silver Medal.

The exhibition of this engine at Paris in 1867 gave such an impetus to the demand for this class of steam motors, that when the exhibition of 1878 opened, the display of engines of the Corliss system built in Europe was so great in those sections, that the entire space allotted to the United States in the Machinery Hall would hardly have sufficed to exhibit them to advantage.

The engine referred to as having been exhibited in 1867, occupies to-day one of the most magnificently fitted-up engine rooms in Europe.

Heretofore English portable engines have been almost without competition at international exhibitions. At Melbourne, however, an American portable engine was exhibited, which, although very plain in appearance, and having little or no "finish" about it, and being of ordinary stock, successfully challenged competition with the best English portables in those essential points of design, material and workmanship, which are so necessary in a good portable engine. This engine was awarded the Gold Medal.

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The success which American cutlery is carving for itself is also largely due to the employment of special machinery and therefore claims our notice. A sample card of some 800 specimens of pocket cutlery was sent to the last exhibition at Melbourne by a Connecticut firm. Some half-dozen of these knives were sent to the jury room, accompanied with a half-dozen ten-penny cut nails and a small package of fine iron chips. The chips had been cut from the nails by the knives. The test was repeated in presence of the jury, and a request made that the best English cutlery be subjected to a similar test. This the exhibitors of English cutlery declined to allow.

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I felt fully justified in this course, because, first, the French Government Commission had promised that no photographing or drawing should be allowed except by permission of exhibitors. Secondly, that if the French Government wanted such a machine it would be much better for it to have an "original" than a "copy" or drawing. As the young man has not yet called for his sketch I feel at liberty to show it to the Society (Mr. Pickering exhibited the paper), and I would also state that before I left Paris the French Government had bought and paid for that machine.

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So much for German evidence. The following is from a French authority. "The Chronique de l'Exposition, July 2, 1873, but referring to an American machine exhibit of 1867, says:

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That this evidence, as given by Prof. Reuleaux and other witnesses, was considered trustworthy is shown by the wording of the recommendation of the group jury, which was endorsed by the council of Presidents, giving to the representative American firm the Diploma of Honor "*For pre-eminent achievement in the invention and construction of machine tools, many of which have been adopted as patterns by the constructors of tools in all countries.*

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Let us cherish and appreciate these advantages, and while we take the lesson to ourselves, let us show to the world at large that, in the wise economy of nations, ideas are better than blows, and brains are better than blood; and that while we believe in what we consider judicious protection for our manufacturers, we also believe in free trade in genius.

DISCUSSION.

The President.—I am sure, gentlemen, we have all been very much interested in Mr. Pickering's paper; it is a paper perhaps that it would be difficult to discuss. I would like, however, to relate a little anecdote which bears on this matter and which was recently told me by Mr. Corliss. You will remember that at the Centennial there was a very handsome pair of horizontal Corliss engines built by a Belgium firm. I remember them especially because of a criticism made by Mr. Porter. The cranks had very thick hubs, and there was about two inches of collar between the hub and the bearing. Mr. Vanderkerchove, on viewing Mr. Corliss' engine, made an arrangement by which Mr. Corliss was to furnish him with plans of Corliss engines as built by himself and also to furnish plans of all subsequent improvements that he might make. Consequently it was Mr. Corliss' practice to send him drawings of everything new that was brought out in his own establishment. Mr. Vanderkerchove was employed by one of the large manufacturers of Belgium to construct an engine which is in all respects similar to a single one of the Centennial engines. This engine on being started gave such admirable results that the attention of the King of Belgium was brought to it, and he appointed a committee to investigate the matter and report upon it, which they did. The result was that the king knighted the manufacturer who bought the engine and Mr. Vanderkerchove who built it, with the statement that this dignity was conferred because of his introduction into Belgium of the invention of "one Corliss, an American." (Applause.)

Mr. Holloway.—I want to say in regard to this paper of Mr. Pickering's, although it applies more especially to international exhibitions, I am pleased to say that on a recent hasty visit I paid to Louisville, where the Southern Exposition is being held, which by the way is a very fine one, I noticed that the suggestion he makes of having the engines in motion is carried out very fully. There can be seen there some of the best engines of some half a dozen makers all driving machinery. The exhibition, as a whole, is a very fine one indeed.

Mr. Strong.—I was in Vienna the year following the exhibition and I met an engineer who was one of the commissioners at that exhibition, and he remarked on the extreme ingenuity displayed in the design and in the carrying out of the mechanical ideas in all the American machines he had seen, or the greater portion of them. Another remark he made was in regard to the difference between American mechanics—that is, laboring mechanics—and their mechanics. He wanted to know why it was that American mechanics in doing their work used their heads as well as their hands. He said an exhibit was made there of a barrel-making machine, and the man in charge of the work was a one-armed man. "Now," he said, "that one-armed man would do more work than any four mechanics I could pick up here."

Mr. Charles E. Emery.—Mr. Holloway's remarks remind me of the method adopted at the first fair of the American Institute in 1868. I was the General Superintendent at that time, but my attention was turned particularly to the machinery. The arrangement made there fulfilled, I think, the conditions suggested by the writer of the paper. All the engines were belted to pulleys on the main shaft, and there was a clutch provided for each of the pulleys. Only one clutch was engaged at one time, though the other engines ran their pulleys as usual. A card was hung over the particular engine which was doing the work, and the load was changed daily from one engine to the other. The system worked very well there. The reminiscences of exhibitions given by our friend Mr. Pickering are very interesting and instructive. There are a number of special features which could be added that might not be pertinent here. The little bickerings that occur at all exhibitions are interesting as showing the competition between one country and another, and even between one exhibitor and another, and accounts of the amusing and instructive incidents would, in themselves, make a large book.

Dr. Grimshaw.—I remember calling attention to several cases of very flat piracy during the Paris Exposition in 1878. In one case there were two agricultural machines, one from Canada and one from Sweden, both of which had the same pattern marks on the castings as the American machines with which they were in competition. Later on, my attention was called by an exhibitor to some locks which were exhibited by an Austrian firm in the Austrian Department, in competition with that exhibitor's American locks. A careful inspection revealed beneath the enamel—the very sheet marks of the American firm—the paint marks on the iron sheets. In other words, they were American locks, exhibited in competition with our own locks. I will speak of another case in which a piece of wood-working machinery from Cincinnati was purchased by a Manchester firm, almost with the avowed purpose of copying it. It was copied part for part. It is known that American castings are not usually much heavier than the law allows. The fact is, complaint is sometimes made that we do not put quite enough metal in. This wood-working machine, which was copied square inch for square inch of section, went to pieces in about three months' use. They had not calculated for the difference in the quality of the iron on the other side, and on the inexperience of the hands on the other side in making tough light castings. It was an awful warning which may be of use in the future.

Mr. Hobbs.—I happened to be at the exposition in London, in 1851. Whitworth & Company had some very nice shaping machines there and some very nice planing machines, and I have seen the very same patterns made by Mr. Sellers in Philadelphia. Mr. Sellers began by copying some very nice machines, and he has made better machines since. So I think that Mr. Sellers and Mr. Whitworth are about even in that particular. As to locks—I don't know much about locks; but I will tell a story, too. I went to the exhibition in 1851. I went out in an American ship. I had a little box with me which was full of various instruments, all honest, but they looked like burglar's tools, and when I landed at Southampton the Custom House people did not want to pass it. I went to the American Consul and told him what it was and he got me through. When I got through, I told the Consul what I intended doing, but I said, "Do not say anything about it." Said he, "Are you an American?" I replied that I was. He said, "I never met an American of that kind before; they always talk to us and want us to tell everybody what they say. He said, "For goodness sake, do something to help us up at the exhibition. The Americans have about one-eighth of the building in London to exhibit in; there are about three barrels of shoe-pegs and a bundle of brooms. It is a total failure. Do something, if you can, to help us out." I then went to London and saw Mr. Lawrence, who was our minister at that time. I told him the story. He put his arms around me and said, "If you can help us out, do it." After leaving him, in walking through Piccadilly, I saw in a window a lock hung up, with a sign upon it reading, "The artist who will produce an instrument which will pick this lock shall receive two hundred guineas reward." I knew of that before I left New York, and I made a bet of a basket of champagne with some friends that I would get that money. I went to the exhibition and looked at the locks, and thought I knew just how that lock was made. I went in to Bramah's store one day, and I asked the man if that was a lock in the window. He said, "Of course it is." I said, "I would like to look at it." He threw it down on the counter. It was a large padlock. It looked very nice. I had been told that it was bogus. I took my pen-knife out and was feeling the little slides. He said, "What are you doing there, sir?" I said, "I was feeling the thing to see if it would move." He said, "You must not do that, it will injure the lock." I told him I did not think it would, but that I would not do it again. I said, "Do you really offer two hundred guineas to anybody who will open that lock?" He said, "Certainly; are you a lock-maker?" I said, "No." "What do you know about it, then," he asked. "Not much," I replied, "but I am a little curious and would like to try it." A gentleman standing near who looked rather troubled came up and said, "Are you a lock-maker?" I said, "No, sir." He said, "Do you think you could open that lock?" I said, "I do not know, but I am going to try." I then turned to the man and said, "You have hung that lock in the window with a challenge, and it is understood that you offer two hundred guineas reward to anybody who will pick that lock. Now, you don't mean it; it is bogus. You may either take that lock out of the window and withdraw your challenge, or else you shall have it fairly decided. If you do not consent to this I will publish you in every paper in the kingdom. I mean business, and I will either fail or you shall take it out of the window." He said, "I am not the right man. If you come to-morrow perhaps you can hear more about it." It happened the very next morning that the London *Times* had an article describing a case of Hope's jewels—Hope, the banker. He had some thousands of pounds worth of jewels in the case. "The case," the paper said, "is locked with one of the Bramah locks, and we understand that a gentleman picks up the gauntlet and offers to open both Chubb's and Bramah's. I went up the next morning and saw the gentleman. I said, "I am glad to see you mean to come out fairly and have written a letter and offered a reward and I am going to give you a trial, and I think the best way to do it is to appoint arbitrators. You appoint one, I another, and they shall appoint a third. Professor Cowper and Mr. George Rennie were appointed by Mr. Bramah, and I appointed Doctor Black. Professor Cowper said to me, "This is not right, you have chosen only one." I said, "You are the very man I wanted; if Bramah has chosen you, I will choose you too." They made the arrangement that the lock should be secured to a door, so as to have it connected with the door. I told them that when we had locks tested in America we gave them thirty days. The party attempting to open the lock could do anything he pleased in that time, but he must leave the lock in perfect condition when through, and that the owners might use the key in the meantime if they wanted to; so it was agreed that I should have thirty days. I had got to take my measures, make all the instruments myself and do the work, and the chances are always against the operator. I went to work at the lock. The first day I took the dimensions of the key-hole and what slides were in. I then made an instrument which I thought was right, and on the third day I went to operate on the lock; I found in the key-hole, which was three-eighths of an inch in diameter, a pin, one-eighth in diameter, leaving an eighth on the outside. There were eighteen slides. There was a disk under those slides that drew them up and that disk was pressed in by the end of the key as the slides went in at different points. When I put the pressure on that spring I could not move the disk and I did not know but that it was blocked up; but by feeling around I found I could get a pressure on it. I then made an instrument by which I could put a pressure of fourteen pounds on that disk, and then I had to have the fork or prong so that it would pass in one-eighth of an inch diameter. I made the instrument and fitted it with a thumb-screw and went to work; in about five hours I had got the slides all adjusted and I commenced to turn the cylinder around. This little thumb-screw, in working the cylinder backward and forward, had worked up so that when I turned the cylinder around about a quarter of the way it stuck. Everything I had done was lost. Then the difficulty was to find how I should get the pressure. The key had a nib on it which put the pressure on the cylinder and you could turn it around, and when you got it partly turned round that part of the key-hole was covered up. I thought the matter over and I decided to drill a hole in where this key-hole was, so that I could put a pin in and get my pressure. I did so, and in about four minutes, when I got the pressure on, I got the slides right again. I then threw it back to the original position. I got a drill made of the right size and a bit of brass wire to tap into its hole. I twisted the lock back again and got to that hole. I re-drilled the hole, tapped it, screwed in the brass wire and marked where it was to come. I broke the pin off in the hole and worked with the end of a file and got it smooth. I then put a little sulphuric acid on and made it look old I thereby covered up what I had done to get my pressure on the cylinder. I went the next day and opened the lock. I unlocked the lock and locked it again, and left it covered up. But it seems, all this time, there was a window a short distance from where I was working, and some fellows with a telescope were observing me, and they reported when I got it open. The next day I sent for the arbitrators. They came there and saw the lock open. I locked it with my instruments and took my instruments out. Professor Cowper tried to unlock it with the key, but he could not do it. I said, " Professor, you do not work that right. Now, you take that key and press it in as hard as you can before you turn it." The professor did as I said and the lock opened. We had a meeting a day or two after, and Mr. Bramah declined paying the two hundred guineas for the reason that I had not complied with the challenge—which read, the artist should produce "*an instrument.*" Mr. Hobbs had produced *instruments*. Dr. Black and Professor Cowper decided it was rather a dirty piece of business, and decided that he was to give me the two hundred guineas. That finished the Bramah lock.

Before I picked the Bramah lock, I had a letter from Mr. William Brown, the head of the firm of Brown, Shipley & Company, of Liverpool, to come and see him at the St. James Hotel; and he talked with me about everything but locks for a long while. Finally he said, " Mr. Hobbs, I wanted to see you; I have invented a lock and I wanted to show it to you, but I cannot do so for it is in Liverpool, but I think I can describe it. He commenced his description. I understood perfectly everything he said, and I understood just exactly what kind of a lock he had. He had six dials on the door and a little pointer in each that was turned around to different letters, whatever they might be, to open the lock. I said, " Mr. Brown, your lock looks very much to me like a letter padlock flattened out, and if that is the case it can be easily opened." "Oh," said he, " you do not understand me." He then went over the explanation again, and I understood him as well as I did before. I knew that Mr. Brown had made a lock and that it was of no interest for me to tell him that it could be picked, and I thought if he likes his lock I am satisfied and I will say very little more about it. He invited me to see him at Liverpool to show me his lock. About two months after, I received a pamphlet with the proceedings of the Archæological Society, and among other papers given in it was one by Mr. Brown on ancient and modern locks. He said that while in London he had invited Mr. Hobbs to come and see him, and he explained his lock to Mr. Hobbs; that after the first explanation he asked Mr. Hobbs if he thought he could open it, and he said he could easily; but after a more thorough explanation, when he understood it better, he rather drew back; and Mr. Brown was perfectly satisfied, from the evasive way in which he answered him, that he could not open the lock. I thought it was about time for me to go to Liverpool. I called at the banking house and saw Mr. Brown. He said, "Mr. Hobbs, you are just the man I want to see; I will show you the lock." He led the way into the strong room. " Now," he says, " how do you suppose such a lock as that can be opened?" I said, " I don't know that it can be." He had a handle that threw the bolt after adjusting these combinations. I said, " You do not think anything of this?" He said, " No, we leave that on the safe every night." " Now," I said, "if I was to try to pick that lock I should do it so." The inner dials had one notch in each to let what we call the stump of the bolt pass in and they also had a series of false notches. Now, on the first turning of those I might bring them around to the false notch. Now, as long as I found the pointer on the dial free I went to another. He said, " That is not right." I said, " I know it isn't right, I don't want to pick the lock; I am only showing what I should do if I were going to try." The cashier called him at this point and said, " Mr. Brown, there is a gentleman wishing to speak to you," and just as he turned his back the bolt went back. I said, "Mr. Brown, please don't leave me here with your door open." He said, "Why, how did you do it?" I said, "I don't know, I think it is a mistake; lock it again." He locked it again, and I opened it again. Mr. Brown was dubbed "William Brown, Esquire, M.P., lock-maker to her Majesty."

While at the Exhibition in London many gentlemen used to come around where I was—many whom I liked very much—but there was one man who came almost every day. One morning he said, "Mr. Hobbs, have you seen the French locks?" I said, "I have not." He said, "They are very curious, indeed—very clever." I said, "I will go and see them sometime, but as I do not speak French I do not suppose that I shall be able to get any information." He said, "I speak French; I will go with you." We went. The French had a very fine display of locks, but what attracted my companion's attention was one of those letter padlocks. It had twelve rings and a whole alphabet on each ring. My friend said, "Is not that a good lock?" I said, "Yes." He said, "Will you open it?' I said, "I will." I raised the cloth on the bench and cut off a bit of wood for a wedge, and while the exhibitor was absent for a few moments, I unlocked the lock, took off the two outer rings, changed the combination and locked it again. When the exhibitor returned I said, pointing to this lock, "What is that?" He said, "A lock." I said, "Where is the key-hole?" He said, "Ze no key-hole." "I said, "How do you lock it?" He said, "Ze letters, ze letters." I said, "Let me see you do it." He tried to unlock it and couldn't do it. He went to the drawer and took out a memorandum book, thinking he had lost the combination. I said, "Where *is* the key?" He said, "Damme, ze no key!" I went away and left him. My companion told him who I was and what I had done, and the poor fellow was in great trouble; people used continually to make fun of him. I went back in the afternoon to see him and asked if he had got his lock open yet. He said, "Sacré!—damn Yankee!" I said, "Never mind," and I unlocked the lock for him.

Dr. Grimshaw.—There are a few minutes left and I think it would be well to call on Mr. Pickering to give a little of his experience about the tricks of exhibitors, and some of us would like to hear something about Mr. Pickering's own tricks in getting that Pullman car in.

Mr. Pickering.—Many of you know that at the last exhibition at Melbourne a large proportion of the goods were sent by a ship sailing from New York. That ship made one of the quickest trips on record until she got within one hundred miles of Melbourne. Then by a miscalculation of distance she struck on a reef and went to pieces. Sixty of the American exhibits were on that vessel, and yet there were just as many exhibits in the American Department as if that vessel had not gone down. The way it was done was this: First of all I got on the committee of jurors. Then we had embodied in the regulations that, in order to obtain an award or to be examined for an award, every article must be exhibited in the department of the country in which it was produced. All that I had to do was to go through the other departments of the exhibition and transfer the American exhibits to our own department and we had our full quota of exhibits.

In response to numerous calls for "another story," Mr. Hobbs said:

Before I went to England for the Exposition of 1851, I used to sell locks. I was at Lancaster, Pa. I had been putting a lock on a bank and the cashier came to me in the morning and said, "Mr. Hobbs, have you seen that?" It was a communication in a New York paper from a man by the name of Woodbridge offering $500 to anybody who would open his lock. I said to the cashier, "That is my money." I came to New York at once. This lock was in the Merchant's Exchange Reading Room and this notice was stuck up on the safe. I saw Mr. Woodbridge. I said, "You had better have a committee appointed about this." A committee was appointed composed of Maltby Weed, Worrell, the iron founder, and Hammond, the watch maker. After everything was ready, I said to Mr. Woodbridge, "You are a younger man than I am; besides, the money in there doesn't belong to you; it belongs to your father (his father was a minister in New Jersey, I think); if you leave that money there I shall take it." He said, "I know just what you are going to do; you go ahead." I said, "All right." I got permission from the janitor of the Exchange to have the room at night. The safe was too low for me to work on. I went down to the carpenter shop and got some pieces of plank and I went to work raising that safe. There were two men standing near and one said, "I bet you a thousand dollars he opens that lock." "Why?" says the other. "Don't you see the way he is raising that safe," was the reply. By the way, there was an arrangement in the lock of such a character that if I tried to draw the bolt back and did not do so, whatever I used would be caught in the key-hole. Mr. Woodbridge thought I didn't know that. I commenced on the lock at half-past nine and at twenty minutes past eleven that night I was at a saloon on Broadway having an oyster stew, but I had everything right. I had left a piece of crooked wire in the key-hole. I got a man and told him to stay there. "Don't you touch that wire and don't let any one touch it," I said to him. The next morning I saw Mr. Woodbridge and told him there was something the matter with his lock and that I wanted to see him there at ten o'clock. I notified the arbitrators also. Some wanted to bet that I had opened, and some that I had not opened, the lock. By and by Mr. Woodbridge came in and said, "What's the trouble with the lock? What ails it?" I said, "It will not keep your door shut," and I pulled the door open. I took the $500 check and disappeared very quickly to draw the money.

CXXXVII.

A POWER CRANE.

BY W. F. DURFEE, BRIDGEPORT, CONN.

WHILE acting as engineer for the Milwaukee Iron Company, in the year 1867, the author of this paper designed and had constructed for the machine shop of that company the power crane to the description of which he now asks attention. The location of the crane in the shop was near one of its sides and between the head-stocks of two large lathes for turning heavy rolls. The duty required of it was to lift the work done in those lathes in and out of the same, and also to handle other heavy castings as occasion required.

As will be seen from the elevations, Figs. 32 and 32*a*, there is nothing peculiar in the general character of the framing of this crane, but the mass of drums, gears, shafts, cranks, etc., and their supporting castings, usually found at the intersection of post and brace of hand-worked cranes, are here conspicuously absent.

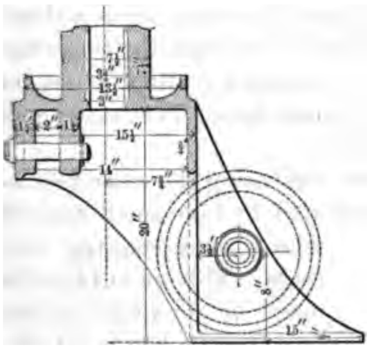

FIG. 33.

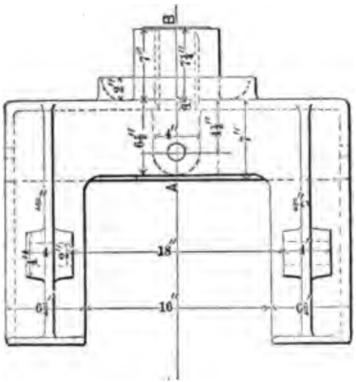

FIG. 34.

The crane post is made of two timbers, 6"x 14" in cross-section, placed parallel with each other, sixteen inches apart. These timbers are securely bolted to castings at their top and bottom, which we shall call respectively the head and foot of the crane; they are shown in detail in Figs. 33, 34, 35, and 36. The crane arm consists of two timbers, 8"x 15" in section, for the distance from the crane post to the outside of the brace, and from thence they are reduced in depth by a gradual curving of their lower edges until at their ends they are 8″x10″. These timbers are bolted to the crane post and also to angle-plate projections from the crane head, designed for the purpose of securely uniting the crane arm with the post. Each timber of which the crane arm is composed is supported near its middle by a brace, 6″x12″ in section. This brace has a bearing in a shoe, projecting from the foot of the crane, and is also bolted to the crane post, its upper end being received by a socket casting which is bolted to the brace and also to the crane arm. This casting is shown in detail by Fig. 37.

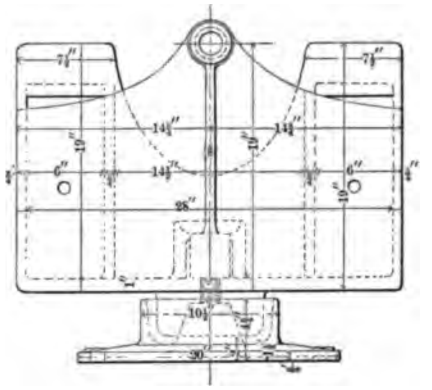

Fig. 35.

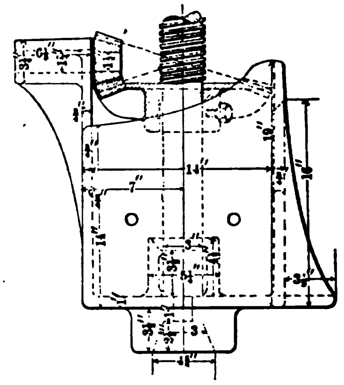

Fig. 36.

On the top of each of the timbers composing the crane arm is placed a rail, and on these rails, supported by four small flanged wheels, runs the top carriage. This carriage is made to traverse the available length of the arm, in either direction, by appropriately maneuvering the endless rope which acts upon the "bull wheel," seen near the crane post in Figs. 32, 32*a*, and 38. This bull wheel has a worm upon its shaft, which acts upon a worm-wheel, on whose shaft, between the timbers of the crane arm (Fig. 38) is a

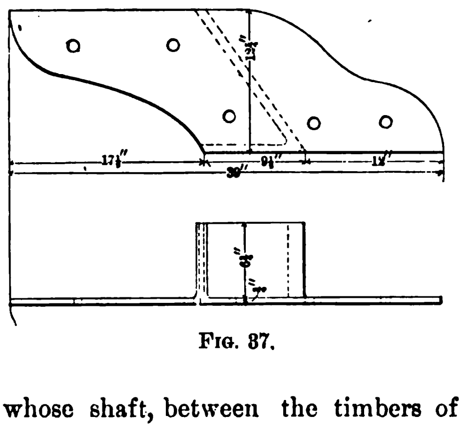

Fig. 37.

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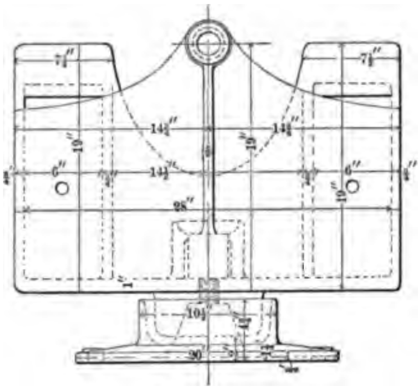


FIG. 35.

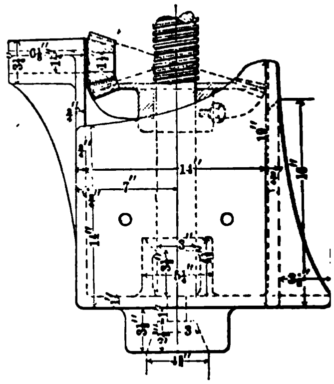


FIG. 36.

On the top of each of the timbers composing the crane arm i placed a rail, and on these rails, supported by four small flange wheels, runs the top carriage. This carriage i made to traverse th available length of th arm, in either direction, b appropriately maneuverin the endless rope which act upon the "bull wheel,' seen near the crane post i Figs. 32, 32*a*, and 38. Thi bull wheel has a wor upon its shaft, which act upon a worm-wheel, o whose shaft, between the timbers of the crane arm (Fig. 38) is

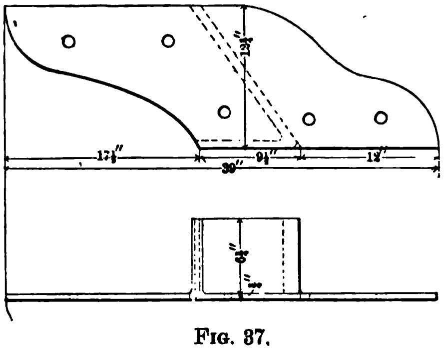


FIG. 37.

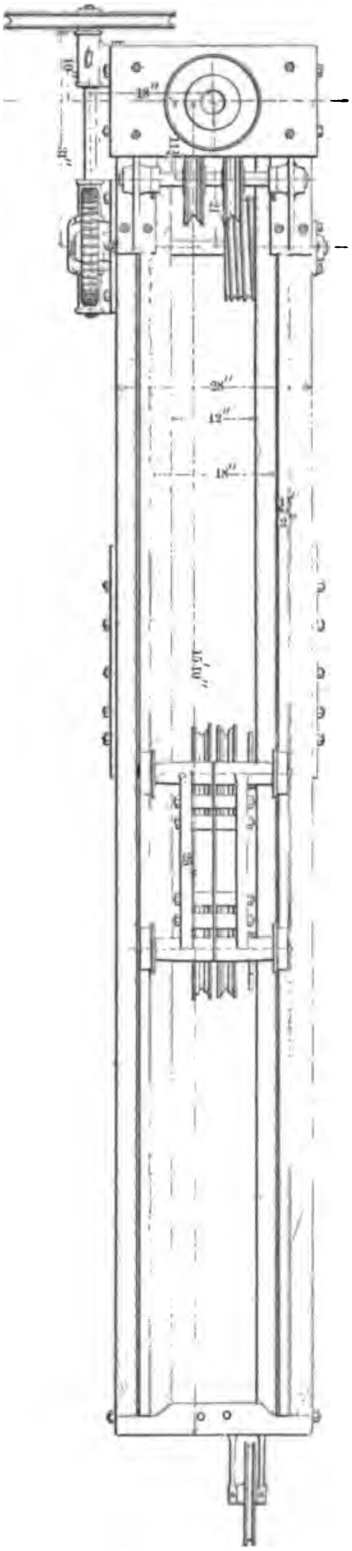


FIG. 38.

grooved drum, around which passes a couple of turns of an endless wire rope, $\frac{3}{8}''$ in diameter. This rope also passes around the grooved pulley at the end of the crane arm, and is attached to the carriage before named. This combination of mechanism for moving the top carriage is believed to have an advantage over the spur gear, and rack and pinion movements in common use for moving the top carriages of cranes, in that the carriage is not liable to accidental movement by any diagonal strain which it may be subjected to before the weight is fairly lifted; and also from the fact that the man who moves the carriage stands with his face to the suspended load and can readily note the progress of its movement. The foot of the crane is supported on a conical step which is cast in the bottom of a cup (Fig. 35), which is kept full of oil, thus insuring the constant lubrication of the step.

The pivot on the crane head turns in a bearing in the lower part of a hollow casting, whose details are shown in Figs. 39, 40 and 41; this casting is bolted to one of the heavy floor beams which support the second floor of the building, and is further secured by a brace, which, entering a socket on the side of the casting, extends horizontally under the floor to the adjoining beam. In the sides of the above-named casting are two holes, six inches in diameter, through which access is had for the purpose of observing and oiling the machinery within.

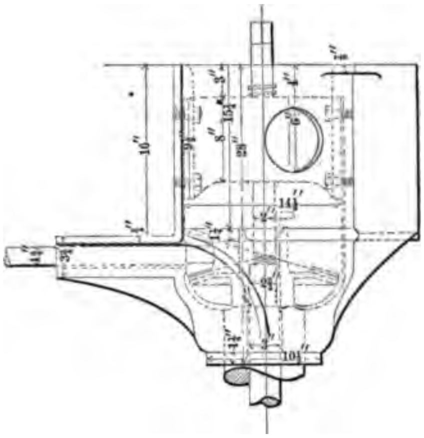


FIG. 39.

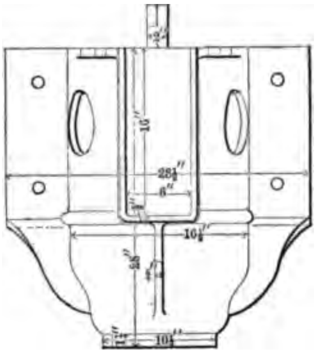


FIG. 40.

Attached to this hollow casting, underneath and at right angles with the floor timber to which it is bolted, is a long bearing for a

1¾″ shaft (Figs. 39, 41 and 42), on the outer end of which are placed two loose pulleys (Fig. 32*a*), 12″ in diameter and 4½″ face, between which is located a friction clutch, made to gear with either or neither of them by an inclined lever (Figs. 32 and 32*a*). These pulleys are constantly driven at 240 revolutions per minute by two 4″ belts, one of which is crossed, thus causing the pulleys to turn in opposite directions. When the friction clutch is in gear with the pulley driven by the crossed belt, the crane hoists its load; when in gear with the other pulley the load is lowered, and when the clutch occupies a position midway between the two pulleys the load remains stationary.

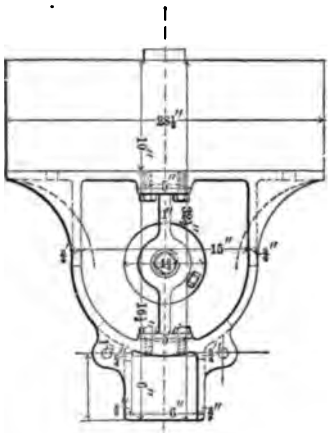


FIG. 41.

The mechanism by which the rotation of the above-named shaft is made to raise, lower or sustain the load on the crane is of the following character. On the shaft just named within the hollow casting with which it is associated is a beveled bronze pinion of fifteen teeth, which gears with a cast-iron bevel wheel of forty-five teeth, the lower end of whose hub is keyed fast to the upper end of a vertical shaft which turns in a bearing formed in and concentric with the pivot on the crane head. The upper end of the hub of the cast-iron gear is bored out somewhat smaller than the lower and forms an oil-tight socket in which enters the lower cylindrical, end of an abutment spindle, whose convex extremity rests upon a hardened lenticular steel disk, which bears upon the upper convex end of the shaft to which the bevel gear is keyed. Above the socket, in the hub of the gear wheel, the abutment spindle is enlarged, and on this portion is cut a square-threaded screw which passes through a corresponding nut formed in the centre of a diametrical rib, so fitted and secured by bolts in the hollow casting that the axis of the abutment spindle passing through it coincides with the prolongation of the axis of the shaft to which the cast-iron bevel gear is keyed. The above-named rib, through the intervention of the abutment spindle, is subjected to a strain equal to the weight being lifted by the crane. The relations of the several parts of the foregoing mechanism are clearly shown by Fig. 42, which is a vertical section made by a plane containing the axis of the driving shaft and that of the vertical shaft before named. The diameter of the vertical shaft where it passes through the pivot of the crane head is three inches, but below the crane head its diameter becomes 3⅜ inches, and so continues until near its lower bearing in the centre of the crane's foot; there it is reduced to the former size. This upright shaft occupies a position midway between the timbers composing the post of the crane and on the entire

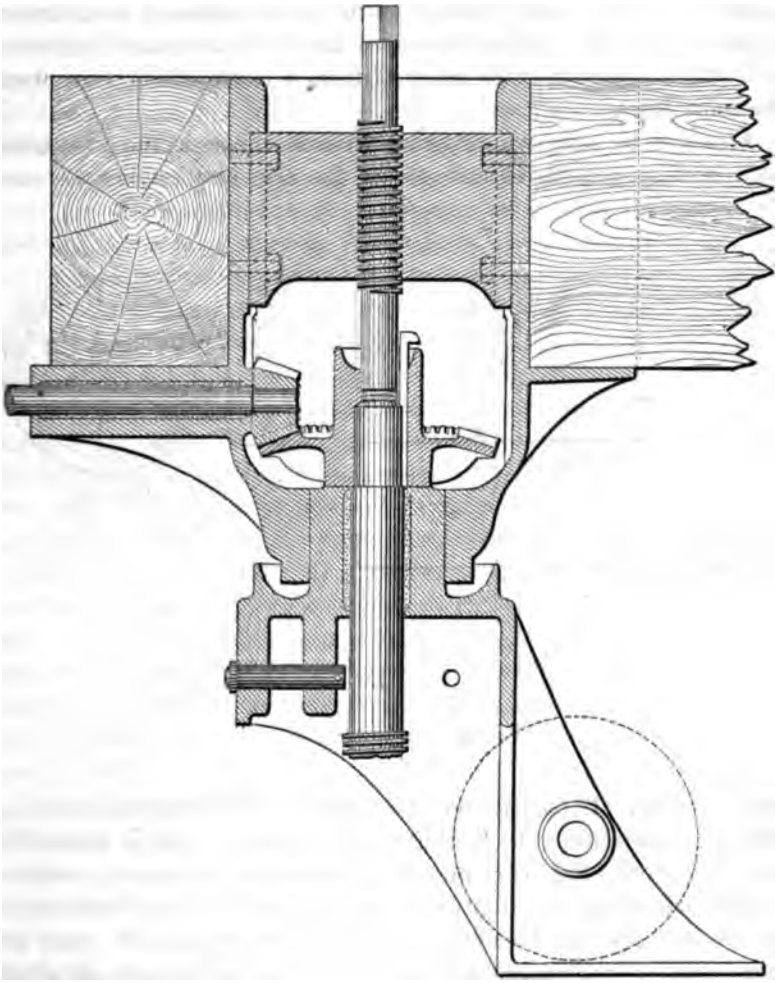


FIG. 42.

length of its largest diameter is cut a square-threaded screw of ½ inch pitch, to which is adapted a bronze nut, attached to and beneath which is a wrought iron cross head (Fig. 32*a*) whose cylindrical ends enter holes in the middle of slide blocks which are fitted to planed cast iron guide grooves bolted to the inside of each timber of the crane post. Midway between the slide blocks and the bronze nut before named are two grooved chain sheaves which turn upon the cross head.

Attached to pins which pass vertically through the two holes seen in Figs. 38, 43, 44 and 45, in the arm end casting, are two ½-inch chains. These pass over the nearest pair of sheaves in the top carriage; descending thence, they pass under a pair of sheaves in the hook block (Fig. 32*a*), and then, ascending, are carried over the second pair of sheaves in the top carriage, whence they are carried to and over the two sheaves shown in Figs. 32a and 38 near the crane post; then, descending, they pass under the two sheaves which turn upon the cross head, and are finally carried up and secured to the ends of an equalizing lever which has for its fulcrum the horizontal pin shown at the left hand of the sections of the crane head in Figs. 32*a* and 42. The function of this lever is to compensate for any slight difference in the length of the two chains, and also for any irregularity of tension between them arising from a side or inclined pull before the load is raised.

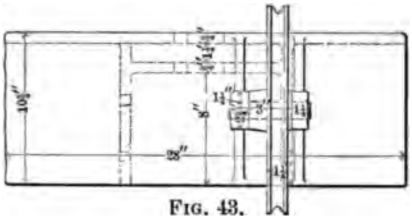


FIG. 43.

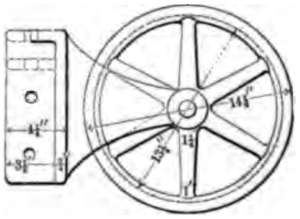


FIG. 44.

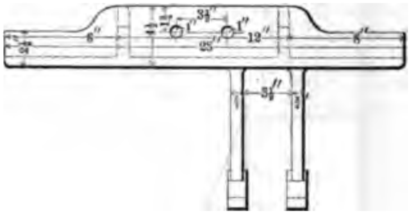


FIG. 45.

When the long screw shaft between the crane post timbers is turned to the left, the cross head is depressed and the load attached to the hook-block is raised. When the shaft ceases to turn, the load is held suspended, and when the shaft is turned to the right the cross head is raised and the suspended load is lowered.

In addition to the means already described for turning the screw shaft and hoisting the load by power, provision is made for working the crane by hand if desired. This consists of a light hand wheel (seen in Figs. 32 and 32*a* at the foot of the crane) which is geared to the screw shaft by wheel and pinion, as shown in Fig. 36. This hand gear was intended more particularly for moving the suspended load through a small vertical distance, as when a roll or other casting was being adjusted in the lathe, but its use was unnecessary, as it was found that the power gearing worked with entire satisfaction even for small movements of the work in hand. It will be noted that in this crane all the vertical thrust of the screw shaft comes upon the floor timber to which the crane head bearing is bolted; but while, in the particular location occupied by the crane, this was admissible, this construction is not essential, as the thrust can easily be rebutted by collars or friction wheels within the crane post and the whole strain upon the screw due to the load confined within the crane itself. It is of course not necessary that power should be transmitted to this kind of crane by means of belts, as the shaft to which the power is applied can be prolonged and connected to a small direct acting reversible engine, which could be readily supported by a strong wall bracket. This engine could be run by water or steam pressure, as was most convenient in the locality where the crane was erected.

In the crane as actually constructed, the space between the crane post timbers is closed by doors in order to exclude dust from the screw.

At the date of the construction of the crane just described, the writer of this paper had never seen a power crane intended for shop use, and is certain that in the leading establishments for the manufacture of machinery nothing of the kind was employed; and he thinks it extremely probable that the crane whose drawings are before you is the pioneer power shop-crane of this country. In its design he attempted to embody and combine strength, simplicity, cheapness and effectiveness, and the fact that it has for the past sixteen years unfailingly responded to all demands made upon it is pretty good evidence of his success.

DISCUSSION.

Mr. Towne.—One of the chief merits of any mechanical contrivance is its simplicity, and in Mr. Durfee's design we certainly have an exceedingly simple construction. Considering the time when that design was made (sixteen years ago) it is certainly a beautiful piece of work, and one that is very much in advance of many cranes that are being built to-day. Possibly, Mr. Durfee may be correct in asserting that it was the first revolving power crane in the country, but, if so, it had successors a very short time afterward. I know of power cranes that were built in Philadelphia within one or two years of that time, of somewhat larger size than that, but having a good deal more complication of parts. The machine before us is so beautifully simple that it leaves but little room for comment of any kind, and I think it is as high a compliment as can be paid to it and to the designer of it, to say that, in the light of experience with larger and more recent cranes, there are only two trifling criticisms which suggest themselves in regard to it. One of these would be the fact that, in the hoisting mechanism, the arrangement of the chain leading from the sheave at the head of the mast is such that the travel of the trolley makes a slight change in the vertical position of the load. In the work, for instance, of settling patterns in foundry work that would be an objection, though one easily remedied by dropping the mechanism so that the chain would be horizontal. The other criticism is that a considerable length of the jib is lost as effective radius. Taking half of the width of the trolley, and the projection of the return sheave at the outer end of the jib, there are some three or four feet that are not available. It is, of course, necessary that the hook should reach out nearly to the end of the jib. As I said before, I think the fact that these points are almost the only ones that can be criticised in the design are as strong a testimony as can be given to its general excellence.

Mr. Barnes.—It may be interesting to know that when, in the latter part of 1861, I went to the Novelty Works in this city, which have disappeared, I believe wholly, power had been adapted to an erecting-shop crane which had been in use for some time. The apparatus which was involved was simply a vertical shaft coming down through the head pin. That crane was there in November, 1861, and I am certain that it had been there for one or two years previous. I saw the other day in Joliet, in the foundry, a very convenient adaptation of power to an every-day crane. It consisted of the use of a very simple shaft connection coming down through the head pin, with a bit of a horizontal shaft, at the outer end of which there was a drive chain. This chain came down alongside and took hold of a similar wheel on the shaft below. I might say that the only reason why that crane at the Novelty Works was not in use was that the worm-gear mechanism employed in it was too slow.

Mr. Durfee.—This crane was designed to meet a particular case, and is the only one of its kind that has ever been built. The two points named were considered at the time it was designed, but as it was intended for a particular work, the angular position of the chain was not thought to be of sufficient moment to justify the dropping of the sheaves lower down in order to obviate that objection; and in regard to the top carriage not going out as far as it might if the arm was arranged differently, in the particular case where the crane were used there was no trouble occasioned by that fact, for it commanded all the area of the floor that there was to command, and that was all that was required.

CXXXVIII.

A MACHINE FOR TESTING THE PHYSICAL PROPERTIES OF METALS AND ALLOYS.

BY T. EGLESTON, PH.D., NEW YORK.

In the course of many examinations of metals that I have been called upon to make during the last ten or twelve years, I have frequently noticed changes which they have undergone, and also that many times they become brittle or unsound without any apparent cause. I have several times called attention in the meetings of the different engineering societies to some of these peculiarities. As the result of observations made up to that time, I announced in the year 1879, at the Montreal meeting of the Mining Engineers, the law of fatigue and refreshment of metals, and I further stated that under certain conditions, likely to happen frequently in anything made of iron and steel, the uncombined carbon in cast-iron, steel, and iron would become combined and the metal become brittle. This fatigue I first observed in steel rails, on which I made a number of curious observations, showing not only the different phases of fatigue, arising sometimes from overwork of the metal before it got into the rail, and sometimes from overwork on the rail after it was placed on the road-bed. I have a collection of photographs of these observations, with the fatigue and the physical changes made apparent to the eye, which I hope at some time to publish. They show quite unmistakably that metals or alloys may be fatigued in the process of manufacture, before they have undergone any use whatever, and that blow-holes are not, as is usually supposed, welded together, but are only flattened out without welding. What led me to make these investigations was finding a broken rail on the Northern Railway of France, in 1873, which was, by the conditions of the contract, replaced by the manufacturers, but which I found, when I came to examine it, was far beyond the limits required by the specifications, except at the point where it broke. It was a long time before I found the cause of the fracture. It was a defect in one particular spot, owing to a blow-hole which had been pressed flat and had come to the surface, thus forming a flaw in the flange and the commencement of a crack which, aided probably by the strain of gagging and the fatigue of passing trains, had spread through the rail in a very crooked line through the flange and web, and finally broke directly through the head. This observation led me to study other defects. It has been shown by careful observations made on the same railroad that a good steel rail should wear only one millimetre for every twenty million tons of traffic, and no more. But I have seen rails worn out before they had even supported as much as ten million tons traffic, while others, apparently under the same conditions, have borne many times that amount. These facts were what first suggested the idea of studying these physical defects of metals. I have seen similar defects not only in iron and steel, but also in copper, tin, and brass, and in almost every other commercial alloy.

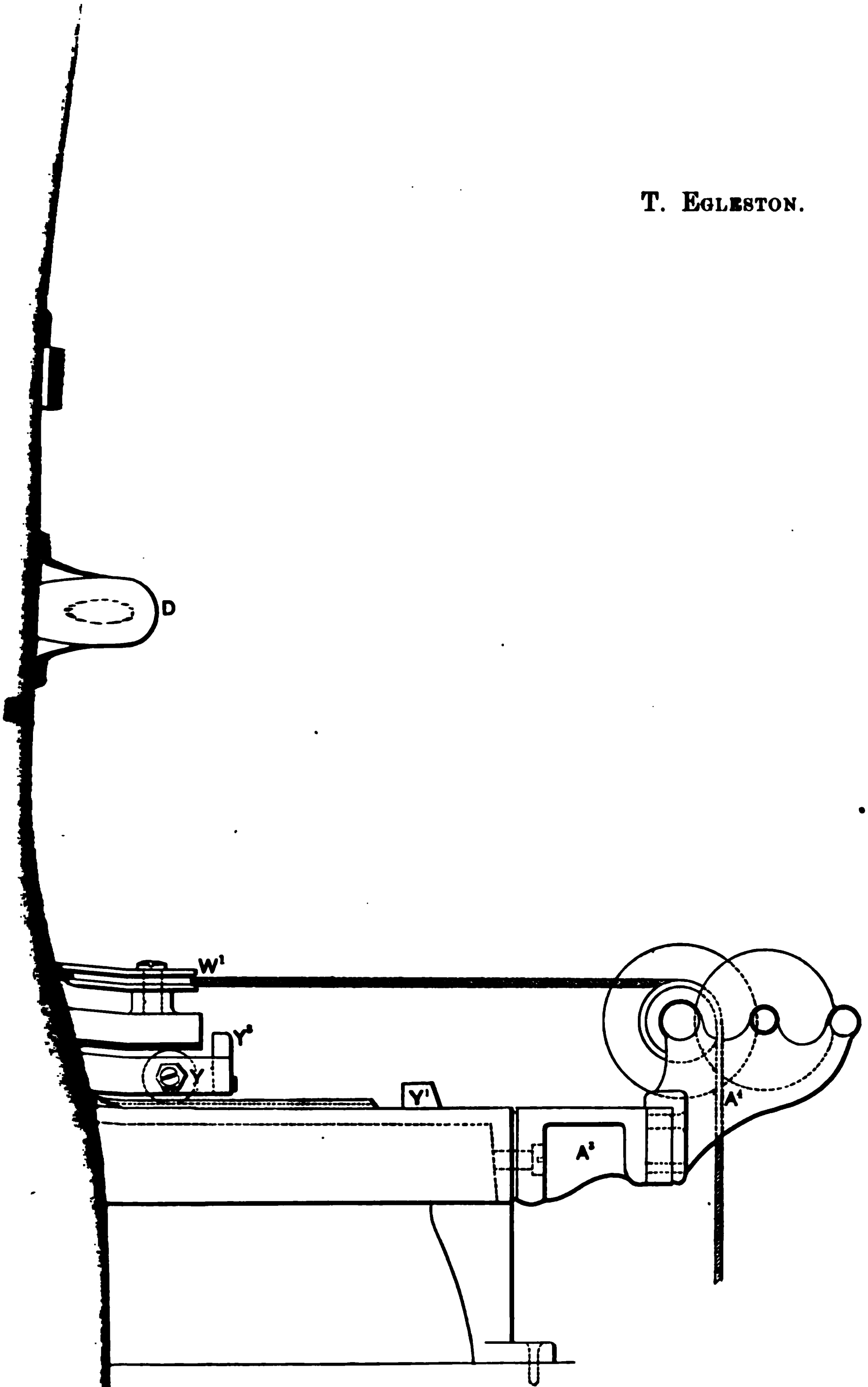
In the year 1878, having been called upon to serve as expert in a very important matter concerning steel rails, I devised a machine for the study of fatigue, in order to investigate the conditions under which various kinds of metal went into service. My intention was to get a scale of hardness suitable for the use of mechanical engineers, with the diamond at the extremity of a scale of 100, instead of 10 as now. There has been great delay in the construction of the machine, and it has only recently come into my possession, and in the interval some one in Europe has proposed a scale of hardness of from zero to 50°. I do not, however, propose to adopt this scale unless I find that it is better than the one I first proposed.

It was not long before I began to notice the change of shape which metals often undergo when submitted to strain to which the name of "flow" has been given. Every one knows something about the *hot* flow of metals, but one of the first things that I observed in studying metals was some of the peculiar conditions of *cold* flow—not only that metals would themselves flow cold, but that each one had a rate of flow which was different under different circumstances, and that very frequently alloys composed of different metals, when fatigued, would separate, each metal taking its own rate of flow, and separating from the original mass in such a way as frequently to lead to disastrous results. It has sometimes been the case that pieces manufactured proved themselves to be quite up to the standard at the time of manufacture, but afterward became worthless by rest. At other times, of two pieces of metal of apparently the same composition, one would stand the strain of the manufacture and the other would not, the metals composing the alloy separating from the difference of their rate of flow. In some cases I have found that sheet metal delivered from the manufacturer had become so fatigued in the rolls that it could not be treated any further without breaking. I have often found metal in stock which was fatigued by this flow so that it would not bear all the necessary pressure in the process of manufacture, and it was considered impossible to explain the reason why it failed. In some cases I found that if the metal had been annealed or had been allowed to rest a certain time, if the cold flow had not commenced it became strong and serviceable, but in others that it was brittle even after annealing. This flow of the metal may be made to take place either by pressure or by heat, and is usually called in the brass works "the starting of the zinc." Every time any of these changes took place, I noticed differences in physical structure, as well as in the qualities of the metal, which it was desirable to study, and after careful examination of the whole subject, I devised in July, 1878, an instrument for investigating these phenomena.

The machine which I propose to use for studying these phenomena has been constructed for me by Brown & Sharpe, of Providence, R. I., and I wish at the outset to express my indebtedness to Mr. George H. Smith, our fellow-member, who has made many useful and important suggestions to me in regard to the details of the machine during its construction.

It consists of a bed-plate A, standing on three legs (Fig. 46). On this bed-plate a standard, $A'\ A'\ B$, is securely fixed (Fig. 47), and carries a slide-rest $C\ C$, to which two sets of tools for different purposes are attached. On the bed-plate, a rectangular table, $Y\ X\ Y$, with four wheels, $Y\ Y\ Y\ Y$, rolls, being guided on one side only, in the slot Y^2 (Fig. 47), so that it can move only in the direction of the axis of the machine. Its motion is limited by the stops $Y'\ Y'$, at each end of the bed-plate A. On this table, moving upon the same rollers, is another one, $U\ U^2\ U'$, which moves on the frame $Y\ X\ Y$. The table is prevented by the stop A^2 from moving more than a given distance in that direction, while the upper one moving in the direction A^3, carries the lower one with it as soon as it reaches the stop Y^3 on the end of the platform. Both of these platforms may be made stationary by the two pins $U'\ U'$ (Fig. 47), which fit into the holes U^2 on each side of the bed-plate. In the centre of the upper table $U\ U^2\ U^1$ is a circular depression, in which a ring $V\ V$

T. EGLESTON.



AND ALLOYS

46.

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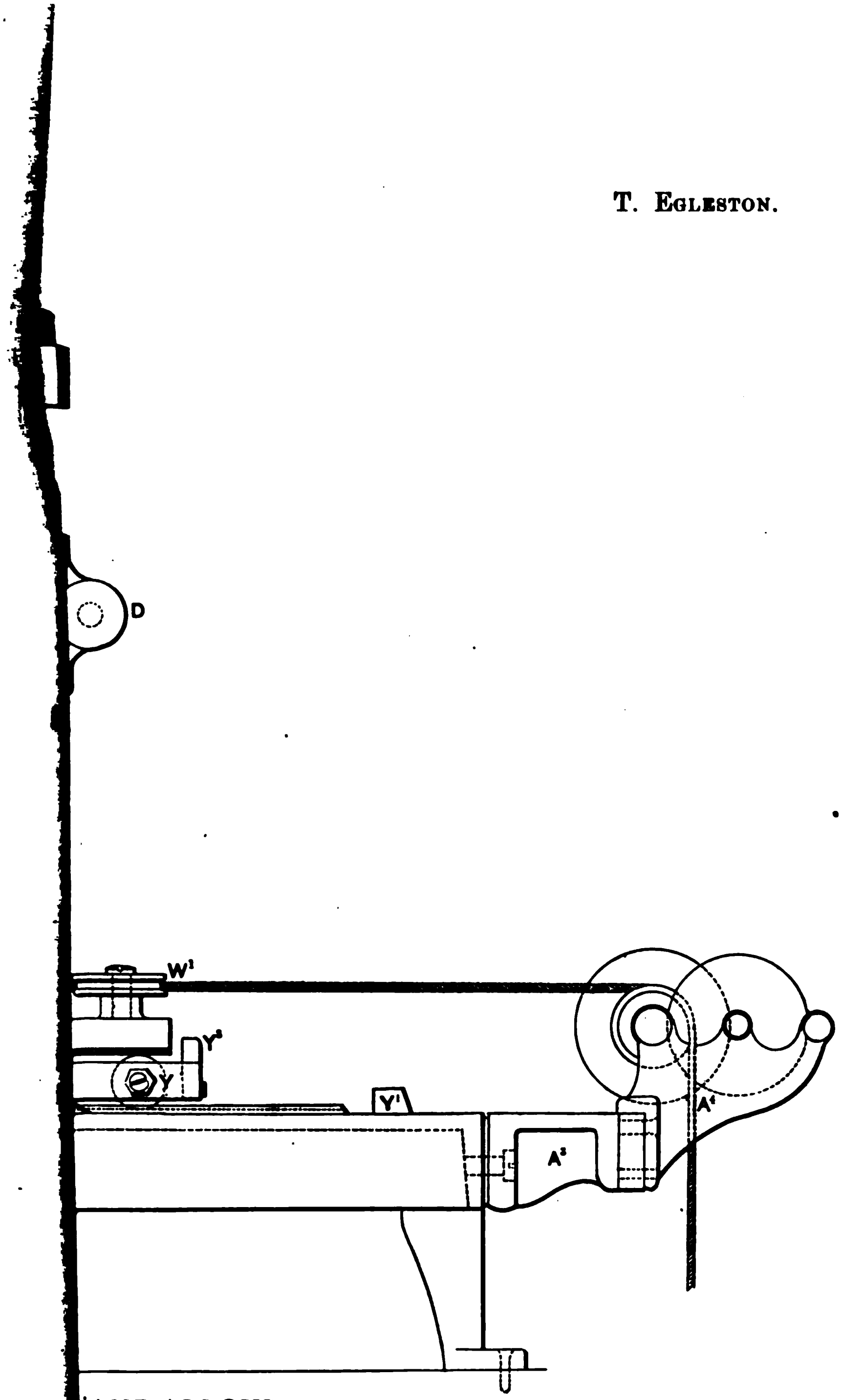


AND ALLOYS

46.

(Fig. 46), is placed, set upon four friction rollers and moving in a groove. Pivoted upon a step which rises in the centre of the depression, is the chuck *P R S* (Fig. 46), which turns on the central pivot, the ring below moving freely on the friction rollers. A rotary motion may be given to the chuck by means of the cord *W′*, which after passing around the groove *S* and the friction rollers *W* and *W′* is made taut at one end by a weight passing over the wheel on the bracket A^2, which is securely bolted to the machine, and is carried by a weight attached to a cord passing over a drum connected to a series of clock-wheels A^4, attached to the bracket A^3, also bolted to the machine, whose velocity is regulated by flies of different sizes, according to the speed required. To this plate is attached the chuck-work shown at *R*, for fastening the pieces to be examined. The rotating chuck may be made stationary by two pins, *T*, at opposite diameters. In order to adapt the chuck to a piece of any size, it is fitted with slides *R*, and screws *Q*, and thumb-screws *P*, for holding any piece in any position to the chuck. It will thus be observed that if the pins *T* are in position, the bed-plate has movement only backward and forward, in the direction of the axis of the instrument, while if the pins, *U′*, are in position, it may be made to have a rotary motion only, thus allowing every part of the piece to be brought under examination by the tools above. When all of the pins are in position, the bed-plate and chucks are at rest.

In order to examine the different conditions of the metal, two tools have been attached to the slide-rest *C*, movable by the screw *E′*. One of these, *N N*, Fig. 47, carries a disk, the lower half of which is movable on the hinges *N N*. Through this disk two arbors, *L F* and *J*, pass. The lower one, *L F*, is counterpoised by means of two weighted arms, *M M*, which catch under the head of the mandrel. The upper one, *J*, has a scale-beam, *I*, and a pan, *I′*, attached to it, so that by counterpoising *L F* with the weights *M*, a given pressure may be applied to the mandrel, *L F*, by placing known weights in the scale-pan *I′*.

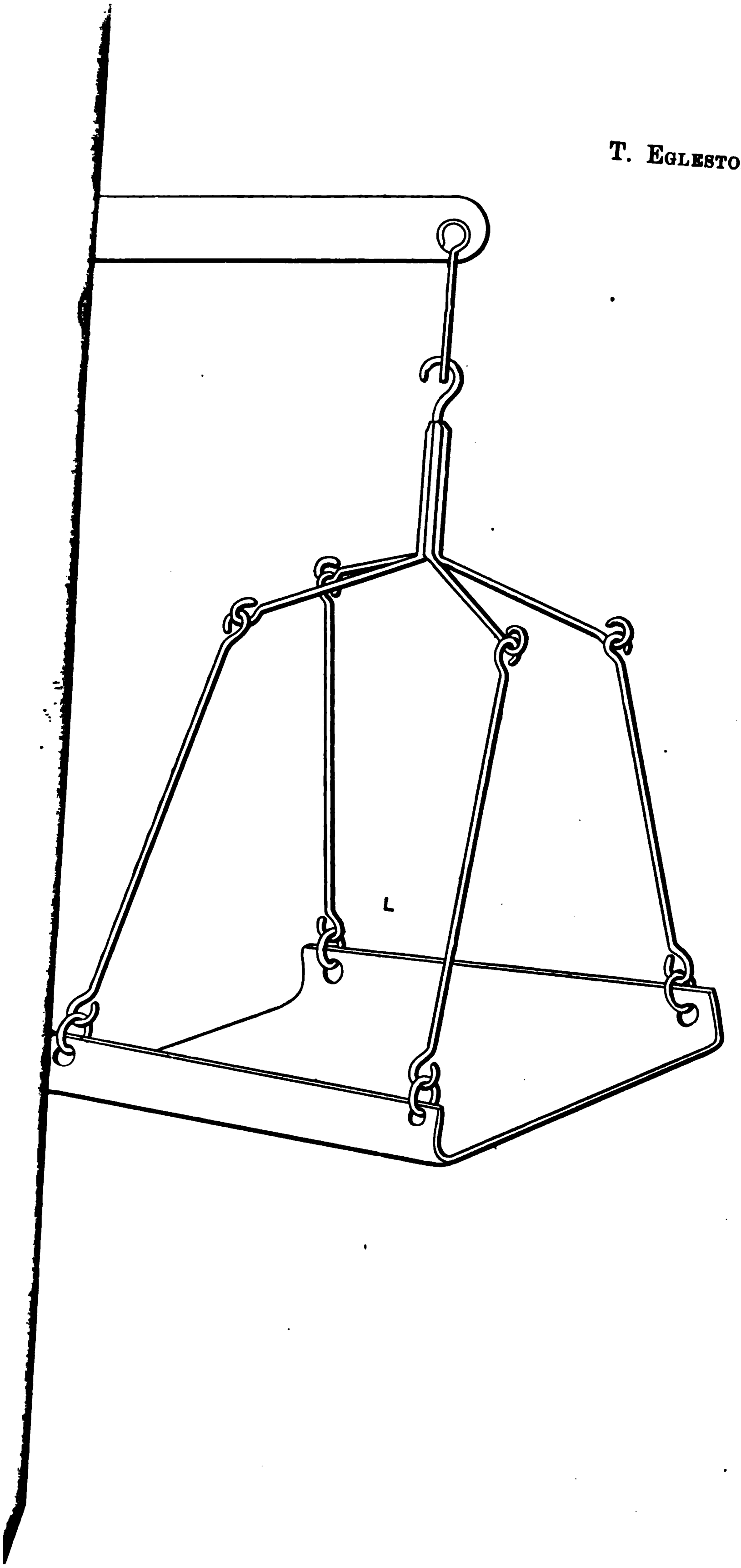
The mandrel, *L*, carries a chuck, *L′*, in which stones of the usual degree of hardness, ground to the angle of an octahedron, are held. A series of points of known composition and of known hardness, made of steel manufactured by Miller, Metcalf & Parkin, for the United States Test Commission, are used as a means of ascertaining the different hardness of different kinds of steel, and it is proposed, starting with the diamond as 100, to interpose a series of metals and alloys whose hardness should be determined in degrees of ten between each of the usual grades in scales of hardness.

The piece to be examined being placed upon the chuck *R*, the slide-rest *C* is brought down by means of the thumb-screws *D* to the proper position, and a known weight is placed in the scale-pan I', so that the point will be free to work upon the metal, and the bed-plate is then made to traverse or rotate at a given velocity by means of a weight attached to the cord passing over the clock-wheels A^4. The conditions in various parts of the plate are then carefully examined and the penetration of the point observed. The abrasion being studied in this way with different weights and with materials of different composition and hardness, the exact condition of the metal in different parts will then be known and what Prof. Langley calls its "abrasive resistance," will be determined. Its physical condition can then be given in terms of a scale of hardness which have been previously determined.

It has been definitely ascertained that generally, as the metal becomes fatigued and the carbon becomes combined, iron and steel will become harder and the other metals will undergo a change which will be ascertained and defined in terms of the various degrees of abrasive resistance. This will show, as the slide-rest is capable of making the various points traverse or rotate on different parts of the metal, how far any physical change has gone on in various points of the metal, as all parts can be traversed by the rotary and rectilinear motions given to the bed-plate.

In order further to ascertain what the change has been, another instrument, Figs. 48 and 49, adapted to the same slide-rest, has been devised, in which it is proposed to use drills whose chemical composition and hardness are known. These drills are attached to a mandrel *B B* and are held by a chuck, the power being applied to the drill by the pulley *C*. On the mandrel which carries the chuck and the pulley is a Neer's dynamometer. The construction of this dynamometer is shown in the figures *H*, *N*, *F*, *G*, *M*. The drill is exactly counterpoised by the weight *K*. The scale-pan *L* is attached to the beam *I*. A given weight is then placed in the scale-pan *L* and the power applied to the pulley *C*, and the penetration into the metal observed. This penetration-resistance will be marked by the hands on the dial *G*. One of these, *O*, is loose, and is carried round by *P*, which, when the drill is not working, is carried back to *O* by the weight *M*. The other *O* remains stationary, so that the power exerted will always be shown by

T. EGLESTON.



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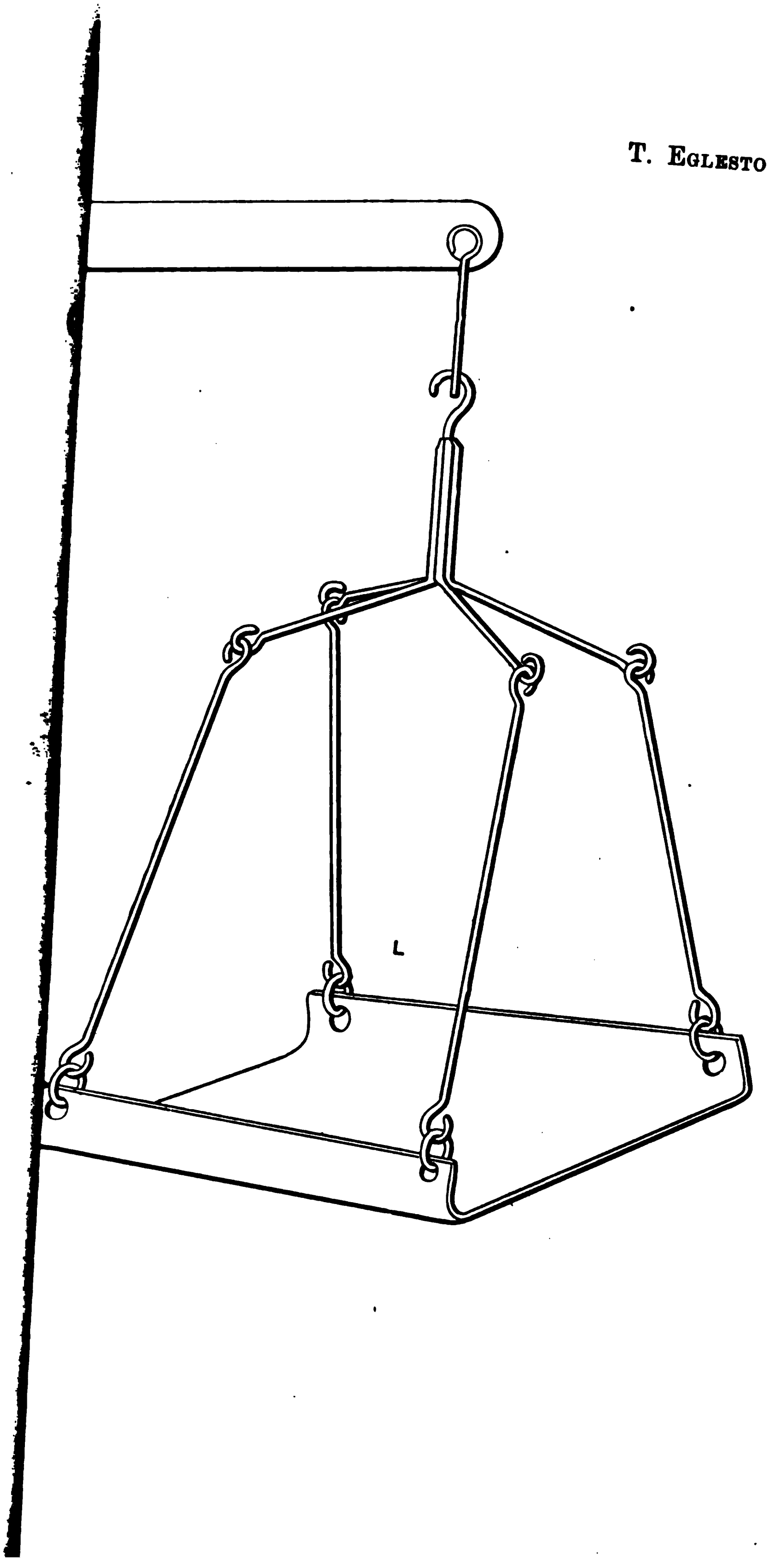


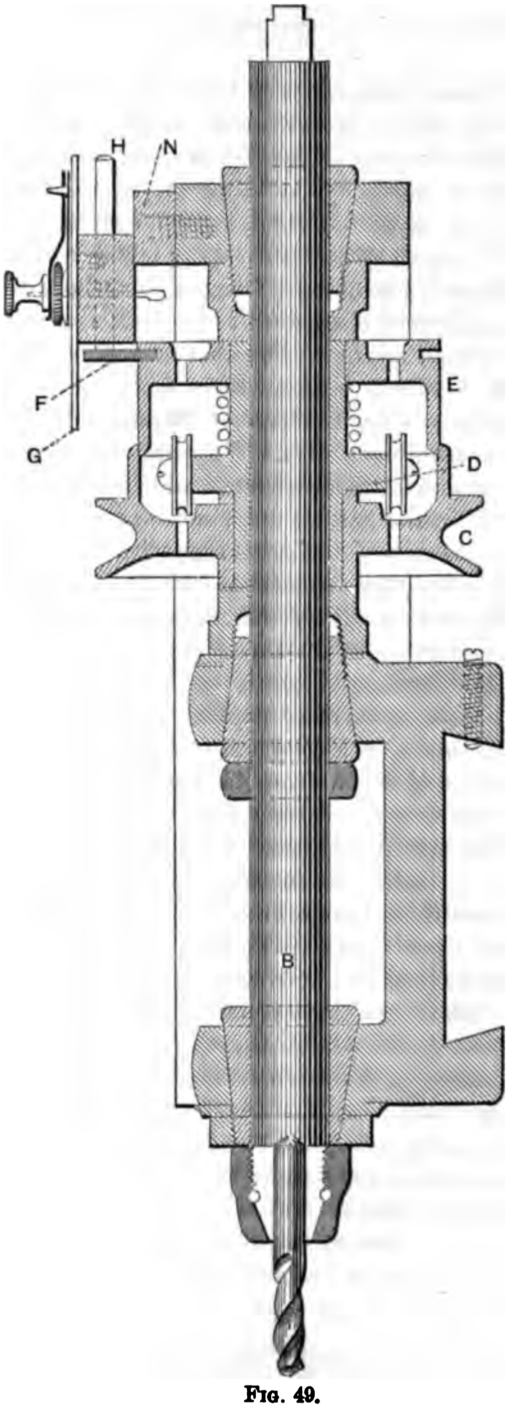


FIG. 49.

the movable hand, and a comparison of the metals can thus be made.

For all of these observations I have plates from the material manufactured by Miller, Metcalf, and Parkin for the U. S. Commission, and also the standard steels and other metals of commerce. I also propose to use alloys of different compositions, which I intend to fatigue under different known conditions, in order to study them. I expect by going over any metal in this way to find out exactly where it has become fatigued, or where there is a difference in the physical character of the metals which we usually call hardness. I am not quite sure that we any of us know exactly what the term hardness means. This fact I pointed out at the Baltimore meeting of the Institute of Mining Engineers, in 1878, during the discussion upon rails. We sometimes mean by hardness the penetrating power of a substance which is itself brittle as the diamond, which is the hardest of all substances, and is at the same time brittle on account of the great ease with which it cleaves parallel to its octahedral face; sometimes we mean toughness; sometimes we mean the quality which makes a substance difficult to scratch or the resistance to abrasion.

I have frequently gone into the stock-rooms of companies for whom I have been consulting engineer, and have found that before it came into the establishment a large quantity of the stock was so fatigued that it could not be put through the processes of manufacture with safety. I have also often found that, after having been put through the process of annealing, and also after long rest, the metal which was formerly bad frequently becomes fit to work, provided there has been no actual separation of the metals. In some cases I have found that alloys which had broken in the course of manufacture had started to separate from the difference in the rate of flow of the two metals which compose them. This flow may be caused either by pressure or heat. I remember, a few years ago, standing by the waste pile in the scrap-room of an establishment not far from New York, which I think represented $2,000 worth of labor, and pointing out to the owner that the zinc in nearly every case had commenced to separate from the other metals in the brass, or as the workmen said, "the zinc had started." Many of these pieces, almost ready for market, were destroyed by the last stroke. In many cases, a proper annealing before this last stroke was given would have saved a large sum of money.

DISCUSSION.

Mr. Oberlin Smith.—I would like to ask Prof. Egleston if the esults obtained by the drill are as good as they would be with a raight-cutting tool. The drills have this defect, that they force eir way into the metal by pressure merely, at the centre where ere is no motion, while at the outside they cut.

Prof. Egleston.—I am groping in the dark, and I adopted the rill as a means of studying the effects to be examined simply ecause that seemed to be the simplest way to begin. If I had ought at the commencement of a way of registering the power, I ink I should have used a planer tool.

Mr. Durfee.—I understand that this drill is rotated at the same me the carriage is traversing.

Prof. Egleston.—No; the carriage is fixed.

Mr. Oberlin Smith.—I would suggest that the difficulty referred might be obviated by drilling a hole first with a small drill—rilling a very small hole first, and then with the testing-drill folwing that hole; or by letting the testing tool be a hollow drill, aving a core in the centre.

Mr. Grant.—I would suggest to the Professor that he fasten his aning-tool to a movable slide that has a compression spring. I ink it would be far preferable to a drill.

Prof. Egleston.—The suggestions are good ones, and I shall be ly too glad to adopt them or any other suggestions that are made. he only reasons that I have for simplifying the work as much as ossible is the question of expense, which is a very serious matter hen there are but few people interested, and there is apparently othing to come of the investigation beyond theoretical results.

Mr. Towne.—It is always easier to criticise than to suggest in ese matters, and I am afraid that that is all I can do in the presnt case. It seems to me, however, that any cutting tool is a very ncertain measure in a matter of this kind, from the fact that the nditions of its use are so variable and so indeterminable; not nly the speed at which the materials meet one another, but the ngle of the tool, the temper of the tool, and of course the material ith which you are working. But I would merely ask Dr. Eglesn if it has occurred to him whether there may not be some method ther than cutting for gauging hardness, as by pressure or rolling ontact? Is cutting the only way, or the best way, of measuring e hardness of a material?

Prof. Egleston.—It is the only way that I have yet been able to devise that my pocket will allow of.

Mr. Hobbs.—We have been suffering very severely for the last sixteen years on account of the inequality of metals. The metal we use is called brass. The mixture is two of copper and one of spelter, and we are trying experiments all the time, at great expense, and we find that the only way by which we can tell what is good or bad is to work up the metal. I think there should be some way of ascertaining whether a piece of metal is good or not, but there does not seem to be any. They take a piece of copper and roll it out. When they start, it is about thirty inches long. They anneal it, and roll it out ten or fifteen feet long. They anneal it again, and roll it thirty or forty feet. They cut it into strips about five feet long. Now, we can take strips from the same sheet that will vary in quality. The way that we test is to examine the shells carefully. I am speaking now of cartridge-shells. We examine every one of the shells carefully. Those that appear to be good we pass. They are then fired. Sometimes we will fire ten, fifteen, twenty, or a hundred thousand consecutively and not have a bad one; and then perhaps we will strike ten, twenty, or thirty thousand which we will have to throw away. My impression is that there is no way to find out whether it is bad or good. No man can take a few pieces and find out whether a lot of metal is good. At the Frankfort Arsenal they had a machine for testing metal. By it they supposed they could tell whether the metal was good or whether it was bad, and at one time they selected by testing some very choice metal. It answered all the squeezing, and it was, as they supposed, just exactly the stuff. They made it into cartridges, and over thirty-three per cent. burst. They took the common metal that looked bad, and it made good cartridges. So I do not think that any test of pinching and squeezing can be good. I am looking forward to see some improvement or change in the manufacture of brass. There is capital enough invested in it, and there is work enough done for good brass to be produced. There are very few uses to which metal can be put where the test is so severe as in cartridges. The metal has to stand the drawing, the bending, the punching, and the force of the explosion. I believe, in making brass, they take a certain quantity of copper and put it into a pot. They put a certain quantity of coal around that pot and light the fire. The draft may be very good indeed at certain times, which they ascertain by looking. They look oftener in cold weather than they do in hot. When they think that the copper is just about right, they take a certain quantity of spelter and put it in the pot. Now, is there a gentleman here who ever saw a brass manufacturer's chimney that was not whitewashed with zinc? They know what they put in, but they do not know what they take out. The man begins with ten pots of metal. He looks in one and then looks in another. If he finds one that is about right, he pours it, and by the time he gets that poured the other is too hot; and that rule of thumb—no, it is not even a rule of thumb—is carried on in all the brass works of the country. If Prof. Egleston, or any other man, can get any machine by which he can improve this method of making brass or mixing metal, he will be the man I want to see. Prof. Egleston came to our place by the advice of myself, to have some experiments conducted. I sent him samples of metals of various kinds. The Professor made his experiments very elaborately, and I believe very correctly. He made a report to me, summing the matter up in about these words: "Your annealing furnaces are wrong." I asked the Professor for a more elaborate report, and it was still that furnace, while about one-third of the metal I sent had never been in that furnace. Now I do not say this to blame him, but I do not believe he knows anything about it.

Prof. Egleston.—I agree with you *most thoroughly.*

Mr. Hobbs.—I have recently been making examinations with the microscope, and there is very little difference in appearance between the good and the bad, but you will find some. The mixture, seen through the microscope, looks as if you had taken a sheet of copper, torn it into strings and scraps, and thrown the spelter in with it. There is no real mixture. Until they get over that miserable system of looking into the pot and trying whether it is good or not, I do not believe they will ever make better metal. I have talked a great deal to the brass makers about it. I want them to try the Siemens furnace. There are three elements to be considered, the copper, the spelter, and the heat; and the heat is just as important as any of the other elements, if not more so. The Ansonia Brass and Copper Company decided, for the purpose of refining copper, to have a Siemens furnace. The furnace was built, and the men did not like it. They thought it was an encroachment on their rights, and things were done to that furnace which make me fully believe that the men were determined that that furnace should not work. They told me that, at any rate, they would have an annealing furnace—a muffle they called it. They build an oven thirty or forty feet long, and they build fires at each side and have the flame come over the furnace. They put the brass in that. They run the sheets in long when they are long, and short when they are short. I want my friend Durfee to tell you just exactly what is done with that furnace, because I want you to know what people will do in order to show that a thing will not work. I want some institution to try to make those brass manufacturers do their work right. I know the Professor will excuse me for what I said about his experiment.

Prof. Egleston.—You did not tell it right; that is the only trouble.

Mr. Hobbs.—If you can do anything by which you can bring the manufacturers of what is called brass up to a standard which will be either good or bad, it will be a great benefit.

Prof. Egleston.—Before the next speaker, I want to correct one or two things that Mr. Hobbs has said. In the first place, I feel very much like telling you what I do *not* know about brass. Mr. Hobbs speaks about the brass manufacturers of this country. It has been my great misfortune to follow the brass manufacture from this country to England, and from England to France, Belgium and Austria, and while I have not been to Russia, I will guarantee that there also it is as bad as it is here.

Mr. Hobbs says that my report to him was that his furnaces were wrong, and so I did say at first, but after a more extended examination, my report was that the *manufacturers'* furnaces were wrong, and that the metals were *fatigued* before he received them.

Mr. Durfee.—To continue the discussion of the methods adopted by the manufacturers of brass, I will say that my friend Hobbs has not gone into the details of the matter as fully as might be done. Instead of putting copper into the pots, they really do not know what they put in. They put something in that has the *color* of Copper, and something which they *think* is Zinc. In the furnace they have a melting-pot, and to save money and time and some other things, they put two or three ingots of copper inside the furnace on top of the coal around the pot. Sometimes free oxygen gets into that furnace, and sometimes it does not. The copper on the coal gets red hot, and sometimes it oxidizes, and when that goes into the pot, the oxygen is retained by the metal that is produced. I never knew of a brass manufacturer having a piece of zinc analyzed. Then, as to the matter of annealing: when there is a great deal of metal to be annealed, they pile up perhaps twenty-five or thirty sheets on top of each other. The top sheet may or may not be colder than the bottom, but no one who has had any experience at all in the manufacture of metals—in the iron trade—would expect for a moment to get a sheet uniformly annealed in that way. Those sheets remain in the annealing furnace a longer or a shorter time, and the man who feeds the fire feeds it according to his judgment. Sometimes he gets a big fire and sometimes he does not. Another thing in connection with the manufacture of brass, is scrap. All manufacturers use their own scrap, and scrap which comes to their works. The scrap is of a very miscellaneous character, and it is put into a big iron mortar and pounded into a mass that will fit a pot.

In regard to the Siemens furnace for the refining of copper, at one of the works in the Naugatuck Valley they decided to put up such a furnace for that purpose. I went to France and saw some Siemens furnaces that had been in successful use for four years in works where there were unusual obstacles to overcome. One obstacle was the proximity of water to the furnaces. All those obstacles had been overcome, and they were making copper that was used for the most trying work. In another works, in England, I saw a Siemens annealing furnace that had been used for sixteen years for annealing brass, and the proprietor of the works assured me that it was only by the use of those furnaces that he was enabled to maintain his standing in the trade. He was enabled by the use of those furnaces to do his work with much greater economy than his neighbors, as regards the quantity of the work and the waste in the work. Aside from this one Siemens furnace in use at Ansonia, I do not think there has been a change in the process of manufacturing brass since the manufacture was started in this country. With regard to the starting of this furnace, at the outset there was a good deal of trouble with it. Some of the men insisted that they knew more about it than anybody could tell them. I think, in the " poling " operation, they at first used twenty-five or thirty poles; but now the furnace is working very satisfactorily, and they get along with about six poles.

Mr. Hobbs.—In making the metal that we use, the manufacturers always take their own scrap and put it in. The brass they make for the market is quite a different thing. Where they attempt to make good brass, they are very particular about their scrap.

Prof. Egleston.—There is one thing alluded to which, I think, every mechanical engineer ought to know, and that is the variable quality of merchant copper. I have been analyzing copper for

many years, and when I get a sample of copper, I never kno
whether it is copper or something else. I sometimes get coppe
that is 99.999 fine. Merchant copper is never finer than tha
Then, too, I often get commercial copper—98 per cent. If the
is any one thing that is disastrous to copper and all its alloys, it
oxygen. This oxygen may be absorbed either in the furnace o
after the metal is taken out when it is brought to a red heat. I
have often seen copper in this same Naugatuck Valley, which whe
it came out of the furnace contained, judging from the fractur,
from 1 to 1.5 per cent. of oxide of copper.

With regard to brass—the manufacture of brass is extremely defective; but I must say a word for the brass manufacturer. The competition in brass is such that if any manufacturer undertook to make a series of experiments with the view of improving his product, the saving would be so small that he could not feel himself justified in doing it. I have been consulted by a very large number of brass manufacturers, and I have shown them, as I think, reasons why they should make changes, and they have replied: We admit all this, but we cannot do it simply because we cannot afford to put on the market a better material, for the reason that we cannot get anybody to buy it at a price which would be remunerative. If you *will* pay for *trash*, gentlemen, and not throw it back on the manufacturers' hands, you will never get good brass. The *purchaser*, not the manufacturer, is to blame. When he is willing to pay a good price he will get a good article.

Dr. Grimshaw.—I am a good deal disgusted with attempts to get good copper castings. I do not think I ever saw good ones until about two years ago. I have made some good copper castings by using horn filings as a flux. It will make an addition of about 10 to 15 per cent. in the weight, and the color will be a fine salmon, while the grain will be like statuary bronze.

Mr. Leavitt.—There is a great difficulty in the furnace. Mr. Cooper, of the Detroit and Lake Superior Copper Company, has discovered a process by which copper can be refined so as to give 99.998 fine. It is, I believe, quite an expensive process, so that such copper is not usually put on the market. Unless copper is kept from the air in poling and in all the subsequent processes, there are likely to be hard particles formed. I have had experience with Babbitt-metal which was very remarkable, because, while I thought the mixture was thorough, copper was found in fine granules, and I suppose the brass makers find a similar difficulty. There are impurities, and the impurities do not get thoroughly diffused. Many years ago I traveled with Mr. Grout, who was, I think, connected with the Detroit and Lake Superior Copper Company, and he related to me the extreme difficulties they had had in their early days. He said that their copper was miserable stuff, and that the use of a reverberatory furnace (which, practically, is a gas furnace) has resulted in the improvement of the copper standard very decidedly. At the same time Mr. Cooper has found in his practice that there were further improvements to be made, that they were practicable, and that if consumers will pay for pure copper they can have it.

Mr. Durfee.—With a reverberatory furnace, using solid fuel, I believe it is a practical impossibility (I do not care how many air passages you put in, or what combination of air passages there may be) so to regulate the furnace that a uniform result can be obtained. Unless the fire is kept with wonderful care, air will pass over the bridge wall into the furnace, and fragments of coal will be carried over the bridge by the blast, and injure the metal under treatment. In regard to Babbitt-metal, I remember a very curious circumstance which happened some years ago, which illustrates very well the importance of accurate analytical investigation. A friend of mine, engaged in the manufacture of "brasses" for a large railway, was likely to lose his business, because somebody came along with a cheaper white metal composition which would answer the purpose fairly. He showed me a piece of this white metal, and he said, "I wish I knew what that was made of." I said, "Why don't you have it analyzed?" Three or four weeks afterward he said to me, "I don't want you to talk to me any more about chemistry," telling me that he cut that piece of metal into three pieces, and that he sent one piece to a chemist in Philadelphia, another to a New York chemist, and the third to a Boston chemist. These men were all eminent men (the New York and Boston chemists are now dead), and he showed me the reports received from them, in which there was so much difference between the results that you would not regard the samples as being taken from the same metal. In each analysis there appeared a metal not found in either of the other analyses. Each of the chemists employed was eminent in his profession, and their age and reputation entitled them to confidence. The fact that sometimes analytical work is put into the hands of incompetent assistants, and the results, whatever they may be, are forwarded to the client, may afford a clue to an explanation of the singular diversity of results in the above case.

As an illustration of the trouble we sometimes have with ignorant workmen, I will relate a circumstance which occurred some years ago, while I had temporary charge of a plate-glass factory, in which for polishing the plate-glass we used "crocus." The superintendent of the polishing department came to me with a serious complaint about the crocus. I said, "What is the trouble with it?" He said, "Well, Mr. Durfee, I have been thinking about that crocus, and I have made up my mind that there is too much *oxide of iron* in it." (Laughter.)

Mr. Towne.—I may mention that in the effort to attain proximate uniformity in color in some work I have to do, we have tried pigging our metal two or three times, starting with an original composition, usually copper, tin, and zinc. The alloy has been melted, pigged, re-cast, pigged again, and a third time melted before being put into the molds. Of course more or less oxidation takes place, but greater uniformity has been obtained in the colors of metals in that way. In casting alloys of copper, particularly those known as bronze, it is a practical difficulty, and a very serious difficulty, to get anything approximating uniformity of color. Often the same casting will vary in color in different parts, and thus far I have learned of no process which overcomes that difficulty.

Mr. Samuel Webber.—This discussion on the constitution of metals recalls to me an experience of my own many years ago. When I was quite a young man, I was engaged in the construction of a machine for tracing figures on copper rollers, which were afterward to be etched by nitric acid and used in calico printing. We spent months over that work before we could produce a perfect impression on the cloth. For the purpose of making the tests, we ruled the varnished copper diagonally with bands of parallel lines. The rollers were then etched and placed in the printing machines, and an impression taken. We thought the trouble was an inequality of cut in the diamond-points. We got up a diamond machine which should grind all those points, so as to make them exactly alike. We then spent six months almost in cleaning the copper, and trying various methods to equalize the coat of varnish. At length I observed the appearance of an impression from an old roller. It had been worn down nearly to the mandrel. The appearance was a cloud, as we call it, running across the whole width of the cloth, looking as near as possible like a common claw-hammer, with the claws knocked off. I said to the superintendent of the print works that I believed I had discovered the cause of the trouble. I said, "I want permission to have this roller turned off and to re-rule and re-etch it and print it again." He said, "Certainly, do anything you like." I repeated the experiment, and the second result was precisely like the first one. The roller was turned off again, and I went over the whole surface of the roller, burnished it down to uniform density, re-ruled it, and produced the first perfect impression, and the process is now used throughout the United States, in all calico print works where etching is employed.

Prof. Egleston.—I am delighted to hear what Mr. Webber has said. I have a dozen photographs in my pocket which show the very thing he speaks of. I have stamped into metals with a perfectly plain surface certain names and figures; have filed them out, and made them perfectly smooth, and etched them with acids, and have found that the fatigue of the metal penetrated sometimes a quarter of an inch. The action of the acid on the surfaces rendered unequally hard was such that it could be printed in a press, and I had quite a number of copies struck off in this way. The same thing may be said in regard to your rails, or any kind of iron or steel which has been fatigued. Every place where gagging has been done upon a rail can be made visible in the same way. I wish I had known beforehand that this discussion would take this turn, because I should have been able to show a number of phenomena that are extremely interesting to mechanical engineers.

I do not want to have the members go away with the idea that the copper people are the most careless people in the world. There is nothing to be done in the whole range of metallurgy that is so difficult as the refining of copper. I have seen one of the best refiners living change his pitch and put up his door four times in the course of two hours. There is no necessity, however, for that being done. It is possible to keep the pitch of the copper exactly the same, from the time it is ready to ladle until the last drop of the copper is out of the furnace. I have sampled and analyzed copper which, month after month, would give 99.97, 99.98, and even 99.99 of copper. I believe that the Siemens furnace is the furnace that is going to solve the problem for copper. I do not feel quite so sure for brass. I am certain that a furnace can be built which will give much better results than any thus far obtained. There is not a single other metal which is so sensitive as copper to oxygen. The copper people do not progress, and the reason that they do not is that they are dependent on their refiners, and the refiner is one man, and he is generally the only man about the establishment who can make the copper. In Lake Superior they are better off. They do not depend on one man. They have half-a-dozen men, any one of whom can take the place of any one else in the establishment. But in our part of the country they have one man, and if that man is determined that no improvement shall be adopted, he can succeed in preventing its adoption. I have told manufacturers in some cases that it was of no use for them to go to the expense of applying improvements, because their refiner was determined they should not succeed, and under such conditions they would not.

Mr. Samuel Webber.—I would like to add that the rollers on which the experiments I described were made were old rollers that had been standing the wear and tear of years. The outer surface had all been turned down, and the part we used was the thin part which lay next to the mandrel, which of course received the full strain of the mandrel upon it before it was transferred to the outer surface, and very likely there might have been what Prof. Egleston calls an actual fatigue of the copper.

Mr. Durfee.—In regard to what Mr. Webber said, I do not wish to be understood as saying that the copper used for calico rolls *is* the purest copper that we find. I intended to say that the copper that should be used for calico rolls should be the purest copper that can be produced, and the reason why the party in France used the Siemens furnace was that by its use they could make a better copper for the manufacture of rolls than they could make with the ordinary furnace before used.

Mr. Webber.—I understand that perfectly, Mr. Chairman.

Mr. Grant.—Was the roll cast on a core?

Mr. Webber.—Cast on a core originally, and then rolled out on a mandrel.

Mr. Hobbs.—There is one other point I would like to allude to, and that is the deterioration of brass after its manufacture. In one instance a quantity of shells were packed in their boxes and stored away, and in the course of three or four months we began to have complaints about the heads breaking off. It led to an examination, and we found that shells which had been laid aside more than four months showed signs of cracking, and they actually broke in the course of a year, the heads tumbling to pieces themselves.

PAPERS

OF THE

PITTSBURGH MEETING,

MAY, 1884.

PROCEEDINGS

OF THE

AMERICAN SOCIETY OF MECHANICAL ENGINEERS.

PROCEEDINGS OF THE PITTSBURGH MEETING: MAY 20 TO 23, 1884.

Executive Local Committee:—Wm. R. Jones, *Chairman*, Wm. Metcalf, Jacob Reese, Harris Tabor, J. P. Witherow.

The opening session was held by invitation of the Engineers' Society of Western Pennsylvania, in the rooms of the Y. M. C. A. of Pittsburgh, to listen to a report by a committee of that society on "Natural Gas for Industrial Purposes." The session was called to order at eight o'clock by President Miller, of the local society, who by a few prefatory remarks introduced President Sweet of the Mechanical Engineers. The latter made a short speech in response, and called for the Report which was the business of the evening, This report will be found in the Appendix, and was presented by Mr. Hunt, of the local society. In the discussion which followed, Messrs. Kent, Metcalf, Reese, Jones, Henning, Strong, Towne, Durfee, Webb, and Kirchhoff, of the Mechanical Engineers took part, and Messrs. Jarboe, Robertson, Hunt, Hahn, Painter, Morton, and Dempster of the local society. At the close of the discussion, President Sweet gave some notices as to the sessions of the society, and an adjournment was taken till the next morning at ten o'clock.

Wednesday, May 21st.

The regular sessions were opened at quarter-past ten o'clock, on the morning of Wednesday, May 21st, at the rooms of the Western Iron Association, No. 77 Fourth Avenue.

The names of the following gentlemen appeared on the Secretary's registers of members in attendance:

Allison, Robert	Port Carbon, Pa
Baldwin, S. W.	N. Y. City.
Barnaby, C. W.	Salem, O.
Barrus, Geo. H.	Boston, Mass.
Betts, Alfred	Wilmington, Del.
Bond, Geo. M.	Hartford, Conn.
Brown, A. G	Pittsburgh, Pa.
Bullock, M. C.	Chicago, Ill.
Byllesby, H. M.	Hamilton, Ont.
Cheney, W. L	Boston, Mass.
Cloud, J. W	Altoona, Pa.
Cogswell, W. B.	Syracuse, N. Y.
Cole, J. W.	Columbus, O.
Comly, G. N.	Wilmington, Del.
Cooley, M. E.	Ann Arbor, Mich.
Dean, F. W.	Phila., Pa.
Donovan, W. F.	Chicago, Ill.
Douglas, Wm.	Phila., Pa.
Drummond, W. W.	Louisville, Ky.
Durfee, W. F.	Bridgeport, Conn.
Emery, C. E.	N. Y. City.
Fay, R. C.	Hopedale, Mass.
Fawcett, Ezra	Alliance, O.
Forsyth, R.	Pittsburgh, Pa.
Francis, W. H.	N. Y. City.
Fritz, John	Bethlehem, Pa.
Giddings, C. M.	Massillon, O.
Gray, G. A., Jr.	Cincinnati, O.
Hamilton, H.	Youngstown, O.
Hand, S. A.	Toughkenamon, Pa.
Hawkins, Jno.	Taunton, Mass.
Hemenway, F. F.	N. Y. City.
Hemphill, Jas.	Pittsburgh, Pa.
Henning, G. C.	N. Y. City.
Hill, Wm.	Collinsville, Conn.
Hollerith, H.	Washington, D. C.
Hollingsworth, S.	Boston, Mass.
Holloway, J. F.	Cleveland, O.
Hunt, R. W.	Troy, N. Y.
Hutton, F. R., *Secretary*	N. Y. City.
Jones, H. C.	Wilmington, Del.
Jones, Washington	Phila., Pa.
Jones, Wm. R.	Pittsburgh, Pa.
Kaffenberger, G.	Hamilton, O.
Kane, J. S.	Rochester, N. Y.
Kent, Wm.	N. Y. City.
Kirby, F.	Detroit, Mich.
Kirchhoff, C., Jr	N. Y. City.
Lane, J. S.	Akron, O.
Leavitt, E. D., Jr.	Cambridgeport, Mass.
Le Van, W. B.	Phila., Pa.

Loiseau, E. F.	Phila., Pa.
Mattes, Wm. F.	Scranton, Pa.
Metcalf, Wm.	Pittsburgh, Pa.
Moore, C. A.	N. Y. City.
Morgan, Jos., Jr.	Johnstown, Pa.
Morgan, T. R., Sr.	Alliance, O.
Morgan, T. R., Jr.	Alliance O.
Morgan, J. R.	Alliance, O.
Morris, H. G.	Phila., Pa.
Nagle, A. F.	Chicago, Ill.
Nicholson, S	Providence, R. I.
Parks, E. H.	Providence, R. I.
Porter, C. T.	N. Y. City.
Reese, J.	Pittsburgh, Pa.
Robinson, S. W.	Columbus, O.
Rogers, W. A.	Cambridge, Mass.
Saulles, De A. B.	Dunbar, Pa.
Schuhmann, G.	Reading, Pa.
Scott, Olin	Bennington, Vt.
Smith, O.	Bridgeton, N. J.
Smith, J. M.	Detroit, Mich.
Smith, W. F.	Carlin, Nev.
Snell, H. S.	Philadelphia, Pa.
Spies, A.	N. Y. City.
Stearns, A.	Brooklyn, N. Y.
Strong, G. S.	Philadelphia, Pa.
Stutz, S.	Pittsburgh, Pa.
Swasey, A.	Cleveland, O.
Sweet, Jno. E., *President*	Syracuse, N. Y.
Tabor, H.	Pittsburgh, Pa.
Thompson, C. T.	Philadelphia, Pa.
Thompson, E. P.	Elizabeth, N. J.
Towne, H. R.	Stamford, Conn
Walker, Jno.	Cleveland, O.
Webb, J. B.	Ithaca, N. Y.
Weeks, G. W.	Clinton, Mass.
Wellman, S. W.	Cleveland, O.
Wiley, Wm. H.	N. Y. City.
Williams, S. T.	Tacony, Phila., Pa.
Wood, W.	Philadelphia, Pa.
Woodbury, C. J. H.	Boston, Mass.
Zimmermann, W.	Pittsburgh, Pa.

Besides the members of the society present, Mr. John Gjers of Middlesboro, England, was an invited guest, and Messrs. McCandless, Kirk, Miller, Harlowe and others of Pittsburgh accompanied the Society on its excursions.

On motion, the reading of the minutes of the last meeting was dispensed with.

The Secretary by direction of the Council presented the following report to the Society, of business which had been transacted since the annual meeting in November:

REPORT FROM THE COUNCIL TO THE SOCIETY.

The Council would report to the Society that it has held meetings for the transaction of business on Nov. 22, Feb. 27, April 17, and May 20. These meetings have been long and have been largely attended. The standing committees have reported regularly, and their action has been approved.

Beside the routine business of the Society, the scrutiny of applications for membership and like duties, the Council has appointed various committees for special service. Carrying out the resolution of the Society, a committee consisting of Messrs. E. B. Coxe, W. F. Durfee, and Joseph Morgan, Jr., is acting on the matter of joint invitation to the British Iron and Steel Association. A committee of three is preparing memorial resolutions on the death of Dr. C. W. Siemens and O. Hallauer, Honorary Members of the Society, to be submitted later. A communication from a committee of the American Institute of Mining Engineers has been received, in reference to uniformity in size and shape of specimens for test in testing machines. Prof. T. Egleston and Mr. E. D. Leavitt of the committee were directed to represent this Society also in that service, and the Secretary was directed to present the communication before the Society, at its next meeting. The Committee on the Revision of the Rules of the Society reported to the Council on a method of submitting the proposed amendments to the Society, and their recommendations were adopted, and the Secretary was directed to send out the proposed changes in the Rules for the information of members.

The Nominating Committee to present nominations for the offices falling vacant in November, 1884, has also been appointed by the President with the approval of the Council. It consists of Messrs. S. W. Baldwin, Hague, Martin, Morris, and Stetson.

The communication of Mr. Charles W. Copeland, as to the founding of the library of the Society for the use of members, has been referred to the Committee on Permanent Site, consisting of Messrs. Trowbridge, Copeland, Egleston, Merrick and Hoadley.

The Council has also passed a resolution, that the annual meetings hereafter be opened by an address from the retiring President, and that such address be printed as part of the proceedings of that meeting.

The followin semi-annual report from the Treasurer was presented and read by the Secretary:

To the American Society of Mechanical Engineers.

GENTLEMEN:—I herewith enclose you my report of the receipts and expenditures of the Society, during each month since the last annual meeting, with other items connected with the office of Treasurer of the Society.

REPORT OF THE TREASURER OF AMERICAN SOCIETY OF MECHANICAL ENGINEERS.

1883.		
Nov. 1, Balance on hand		$303 18
Nov. 30, Receipts during this month		2,326 26
Dec. 31, " " "		788 99
1884.		
Jan. 31, Receipts during this month		418 27
Feb. 29, " " "		212 90
March 31, " " "		528 71
April 30, " " "		309 27
May 19, " " "		811 18
1883.		
Nov. 30, Disbursements this month	$734 33	
Dec. 31, " "	827 04	
1884.		
Jan. 31, Disbursements this month	1,959 28	
Feb. 29, " "	775 59	
March 31, " "	318 76	
April 30, " "	363 00	
May 19, " "	349 28	
Balance on hand this date	871 48	
	$5,698 76	$5,698 76

There are in my hands at this time the following bills, which have been audited by the Finance Committee of the Society, but have not yet been paid, viz:

J. J. Little, March 31	$424 00
J. J. Little, March 31	77 00

Making a total of five hundred and one dollars ($501 00).

CHAS. W. COPELAND, *Treasurer*

In the absence of the chairman of the Committee on Revision of Rules, the statement of the labor and plans of that committee was made by Mr. C. J. H. Woodbury, one of its members, as follows:

The Committee on the Revision of Rules have held a number of meetings, and, in addition to that, have done a large amount of work by examining the rules and regulations of every engineering society in Great Britain and in this country, of which the rules could be found. As a result of that work, they made a number of amendments, which were generally very slight in their character, and submitted them to the membership by mail last March. Since that time they have received a number of communications suggesting further alterations and modifications, and they desire to examine them very carefully. The committee will further report in this matter, and finally, during the summer, will send the amended rules around to the membership, accompanying them with a ballot, so that the whole Society, and not merely those present at any meeting, will have an opportunity to vote upon the question ot adopting the proposed amendments.

The Secretary read the following communication from Professor Egleston, chairman of a committee appointed by the American Institute of Mining Engineers at its session last February:

NEW YORK, *April* 3, 1884.

American Society of Mechanical Engineers.

GENTLEMEN:—The American Institute of Mining Engineers have recently appointed a Committee on Uniform Standards for Test Pieces of Iron and Steel, and other constructive materials, consisting of Mr. Clark, formerly of Clark, Reeves & Co., Mr. A. P. Boller, lately connected with the Manhattan Railway Co., Mr. Hill, of the *Iron Age*, Mr. E. D. Leavitt, Jr., the well-known hydraulic engineer, and myself as chairman.

This committee is desirous of learning what has been done on the subject by other societies, and would like to be put in communication with any committee of your Society which may be directly or indirectly concerned in this matter. I should also like to know if you have any publications in the transactions of your Society, or know of those of any other society or individual which have any bearing on this subject. If so, we shall be greatly obliged if you can put us in the way of obtaining them. We are anxious to do what we can to bring about a uniformity of standards for test pieces, not only throughout the United States, but also in other countries, so that the results of tests made all over the world may be comparable, and all tests made may therefore be of universal value.

Any information or aid that you can give us therefore in this important matter will be gratefully received.

Yours truly,
Thos. Egleston, *Chairman.*

The Special Committee of the Council, appointed to count the ballots cast for new members under Rule XIII, presented the following report:

New York, *May* 14, 1884.

To the Council of the American Society of Mechanical Engineers.

The undersigned, who were appointed a Committee of the Council to act as tellers to count ballots cast for or against each of the persons proposed for membership in the Society of Mechanical Engineers, to be voted for previous to the spring meeting of 1884, hereby certify that we met this day at the office of the Society, and proceeded to the discharge of our duties.

There were cast in all two hundred and forty-seven ballots, and all the persons whose names appear on the ensuing list were duly elected, in accordance with the rules, to their respective grades.

Chas. T. Porter,
Allan Stirling,
Tellers.

Albrecht, Otto	Philadelphia, Pa.
Allison, Robert	Port Carbon, Pa.
Anderson, J. W	South Bend, Ind.
Anthony, G. C	Providence, R. I.
Barnaby, Chas. W	Salem, Ohio.
Booraem, J. V. V	Brooklyn, N. Y.
Boyd, James T	East Boston, Mass.
Broadbent, Sidney	Scranton, Pa.
Briggs, J. G	Terre Haute, Ind.
Brooks, E. C	Cambridge, Mass.
Brown, A. G	Pittsburg, Pa.
Burr, J. T	Brooklyn, N. Y.
Bushnell, R. W	Cedar Rapids, Ia.
Caird, Robert	Pullman, Ill.
Carr, C. A	Hoboken, N. J.
Cartwright, Robert	Stamford, Conn.
Chamberlin, F. L	Cleveland, Ohio.

Church, B. S.	New York.
Clarke, S. J.	New York.
Cooley, M. E.	Ann Arbor, Mich.
Corbett, C. H.	Brooklyn, N. Y.
Cullingworth, G. R.	New York.
Cummings, A. W.	Steelton, Pa.
Dickey, W. D.	New York.
Dixon, Chas. A.	Newburgh, N. Y.
Dodge, J. M.	Philadelphia, Pa.
Douglas, Wm. M.	Beaver Falls, Pa.
Fisher, C. H.	Boston, Mass.
Ford, John D.	Baltimore, Md.
Fowler, John	Louisville, Ky.
Francis, W. H.	New York.
Freeland, F. T.	Leadville, Col.
Gaskill, H. F.	Lockport, N. Y.
Gaunt, Thomas	Cold Spring, N. Y.
Geer, James H.	Johnstown, Pa.
Gold, S. F.	Englewood, N. J.
Graham, J. S.	Rochester, N. Y.
Gray, G. A. Jr.	Cincinnati, O.
Greenwood, J. H.	Columbus, O.
Hammer, A. E.	Branford, Conn.
Hemphill, James	Pittsburgh, Pa.
Hanscom, W. W.	San Francisco, Cal.
Harmon, O. S.	Jersey City, N. J.
Hawkins, J. T.	Taunton, Mass.
Henney, John, Jr.	Hartford, Conn.
Herreshoff, J. B.	Bristol, R. I.
Hill, James W.	St. Louis, Mo.
Hill, Warren E.	Brooklyn, N. Y.
Hollis, Ira N.	Schenectady, N. Y.
Johnson, C. R.	New York.
Jones, W. H.	New York.
Jones, R. R.	Chicago, Ill.
Knight, C. A.	Glasgow, Scotland.
Kane, John	Rochester, N. Y.
Lacy, John H.	Pullman, Ill.
Ladd, James B.	Philadelphia, Pa.
Lewis, J. L.	Pittsburgh, Pa.
Lewis, Wilfred.	Philadelphia, Pa.
Loiseau, Emile F.	Philadelphia, Pa.
Magruder, W. T.	Taunton, Mass.
Mahony, James	New York.
Manning, C. H.	Manchester, N. H.
Mirkil, T. H., Jr.	Philadelphia, Pa.
Morgan, James	Pittsburgh, Pa.
Morgan, John R.	Alliance, O.
Morgan, T.R., Jr.	Alliance, O.
Morse, C. J.	Youngstown, O.
Morse, C. M.	New York.

Mudge, B. C.	Boston, Mass.
Parsons, W. P.	Hoosick Falls, N. Y.
Peabody, C. H.	Boston, Mass.
Pendry, W. A.	Detroit, Mich.
Rae, Charles W.	Washington, D. C.
Randolph, L. S.	Susquehanna, Pa.
Reynolds, Edwin	Milwaukee, Wis.
Rogers, Prof. W. A.	Cambridge, Mass.
Rowland, T. F., Jr.	Brooklyn, N. Y.
Sancton, E. K.	Scranton, Pa.
Schuhmann, George	Reading, Pa.
Schwamb, Peter	Boston, Mass.
Sheldon, T. C	Boylston, Mass.
Skinner, L. G.	Erie, Pa.
Small, H. J.	Brainerd, Minn.
Smith, Charles D.	Plantsville, Conn.
Snell, Henry J.	Philadelphia, Pa.
Sorzano, J. F.	New York.
Spiers, James	San Francisco, Cal.
Spies, Albert	New York.
Springer, J. H.	Pittsburgh, Pa.
Stalmann, Otto	Lake Linden, Mich.
Starbuck, G. H.	New York.
Stone, Joseph	Lawrence, Mass.
Stutz, Sebastian	Pittsburgh.
Sunstrom, K. J.	Providence, R. I.
Thompson, E. P.	Elizabeth, N. J.
Thompson, E. W.	Thomasville, Ga.
Thorne, W. H.	Germantown, Pa.
Thurman, G. E.	Louisville, Ky.
Trautwein, A. P.	Brooklyn, N. Y.
Upson, L. A.	Thompsonville, Conn.
Van Winkle, Franklin	New York.
Vogt, Axel S.	Altoona, Pa.
Wall, E. B.	Columbus, O.
Waterman, J. S.	Ithaca, N. Y.
Webster, J. F.	Springfield, O.
West, Thos. D.	Cleveland, O.
Westinghouse, H. H.	Pittsburgh, Pa.
Whitaker, E. T.	Sacketts Harbor, N. Y.
Wightman, D. A.	Pittsburgh, Pa.
Williams, S. T	Philadelphia, Pa.
Wood, S. A.	Springfield, Ill.
Wright, J. Q.	New York.
Zimmermann, W. F.	Pittsburgh, Pa.

ASSOCIATES.

Brown, D. N.	New York.
Delano, T. H.	New York.
Douglas, E. V.	Philadelphia, Pa.
Gibson, Wm., Jr.	New York.
Worthington, Geo.	New York.

JUNIORS.

Day, F. M.	Milford, Mass.
Guthrie, E. B.	Buffalo, N. Y.
Warrington, J. A.	Chicago, Ill.

PROMOTIONS.

Vanderbilt, Aaron (Associate A.S.M.E.)	New York.
Porter, H. F. J. (Associate A.S.M.E.)	New York.
Giddings, C. M. (Junior A.S.M.E.)	Massillon, O.

No further new business being presented, the President called for the first paper, by Mr. Jno. W. Cloud, of Altoona, Pa., on "Helical Springs." In the discussion which followed, Messrs. O. Smith, Webb, Metcalf and Emery took part. The succeeding paper was presented by Prof. Wm. A. Rogers, of Cambridge, Mass., entitled "A Practical Solution of the Perfect Screw Problem." In the extended discussion which it elicited, Messrs. O. Smith, Towne, Webb, Hand, Sweet, Bond, Reese, Woodbury, Kent, Robinson, Porter, Durfee and Emery were heard, and also Mr. Brashear, of Pittsburgh, by invitation.

The hour for morning adjournment was reached before the discussion seemed to be closed, so that its completion was postponed to the next session, and an adjournment was taken till eight o'clock in the evening. President Sweet, before putting the motion to adjourn, announced that the Engineers' Society of Western Pennsylvania had courteously extended the use of its rooms and library to the visiting Society during its stay in Pittsburgh, and conveyed an invitation from the hosts to make use of the rooms for social or business purposes.

The afternoon of Wednesday was purposely left unappropriated, in order that the members of the Society might have an opportunity to visit any special places of individual interest, beside those offered by the general excursions. The local committee had made arrangements that the following places might be visited by certified members of the society, and a map of the city had been printed with these points suitably marked for easy reference: Moorhead Bros. & Co., Spang Steel Co., Spang, Chalfant & Co., Graff, Bennett & Co., Wm. Clark & Co., Carnegie Bros. & Co., Wilson, Walker & Co., Shoenberger & Co., Zug & Co., Brown & Co., Lindsay, McCutcheon & Co., Manchester Steel & Iron Co., Oliver Bros. & Co., Pittsburgh Forge & Iron Co., James Wood's Heirs, J. Painter's Sons, Phillips, Nimick & Co., J. Dilworth, A. M. Byers & Co., H. Lloyd's Sons, Everson, Brown & Co., Chess, Smythe &

Co., Republic Iron Co., Jones & Laughlins, Elba Iron & Bolt Co., Tin Plate Co., N. D. Wood & Co., National Tube Works, Crescent Steel Works, Anderson, Linden & Blair Steel Co., Park Bro. & Co., Steel Casting Co., Hussey, Binns & Co., Hussey, Howe & Co., A. J. Nellis, Smith, Sutton & Co., Singer, Nimick & Co., Crown Steel Works, Pittsburgh and Edgar Thomson Bessemer Works, and Oliver & Withcrow. Of machine shops and manufacturing concerns, the following might be visited: H. M. Bole, Jas. Rees & Co., Mackintosh, Hemphill & Co., Totten & Co., Westinghouse Machine Co., Union Switch & Signal Co., Westinghouse Air Brake Co., Robinson, Rea & Co., A. Garrison & Co., Cavett & McKnight, A. Fulton's Sons & Co., Pittsburgh Locomotive Works, H. K. Porter Locomotive Works, Keystone Bridge Works, A. French & Co., Crescent Spring Co. The various blast furnaces about the city might also be inspected.

The evening session was called to order at 8 P. M. in the rooms of the Western Iron Association. The Secretary read various announcements as to the excursions on the following days. The chair called for any further remarks as to the paper by Prof. Rogers, but no discussion springing up, the paper by Mr. Wm. Kent, of New York, was called for, on "Rules for Conducting Boiler Tests." Messrs. Le Van, Emery, Woodbury, Nagle, Porter, Webb and Leavitt took part in the discussion, and Mr. N. W. Pratt, of New York, had sent a suggestion in writing which was read by the Secretary. In closing the debate, Mr. Kent spoke at some length, closing as follows:

"If the subject of boiler tests had been discussed more fully, and especially if it were written upon by the engineers of this society, we would find still greater difference of opinion than has been expressed in regard, first, to general principles, and secondly, and far more largely, as to detail. These differences show that engineers need to-day some set of boiler rules, and I have not brought in this particular set of rules with the idea that they are perfect and that none others should be adopted; but I want *some* standard set of rules, so that when engineers make a test, they can say it is according to an approved standard. I think it would be in order to move the appointment of a committee of this Society, to report on a standard set of boiler testing rules, which could be reported to our Society at the next meeting, whenever that is to be held. I do not suppose this Society will take any action in the matter further than to receive the report of the committee, but I think it would be a valuable thing to have such a report signed by a committee known to be composed of engineers of high standing."

The motion as amended, that the committee be appointed by the chair, was duly seconded and carried.

The President, before the close of the meeting, appointed this committee, consisting of Messrs. Kent, Hoadley, Thurston, Emery and Porter.

The paper by Mr. W. B. Le Van, of Philadelphia, Pa., was next read, entitled "New York to Chicago in Seventeen Hours." Messrs. Emery, Kirby, Leavitt and Kent took part in the discussion at its close.

The paper by Mr. C. E. Emery, which followed, entitled "Estimates for Steam Users," elicited discussion from Messrs. Kent, Leavitt, O. Smith, Le Van and Cole. When the discussion was completed, Mr. H. R. Towne, of Stamford, was called on for his paper on "A Drawing Office System." Messrs. Emery and Walker spoke on the topics suggested by it.

The session had by this time been prolonged to a late hour. Prof. Webb presented his paper on "Cross-sectioning with the Right-line Pen" by brief abstract, illustrating it by distributed blue prints. The paper by Oberlin Smith, of Bridgeton, N. J., on "A Positive Speed Indicator," was read, but elicited no comment, and Prof. Ordway's paper on "Non-conducting Coverings for Steam Pipes," a supplement to his previous paper, was presented and read by title only.

On motion, the Society adjourned.

Thursday, May 22d.

This day was devoted to a railroad excursion up the Allegheny valley. The members were conveyed from the headquarters of the Society at the Monongahela House to the station of the West Pennsylvania Railroad in Allegheny City by omnibuses furnished by the local committee. A special train conveyed the party, stopping at the Isabella Furnaces, the Creighton Plate Glass Works, and the Natrona Salt Works, on one side of the river. After lunch, served in the forward car, the train crossed the river, and stopped at the Pittsburgh Water Works, at the Keystone Bridge Co., the Crescent Street Works, Park Bro. & Co., and the Pittsburgh Locomotive Works. The train reached the Union Depot at about six o'clock.

In the evening, a subscription dinner was held at the Monongahela

hela House, at which seventy members and their friends were present. The responses after the meal were made by Messrs. Metcalf, Hunt, Gjers, Jones, Smith, Spies, Kent, and Hutton.

FRIDAY, MAY 23d.

This day was spent on the Monongahela River. The steamer Elizabeth had been chartered, and left the wharf a little before ten. Stops were made at Jones & Laughlin's, at the Duquesne Forge and at the Edgar Thompson Bessemer Steel Works.

After lunch had been served in the saloon cabin of the boat, a special session of the Society was held in the after part of the cabin to listen to the paper by Mr. G. H. Barrus of Boston, Mass., on "A Comparison of Three Modern Types of Indicators." In the interesting discussion which followed, Messrs. Porter, Hand, Robinson, Le Van, Emery, Moore, Kent, Sweet, and Holloway took part.

At the close of the discussion, Mr. Porter presented the following resolutions, which were put and carried with enthusiasm:

Resolved: (1.) That the American Society of Mechanical Engineers, through its members present at the Pittsburgh meeting, hereby extends its most cordial thanks to the Pittsburgh Local Committee, and through them to the Pittsburgh members generally, for the many provisions so thoughtfully made for the comfort, convenience, and pleasure of the visiting members, for the numerous and interesting excursions arranged by them among the industries of Pittsburgh, and, generally, for the many hospitalities extended to the Society.

(2.) That the Secretary be instructed to transmit a copy of these resolutions to the chairman of the Local Committee, and that the resolutions be entered at length on the Records of the Society as a permanent testimony to its grateful appreciation of the hospitality extended to its members at the Pittsburgh meeting of 1884.

Mr. Oberlin Smith presented the following resolutions, which were passed with similar acclamation:

Resolved: (1.) That the American Society of Mechanical Engineers begs hereby to convey to the several firms and corporations that have so kindly opened their establishments to the members, its most sincere thanks for the opportunities so accorded for visiting and inspecting the works wherein the great industries of Pittsburgh have been created and developed to the benefit not only of that city, but equally of the nation, which now heads the list of the steel-producing countries of the world.

(2.) That the thanks of the Society be also extended to the officers of the Pennsylvania R.R. Co., and the A. V. R.R. Co., through whose courtesy the excursions of the Society have been so pleasantly accomplished; and

(3) That the Secretary be instructed to communicate these resolutions in a fitting manner to those to whom the thanks of the Society are hereby expressed.

Mr. J. F. Holloway presented the following resolutions, which were unanimously passed:

Resolved: (1.) That the American Society of Mechanical Engineers tenders its warmest thanks to the Western Iron Association, and to the Engineers' Society of Western Pennsylvania, for the facilities so kindly provided by them for the sessions of the Pittsburgh meeting; for the opportunities offered to the members, on the opening night, of listening to the very interesting paper on Natural Gas, and of participating in the discussion thereon; and for the assistance so generously given by the two local societies to the visiting society in promoting the objects of the latter during its Pittsburgh meeting.

(2.) That the Secretary be instructed to convey to the officers of the two societies named, the purport of these resolutions.

After these resolutions had been duly voted, the President made a few closing remarks, and on motion the Society adjourned.

The boat had not quite reached the wharf at the close of the session, and impromptu speeches and expressions of kind feeling were called out from many sources, in recognition of the efforts put forth for the entertainment of the visiting members.

CXLII.

HELICAL SPRINGS.

BY JOHN W. CLOUD, ALTOONA, PA.

Having had recent occasion to investigate and study the matter of springs in the interest of a large consumer, and having had some experience for ten years past with the testing and the service of a great variety of types of helical springs, it has seemed to the writer that the results of the inquiry might be of interest.

Helical Springs have passed through a great deal of variation in shape of bar during the past few years, and it will be a large part of the burden of this paper to show that most of the variations alluded to are improper, and that the round bar is the best shape for the steel as distinguished from rectangular sections.

A helical spring is just as legitimate a subject for proper design, and adjustment of stresses and strains, as a bridge, a machine, or any other structure, and data are available which will render it just as easy for the engineer to say, with confidence in the result, how the spring should be made, with an assumed factor of safety, as it is to design the bridge or the machine. The data required are the elastic limit and modulus of elasticity of the tempered steel, and are not perhaps as voluminous as is desirable for various grades of steel variously tempered. Enough data are on record, however, to make the results of design valuable, and it is doubtful whether the factor of safety need cover any greater margin of ignorance than in the average structure. In order, however, to have reliable data for the grade of steel to be used for springs, twenty samples, ten tempered and ten untempered, and of sizes varying from $\frac{3}{4}$ of an inch to $1\frac{5}{16}$ inches diameter by five (5) feet long, were tested by the Government Testing Machine at Watertown Arsenal with the results which are given in Table No. I., which will be again referred to. The tempering was done in oil.

The formulæ showing the relations of stresses and strains in helical springs are given in a concise form by Reuleaux in "Der Constructeur," but without any explanation as to how they are obtained. The following mathematical discussion is, therefore, made, to show how these formulæ may be obtained, and, for the sake of

TABLE I.

CONDENSED STATEMENT OF TENSILE TESTS OF SPRING STEEL AT WATERTOWN ARSENAL.

TEST NUMBER.	DIAMETER OF BAR.	TEMPERED OR UNTEMPERED.	ELASTIC LIMIT.	TENSILE STRENGTH.	ELONGATION, PER CENT.	CONTRACTION OF AREA, PER CENT.	MODULUS OF ELASTICITY
	Inches						
3359	0.74	Untempered.	78,000	130,000	8.20	15.60	32,600,000
3360	0.74	"	77,000	152,090	6.40	10.50	33,000,000
3361	0.74	Tempered.	140,000	226,980	2.40	10.50	32,500,000
3362	0.74	"	140,000	232,790	3.30	7.90	31,300,000
3355	0.87	Untempered.	70,000	131,830	6.50	5.50	32,000,000
3356	0.87	"	73,000	146,330	5.60	7.70	31,000,000
3357	0.87	Tempered.	125,000	197,830	1.40	5.50	31,700,000
3358	0.87	"	125,000	160,000	0.55	5.55	31,600,000
3343	1.01	Untempered.	90,000	143,000	6.80	11.40	30,700,000
3345	1.01	"	82,000	134,750	3.60	5.70	31,300,000
3344	1.01	Tempered.	120,000	167,100	0.60	3.80	31,800,000
3346	1.01	"	120,000	189,375	1.30	8.80	31,900,000
3347	1.13	Untempered.	63,000	108,500	1.30	3.20	31,900,000
3349	1.13	"	66,000	94,500	0.55	1.50	30,700,000
3348	1.13	Tempered.	120,000	159,600	0.45	3.20	30,500.000
3350	1.13	"	125,000	164,200	0.55	3.20	32,500,000
3351	1.32	Untempered.	67,000	117,750	2.90	8.60	32,600,000
3352	1.32	"	64,000	104,850	2.40	3.60	30,800,000
3353	1.32	Tempered.	120,000	122,500	0.25	Hardly appreciable.	32,800,000
3354	1.32	"	115,000	141,000 applied. Test discontinued. Bar not fractured.	0.15	......	31,400,000

comparison with formulæ given by Reuleaux, his notation will be adopted. Some general deductions will also be made which are not referred to by him.

It is universally recognized by the authorities on Mechanics that the bar of a helical spring is subjected, mainly and almost wholly, to torsion. This is not a question needing further argument, but if any one interested cannot fully comprehend the fact, he may by a little careful experimentation satisfy himself upon this point, one of the most satisfactory methods perhaps being that explained by Mr. Oberlin Smith in an interesting paper read before this Society at Cleveland in 1883. [Trans. Vol. IV., p. 335.]

To examine a bar in torsion let

l = length of bar.

P = force twisting its free end.

R = radius at which P acts.

a = distance of most remote fibre from neutral axis, $= \frac{d}{2}$ for ound bar (Fig. 50), or $= \frac{\sqrt{b^2 + h^2}}{2}$ for rectangular bar (Fig. 1). Then,

The acting moment $= PR$ and resisting moment $= \frac{S\,I_p}{a}$, where S is the stress in a normal section at the distance a from

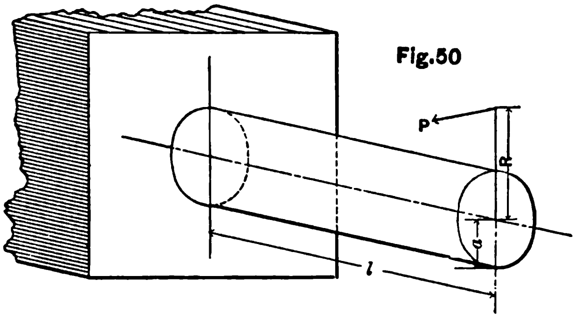


Fig.50

neutral axis, and I_p is the moment of inertia of the section about this axis, *i.e.*, the polar moment of inertia.

But to produce equilibrium the acting and resisting moments must be equal, *i.e.*,

$$PR = \frac{SI_p}{a} \text{ and } P = \frac{SI_p}{a\,R} \quad . \quad . \quad . \quad . \quad (1)$$

If θ represents the angle of torsion in length, l, then the relative angular displacement of any two sections dx apart, will be:

$d = \frac{PR}{I_pG}\,dx$, where G is the modulus of elasticity for torsion.

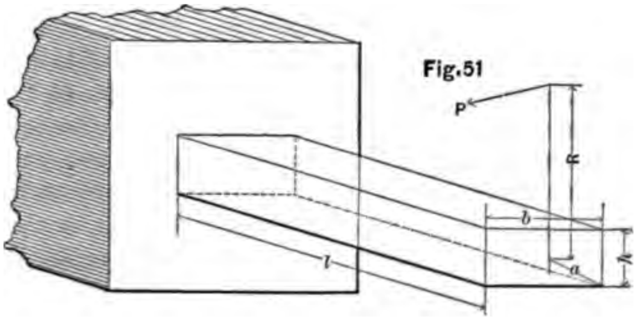


Fig.51

The whole angle θ, therefore, for length l is found by integration, between zero and l, to be

$$\theta = \frac{PR\,l}{I_pG} = \frac{Sl}{aG} \quad . \quad . \quad . \quad . \quad . \quad (2)$$

the constant of integration being zero.

In order to apply these formulæ to helical springs in compression, or extension, it is necessary to remark that the load on the spring, or the pull exerted, in either case respectively, is the force P; and the radius of the spring, or half its diameter to neutral axis, is the radius R, while the compression or extension is equal to $R\,\theta$.

In the following formulæ, therefore, which we will consider for compression only, though equally correct for extension if properly worded:

P = load on spring.
R = radius of spring to neutral axis.
a = distance from axis of bar to most remote fibre.
l = length of bar.
S = maximum stress in normal section under load P, *i. e.*, stress at distance a.
G = modulus of elasticity for torsion.
f = compression of spring under load P, whence equations (1) and (2) show the relations of stresses and strains, *i. e.*,

$$P = \frac{S\,I_p}{aR} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

and,

$$f = R\,\theta = \frac{R\,S\,l}{aG} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

Formulæ (3) and (4) will serve properly to design a helical spring to carry any maximum load, P, and produce any maximum compression, f, desired.

The maximum stress in a normal plane, S, is to be determined by fixing a proper factor of safety, the elastic limit of the tempered steel being supposed known, as well as the modulus G, from test of the material. It is requisite to remember in this connection that the stress in the normal plane at any point is not the maximum S at that point in the bar, as the maximum S acts in an oblique plane and is about five-fourths ($\frac{5}{4}$) as great as in the normal plane. The maximum value of S, used in the formula (3), should, therefore, be only four-fifths ($\frac{4}{5}$) as great as would be considered a safe, true maximum stress to be allowed in the steel, as when the spring is solid.

Similarly, the modulus of elasticity, G, for torsion, should not be called $\frac{1}{2}\,E$ (modulus in tension), but only $\frac{1}{2}$ of $\frac{4}{5}\,E = \frac{2}{5}\,E$. This for future use.

Continuing the general discussion:

OR CIRCULAR SECTIONS.

$I_p = \frac{\pi d^4}{32}$; and $a = \frac{d}{2}$,

rmulæ 3 and 4 become,

$$P = \frac{S\pi d^3}{16R}; \quad \dots \quad (5)$$

$$f = \frac{2RSl}{dG} = \frac{32PR^2l}{\pi G d^4}; \quad (6)$$

d hence, the following product,

$$Pf = \frac{S^2\pi d^2 l}{8G}; \quad \dots \quad (9)$$

Let V_c stand for volume of eel in cubic inches for circular rs—

$$V_c = \tfrac{1}{4}\pi d^2 l,$$

en formula (9) becomes,

$$Pf = \frac{S^2 V_c}{2G}; \quad \dots \quad (11)$$

erefore,

$$V_c = \frac{2GPf}{S^2} \quad \dots \quad (13)$$

FOR RECTANGULAR SECTIONS.

$I_p = \frac{bh}{12}(b^2 + h^2)$; and

$a = \frac{\sqrt{b^2 + h^2}}{2}$;

formulæ 3 and 4 become

$$P = \frac{Sbh\sqrt{b^2 + h^2}}{6R}; \quad \dots \quad (7)$$

$$f = \frac{12PR^2l}{bhG(b^2 + h^2)}; \quad \dots \quad (8)$$

and hence the following product,

$$Pf = \frac{S^2bhl}{3G}; \quad \dots \quad (10)$$

Let V_r stand for volume of steel in cubic inches for rectangular bars—

$$V_r = bhl;$$

when formula (10) becomes,

$$Pf = \frac{S^2 V_r}{3G}; \quad \dots \quad (12)$$

therefore,

$$V_r = \frac{3GPf}{S^2}; \quad \dots \quad (14)$$

Important general deductions may now be drawn from these rmulæ, as follows:

1st. Formula No. 13 shows that in round steel of a given ality and of a given factor of safety, as determined by certain merical values for G and S, the volume of steel, and, therefore, e weight of steel required, depends entirely upon the product f and is independent of any arbitrary dimensions chosen, such as e diameter and length of bar or diameter and length of helix, provided, of course, that if any of these are arbitrarily fixed the others must be properly related to them as dictated by the foregoing formulæ.

Formula No. 14 shows the same to be true for rectangular bars with any relations between b and h. Formulæ Nos. 13 and 14, compared together, show that the volume and weight of steel required in the rectangular form, to do the same work, represented by Pf, is just fifty per cent. greater than is required in the circular form if the same maximum S is not to be exceeded, and this is independent of the ratio of b to h.

Both formulæ show that for a given work represented by Pf, the volume and weight of steel required will vary inversely as the square of maximum S allowed and directly as the modulus G, and that, therefore, a steel of high elastic limit and low modulus of elasticity is the most economical in weight required for helical springs.

By reference to Table I., it will be seen that the lowest elastic limit in the tempered bars is 115,000 pounds, and that the modulus of elasticity, E, is not materially affected by the tempering, and averages about 31,500,000, so that $G = \frac{2}{5}E = 12{,}600{,}000$.

In regard to fixing the factor of safety, or the maximum S allowable, we will for the purpose of illustration, assume that S in normal section is 80,000 pounds when the spring is solid as under test, which will, as already explained, be equivalent to a true maximum of 100,000 pounds in an oblique plane. We will also suppose a spring is desired which shall require 32,000 pounds to put it down $1\frac{1}{2}$ inches when it becomes solid. If now the spring be made of round bar steel, equation 13 shows

$$V = \frac{2GPf}{S^2} = \frac{2 \times 12{,}600{,}000 \times 32{,}000 \times 3}{6{,}400{,}000{,}000 \times 2} = 189 \text{ cubic inches.}$$

But as weight of steel per cubic inch is 0.28 pounds, the weight $w = 53$ pounds, if properly disposed.

In order to dispose the steel properly, some further general deductions may be made first and then applied to the foregoing example.

Suppose the spring will be composed of more than one helix, and that they are placed either concentrically or side by side in a nest. Then, if H = height when solid:

$$l = \frac{2\,\pi\,RH}{d}; \quad . \; . \; . \; . \; . \; . \; . \quad (15)$$

and $$V = \tfrac{1}{2} \cdot \frac{\pi d^2 \pi RH}{d} = \frac{\pi^2 dRH}{2}; \quad . \ . \ . \quad (16)$$

are true equations from each helix, R, d, l and V, applying to each in turn. Therefore, in each helix V varies as dR, since the other items are constant. But since S also is to be made the same for all the helices, and f is constant, formula (5) shows P varying as $\frac{d^3}{R}$, whence, dR varies as $\frac{d^3}{R}$, and, therefore, R varies as d, and l is constant for all the helices having the same amount of compression, f, in order to get the same stress, S, in the different helices when they are solid.

The relation that R varies as d is important. There is no absolute ratio necessary between them, except that R be sufficiently great for any d for practical manipulation, coiling, etc.

Let it be assumed for each helix in the spring under consideration that $R = 2d$ and that $H = 4\frac{1}{8}$ inches; also that the springs will be made of four (4) equal helices side by side. The sequel will show that these are practical figures.

The total volume was found to be = 189 cu. inches. For each helix,

$$V = \frac{189}{4} = 47.25 \text{ cubic inches} = 13 + \text{lbs.};$$

and from formula (16),

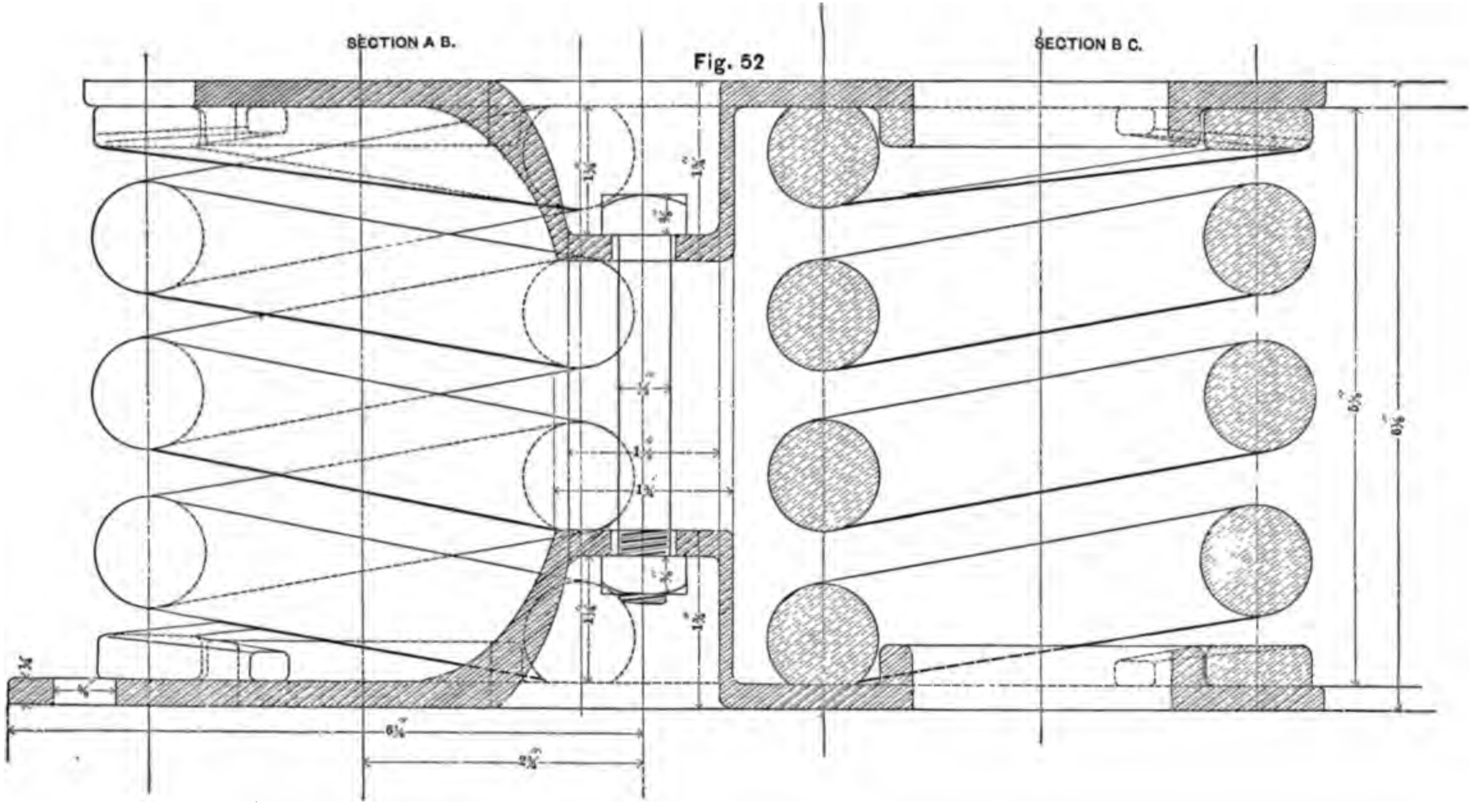
$$V = \frac{\pi^2 dRH}{2}, \text{ or, } 47.25 = \frac{\pi^2 d^2 \times 33}{8} = 40.7\, d^2.$$

Therefore, $d^2 = 1.16 +$ and $d = 1.07 + = 1\frac{1}{16}$ inches.
Hence, $R = 2\frac{1}{8}''$ and outside diameter of helix $= 5\frac{5}{16}$ inches.
The length of bar will be as per formula (15).

$$l = \frac{2\pi RH}{d} = 51\tfrac{3}{4} \text{ inches.}$$

If it is desired to make the spring of concentric helices, the same length of bar will prevail and the same ratio, R varies as d, which relations will materially aid in finding proper values for each helix from formulæ (5) and (6), and when properly related, the total weight of steel should be fifty-two (52) pounds. In order, however, to get this properly proportioned in concentric helices, it will be found that the outside bar must be of a larger diameter than

Fig. 52



is usually practised, and there are indications in Table I. that such large bars would not be as thoroughly tempered through as the smaller sizes.

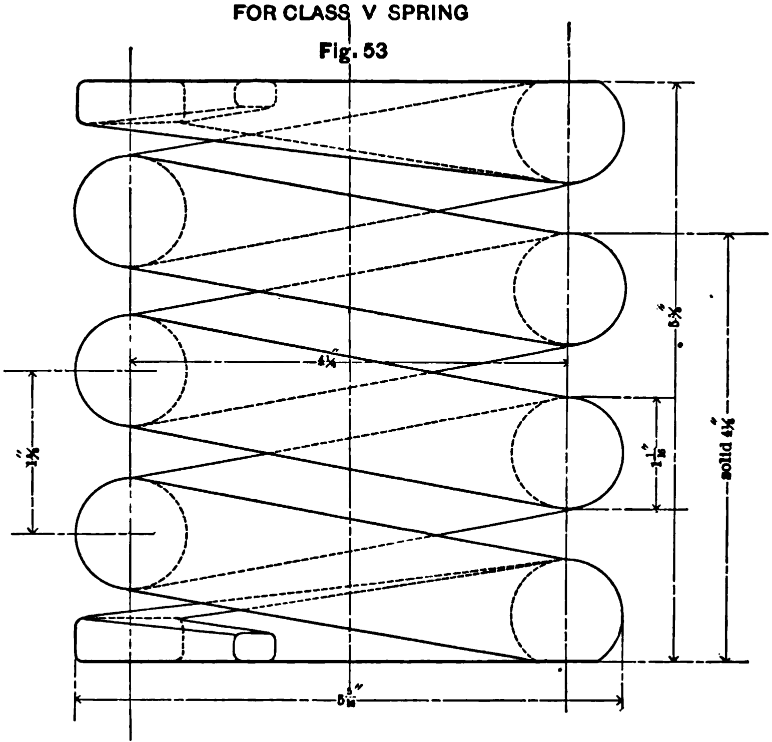
It is, therefore, sometimes better to place the helices side by side in a nest than to make the spring of concentric helices.

Upon examination in the light of the foregoing formulæ of springs which have been made of rectangular bar in large numbers for freight-car service, it has been found that they have been made with forty-two pounds to fifty pounds of steel, when one hundred and fifty per cent. of fifty-three pounds, or seventy-nine and one-half (79½) pounds should have been used. Such springs have broken in service in large numbers, as should be expected, whereas, if they had been designed with reference to the shape of bar to be used, they could have been made to show as long a life as a round

DETAIL OF HELIX

FOR CLASS V SPRING

Fig. 53

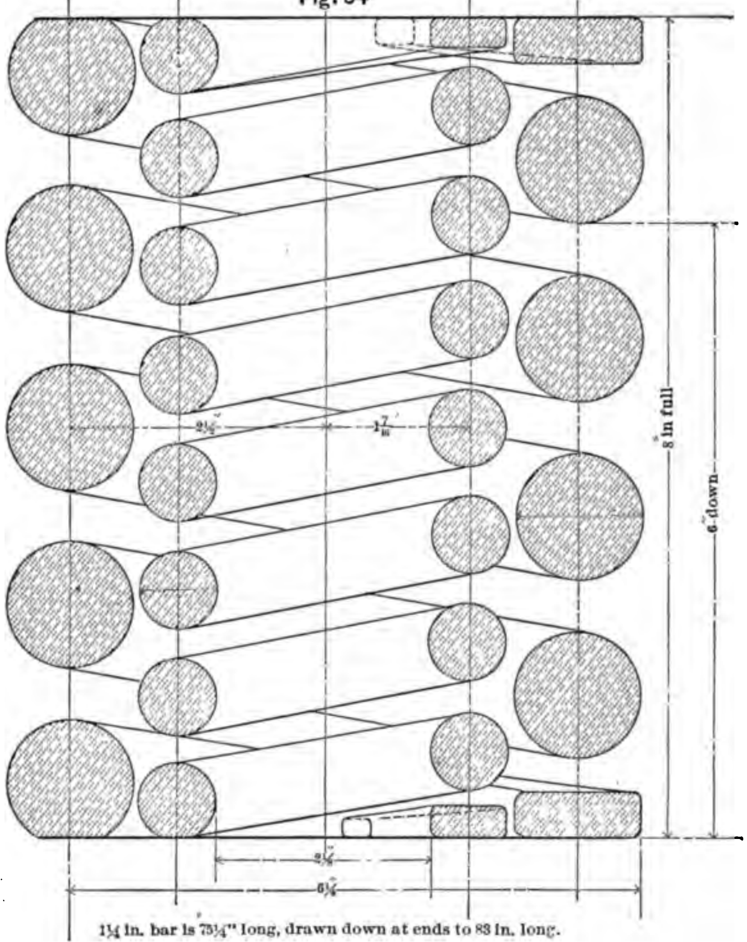


To be made of a bar 1 1/16 in diam. (near) & 51¾ in. long.
Bar tapered at ends so as to be 58¾ in. long.

bar spring with the same factor of safety, but even then, one-third of the steel used and one third of the price paid is just so much paid

CLASS X SPRING

Fig. 54



1¼ in. bar is 75¼" long, drawn down at ends to 83 in. long.
¾ in. ,, ,, 72¼" ,, , ,, ,, ,, ,, ,, 76 in. ,, .

for ignorance, and the surplus steel is frequently in the way by occupying valuable space with unnecessary material.

If it be urged that the corners of rectangular bars are always made somewhat rounding, it can be shown that this slightly modifies the evil and that it does most good when it is practised on square bars, but the conclusion cannot be evaded that the maximum good is only reached when the radius of the round on the corners of square bars is equal to that of the inscribed circle.

In the foregoing discussion the distortion of the bar during the process of coiling has been neglected. It is generally understood that a circular bar assumes a slightly elliptic section, but this is probably too slight to make it an object to roll the bars elliptical to some extent, and to coil them so that they would be circular in the helix. The slight bending action to which the bar is subjected, under load, has also been disregarded, as too small an item to modify the compression materially, and the factor of safety should be sufficient to cover any increased stress from this cause, if there be any.

An examination of Table I. indicates that the bars of smaller size would be most economical in weight of steel required, because the elastic limit of such tempered bars is higher than for the larger sizes, but this again necessitates helices of small diameter, besides a more elaborate case to contain them, and they are liable to bend under load if in compression. The same table shows that the modulus of elasticity is not materially altered by the process of oil tempering, which warrants the conclusion that a spring of given design cannot have its rate of compression per 1,000 pounds altered by this process of tempering, so that the rate calculated by these formulæ must prevail so long as the design and the temper together will not allow the stress to reach the elastic limit when the spring has its maximum load. It is possible, however, that the rate of compression may be largely interfered with and the maximum stress which the steel may be called on to sustain, may be seriously altered by improper workmanship in coiling, so as to make the angle of pitch variable when it should be constant. This is a departure from the design, and if care is not taken in this respect as in every other, the spring will not act under load as the formulæ indicate.

Further, the time of a simple oscillation is fixed for given conditions of compression, and cannot be altered for better nor for worse with the same ratio of R to d except by increase of length l and consequent height of helix; for the time of a simple oscillation of a torsion pendulum is:

$$t = \frac{\pi}{\sqrt{g}} \sqrt{\frac{RSl}{Gd}},$$

but $\frac{RSl}{Gd} = f$; therefore,

$$t = \pi \sqrt{\frac{f}{g}}.$$

As a justification of the foregoing formulæ, it may be stated that designs were made as above for two classes of springs—one as shown in Figs. 52 and 53, which calls for fifty-two pounds of steel, and one as per Fig. 54, which requires thirty-five pounds of steel.

Five complete sets of six springs each have been made from these drawings by five different manufacturers, and these springs have all been found to stand at the desired heights under load, and to compress at the rate calculated, neither could any difference be detected in the quality of the motion.

DISCUSSION.

Mr. Oberlin Smith.—I would like to ask what process was used in tempering after the hardening?

Mr. Cloud.—The springs were made by five different manufacturers, and I cannot say to what extent they were drawn after being tempered. The manufacturers all had a copy of the same drawing to work to, and it was specified nine-tenths of one per cent. carbon, and two-tenths of one per cent. manganese. The tests of the same grade of steel made beforehand at Watertown, indicate that the modulus of elasticity is not affected materially by the process of oil tempering, and I only wished to show by the cases cited that springs designed in accordance with the formulæ will stand as the formulæ indicate, provided the steel is of the same grade. Variation in carbon or manganese in the steel may produce considerable variation in the height under load, but I think there is little opportunity for variation in process of tempering to do the same thing if the tempering be done in oil, as were the samples.

Mr. Smith.—In regard to the breaking of the springs, it is possible that the maker might have got them too hard at first, and may have drawn them down afterwards. It seems to me in tempering steel that there is introduced an element of uncertainty. Improper temper is such a difficult thing to get hold of, that is, it is difficult to get specifications saying how hard a spring steel should be when we have no standard of hardness, such as we ought to have, and although the strength of the spring may not be affected if the hardness is within proper limits, yet there is the danger of getting the springs too brittle, and also the danger of unevenness of temper, getting them brittle in spots.

Professor Webb.—I would like to know whether any experiments have been made in reference to the best diameter for the steel for a given spring.

And there is still another point: whether, in compressing these springs the simple law that the pressure varies with the amount of compression, was observed to hold practically good.

Again, as the spring stretches out so that the pitch becomes greater, the spring will eventually be stretched out to a straight line, the radius will be reduced to nothing, in which case we would have no torsion whatever. As the spring is stretched out the radius decreases, of course. Has this fact been sufficiently considered?

Mr. Metcalf.—Mr. President, I received a copy of this paper a day or two in advance, with a request from Mr. Cloud to discuss it. I only regret that I did not have it earlier, in order to be able to make a more thorough discussion of the subject than I can to-day, because it is a very important one, and particularly so to the unfortunate manufacturer of springs. The number of styles, varieties, sizes, shapes and patents of the simple coil spring is simply legion. I have been engaged in the manufacture of springs for some sixteen years, and I have thought, from time to time, that we had gathered up every possible shape of a coil spring and bar that could be made, and yet there is scarcely a week, if any scrap comes into our place, that we do not find something new. It seems to be the fact that every master car builder, master mechanic, and superintendent knows just exactly what kind of a spring he wants, and just what is the best, till finally I was forced to the conclusion, years ago, to which we adhere very rigidly, that the best spring is that which a man wants. (Laughter.)

There can be no question about the propriety of the discussion of this subject to-day, and I can only say that I am very glad that the gentleman has made a report, and I think it is due to the organization with which we know he is connected, to say that it shows the great intelligence of that company that they allow and expect their engineers to come forward with their reports instead of leaving us all to go to the individuals in the different shops and be subjected to the weight of the *ipse dixit* of everybody as to what they want, without room for discussion, simply "This is so, and it must be had," etc. These gentlemen come before our society and place themselves on the same level with us all. Their reports are common property and open for discussion, and certainly the discussions and the information gained in that way must be of immense value to the company, as the reports given out are of value to other engineers.

In regard to the strains in springs it is a matter that I have considered a great deal, and, I must confess, to my entire confusion. I believe that Mr. Cloud is right in stating that the result of stress in the coil spring is mainly torsion; but when we consider the shape of the bar, it can be treated as a beam loaded at one end and fixed at the other, subject to ordinary deflection, or it can be treated as a shorter beam which would be the length of half of the circumference, subject to flexion, and to show that the treatment of a spring considered as a beam loaded at one end and fixed at the other will give some correct data, I had occasion to take, some years ago, an ordinary volute. Although that is not exactly a helical spring, yet it is spiral and subject to nearly the same strains. These springs are made usually $4 \times \frac{3}{8}$ or $6 \times \frac{1}{4}$ or $6 \times \frac{3}{8}$, flat bars, coiled up on the flat side, and it occurred to me that if that steel was acting at all, there was just about twice too much steel in it. So we made a great many springs of which we split the bars on the diagonal, leaving just enough at one end to furnish material to resist the weight, say perhaps three-fourths of an inch wide, making the spring practically half the weight of the original spring, and then we found that whether we coiled that spring with the broad end or the narrow end in, we got precisely the same resistance as we did with the whole bar. I do not know how that would work when subjected to more torsion, but certainly half the steel gave as much resistance in that shape as the whole amount.

Then again, these springs do vary in diameter. I investigated that matter again carefully yesterday, as I have a number of times before, fixing a spring in the testing machine, and I found that the friction of the plates at the ends held the ends firm. Now, if all the strain in the spring were torsion, the torsion would have to be sufficient to simply close up or compress that steel into the smaller space that the spring occupies when it is set down a distance of two inches and a half, say. But, by careful measurement I found that the Class X spring given in this paper increased in diameter a quarter of an inch in closing it down. That shows a direct transverse strain in the bar. The ends could not get away; they could not slide around, and the diameter was increased that much. Therefore I think the bars are subject to flexion,—that subject to such a load as a beam loaded at one end and fixed at the other, and also subject to torsion. Believing that, I have always felt that I could not work out a satisfactory formula, and so we have in our practice been guided by the demands of our patrons, and governed argely by past experience.

The round-bar spring unquestionably is the stiffest form of steel, taking weight for weight, coiled at the same diameter. I think Mr. Cloud is correct in that. I took yesterday three springs. One was what is called the eye-pin shape. It is a section in the shape of a pointed egg, with the larger edge rolled nearer to the mandrel, when it is rolled on its edge. A bar, an inch and a half by three quarters, 54 inches long, coiled on its edge, gives a spring 6 inches in diameter by $4\frac{3}{4}$ inches high. This spring compresses 333 pounds for $\frac{1}{16}$ of an inch. At 4,000 pounds it stood $4\frac{1}{16}$, at 6,000 pounds $3\frac{11}{16}$, at 8,000 pounds $3\frac{5}{16}$, and it closed at $3\frac{1}{8}$ inches. A bar of exactly the same size coiled flat, made a spring $4\frac{3}{4}$ inches in diameter by $8\frac{1}{2}$ inches high, turning the bar up on its edge. At 4,000 pounds it stood $7\frac{13}{16}$, at 6,000 pounds $7\frac{5}{8}$, the compression for $\frac{1}{16}$ of an inch there being 666 pounds, twice that in the other spring. Then between the weights of 6,000 pounds and 8,000 pounds it compressed $\frac{5}{8}$ of an inch, giving only 200 pounds resistance for a motion of a $\frac{1}{16}$ of an inch, showing that the spring in that form was irregular. The tendency to expand in diameter is not so well resisted in a bar coiled flat.

A round bar of the same diameter of steel, one inch in diameter, which is the same volume per inch, 54 inches long, made a spring $5\frac{1}{4}$ inches in diameter, and 6 inches high. It stood $5\frac{1}{2}$ inches high for 4,000 pounds, $5\frac{1}{8}$ high for 6,000, giving 285.7 for $\frac{1}{16}$ of an inch motion. At 8,000 pounds it stood $4\frac{3}{4}$ inches high, giving 333 pounds for $\frac{1}{16}$, the same as the edge-rolled spring. At 10,000 pounds it stood $4\frac{7}{16}$, giving 400 pounds resistance per $\frac{1}{16}$ of an inch motion and closed at $4\frac{5}{16}$. This spring you will see is irregular. It becomes more and more stiff as you compress it, and so would act more stiffly under a load.

In regard to the springs, Class V and X mentioned in the paper, we made one of those sets. The specification read in this way: "We think that a Class V spring, made according to these requirements, should stand about $5\frac{3}{8}$ inches high with a load of 16,500 pounds, and that they will be $4\frac{5}{8}$ inches high solid, and that a Class X should stand 6 inches high when down, and require 17,000 pounds to 18,000 pounds to put them down. Four coils to a nest makes a load of 4,125 pounds to each coil; divested of castings, Class V should, according to above, stand $4\frac{7}{8}$ inches high with this load."

We made the springs first exactly in accordance with the specifications given. At a load of 2,500 pounds it stood $5\frac{1}{4}$ inches high; 3,000 pounds $5\frac{1}{8}$ inches; at 4,125 pounds, the required load, 5 inches high instead of $4\frac{7}{8}$. At $4\frac{7}{8}$ high it required 5,062 pounds. At 6,000 pounds it stood $4\frac{3}{4}$ inches high; at 8,000 pounds $4\frac{1}{2}$; at 10,000 pounds $4\frac{1}{8}$ practically closed. In the specifications they gave a spring that was 1,125 pounds too strong—937 pounds too strong at the required height. I will say to Mr. Cloud, I do not know what our friends did, but we simply cut that spring down until it met the specification. It had to be reduced; it was too stiff. We may have had our steel too high in temper. The letter enclosing the specifications said that this spring would weigh 52 pounds. It did weigh actually $52\frac{3}{4}$ pounds, which is practically correct. It was stated that the X Spring would weigh 32 pounds. It actually weighs $37\frac{1}{2}$ pounds.

Now, in regard to the elastic limit, I do not think that in any spring, in practice, the elastic limit can be very well exceeded if the spring be properly made and tested before it leaves the works. Our plan of making springs is to coil them—and it is, of course, the same with all parties—to coil them a good deal higher than they are required to be, harden, and temper them in some cases and not temper them in others. That is all governed by the quality of steel—the carbon of the steel. There are three or four different ways of hardening springs. After that is done they are put in a testing machine, which is simply an inverted hammer. It is a 22 inch steam cylinder with 100 pounds pressure, with a cross-bar fixed at the top. A spring is put on the cross-head of the piston rod, and simply snapped up solid, as hard as we can strike it with this 100 pounds pressure on a 22 inch cylinder, and then is thoroughly well beaten until the set is taken out. After that is done, you may keep the spring banging there all day long, and you can't set it any more, and if that spring is put into service, and it is overloaded, it will simply go down solid and be a solid block. I do not see how it is possible to exceed the elastic limit in a spring of that kind.

I think the reason for the breakage of the springs, that Mr. Cloud speaks of, was largely their rectangular shape—due to the fact that there were sharp corners left on the steel, which made it almost impossible to heat the steel uniformly. In heating a spring of that shape the corners will get hotter than the rest of the bar. An internal strain will be set up in the bar, which cannot be taken out afterwards, and when the spring is put in service these little sharp corners will scratch and spread the spring, and your spring will break. A rectangular bar with sharp corners is a bad thing, and should not be used, not only for springs, but for any other purpose for which steel is used. It should be avoided.

But there are other considerations besides the question of load and elastic limit and the quantity of steel required. We have, for instance, to look to the duty that the spring must do. Now, if you take a spring of this Class V, just right for a 50,000 pounds freight car, and put it under a car on one of our Eastern roads, where the road-bed is solid and firm, ballasted with stone, the rails all true and even, where there is no jarring or pounding, or anything of that kind, that spring ought to carry that load indefinitely up and down the track. But if you take the same car and load it with perhaps 60,000 or 70,000 pounds, as is frequently done, and send it off all over the country, out on the inferior and the branch roads, and it is bumped and thumped around under a strain a great deal heavier than 50,000 pounds, very often springs will break under that treatment that would last their full life out with proper treatment. They will break from the actual crushing of the bar under the continued heavy pounding.

Then, again, there is another case I will cite, just to illustrate. We get an order from a man in the logging regions for a spring to put under a car which shall be able to carry a certain load, say 10 tons. But they notify us that they are going to roll big logs off from a heap, and they will drop them down 5, 6, or 10 feet, and they want a spring to resist that. Now, there is no trouble in making a round bar spring, a very light and cheap one, to carry the load, but all that we have sent out to do that work have failed and broken, and the parties have complained of them. We have then been compelled to resort to this edge rolled bar steel in order to make a safe spring, with a soft motion, that will yield, having an ultimate capacity sufficient to carry the load required, so that when these logs come down they shall not break the spring. Due regard must be had to the work that the spring is going to do, if you want it to live out its life.

Another thing. All railroad men say to us, so far as I know, "We don't want a soft spring. We don't want a spring that will set. We want you to leave all the temper you can in the spring, so that when it fails it shall break, for the reason that when it sets under work it simply becomes a solid block of steel, and it knocks the car to pieces, and knocks the track to pieces, and breaks everything up." Due regard must be had to that—to softness of motion and high temper, so that the steel cannot set. This is very important indeed. We all make steel springs, and make them higher than will give the greatest resistance to impact for that reason—that when a spring does fail it shall break. I think that demand is uniform among all the railroads.

I have a little memorandum here that I should have brought in before. I took an inch bar this morning, 42 inches long, and sent it down in order to get a spring of the same height as this edge rolled that I have spoken of. It made a spring 5¼ inches in diameter and 4¼ inches high. At 4,000 pounds it stood 3¾ inches high, at 6,000 pounds 3½ inches high, giving a difference of 500 pounds for a $\frac{1}{16}$ of an inch motion, closing at 8,000 pounds 3⅛ inches high, giving 333 pounds, showing uneven motion also.

I am very glad that Mr. Cloud has come to dissipate the clouds of ignorance in regard to springs. He did not say, and I know he did not mean, the ignorance of the spring-makers (laughter), because we, to a large extent, have to make simply what we are ordered to make.

I will give the history of the Class U spring, for the information of the members. The first demand that was made for that spring was about six years ago. I received a demand for a spring that should be the lightest possible spring to carry the load, a 40,000 pound freight car—a U spring—the idea being that the elliptic spring was too heavy and too expensive, that coil springs were better to do the work, and they desired to have the lightest possible spring that could be obtained, and also one of soft motion. We made a nest spring, consisting of three coils set on the angles of an equilateral triangle, containing in each nest 32 pounds of steel, and they did the work beautifully. But after the spring had been introduced and tested pretty well, a great many thousands were ordered from all over the country, and the spring was too light, and the tempering, probably, was not always evenly done, and the breakages were simply frightful. Then a demand came from the users that they wanted the utmost possible quantity of steel put into a space that should not be greater than 10 or 11 inches in diameter and 6 inches high, and they wanted all the steel they could get there, for greater safety. The result of that was springs that weighed from 52 to 79 pounds, and a great many thousands of them were made and a great many thousands of them broke in service. Then they appealed to their engineers, and Mr. Cloud comes out and gives us the result of his investigations, and he is getting very much nearer right than before, and eliminating the ignorance that we are all laboring under. But I really think the chief difficulty we labor under, and it is a difficulty with all of the railroads, and more particularly almost with all engineers, is one that the engineer cannot control—he can only work out and make his designs and his specifications for the best thing that can be had, and then he is overweighed by the *lowest bidder*.

Mr. Oberlin Smith.—It seems to me that one of the most suggestive remarks made by Mr. Metcalf is that all the new-fangled springs are found in scrap piles.

I have a suggestion here to make to spring makers—I do not know, however, whether it is worth trying—and that is, that the proper form for a spring for bars of round section would be tubular, as a round bar subject to torsion does nearly all its work, of course, on the outside, the interior of the bar being near the neutral line and subjected to no torsion whatever. I do not know whether it has been tried. I have tried it with little pieces of iron pipe occasionally. I have not gone into the mathematics of the matter at all, but it would seem common sense that the best result would be secured with the least weight of steel if the spring was made tubular. I do not know how successful the tube makers are with steel tubes. I do not know what they have done in this country, nor do I know whether anything has been done in the way of welding steel tubes, but if steel tubing is obtainable here, I think it would be a good idea to try some springs made from it. Certainly it is a matter of importance to save dead weight in cars. I think the most work could be gotten in that way from the least material.

Mr. Metcalf.—Mr. Chairman, those springs, if they could be made, would be simply crushed. These cars, when loaded at their maximum, take a very heavy load, and the springs under temper would certainly break.

Mr. Chas. E. Emery.—The circular section is not only of value in providing the least weight of metal for a spring of a given resistance and elasticity, but may also be employed to prevent parts of machines subject to indirect strains from springing. I designed a pair of offset reversing levers for twin screw engines some ten or twelve years ago. Each lever had a straight vertical portion made thin and tapered to a handle as usual, but was offset horizontally, about eighteen inches, one foot out from the reversing shaft. The whole of the horizontal portion, as well as the portion one foot long joining the hub, which was keyed to the shaft, was of round iron, three inches in diameter. The offsets showed prominently above the engine-room floor and occasioned considerable remark, but they are the only offset levers I ever saw that did not spring with links in motion, showing that the circular section is the best form to resist the combined torsional and transverse strains due to an offset.

CXLIII.

A DRAWING OFFICE SYSTEM.

BY HENRY R. TOWNE, STAMFORD, CONN.

In the management of nearly every large works the need is experienced of some systematic provision for the indexing of drawings and patterns. The system herein described has been developed in the works under the writer's management, and has been proved by experience to be simple, effective, and satisfactory. It is believed that a brief description of it may be of service to others in effecting similar arrangements.

A drawing office, like any other department in a manufacturing establishment, should have a recognized head, designated usually as chief draughtsman or as foreman of drawing office. In some cases, owing to the carrying on of distinct lines of work in the same office, more than one such head may be desirable, each, of course, having a clearly defined field of work. Whoever is selected for such position should of course have a well-defined responsibility and discretion, subject to such general direction or supervision from the principals as in each case may be desired, and should have the entire control and direction of his subordinates. In recognition of this principle the system herein described provides for the signature of each drawing by the initials of the responsible head of the department, followed, where the drawings are made by others, by the initials of the draughtsman. In this way the responsibility for all work covered by a drawing is at once indicated on its face.

Uniformity in size of drawings is important as a matter of economy in drawers, boards, etc., and in most cases standard drawings can be restricted to a number of sizes not exceeding two or three. Uniformity in style of title or legend of drawings is desirable as a matter largely affecting the general appearance of the sheets, and as better in every way than leaving the inscription to chance determination in each case. The title should also be always placed in the same position on every sheet, and is best located near the bottom, for the reason that it is then most accessible when the sheets are filed in drawers, as is usual. The style of lettering adopted should be one capable of quick production by hand, and as neat and clear as possible. Fancy type of all kinds are objectionable. The style preferred by the writer is a Gothic type as illustrated by the sample title herewith, all of which, excepting the headline, can be easily done by any fair penman in freehand work, and very rapidly. The illustration is a *fac-simile* reproduction, by photo process, of the title on an actual drawing, the work on which is entirely free-hand and was done in less than one hour. The headline is best done with a brush or stub, and although requiring more time is preferable to a lighter type as giving character and distinctness to the whole inscription. A decided economy can be effected in any large drawing office by having all of the lettering on drawings done by some one person rather than by each of the draughtsmen. A competent person for this work can usually be obtained at much less expense than a draughtsman, while the increased practice he obtains will also soon enable him to do the work in shorter time. In this way also uniformity is obtained in the lettering of all drawings prepared in the office.

A clerk or assistant is a useful addition to the corps of any drawing office employing many draughtsmen, and in some cases two or three such assistants can be profitably employed. Their duties should comprise the lettering of drawings, as above explained, the preparation of blue prints (where the blue process is employed), the book-keeping incident to the indexing and recording of drawings, the preparation of shop orders, and the assorting and filing of drawings and tracings. Experience shows that such work is, as a rule, not only done more cheaply but also better by such assistants than by the individual draughtsmen.

The use of sketch-sheets for orders to the shops is recommended for all minor and temporary matters which can easily be presented in this way, and for much of the details of general drawings, such as the simpler forgings, castings, etc. For this purpose a sheet measuring about 9 × 12 inches is preferred, and should be cross-ruled into squares of one-quarter inch each, thus facilitating the practice of sketching accurately to scale, and dispensing with all need of a ruler. The sketch-sheet should be of quite stiff paper or card-board, and should also have a neat printed heading at its top, with blanks for the insertion of the order number, date, name of draughtsman, etc. All sketches on these sheets should be made in copying ink, and should be numbered consecutively. As soon as finished they should be press-copied in an ordinary letter-book, thus enabling a fac-simile of every sheet to be retained in the drawing office, which, by means of the reference system described below, is conveniently accessible at any time. Persons unfamiliar with this mode of preparing, copying, and indexing sketches will be surprised on trial at the large amount of work which can be conveniently treated in this way, and at the marked economy it accomplishes as compared with the ordinary system of drawings.

Templates or full-size drawings, whenever required for any purpose, should be always prepared in the drawing office rather than by the foremen or workmen in the shops. The chances of error are thereby lessened, and the work can always be done better and more economically than by persons not habitually engaged in it. In like manner all lists of bolts, timber, or materials of any kind should be prepared in and issued from the drawing office, a record being kept by means of the letter-press process, in the same manner as in the case of sketch-sheets.

Pencil sketch books, or "blotters," should be provided for every draughtsman, with instructions that *all* of his preliminary sketches, calculations, and notes of all kinds shall be originally made in these books and not on loose sheets of paper. For this purpose a book is recommended measuring about 7 by 8½ inches and containing about 125 sheets of soft white paper, suitable for use with pencils, but sized so that ink may also be used. The pages should be entirely plain, without ruling or printing. Such books can be had in quantity at a cost of about forty cents each, which is but little more than the cost of paper. A short trial will convince the most skeptical of the great advantage resulting from this plan. By means of it every sketch or calculation is preserved and is easily accessible for future reference. Every engineer knows well how frequently he is required to make over estimates, calculations and detail dimensions where it is his habit to do such work on random sheets which are not preserved, and how constantly his time is spent in doing over work which he has often done many times before, a record ot which, if easily accessible, would save much valuable time. Where these sketch books are used it should be an imperative rule that all sketching and figuring should be done in them and not on loose sheets. To insure this, no effort should be made at neatness or nicety, and under no circumstances should original work be done on loose sheets and transcribed into these books. Their purpose is to serve as original records, and to preserve in their entirety all of the work and notes of the engineer or draughtsman for reference not only in the case of reproduction or alteration of designs, but also for the verification of work and tracing up of errors.

The blue process of printing from tracings is now employed in almost all large establishments, and where it is in use economy in drawings is promoted by the following system: Each original drawing is completed in pencil on the usual drawing paper; a tracing in ink, on linen, is then made from the pencil drawing, and this tracing thus becomes the finished original; blue prints from this tracing supply the requisite duplicates for the shops or for mailing to customers. If at any time an alteration is required, the original pencil drawing is utilized for this purpose, and a new tracing made, the latter receiving a *distinctive* number (thus distinguishing it from the first tracing), the numbers and dates of each successive tracing being noted on the original pencil drawing. The original pencil drawings should be kept at the bottoms of their respective drawers, and the linen tracings above them, in the same drawers. The pencil sheets are not again referred to unless needed for purposes of alteration. The linen tracings thus become in fact the original drawings, and should never, under any circumstance, be removed from the drawing office. Where the expense is not objected to, the original sheets may be inked in, and are, of course, somewhat handsomer and more satisfactory than tracings, but in most cases the increased cost is not justified.

The indexing of drawings, so that they may at any time be quickly found and referred to, is a matter of much importance. A method for accomplishing this is clearly explained in the following "instructions." It requires a few books of record and two brief entries for each drawing or sketch. Experience has proved the system to work well in practice, and to accomplish all that it aims to do. The marking of patterns, so that they may be easily recognized and quickly traced, is almost as important as a system for indexing drawings. The one herein explained has the merit of simplicity and the identification of every pattern with the drawing or sketch from which it originates. This latter feature should be made the basis of any system of marking patterns, and cannot well be done in a manner more simple or more easily understood than that herein explained.

The adaptation of any system of drawings and patterns must of course be made with reference to the special business in which they are employed and to the special class of products they relate to. This paper is merely intended to indicate a few of the salient

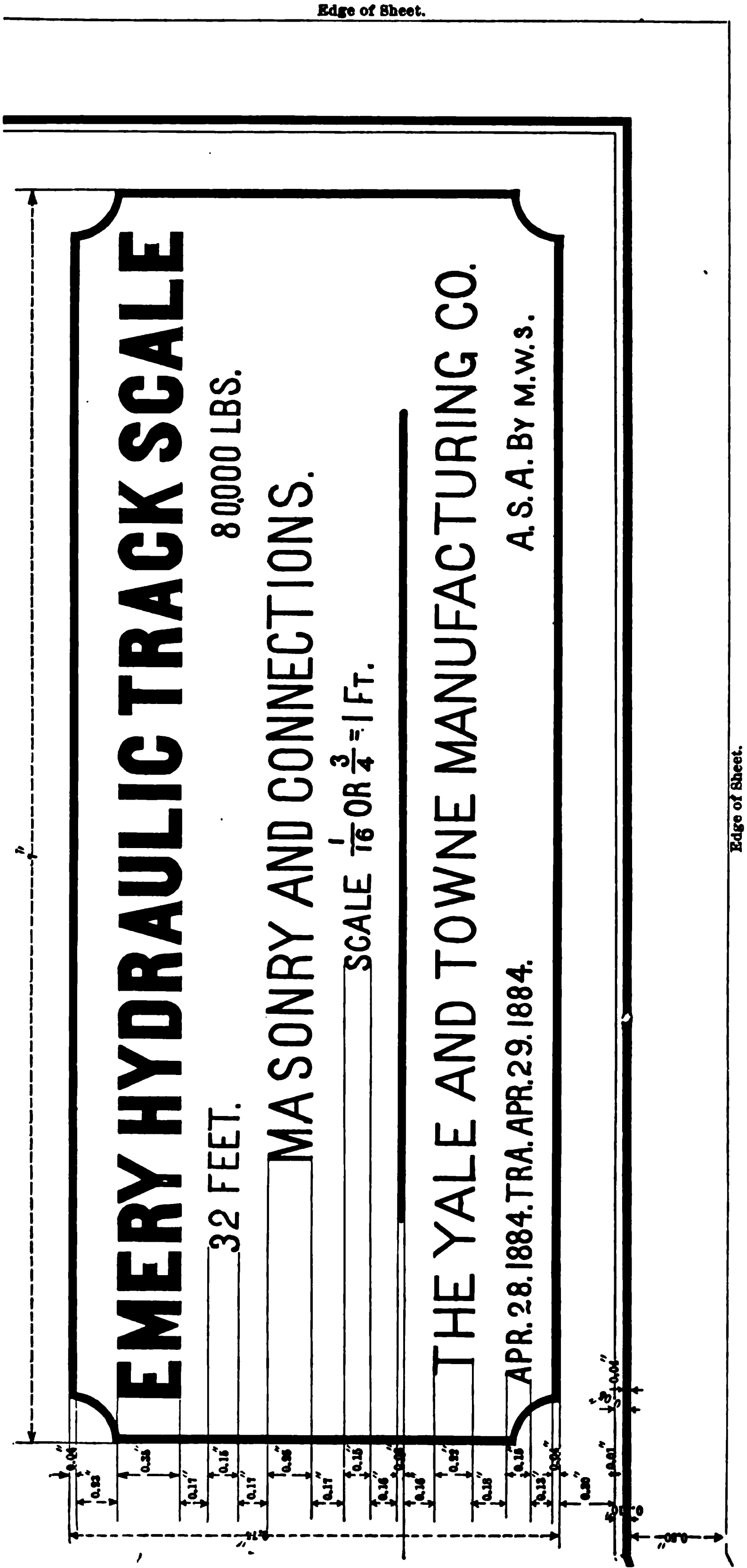

Edge of Sheet.
Edge of Sheet.
EMERY HYDRAULIC TRACK SCALE
32 FEET.
80,000 LBS.
MASONRY AND CONNECTIONS.
SCALE 1/16 OR 3/4" = 1 FT.
THE YALE AND TOWNE MANUFACTURING CO.
APR. 28. 1884. TRA. APR. 29. 1884.
A. S. A. BY M. W. S.
0.04"
0.32"
0.35"
0.17"
0.15"
0.17"
0.35"
0.17"
0.15"
0.16"
0.16"
0.32"
0.18"
0.15"
0.12"
0.04"
0.20"
0.01"
0.04"

Fig. 61.

points which experience has shown to be of value in such work, and to aid in the improvement of drawing-office methods by illustrating a system which has been carefully considered and developed in connection with a large business, and which experience has shown to be well adapted to its purpose.

Appended will be found *in extenso* the "instructions relating to drawings and patterns," and also the design of "titles for drawings" which have been adopted in the works under the writer's management, and which are above referred to.

THE YALE & TOWNE MFG. CO.

INSTRUCTIONS AS TO DRAWINGS AND PATTERNS, 1883.

Size of Drawings.—Drawings for Scales, Gauges, and Testing Machines to measure 23x31 inches, when trimmed.

Drawings of Post Office Work to measure either 27x35 inches, or 17½x27 inches when trimmed.

All other drawings to measure 27x35 inches, when trimmed.

Border.—Every finished sheet, whether drawing or tracing, to have a rule or border around it, with square corners. This rule to consist of a double line, measuring 10·100 in. over all, composed of an outer line .04 in. broad, a space of .05 in. and an inner line of .01 in. A clear space of .50 in. to be left between the border and edge of the sheet.

In the case of *small* P. O. drawings all of the above dimensions to be reduced one-fourth.

Titles.—Every sheet to have a title-panel at its right-hand lower corner.

This panel to measure 7x2¾ inches outside, and to be enclosed by a single line .04 in. wide, with corners cut with a .25 in. radius.

A space of .30 in. to be left between the panel and the border enclosing sheet.

The lettering within the title-panel to conform to the standard model, and to be of uniform style in all cases.

Scales.—All regular drawings to be made to one or more of the following scales, viz :

NATURAL.		BY INCHES.	
1/1 Size	Full Size.	2/3 Size	8 inches=1 foot.
1/2 “	6 inches=1 foot.	1/3 “	4 “ 1 “
1/4 “	3 “ 1 “	1/6 “	2 “ 1 “
1/8 “	1½ “ 1 “	1/12 “	1 “ 1 “
1/16 “	¾ “ 1 “	1/24 “	½ “ 1 ‘
1/32 “	⅜ “ 1 “	1/48 “	¼ “ 1 “
1/64 “	3/16 “ 1 “	1/96 “	⅛ “ 1 “

The scale of each drawing shall be noted thereon both *by inches* and *by proportion ;* thus “ Scale 3 ins.=1 ft. or ¼.”

Every dimension necessary to the execution of the work is to be clearly stated *by figures* on the drawing so that no measurements need be taken in the shop by scale. All measurements to be given with reference to the *base*, or starting point, from which the work should be laid out.

Numbers of Drawings.—Every sheet to be numbered consecutively, as soon as started, with .25 in. plain figures, in black, at the lower left-hand corner just within the angle of the rules or border.

Numbers of Patterns.—On each original drawing containing any new patterns the several pieces which require patterns shall be lettered distinctly A, B, C, etc. The patterns made from such drawing will be marked with the number of the drawing and the indicating letter above referred to. For example, the pattern lettered A on drawing No. 1027 will be marked and known as 1027–A.

When existing patterns are utilized in a new design or machine their original number is to be noted on the drawing in which they are shown in their new employment. Thus, if in drawing No. 1028 use is made of the pattern above referred to it will be marked on the latter drawing “1027–A,” while other new patterns, shown for the first time in drawing No. 1028, will be simply marked A, B, C, etc., on the drawing, and will themselves be marked and known as 1028–A, 1028–B, etc.

Number of Piece.—When it is desired to give a distinctive No.

to each part or piece of a machine, such numbers shall commence with 1 and continue consecutively.

These numbers shall be noted in plain black figures on the drawing in connection with the pattern No., as explained below.

Numbers and Marks.—In connection with each important piece on each drawing there shall be noted the following particulars:

(1) The consecutive *No. of the piece*, where such numbers are given.

(2) The *pattern letter or No.* If the piece requires no pattern, the fact to be indicated by placing a 0 in the position which would otherwise be occupied by the pattern No.

(3) The number required of the piece or part to complete one machine.

These three numbers or marks are to be written on the drawing consecutively, one following the other, with a hyphen between. Thus, if the part is No. 5, the pattern D and the No. required 3, the marks would be "5 D-3." If the pattern were old, the marks would be "5-1027A-3." If no pattern were required (as in the case of a forging), the marks would be "5-0-3."

Wherever the drawing admits of it, these marks are to be placed directly upon the representation of the part they apply to. Where this is not possible, or would lead to confusion, the marks shall be placed at one side, with an arrow or pointer connecting them with the piece to which they refer. All of these marks to be in black ink and considerably heavier than the figures used for dimensions.

Materials.—The material of which each part shown by the drawing is to be constructed shall be indicated thereon by means of the following symbols, viz:

CI.—Cast Iron ;
GI.—Gun Iron ;
Bz.—Bronze ;
TS.—Tool Steel ;
SC.—Steel Casting ;
CRS.—Cold Rolled Steel ;
WP.—White Pine ;
Ok.—Oak ;
Ch.—Cherry ;
Wh.—Whitewood ;
WI.—Wrought Iron ;
Bs.—Brass ;
MS.—Machinery Steel ;
BS.—Bessemer Steel ;
SMS.—Siemens-Martin Steel ;
SP.—Spruce Wood ;
YP.—Yellow Pine ;
As.—Ash ;
BW.—Black Walnut ;
My.—Mahogany.

Finish.—The kind of finish required on each of the different parts shall be indicated on the drawing by means of the following symbols, viz:

D. = Dressed or Tooled, and indicates that the surface to which it applies is to be dressed off in whatever manner its nature or shape best admits of.

F. = Fine Finish, and indicates that, after planing or turning, the surface is to be further finished by file or emery.

P. = Polished, and indicates polishing by means of emery wheels or a similar process.

S. = Scraped, and indicates scraping down by hand.

G. F. = Grinding Finish, and indicates that the only finish to be allowed is that necessary for grinding.

G. = Gauge Dimension, and is to be *prefixed* to the measurement to which it applies. Thus, G 2.75 in. indicates that in finishing the work this dimension is to be obtained by using the 2¾ in. standard gauge.

Bolt Lists.—On every drawing showing either bolts or parts which require bolts for their attachment, there shall be a "Bolt List," written in clear figures and contained in a table or panel placed, if possible, near lower left-hand corner of sheet.

The columns on the "Bolt List" shall give the following information, viz:

Piece Number;
Number to Set;
Metal;
Body, Finish of;
Head, "
Nut, "
Diameter;
Length;
Length of Thread;
No. of Threads;
Kind of Head;
Kind of Nut;
Remarks and Sketches.

The length of bolt shall be the length from under side of head to point, or from point to point in case of a stud bolt. Where standard thread is to be used it may be indicated by an "S" in thread column. If special, give number of threads per inch.

The several kinds of bolts shall be known as follows:

Through Bolts, with head on one end and nut on the other.

Stud Bolts, one end screwed into the work and the other fitted with nut.

Tap Bolts, with head on one end and screw on the other.

Through bolts are always to be used if possible, Stud bolts are to be preferred next, and Tap bolts last.

The kinds of finish of bolts and the symbols therefor are as follows:

B. = "Black"—indicating that the bolt is to be left as forged.

T.=" TURNED"—indicating that the bolt is to be turned, and its head and nut finished bright.

F.=" FITTED"—indicating that the bolt is to be turned and accurately fitted to its hole, so as to be "body-bound" to serve as a dowel.

The style of heads and nuts to be indicated as follows:

HEX. for hexagon.

SQ. for square.

Ro. for round.

Unless otherwise specified, all bolts and screws are to be made in accordance with the Company's tables of standard sizes.

Index of Drawings.—A book, having this title, will be kept in the drawing office by the clerk. Each of its pages will contain five spaces, numbered consecutively.

When starting a new drawing the draughtsman will apply to the clerk for a number, and will be allotted the first unappropriated No. in the Index Book, the name of the draughtsman being written opposite it in pencil to show that it is taken.

Whenever an original drawing is finished, or, if in pencil only, when the original tracing is finished, the draughtsman will hand the same to the clerk, who will immediately enter in the Index the title of the drawing, its date and date of tracing, and the No. of the drawer in which it is to be placed, which will be given him by the draughtsman. The drawer No. will be entered in pencil, so that it may be easily altered if the drawing is changed from one drawer to another.

Drawing Registers.—A series of Drawing Registers, or record books, will be provided, one for each class of drawings. The pages of these books will be ruled to contain the title of the drawing, the name of the parts or pieces it represents, the No. of its drawer (which will be entered in pencil), and the drawing No.

At convenient intervals the clerk will post these books from the "Index of Drawings," at the same time noting in the latter the book and folio in which each drawing is recorded.

Each Register or record book will be paged, and will have an alphabetical index. Accounts will be opened with each important machine for which drawings are started, and sufficient space reserved to contain all the drawings likely to be made. Miscellaneous drawings will be classed under certain general heads and entered in like manner. Sketch sheets (except where reproductions of parts shown on drawings) to be noted in the Registers in the same manner as drawings, by giving the No. of copy-book and page.

These records will thus give a complete list of the drawings and sketches pertaining to any machine, so that, if the latter is to be reproduced, a complete list of the drawings is available, and the No. and location of each drawing ascertained.

Separate Registers will be provided for each of the following classes of drawings, viz.: Cranes, Hoists, Scales, Pressure Gauges, Testing Machines, Post Offices, Tools, and Buildings.

Drawers.—Each Drawer will have a No. and a Title-card (indicating its contents) on its front, and a " Contents-card " inside. The latter shall measure 8x12 inches, and shall either be pasted on the card-board sheet covering the drawings, or upon a thin board slightly larger than the card and attached to the drawer by a cord 24 inches long.

The " Contents-card " will be ruled in three vertical columns, the first stating the No. of the Drawing; the second, the Machine it relates to; and the third, the Parts it shows. Whenever a drawing is first placed in its drawer its No., Title, etc., are to be immediately entered upon the "Contents-card" of such drawer in ink. If at any time the drawing is transferred to another drawer this entry will be canceled by ruling out.

INSTRUCTIONS FOR MARKING PATTERNS.

Patterns.—To be made and finished as heretofore, all core-prints of wood patterns being painted black, and the abutting faces of all detachable pieces being painted red, so that when the pattern is complete no red surfaces are seen. Sinking heads and gates to be colored green.

Numbers.—Each pattern to have plainly painted or stamped upon it its pattern No. or mark, consisting of the No. of the drawing from which it originates, with a letter added thereto, as indicated on the drawing. Each pattern, until altered, will be always designated in the above manner.

Alterations.—Whenever a pattern is altered, under instructions from the drawing office, the drawing or sketch thereof shall indicate the old and new pattern marks.

Where the alteration is intended to be permanent, and where the pattern is retained solely for use in the machine for which it was originally designed, the original mark on the pattern shall be canceled by drawing through it a straight horizontal line, and the new No. be marked on the pattern immediately under or above the old.

Where the alteration is to adapt the pattern to some new machine, without interfering with its subsequent use for its original purpose, the drawing indicating the alterations shall give, *first*, the original pattern marks, and, *second*, the marks of the altered pattern. The latter will consist of the original mark, underscored by a heavy line, and beneath this line the No. of the drawing or sketch showing the alteration, followed where necessary by a designating letter, thus $\frac{1027\ \text{A}}{1128\ \text{H.}}$ The mark on the altered pattern thus indicates both the source of the alteration and of the original. Where the alterations consist in the addition of pieces to the original pattern the alteration marks will be placed only upon such additional pieces. Where, however, the pattern itself is altered the alteration mark must be placed permanently on it and remain until the pattern is again restored to its original condition, whereupon the alteration marks will be obliterated, leaving the original No. or mark on the pattern.

DISCUSSION.

Mr. Emery.—Mr. Chairman, I hear the remark all around me that that is a very good paper. I think we should all say so. I do not think, however, it is a matter admitting of much discussion. I am very much obliged to Mr. Towne for his labors in this direction. It will help us all out once in a while.

Mr. Walker.—In reference to the paper that has just been read, Mr. Towne speaks of taking a drawing in the usual way, with a pencil, taking off a tracing from that, and then taking the blue prints.

Many of our members may not know of a new method of making drawings on bank-note paper, which, since blue prints have come into use, I think deserves notice. To make first a pencil drawing on paper, then to make a tracing from that, and then to make blue prints, for the shop, makes three distinct operations. But Mr. Wellman, of the Otis Steel Works, in Cleveland, has commenced this new plan, taking a thin paper very similar to bank-note paper, made very nice and even. It is stretched on a board with a regular background of white drawing paper below, and it can be worked on very nicely. From this drawing when it is finished the blue prints are taken and it is filed away. The originals occupy very little space in thickness. You can pile about ten times as many sheets in a drawer as you can of ordinary drawings. I think this is worth noticing, and any of our members who will write to Mr. Wellman, of the Otis Steel Works, will get all the information they wish.

Prof. Webb.—I have in my university work found the necessity of keeping myself posted and finding out what practical men are doing, and to me the paper just read is of very great value. I hope the subject will be continued, that we shall have other papers, and that either some conclusion will be arrived at as to the best system, or, at least, that we shall find out from the discussion the different systems that are used.

I feel confident also that you are equally interested in knowing what we are doing in university classes, because you take our students and put them in your drawing-rooms, and it seemed to me that you might be glad to see specimens of our work, and be interested by a description of some of the methods we employ in teaching drawing, and I had hoped to present a brief paper and secure a discussion upon them.

I have had charge of part of the drawing at "Cornell" for only a year, but I have commenced some work of my own, which is entirely different from copying from books or blue prints. It is an original work, and as there was a drawing nearly finished, which I thought would indicate what we are doing, I asked one of the students to loan it, and I tried to take some blue prints of it. They are nearly a failure, but they will show you what the drawing is. I have brought with me some of the simple tools which we use. I do not present the drawing as a specimen of finished work, for it is not finished, nor have the imperfections been taken out, and it is the first piece done by the student, who is a lower classman, admitted to the class by special permission. My object is to explain the methods of teaching—not only the methods of producing the proper effects of light and shade, but more particularly the methods of training men to think, because in a class in drawing, as well as in any other subject, students may be trained to think accurately and correctly, and if this be not done, it is the neglect of a great opportunity.

With these remarks, I will close, and will hope to continue the subject at a future meeting.

CLXIV.

A POSITIVE SPEED INDICATOR.

BY OBERLIN SMITH, BRIDGETON, N. J.

Before describing the instrument referred to in the title, a brief glance will be taken at the various Speed Indicators in common use, noting that the word "speed" is used in the ordinary sense, meaning speed of rotation, the unit of which is *revolutions per minute*. These instruments are used for ascertaining the rotative speed of shafts of all kinds, and consists of two general classes, namely: *recording*, and merely *indicating*. To the former class belong such instruments as record, by means of a diagram, all the variations of speed which have taken place within a fixed period of time. The speed is, however, ascertained approximately only, by means of a centrifugal mechanism, working upon the same principle as an engine governor.

In the second class (indicators merely) we find two general groups; the first of these point out the speed at any given instant by means of an index finger upon a dial, or a mercury column against a scale. They depend for their action upon centrifugal force, and, of course, give but an approximate reading. Those of this class which are portable, and capable of being carried in the hand or applied quickly to any shaft, are arranged with weights to fly outward from the axis by means of centrifugal force, and are probably the most convenient thing to use for ordinary work, within certain speed limits. Such instruments as depend upon the centrifugal action of a cup of mercury are not portable, as far as known to the writer, but have to be driven from the shaft which is being tested by means of a belt or other connection. These would not appear to be capable of accurately indicating very slow motions, say as low as ten per minute, or very rapid speeds, say up near 10,000.

In the second group are found several varieties of the ordinary pocket "Speed Indicator," some of which register a hundred only, while others can be read up as high as one thousand. They are none of them, however, automatic, and consist really of a device to "slow down" the number of revolutions in question, so that the counting can be done by the operator with a watch in his hand. Of course, they are not accurate, as the "personal formula" of the operator has to be taken into consideration, as well as his normal supply of carelessness. There is also an error (varying with the velocity) arising from the inertia and momentum of the spindle of the instrument, which do not allow it to start and stop instantly —that is, as they are usually made, with obtuse driving edges pushed against a smooth conical "center" in the shaft. The chief merits of these little machines are their cheapness and portability. When combined with a watch, they answer a very good purpose for ordinary use about line-shafting, but are not definite enough for such work as testing our modern high speed engines—such, for instance, as a "straight-line" engine recently purchased by the writer, which will vary only about one revolution in two hundred, with a oad variation from nothing to 40 horse-power.

There is one "speed indicator" in use that is positive in its ac-ion (being composed partly of a clock), but it has little in common ith the instruments in question, as it merely shows whether the aft with which it is permanently connected varies from a given andard speed to which the gearing of the indicator is adapted.

To accomplish such work as was before mentioned, and other eed-testing that needs to be *definite*, the writer has contrived a achine to count automatically the actual number of revolutions, ade in one minute, of any shaft to which it may be applied,—hether it be ten or anywhere upward to 10,000. This variable apacity makes it alike applicable to overshot-water-wheels and to otton-spindles; with sundry dynamos, high-speed-engines, shaft-ng, etc., between. It consists essentially of a clock movement so onnected with a dial-indicating-mechanism that it will throw the latter into gear with a spindle which is driven from the shaft to be tested exactly at the *beginning* of a given minute and out of gear exactly at its *end*. After the expiration of the minute there is some "waste" time allowed (preferably about one half a minute) before the clock can commence another counting cycle. At any time during this half minute the instrument can be removed from contact with the shaft, set back to zero, and applied again, there or elsewhere. This "overtime" gives opportunity for a leisurely application and removal, and allows the spindle to attain the full speed of the shaft before it is called upon to do any indicating work.

Upon the accompanying plate, Figs. 62 and 63 show the instrument in elevation and partial sections. When not indicating speeds it answers a good purpose as an office clock. This machine, being

the first of its kind, is considerably larger and heavier than it w
need to be if manufactured in quantities,—in which case it n
be made for an ordinary pocket, whereas this requires an ove
pocket. Its weight is 2¾ lbs., and the extreme size of the ca
× 3¾ × 2 inches. The bulk is greater than it would have bee
a smaller clock been at hand with sufficient power to do the v
It could be reduced still more by using a special "movem
which would run but a few minutes instead of thirty hours.
other reduction in bulk could be made by lessening the size o
dials, and printing upon them only every tenth number,
scratch marks between for units, as in the pocket-indicators. I
thought best, however, to let every number from 1 to 10,000
in plain figures without any mental calculation to connect the
ings of the two dials.

Referring to the drawings: Fig. 62 is a front view, as it st

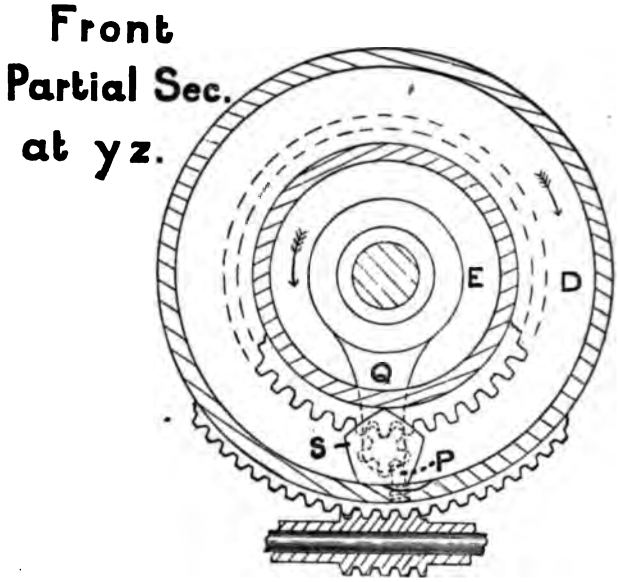


FIG. 64.

upon the desk or is held while operating. Fig. 63 is a se
through the center of Fig. 62 at u v, looking from left side.
64 is a section through the dial-wheels, etc., at y z, Fig. 63, loo
from front. The construction and operation are as follows
case A are mounted spindle B, clock C, dial-wheels D and E, cl
lever F, center-punches G and H, etc., together with their nece
journals and supports. The motions are as shown by arrows.

clock is wound and set by knurled discs at the back without the use of a key. Mounted upon the clock is time-wheel I, which makes one revolution in 1½ minutes. Sunk into I is an annular groove, extending ⅔ way around, whose motion from end to end represents one minute of time. At each end a groove runs out radially, and between the extremities of these latter an annular space extends the other ⅓ way around the wheel, representing the ½ minute of "overtime" before referred to. Working in these grooves and space is a pin, extending back from lever, F. Upon wheel I is a cam actuating F by means of the double spring J, and giving F a *tendency* to stay up and down alternately. Whilst the tendency is *up* the half-minute space comes around, and F flies up quickly as soon as the radial groove will allow. By the time the other radial groove arrives at the pin the tendency is *down*, and F descends quickly from its one minute rest. The groove is not made to act as a cam to push F up and down, because it could not then be radial at its working points, and would have to have a slope of say about 45° with a radius, which would give a slow motion to F. One side of it is sloped off, however, so as not to break the clock in case the spring should accidentally fail to act.

Loose upon spindle B is sleeve K carrying a worm and an automatic-stop-clutch, similar in principle to those used upon power presses. The swinging pawl, L, of this clutch engages by a spring, whenever allowed to do so, with teeth upon a disc M, keyed fast to spindle. Thus the spindle can be placed in contact with shaft to be tested, and will run free while L is held away from teeth of M by the trip-pin N, projecting from F. As soon as F descends, at the beginning of the minute, L engages with M, and the whole indicating mechanism, driven by worm on K, begins to act. At the end of the minute F rises and, catching against the sloping surface of L, pulls it out of gear, and also acts as a positive stop to the further revolution of K by momentum or friction. The shape of L is such that it can also be tripped by N in case of the accidental backward rotation of B and M. The spindle normally revolves in but one direction, and can be applied to a shaft at either end, according to which way the shaft is running. As an improvement upon the usual obtuse-angle shape, the driving edges at ends of spindle are made acute, so that their working faces are radial, and do not require so much end pressure by the operator to keep them from slipping out of the slight grooves which are supposed to exist in the shaft center, and which in this case may really exist by means of a slight tap with one of the center-punches *G* or *H*, which are right and left-handed respectively, and have in them teeth, milled out uniform with those in ends of spindle. These punches can be used while shaft is running, and have upon them ball heads to serve as feet for the office-clock personality of the instrument, when screwed into the sockets in which they are kept.

The dial-wheel D has a 100-toothed worm-gear projecting from its edge, driven by worm on N in the ordinary manner. It is, however, larger than usual, so as to have room upon its annular face for 100 legible numbers—from 00 to 99. It is cup-shaped and revolves upon a hollow stud O, which is a part of the casting that forms the spindle bearings. It is held forward against a stationary arm Q by spring R. It carries a radial pin P, which, once in each time around, 'revolves an intermediate 5-toothed pinion S, having its bearing in a boss upon Q. Upon front of S is a pentagonal disc which locks S from turning by resting against interior of D, except at the proper time, when one of its angles can pass by entering a notch provided in D. Projecting from the slow dial-wheel E is a 100-toothed spur-gear, driven by S. Thus D revolves once for each 100 revolutions of K, and E once for each 10,000 revolutions. Upon the face of E are a set of numbers like those on D, but progressing in the opposite direction. The combination of two of these numbers, read through a rectangular slot in the face-plate of case, shows any number of thousands up to ten. The wheel E has a shouldered stem, running inside of stud O, and is pushed forward by a spring around the stem—being stopped by a knurled head T, screwed tightly on. By grasping T both dials can instantly be set to zero, without running the spindle around a hundred or so times, as in the ordinary instrument. When T is pulled back, it pulls E through about $\frac{1}{64}''$ clearance space and then against a shoulder inside of D. Upon pulling further (against both springs) D is pulled far enough to slide out of gear with the worm and can then be revolved by the friction of E, where the latter bears upon the shoulder. This bearing is of much larger diameter than where spring R presses in the other direction, and therefore, with this increased leverage, D is sure to move—that is, until pin P strikes boss upon Q and stops D at the zero point. After this E alone moves (the friction contact slipping), and when its zero-point is reached T is released by the operator—allowing the springs to return both dials to their normal positions.

As this particular instrument is arranged, it will count whole revolutions only; but it can obviously be made to count fractional parts of a revolution by putting more than one clutch-pawl (L) around about K. This would increase its delicacy at the *stopping* point. Its degree of delicacy at *starting* is determined by the number of teeth in spindle collar M. In this case there are eight, which is sufficient for all practical purposes. Thus the time lost after the minute has begun, before counting commences, is the time necessary for lever F to spring down, plus a wait of less than $\frac{1}{8}$ revolution. The first item is approximately balanced at the end of the minute by the time required for F to spring up again. The wait at this end is something less than one revolution with one pawl; less than $\frac{1}{2}$ with two, etc.

In further reference to a reduction of bulk, the writer had thought of using a "Waterbury watch" for the timing mechanism, but found that its action would be too weak to drive the mechanical connections. It was then planned to operate the clutch lever F by an electro-magnet, making the connections with the watch electrical. This would make a very delicate machine, and would enable the issue of a "vest-pocket edition," so to speak. It would, of course, have to contain a small battery, and in consideration of the bother of keeping this in working order, the preference was given to the present mechanical form.

In conclusion, the advantages sought for in this construction may be summed up briefly as follows: Portability, absolute counting, definite reading, with either direction of motion; large capacity for variation in speed; convenience of manipulation, and moderate cost. Whether these are all attained can perhaps be better determined after some further experience in its use.

CXLV.

NON-CONDUCTING COVERINGS FOR STEAM-PIPES.

Further Experiments

BY PROF. JOHN M. ORDWAY.

[*Second Paper.*] *

Presented by C. J. H. Woodbury, Boston, Mass.

IN making the trial of steam-pipe coverings described in the former paper, the only place available was a room occupied with machinery; and as the steam-pipe could not be extended or changed in place, some desirable arrangements could not be carried out. The removal of the machinery and the enlargement of the room at length gave a chance to mount pipes for the special purpose of making further experiments. A connection was made with the main pipe conveying the steam of three boilers in which the pressure is maintained at about 65 lbs. A valve admits the steam to a short horizontal two-inch pipe provided with a pocket to receive whatever water may come forward. An elbow above the pocket conveys the steam into a slightly inclined two-inch pipe, also provided with a pocket, from which the water condensed in this two-foot length of pipe can be drawn off as it accumulates. The steam passes upward and through an elbow to a thirty-foot length of two-inch pipe, likewise provided with a pocket, and thence into a horizontal pipe with some side connections, and through a smaller descending pipe into a trap connected with the return pipe. The side connections are inclined two-inch pipes, 2½, 5, and 10 feet long, capped at the outer ends, the caps being provided with small stop-cocks for drawing off the condensed water. It is thus made possible to determine the condensation in two feet and thirty feet of transmitting pipe, and in 2½, 5, and 10 feet of blind pipes simply receiving steam. It had been a question whether condensation would be proportionally the same in long blind pipes as in short ones. It is conceivable that with very long pipes of small diameter there might be a difference. But it is found that ten feet of two-inch pipe condenses very nearly four times as much as 2½ feet.

The determinable condensation in the transmitting pipes has been

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* See page 73 of present volume.

found anomalous, and by no means proportionate to the lengths. I have been much puzzled to account for the strange behavior of these pipes, and have even gone so far as to change the arrangement. But the irregularity still continues. It is evident that the water formed does not all find its way into the proper pockets, and that moving steam must sometimes carry forward not a little mist.

Before placing much reliance on the results obtained in the way of condensation, it is proper to ascertain the quality of the steam at various times. For this purpose small cocks were screwed into the fittings in three places, and to them there were attached spiral coils of brass tube of $\frac{1}{12}$ inch bore, open at the end. Each of the coils is inclosed in a calorimeter of about twelve litres capacity. A weighed quantity of cold water is introduced into the calorimeter, and the steam is allowed to blow in for some three minutes. From the temperature of the steam and the increase of the water in weight and temperature we may easily calculate the percentage of mist in the steam. In many trials the steam has been found to be dry, while in others the proportion of mist ranges from two or three up to forty-two per cent. of the whole. This "priming" of the steam comes unexpectedly, and may last but a short time; but even a short continuance is sufficient to vitiate any determinations of lost heat based on the latent heat of the supposed steam. As there is no instrument which, like the thermometer, renders variations visible, changes may come and go unsuspected and unknown.

There is another source of inaccuracy in trials by the condensation method. The water must be drawn off frequently, and let off while it is far above 100° C. Consequently, much of it changes into vapor and escapes. I have endeavored to obviate this difficulty as far as possible by letting out the boiling water slowly, and running it through a long, twice-bent glass tube into a flask. But the precautions are by no means perfectly effectual. Any more complicated apparatus for drawing off the water would add a mass of cooling metal that would of itself be a source of error.

It was desirable to try the relation between the condensation that occurs when the heat is transmitted to the air and that which takes place when the covering is surrounded by water. Having a blind pipe thirty inches long, a new calorimeter was made twenty-eight inches long,—not in halves to be clamped together, but whole, to be slipped over the end of the pipe. With this arrangement it is possible to determine the whole condensation of the pipe when the calorimeter surrounds it, and then again when the transmitted heat goes into the air. The trials with this apparatus have not been so numerous as I could have wished, but they go to show that the radiation into air and that into water are very nearly the same.

Being confirmed in my belief of the greater reliability of the calorimetric mode of testing, I have tried several coverings of substances not used in the former trials, or used in a different way. Among others, one covering of cork was tried, as it was furnished by the "Société Anonyme des Liéges appliqués à l'Industrie," of Paris. This covering consists of long strips of cork with the edges nicely beveled, so that when they are laid side by side around the pipe they make an accurately fitting hollow prism, touching the pipe along the median line of each inner side. In the case of a two-inch pipe, ten strips are furnished. These are first tied on temporarily till the cork is well dried, and they are then bound on firmly with tinned iron wire. Such a covering is neat and strong, easy to put on and easy to take off. It is particularly suitable for pipes or boilers that are subject to concussions or jarring, like locomotive boilers. Cork is a good non-conductor, but the specimen sent me was too thin, being only five-eighths of an inch in thickness. An average of five trials showed a transmission of 26.54 kilogram Centigrade heat units per foot per hour. Much better results were shown by a thicker covering of cork chips coated and cemented together with waterglass. This makes an admirable covering,—one of the best ever devised.

It is difficult to apply a perfectly uniform and definite thickness of any covering to a round pipe, nor is it easy to impart a precise degree of compression to a cylindrical covering. Therefore, a new apparatus was set up for experiments with exact thicknesses and densities. A short piece of six-inch steam-pipe was provided with a malleable iron cap at each end, one of the caps being turned to a true face. The other cap was furnished with one pipe for introducing steam and another to carry off water and excess of steam. The turned cap is 7¼ inches in diameter. A canteen-like calorimeter of brass, six inches in diameter, can be adjusted with its face at any desired distance from the turned cap. The vacant interval may be surrounded with a strip of pasteboard cemented to the cylindrical sides of the cap, so as to make a round box with a narrow opening on the upper side. Through this opening any substance in powder may be introduced, and either left loose or rammed in.

This apparatus has enabled me to try the transmissive power of various powders and fibrous substances under various degrees of compression.

The following list gives the kilogram-centigrade heat units transmitted per hour through a thickness of twenty-five millimeters :—

Fine table salt	36
Plumbago	35
Fine washed sand	30.7
Coarse washed sand	30.6
Fibrous Asbestos	24.2
Air alone	23.7
Anthracite Coal	17.6
Finest Sand	15.7
Flour of Pumice-stone	15.4
Plaster of Paris	15.3
Sulphate of Barium	13.2
Paris White	10.2
Zinc White	8.5
Fossil Meal, compressed	7.7
Fossil Meal, loose	7.2
Magnesia alba, compressed	7
Magnesia alba, loose	6.7
Pine Charcoal	6.8
Calcined Magnesia	6.2
Cork Charcoal, coarse	6.2
Cork Charcoal, fine	5.9
Live geese feathers, loose	5.8
Live geese feathers, compressed	4.8
Cotton, loose	5.4
Cotton, compressed	4.5
Wool, loose	5.3
Wool, compressed	4
Wool, compressed more	4.5
Lampblack	4.8

Peclet speaks of cotton and other filamentous substances, as having the same transmissive power, whatever may be the degree of compression,—"quelle que soit sa densité." And this seems to be approximately correct for moderate degrees of crowding, but it is by no means exact. Moderate condensation somewhat enhances the non-conductive power, because it more fully prevents any motion of the entrapped air, and hence any convection. But we soon arrive at a point beyond which farther compactness does no good.

It is interesting to observe that at the temperature of 155° C. a mere air space is of little service. Probably the greater the heat the greater is the need of something to prevent the lively motion of the air. Were the arrangement such that the heater was horizontal, the air space below the level face, and the calorimeter at bottom, of course the result would be very different, for then convection would have no influence. But in any practical use of air spaces such an arrangement is rarely possible. Unarrested air cannot be ranked among the best of non-conductors. Mere air spaces are not to be recommended, except when light is to be admitted while heat is retained, as in the case of double windows.

[NOTE.—The necessary stoppage of the steam circulation in the buildings where these experiments are made has compelled the author to defer several further investigations to a future paper, but these memoranda are given as they stand to supplement some details of the previous notes.]

CXLVI.

ON A PRACTICAL SOLUTION OF THE PERFECT SCREW PROBLEM.

BY WILLIAM A. ROGERS, CAMBRIDGE, MASS.

At the outset of a discussion of the problem indicated by the title of this paper, it is clearly essential that the term "perfect screw" shall be defined in the most explicit way. Perfect is a relative term. For certain purposes a piece of mechanism may be perfect, while it might fail to meet the most simple requirements of another problem. In another paper the writer has used the illustration furnished by the carpenter who was called to level up his comparator, but it will bear repeating in this connection. He had been furnished with an astronomical level, but in a short time he returned in great disgust, saying that "the level was good for nothing—that it bobbed all about." "But," said he, "I have a level at home which will settle at the same spot every time," and he insisted that he should be allowed to go home and get the level that would "settle at the same spot every time." He was allowed to do the work in his own way, and shortly afterward he triumphantly pointed out the evidence that the bed of the comparator was perfectly level. It need not be said that the most elementary test showed that the bed was *not* level, notwithstanding the evidence pointed out by our good friend the carpenter.

A piece of mechanism of the class which the French would call mechanism of precision may be termed perfect *when it meets all the requirements of the purpose for which it was constructed.* Let us apply this definition to any mechanism which involves the use of a good screw.

The cross-head of a planer receives its vertical movement through two screws. If one screw has a pitch differing from the other, it is evident that new adjustments will be required for every elevation. But if, after the proper adjustment has been made at one elevation, it is found that the working parts of the planer remain constant at whatever height the cross-head may be raised, the screws may in this case properly be called perfect. Yet, if these two screws were removed from their connection with other working parts, it would without question be found that measurable errors of pitch could be detected and measured.

A short screw made at the works of the Waltham Watch Company will be presently described. If reliance could be placed upon the severe tests of direct measurement which have been applied to graduations produced by this screw, it might be fairly called perfect. On this bar of speculum metal 5,000 lines are ruled within a space of half an inch, producing what is called a diffraction grating. When this grating is subjected to examination under the spectroscope, there are certain optical tests of the accuracy of the spacing for short intervals which will at once detect errors which must always elude the most careful tests by direct measurement. To the naked vision there would not seem to be much difference between this grating and those produced by Rutherford, and especially the magnificent gratings from the machine of Professor Rowland, but tried by optical tests the difference is really so great that if all the errors could be charged to the screw itself the claim of perfection could not hold for a moment.

It has been intimated that the errors shown by optical tests in diffraction gratings may not after all be entirely chargeable to the screw. The flatness of the surface ruled, any unequal friction between the nut and the screw, the character of the groove cut by the ruling diamond—these and many other considerations determine the character of the grating. It is now well known that, severe as the optical test is, in the detection of periodic errors depending on single revolutions of the screw, it fails in the detection of errors separated at wide intervals. Indeed, even in the most perfect of all machines, Professor Rowland's, he is obliged to employ a "corrector" to eliminate the errors which are beyond the limits of direct measurement. The writer is well aware that he should speak with a good degree of moderation in this connection, since he has to a certain extent failed where Professor Rowland has succeeded; but Professor Rowland has a supreme knowledge of the problem both as a physicist and as a mathematician, and his success has been achieved by the power of keen analysis, aided by his little "corrector" and a precise knowledge how to use it, having as the basis of his work a most excellent but not a perfect screw.

Let us take another illustration. A cathetometer is an instrument for the measurement of vertical distances by means of one or more telescopes attached to a vertical standard upon which there is a graduated scale, usually one meter in length with subdivisions to millimeters. There are several well-known manufacturers of physical apparatus in Europe, who advertise that these graduations are without sensible error. An investigation of the errors of several of these graduated scales during the past three years has shown that in every case they were nearly within the requirements of the optical power of the telescopes employed, but it needed only the most superficial examination under the microscopes of the comparator to place them instantly far below the lowest limit required in an exact standard of length.

Illustrations almost without limit might be multiplied to show the necessity of defining the limit of accuracy with which one ought to be content in mechanical construction. It goes without saying that real progress begins when the mechanician recognizes that there is such a limit. A short time since, the writer asked Mr. Sharpe, of the firm of Darling, Brown & Sharpe, if he would undertake to grind a perfect cylinder. His reply was very suggestive. He said: "We are not making perfect mechanism of any kind any longer in this establishment. A few years ago we felt competent to undertake perfect work of any and every kind, but we have grown wiser since then." Need it be said that the work done by this company is in many respects of a higher grade than it was ten years ago?

Five or six years ago, the writer was ruling lines 120,000 to the inch more or less, and he thought nothing of obtaining for the probable error of a set of measures of graduations, figures low down in the millionths of an inch. It has since been learned by some not very pleasant experience that figures do not always tell the truth, especially figures which represent what are known as "probable errors"—that while straining at very small gnats, several very large camels walked by unperceived.

Let us now endeavor to answer the question — *What ought we to expect of a perfect screw?* Those of you who are accustomed to make screws will at once say that the answer depends to a large extent upon the length of the screw. And so it does under the ordinary methods of construction, but in the Rogers-Ballou process, which will presently be described, it is claimed that a screw 6 feet in length can be cut with nearly the same accuracy as a screw 6 inches long.

At this point it is important that the errors to which screws are subject should be defined with the utmost clearness. They are of three kinds:

(*a*) An error in the total length. Supposing the pitch to be uniform at every point between the terminal threads, the whole length may either exceed or fall short of the unit of length adopted, *e. g.*, the yard at the standard temperature, 62 Fahr.

(*b*) Even if the whole length is correct, the pitch of the screw for even revolutions may not be uniform. In a perfect screw the distance from face to face of every thread in a line parallel with the axis of the screw will be the same. That is, the inclined planes formed by the threads are everywhere parallel and equidistant.

(*c*) Even if conditions (*a*) and (*b*) are fulfilled there may yet remain a very troublesome class of errors, which are a function of single revolutions of the screw. If I rule 11 lines corresponding to even tenths of a revolution of the screw, I may find, from an examination of the spaces formed, that there is a gradual but very small increase in the length of each successive space up to a certain point, when a maximum value is reached. After this a diminution takes place which goes on until the amount of decrease is equal to the amount of the previous increase. Errors of this class are usually designated "periodic errors," since they are a function of a complete revolution of the screw. Expressed in mathematical language, every measured space gives an expression of the form

$$\Delta = m + a \sin. x + b \cos. x + a' \sin. 2x + b' \cos. 2x, \textit{etc.},$$

in which:

Δ = the required error.
m = a constant.
x = the angle of revolution.
a, b, a', b', *etc.* = unknown coefficients to be determined from a series of equations by the process of Least Squares.

It is important that we shall ascertain what efforts have been made to overcome these errors in the construction of screws.

It is well known that the earliest systematic efforts to place the screw problem upon a substantial and scientific basis were made by Whitworth, but he profited by the labors of still earlier investigators. The following account of the early efforts in this direction, communicated to me by Mr. H. J. Chaney, Warden of the Imperial Standards of Great Britain, is such a clear and concise statement of what was accomplished by the early investigators in this field that it is quoted entire, although it was not communicated for the purpose of publication:

"In the rapid development of steam machinery there was felt a necessity for accuracy and interchangeability in parts, which in the screw took practical form nearly half a century since; first in the production of a standard guide screw, and subsequently in the demand for a uniform system of screw threads.

"In this country it is perhaps to the eminent engineering firm of Messrs. Maudslay & Co. that we are indebted for the first attempt to construct a perfect system of screws. For his dividing engine, however, Mr. Bryan Donkin had constructed in the year 1828 a standard screw fitted with a compensating bar, by means of which the errors of different parts of the screw were allowed for. Many screws were cut by this machine, some of which were given to various scientific friends. Sir Joseph Whitworth among others had one of these screws in the year 1843.

"Messrs. Maudslay had the advantage of the assistance of a workman whose name is now identified with all that is systematic and accurate in screw work—Whitworth, and who subsequently left them to take part under Mr. Clements, of Lambeth, in the construction, as I understand, of Babbage's Difference Engine, and there produced with Clements the first standard guide screws.

"In a paper communicated to the Institution of Civil Engineers in 1841, Whitworth discussed the question of the want of uniformity of screw threads, and put forward a series of sizes adapted to the use of engineers. These sizes differed from Maudslays', and appear to have been a compromise between sizes then generally in use. For iron piping, Whitworth took, as is well known, some sizes which had been adopted by Messrs. James Russell & Son, pipe manufacturers.

"For engineering purposes the Whitworth thread appears now to be generally adopted. For many other purposes the want of a common standard gauge for screws is much felt. A committee of the British Association appointed in 1881 for the purpose of determining a gauge for the manufacture of small screws used in electrical apparatus and clock-work, adopted a pitch similar to the Whitworth pitch for all sizes down to a ¼ inch, and also adopted the Whitworth thread above or below ¼ inch. This committee have made no definite report, and there appears to be much difference of opinion on the questions as to the inch or millimeter units, the angle of the threads, descriptive number of each size, etc."

At the outset of a discussion of the screw problem, and especially as a preliminary to any attempt to improve upon existing methods of construction, it seemed important to ascertain just what degree of accuracy had been attained thus far in the manufacture of precision screws. Accordingly, in 1879 the writer visited Baltimore, Philadelphia, Schenectady, New York and Providence, and obtained transfers from screws by Perreaux, Bianchi, Clement, Brown & Sharpe, and Rutherford. As far as could be learned, these were the only screws at that time in this country possessing any claim to more than ordinary accuracy. In London, a yard with subdivisions into inches was obtained from the dividing engine used by Troughton & Sims in ordinary work. In Paris a meter with subdivisions to decimeters was obtained from the dividing engine of Desmoulins-Froment. Access could not be obtained to the dividing engine of Brunner Frères, but a standard centimeter subdivided to tenths of millimeters was obtained from this firm.

Application was made to Sir Joseph Whitworth & Co. for a screw one meter in length, but the reply was returned that the company was not prepared to do work of this class with the degree of precision required. Froment, of Paris, however, accepted the order, but it was not until after two years that the screw was delivered.

It does not seem necessary to include in this paper a full account of the investigation of the errors of these screws. The results can be stated in a few words.

(*a*) In only two cases was the total length found to be substantially correct,—viz., in a yard and meter made by Brown & Sharpe and in a meter by Froment. But in both of these cases the total length was varied to correspond with the unit of length adopted by means of a "corrector." Brown & Sharpe have always exercised their undoubted right of declining to allow a personal inspection of their processes, but I cannot be far from right in saying that a corrector was employed not only in the correction of the total length, but also in the correction of errors due to the irregularities of the screw. In Paris, Froment accorded the rare privilege of a personal inspection of his dividing engine. It was estimated that the corrector eliminated errors amounting to about one-tenth of a millimeter, or about one-two hundred and fiftieth of an inch. In the remaining cases the error in the total length was in no case less than one two-hundred-and-fiftieth of an inch, and in one case it reached one-tenth of an inch in one yard.

(*b*) In every case in which a corrcetor was not employed the errors depending on single revolutions of the screw were very large, while the variation in the pitch at different points along the screw varied between $\frac{1}{100}$ inch and $\frac{1}{2000}$ inch.

If one can judge of the screws made by German manufacturers by the graduations of German cathetometers, they would appear to be at least of no higher grade than those of French or American manufacture.

It appears safe to conclude that with the exception of the Rutherford screw, of a few micrometer screws by Alvan Clark & Sons, and perhaps of a small number of screws of the same class by Hilger, of London, by Brunner Frères, of Paris, and by Repsold, of Hamburg, there was not in the year 1880 a single screw in existence which could be shown by a published discussion of its errors to be sufficiently uniform in pitch to entitle it to the rank of a precision screw.

One does not need to go very far in assigning a cause for the failure to make any important advance in the construction of screws. According to the existing methods of manufacture, the maker of a screw has absolutely no precise knowledge of the form and dimensions of the thread which he cuts till the screw is completed. It is well known that several devices have been employed to test the accuracy of the screw during its construction, but they nearly all involve *the errors of a combination of threads, instead of the errors of single threads.*

It has been the custom to assume that the residual errors of a screw can be worked out by grinding with a lead nut. The ordinary methods of grinding are wholly inadequate, especially in the elimination of the errors depending on one revolution of the screw, when combined with uniformly increasing or decreasing variations in pitch for successive threads.

The action of a grinding nut may be likened to that of a harrow upon a ploughed field. The harrow will easily smooth down the furrows freshly turned by the plough, but it would be making a too serious demand upon it to require that it should level down hills or even hillocks. The whole difficulty in grinding consists, first, in the fact that the action between the nut and the screw is to a certain extent mutual, and, second, that the threads of a screw are ground *in combination, and not each by itself.* A grinding nut will easily work out short and irregular variations in pitch, but it will not eliminate long sweeps of errors except by accident. I shall presently recur to this matter in connection with the discussion of two typical screws.

(c) *The apparent irregularities in screws are due, first, to the errors in the pitch of the screw itself, and, second, to the unequal friction between the nut, the screw, and the ways upon which the carriage driven by the nut moves.* Abundant experience has proved this statement to be true. In the first dividing engine constructed for the writer by Buff & Berger, of Boston, the nut was at first connected rigidly with the carriage. After an experience of two years it was found impossible to get the same system of errors for the screw in successive trials if the slightest change was made in the relation of the working parts of the machine. By touching a screw here or there it was found possible even to reverse the sign of the correction depending on one revolution of the screw. About 1878 a *free nut* was first employed, *i.e.*, the nut now travels freely upon the screw, without the slightest binding, pushing the carriage before it. From that time to this, the system of corrections required for the screw has remained unchanged, and I now use precisely the same values as were computed five years ago. The constancy of the corrections was greatly aided by the use of finely powdered graphite as a lubricant. A similar experience with some micrometer screws made for the meridian circle of Harvard College Observatory by Mr. Geo. Clark, of the firm of Alvan Clark & Sons, was very instructive. One of the screws was mounted and dismounted nine times, and in every case different systems of errors were obtained, the extreme difference being about $\frac{1}{2000}$ inch.

There are a few fundamental requirements which must be absolutely met in the successful construction of a screw. Let us try to state these requirements in the most simple and positive terms:

(1) The shaft to be threaded must maintain a true cylindrical form during every part of a revolution and during every successive revolution, *i. e.*, the axis of motion must be a straight line.

(2) The cutting tool must travel in a line exactly parallel with the axis of the screw to be cut. This requirement demands first that the ways upon which the carriage of the screw-cutting machine travels shall be straight, allowing the carriage to move in a horizontal plane. Especial attention must therefore be paid to the elimination of the flexure of the bed-plate upon which the ways are cut. Second, the ways must be free from horizontal curvature, allowing the carriage to move in a true vertical plane. Expressed in general terms, the conditions to be fulfilled require that every movement of the cutting tool with respect to the screw to be cut *shall be referred to an invariable reference plane.*

(3) The cutting tool must give to each single thread approximately its proper form and pitch during each successive operation of cutting, independently of every other thread.

If these conditions can be fulfilled in the construction of a screw, there is no reason why the residual errors may not be reduced far below the limit reached in our present practice.

Let us now venture to define the limit which ought to be reached. First, a screw ought to be capable of measuring as closely as a skillful mechanic can calliper. In ordinary practice that limit may be placed at about $\frac{1}{20000}$ inch, but in the hands of a person in which the sense of feeling has been cultivated even but slightly, a good calliper will detect variations in the diameter of a small cylinder amounting to about $\frac{1}{40000}$ inch.

Second, experience has shown that the limit of *certainty* in measuring short spaces, *e. g.*, two or three inches, is about $\frac{1}{50000}$ inch, while for longer intervals, in which flexure comes into play, the limit should be placed at between $\frac{1}{20000}$ and $\frac{1}{30000}$ inch.

It would appear, therefore, that we ought to demand of a precision screw that it shall have no error much exceeding the lowest limit named. Of course, the average error of adjacent threads would in this case be far less. If it is possible, therefore, to construct a screw whose error shall not rise above this limit, it may fairly be termed a screw of precision, or, if you choose, a "perfect screw."

In the fall of 1882, Mr. Geo. F. Ballou, who is now superintendent of the Ballou Manufacturing Co. of Hartford, Conn., which has undertaken the manufacture of improved lathe and precision screws by what has been designated the Rogers-Ballou process, joined the writer in an attempt to give a practical application to the principles of construction which have been outlined in this paper. For about four years previous to this time, Mr. Ballou had been engaged in making a dividing engine which Mr. Chas. Van Woerd, at that time mechanical superintendent of the Waltham Watch Company, undertook to construct upon my order.

A high limit of precision was soon reached in the construction of a short screw, having a working length of about 4 inches, although the hope of obtaining from it an improvement in diffraction gratings was not realized. But in every attempt to make a screw having a length of half a meter it was found impossible to go beyond a certain limit by the ordinary methods of construction and correction. After experimenting in various ways for nearly two years, Mr. Van Woerd decided to adopt the form of a sectional screw. Threads were cut upon ferrules 1¾ inches in length, each ferrule being cut from the same part of the leading screw. These ferrules were then placed upon a cylindrical shaft, and adjusted in such a way that the threads of the adjacent ferrules would match.

The method itself is not new; Whitworth tried it and abandoned it many years ago. Mr. Van Woerd, however, by the method described in patent No. 293,930, claimed that the difficulties encountered by Whitworth were entirely overcome.

Mr. Ballou had done all of the actual work of construction up to the point of the application of the new method of making the ferrules.

About this time an order was received from Professor Wm. A. Anthony for the construction of a dividing engine for the Physical Department of Cornell University. Upon accepting the order, a shop was fitted up in Boston with tools of the best quality, chiefly from the establishment of Pratt & Whitney, and Mr. Ballou undertook the construction of the engine, mainly from his own designs, and of the screw which is its essential part. The completed machine was shipped in just 35 weeks after the actual commencement of the work; and the screw, which will be presently discussed, was cut and ground in 27 hours from the time the first tracing of a thread was made. It was at that time practically perfect for about 20 inches, and nearly as perfect as it afterward became by the process of grinding adopted. Notwithstanding the fact that the work was done upon a common lathe in which the errors of the leading screw were enormously large, the result showed that the method employed was based upon correct mechanical principles, and was entirely feasible.

This method can be described in a very few words.

Let the reader hold clearly in mind the following:

There are:

(*a*) An ordinary lathe, the ways of which have been made as nearly straight as possible.

(*b*) A shaft between dead centers which maintains a cylindrical form during every revolution and every part of a revolution.

(*c*) A microscope provided with Tolles' opaque illuminator for viewing opaque objects, attached to the carriage moved by the leading screw of the lathe.

(*d*) A graduated bar mounted independently of the carriage with subdivisions which are multiples of single threads of the leading screw.

(*e*) A slide moving parallel with the leading screw, by means of a very short and firmly mounted micrometer screw of comparatively large diameter and attached firmly to the carriage. The tool-post is secured firmly to this secondary slide.

(*f*) A mechanical means of determining when the leading screw has made a complete revolution.

The method of proceeding was as follows:

(1) The graduated bar having been leveled up and set parallel to the axis of the screw to be cut, the micrometer of the microscope was set upon the initial line. The lathe was then started with the leading screw "in feed." After the screw had made, for example, nearly ten revolutions, the lathe was stopped and the remainder of the even revolution was completed by hand manipulation. The deviation of the micrometer line from the corresponding graduation upon the bar was then measured in terms of the screw-head of the secondary micrometer screw. In this way the errors of the leading screw with respect to the graduations of the standard bar were determined and written down upon a strip of paper pasted to the vertical face of the bar.

(2) The carriage was then started again with the cutting tool in operation, and by means of a rough pointer, the micrometer screw working the secondary slide was fed either forward or backward, in accordance with the corrections before determined. Hence, when any even revolution was completed, it would be found that the line of the bar would be nearly under the cross wire of the microscope. This operation was kept up until the screw was finished.

At the completion of the operation of cutting, it was found—

First, That the total length of the screw corresponded nearly with the length of the line standard from which it was cut.

Second, That there were at many points minute irregularities of pitch, due to the fact that the application of the corrections intermediate between the main divisions had not been exactly made.

Third, That the crucial test of the removal of these irregularities by grinding with a brass nut was a complete success. As had been predicted, they were for the most part removed after an hour's grinding.

The method of testing was as follows: Two half nuts with projecting arms resting upon the ⌒ shaped way were first placed at a fixed interval apart. A microscope was mounted upon one nut and coincidence was made between the micrometer wire of the microscope and a line drawn upon the upper surface of the other nut. It is obvious that if the relation between the different threads of the screw remained constant, the line under the microscope would remain constant. This constancy under a half-inch objective was maintained for about twenty inches. Then the nuts began to separate, and the separation continued until the maximum deviation amounted to about $\frac{1}{1000}$ of an inch; but near the end the nuts came back to their first relation.

In order to eliminate these residual errors, together with the remaining errors which were a function of one revolution of the screw, the following method was employed. The grinding nut was made in two halves, in such a manner that a constant relation was maintained between the two halves, both in their normal and in reversed positions. By grinding the screw first with the two halves of the nut in their normal relation and then in reversed relations, the tendency was to continually work out the periodic errors of the screw, with the exception of minute errors which were transferred from the screw to the nut during the operation of grinding.

In order that the nut might grind without disturbing the general relation between the threads, a cast-iron cylinder with a centre at the bottom was filled with the best sperm oil, and the screw was mounted vertically upon this centre. At first two broad fans were attached to the nut in the hope that the resistance of the oil, which in this case would be symmetrical with respect to the axis of the screw, would be sufficient to drive the nut upon the screw. As this movement was found to be too slow, a guiding rod was used.

The grinding process was continued for three weeks, and the results obtained confirmed previous experience. At the end of the first week the maximum error of $\frac{1}{1000}$ of an inch had been reduced about one-half, but it was found that small errors had been introduced in the mean time at other points, through a slight transfer of the errors of the screw to the nut itself and from thence back to the screw. Near the end of the second week the screw was clearly less perfect as a whole than at the commencement of the operation of grinding. Mr. Ballou then recut the nut, making its diameter a little less than than that of the screw. Within a few hours thereafter a decided improvement was observed. A new nut was made at the end of the second week having its diameter still a little less than before. During the third week the gain consisted for the most part in eliminating the errors which had been introduced during the second week. Throughout the entire operation of grinding, reversals were made every hour both of the two halves of the nut and of the screw upon its centres.

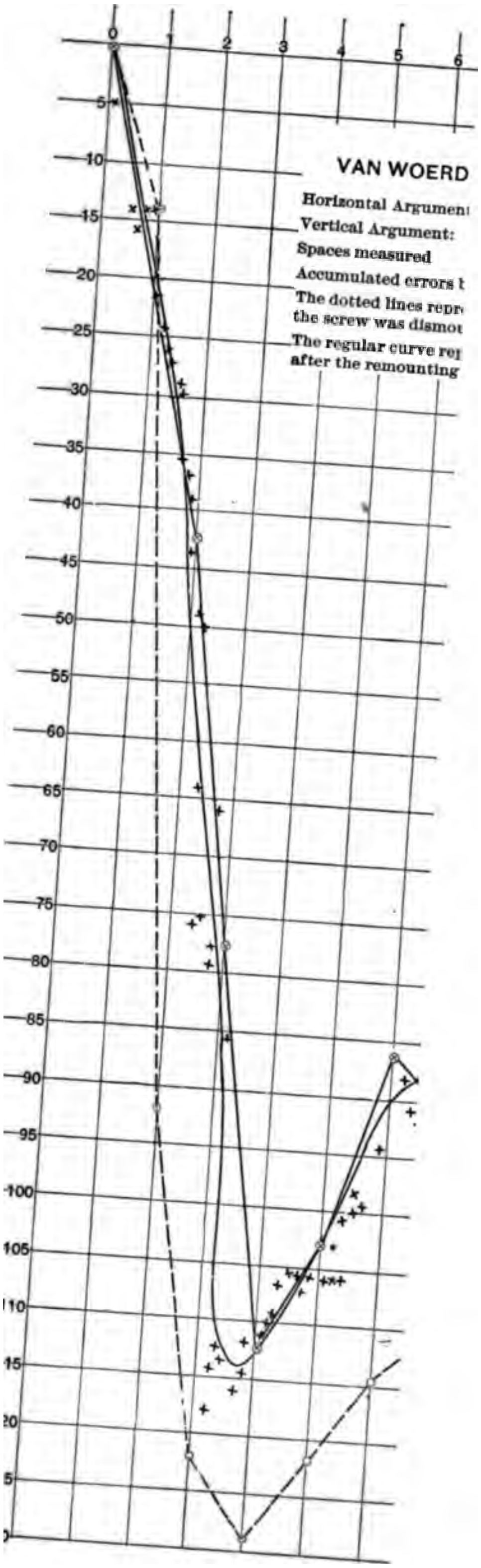
For a comparison of the old with the new method of cutting screws the sectional screw made by Mr. Van Woerd has been chosen; *first*, because its errors are less than those for either of the long screws which preceded it, and, *second*, because it seemed important to ascertain whether a sectional screw can be made which possesses decided advantages over the ordinary form. It is pretty certain that this particular screw is the best of its class ever made. The workmanship upon it could hardly be better. If it is found that errors of considerable magnitude remain, we may conclude that they should be charged to the method itself.

The method of obtaining transfers from this screw was as follows: It had been found by a direct comparison both with a half meter and a half yard, standard at 62° Fahr., that

The half meter = 400.4210 revolutions of the Waltham screw.

The half yard = 366.1382 revolutions of the Waltham screw.

Since the screw was cut 8 threads to the centimeter, it is therefore $\frac{421}{800}$ of a millimeter too long, or nearly a millimeter in a meter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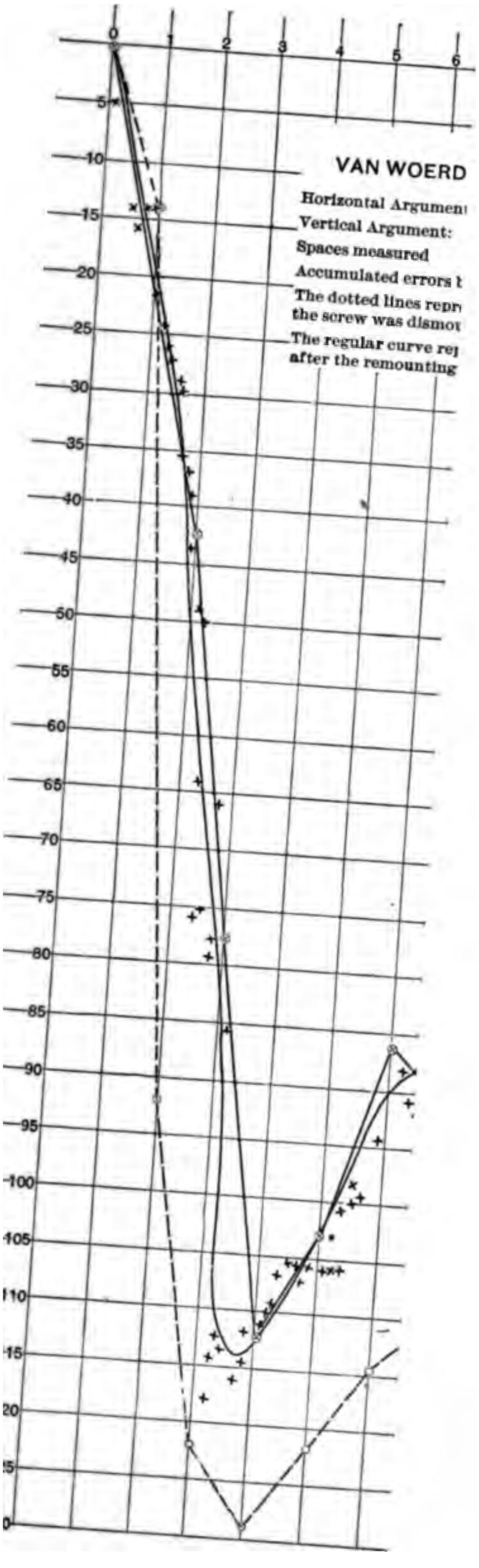
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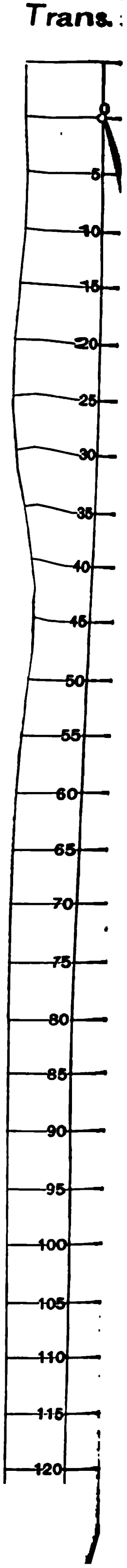
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1 Div. = .0000050 inch.

VAN WOERD SECTIONAL SCREW.

Comparison of the computed with the observed readings for:

Subdivisions of the half meter.

Spaces.	Observed Reading of Index.		Computed Reading of Index.		Δ Expressed in hundred thousandths of an inch.
	Rev.	Div.	Div.	Δ Div.	
1	8	63	84	− 21	− 10
2	16	140	168	− 28	− 14
3	24	196	253	− 57	− 28
4	32	271	337	− 66	− 33
5	40	327	421	− 94	− 47
6	48	369	505	− 136	− 68
7	56	431	589	− 158	− 79
8	64	459	624	− 165	− 83
9	72	533	758	− 225	− 113
10	80	606	842	− 236	− 118
11	88	696	926	− 230	− 115
12	96	782	1010	− 228	− 114
13	104	864	1095	− 231	− 115
14	112	973	1179	− 206	− 103
15	120	1065	1263	− 198	− 99
16	128	1161	1347	− 186	− 93
17	136	1249	1431	− 182	− 91
18	144	1325	1516	− 191	− 95
19	152	1436	1600	− 164	− 82
20	160	1525	1684	− 159	− 60
21	168	1605	1768	− 163	− 82
22	176	1692	1852	− 160	− 80
23	184	1811	1937	− 126	− 63
24	192	1914	2021	− 107	− 53
25	200	2021	2105	− 84	− 41
26	208	2118	2189	− 71	− 35
27	216	2201	2273	− 72	− 36
28	224	2290	2358	− 68	− 34
29	232	2355	2442	− 87	− 43
30	240	2435	2526	− 91	− 46
31	248	2522	2610	− 88	− 44
32	256	2604	2694	− 90	− 45
33	264	2717	2779	− 62	− 31
34	272	2819	2863	− 44	− 22
35	280	2903	2947	− 44	− 22
36	288	2990	3031	− 41	− 20
37	296	3073	3115	− 42	− 21
38	304	3154	3200	− 46	− 23
39	312	3241	3284	− 43	− 22
40	320	3331	3368	− 37	− 18
41	328	3409	3452	− 43	− 21
42	336	3512	3536	− 24	− 12
43	344	3605	3621	− 16	− 8
44	352	3693	3705	− 12	− 6
45	360	3785	3789	− 4	− 2
46	368	3862	3873	− 11	− 5
47	376	3951	3957	− 6	− 3
48	384	4040	4042	− 2	− 1
49	392	4124	4126	− 2	− 1
50	400	4210	4210	+ 0	+ 0

Comparison of the computed with the observed readings for:

Subdivisions of the half yard.

Spaces.	Observed Reading of Index.		Computed Reading of Index.		Δ Expressed in hundred thousandths of an inch.
	Rev.	Div.	Div.	Δ Div.	
1	10	1677	1705	− 28	− 14
2	20	3368	3410	− 42	− 21
3	30	5055	5115	− 60	− 30
4	40	6735	6820	− 85	− 43
5	50	8375	8525	− 150	− 75
6	61	77	230	− 153	− 76
7	71	1708	1935	− 227	− 114
8	81	3416	3640	− 224	− 112
9	91	5129	5345	− 216	− 108
10	101	6845	7051	− 206	− 103
11	111	8559	8756	− 197	− 98
12	122	288	461	− 173	− 86
13	132	1982	2166	− 184	− 92
14	142	3690	3871	− 181	− 91
15	152	5415	5576	− 161	− 80
16	162	7131	7281	− 150	− 75
17	172	8824	8986	− 162	− 81
18	183	580	697	− 117	− 58
19	193	2303	2396	− 93	− 46
20	203	4039	4101	− 62	− 31
21	213	5733	5806	− 73	− 37
22	223	7445	7511	− 66	− 33
23	233	9131	9216	− 85	− 42
24	244	831	921	− 90	− 45
25	254	2534	2626	− 92	− 46
26	264	4282	4331	− 49	− 25
27	274	6013	6036	− 23	− 12
28	284	7697	7741	− 44	− 22
29	294	9390	9446	− 56	− 28
30	305	1102	1151	− 49	− 24
31	315	2849	2856	− 7	− 3
32	325	4530	4562	− 32	− 16
33	335	6246	6267	− 21	− 10
34	345	7960	7972	− 12	− 6
35	355	9776	9677	− 1	− 1
36	366	1382	1382	+ 0	+ 0

The transfer from the Cornell screw was kindly made for me by Professor Anthony after the engine had been mounted upon the firm foundation prepared for it in the new Physical Laboratory of Cornell University. The transfer consists of forty half-inch spaces, but only thirty-six of these have been fully investigated.

It is necessary at this point to define clearly a class of errors which will be designated *accumulated errors.* They will be indicated by the symbol Σ, which is the usual symbol for a summed series.

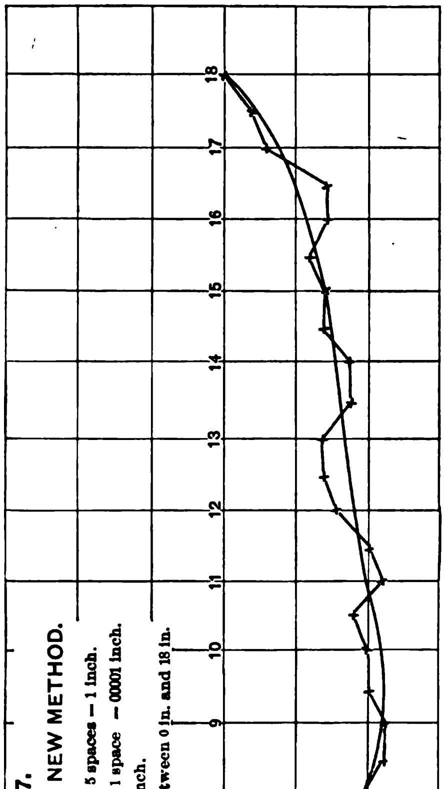

Fig. 57.

CORNELL SCREW, NEW METHOD.

Horizontal Argument: 5 spaces = 1 inch.
Vertical Argument: 1 space = .00001 inch.
Spaces measured = ½ inch.
Accumulated errors between 0 in. and 18 in.

If a given space is subdivided into any number of approximately equal parts, the error of any space with respect to the mean of all the spaces is called a relative error. The accumulated error is the algebraic sum of the relative errors reckoned from the initial line. Thus, if the second space of a given series of subdivisions is one unit longer than the first, the third one unit longer than the second, the fourth one unit longer than the third, etc., the error of the fifth line expressed in aliquot parts of the whole space subdivided will be three units. But in this summation of the relative errors, the accidental errors of observation are carried along with every subsequent summation. Hence the error, for example, of the middle point may not be found to be the same as would be obtained from a direct comparison of the two halves. Indeed, it ought not to be expected that in practice the summation of the errors of fifty spaces should give the same accumulated error for spaces 10, 20, 30 and 40 as would be obtained by a direct comparison of these spaces. If a substantial agreement is obtained, however, whatever the number of sub-divisions, it may be assumed that the errors of the separate spaces have been correctly determined.

Let us now apply this test to the errors of the Cornell screw. The measures given below were obtained by a comparison of each space with a constant distance between the two stops of the comparator. If the distance between the stops differs from the mean of all the spaces, a constant must be taken from each in order to reduce it to the mean of all the readings. This constant has been subtracted in every series given below except in the first. Here, instead of obtaining the constant from the whole series for the reduction of the remaining series, it will be necessary to obtain it for the space measured in that series. The errors at intermediate points are found by adding to the relative errors for each space the uniformly distributed errors of the limiting lines of that space, as will be shown below. The uniformly distributed errors are printed in smaller type than the errors of the main subdivisions. The values given are expressed in terms of the micrometer screw of the microscope employed, in which

1 div. = .000020 inch.

INVESTIGATION OF HALF-INCH SPACES FROM THE CORNELL SCREW.

OBSERVED DATA.

SPACES =	1/2 IN.	2/2 IN. Σ	3/2 IN. Σ	4/2 IN. Σ	6/2 IN. Σ	9/2 IN. Σ	18/2 IN. Σ
	div.	*div.*	*div.*	*div.*	*div.*	*div.*	*div.*
0	+ 0.0	+ 0.0	+ 0.0	+ 0.0	+ 0.0	+ 0.0	+ 0.0
1	+ 2.5	+ 1.9	+ 1.5	+ 0.8	+ 0.6	− 0.1	− 0.3
2	+ 0.5	+ 3.9	+ 3.0	+ 1.7	+ 1.3	− 0.2	− 0.6
3	− 0.1	+ 2.9	+ 4.5	+ 2.6	+ 2.0	− 0.4	− 0.9
4	− 0.2	+ 2.0	+ 3.9	+ 3.4	+ 2.7	− 0.5	− 1.2
5	+ 0.4	+ 1.6	+ 3.3	+ 3.3	+ 3.4	− 0.6	− 1.5
6	− 1.4	+ 1.2	+ 2.8	+ 3.2	+ 4.0	− 0.8	− 1.8
7	− 1.8	+ 0.6	+ 1.7	+ 3.1	+ 3.1	− 0.9	− 2.1
8	− 0.3	− 0.1	+ 0.6	+ 3.0	+ 2.2	− 1.0	− 2.4
9	− 3.1	− 1.6	− 0.6	+ 1.7	+ 1.2	− 1.2	− 2.8
10	− 1.9	− 3.0	− 0.9	+ 0.4	+ 0.3	− 1.8	− 3.1
11	− 2.1	− 3.5	− 1.1	− 0.9	− 0.6	− 2.4	− 3.4
12	− 2.9	− 4.0	− 1.4	− 2.3	− 1.6	− 3.0	− 3.7
13	− 1.3	− 4.2	− 2.0	− 2.9	− 2.0	− 3.6	− 4.0
14	− 1.7	− 4.4	− 2.6	− 3.5	− 2.4	− 4.2	− 4.3
15	+ 0.5	− 4.8	− 3.3	− 4.1	− 2.8	− 4.8	− 4.6
16	− 2.6	− 5.2	− 4.2	− 4.6	− 3.1	− 5.4	− 4.9
17	− 1.6	− 5.0	− 5.1	− 4.7	− 3.5	− 6.0	− 5.2
18	− 0.6	− 6.5	− 6.1	− 4.8	− 3.9	− 6.6	− 5.5
19	+ 0.0	− 6.1	− 5.5	− 4.9	− 3.8	− 6.4	− 5.2
20	− 1.3	− 5.2	− 4.9	− 5.0	− 3.6	− 6.2	− 4.9
21	+ 0.4	− 5.0	− 4.2	− 5.0	− 3.5	− 6.1	− 4.6
22	− 1.5	− 4.8	− 4.3	− 5.1	− 3.4	− 5.9	− 4.3
23	+ 0.3	− 4.3	− 4.4	− 5.1	− 3.2	− 5.7	− 4.1
24	+ 0.6	− 3.8	− 4.5	− 5.2	− 3.0	− 5.5	− 3.8
25	+ 0.2	− 3.8	− 4.5	− 5.2	− 2.8	− 5.4	− 3.5
26	− 1.1	− 3.9	− 4.5	− 5.2	− 2.6	− 5.2	− 3.2
27	− 1.8	− 4.0	− 4.5	− 5.1	− 2.4	− 5.0	− 2.8
28	− 0.5	− 4.0	− 3.3	− 5.1	− 2.2	− 4.5	− 2.5
29	+ 0.0	− 3.5	− 2.1	− 4.8	− 2.0	− 3.9	− 2.2
30	− 0.4	− 3.0	− 1.4	− 4.5	− 1.8	− 3.4	− 1.8
31	− 0.8	− 3.0	− 1.3	− 4.2	− 1.5	− 2.8	− 1.6
32	− 0.8	− 2.9	− 1.2	− 3.8	− 1.2	− 2.3	− 1.3
33	+ 0.0	− 2.4	− 1.0	− 2.9	− 0.9	− 1.7	− 1.0
34	+ 1.5	− 1.9	− 0.7	− 1.9	− 0.6	− 1.2	− 0.7
35	+ 0.5	− 0.9	− 0.3	− 0.9	− 0.2	− 0.6	− 0.3
36	+ 1.0	+ 0.0	+ 0.0	+ 0.0	+ 0.0	+ 0.0	+ 0.0

RESULTS.

ARGUMENT—HALF-INCH SPACES.

(1) Σ	(2) Σ	(3) Σ	(4) Σ	(6) Σ	(9) Σ	(18) Σ	*MEAN Σ	MEAN IN HUNDRED THOUSANDTHS OF AN INCH.	Δ
+0.0	+0.0	+0.0	+0.0	+0.0	+0.0	+0.0	+0.0	+ 0	+6
+3.1	+2.9	+3.0	+2.9	+3.0	+3.0	+3.2	+3.0	+ 6	+2
+4.2	+3.9	+4.0	+3.9	+4.1	+4.0	+4.5	+4.1	+ 8	+1
+4.7	+3.5	+4.5	+4.3	+4.6	+4.3	+5.1	+4.5	+ 9	−2
+4.1	+2.0	+3.4	+3.4	+4.0	+3.6	+4.7	+3.5	+ 7	+2
+5.1	+2.5	+3.9	+4.5	+5.0	+4.5	+5.8	+4.4	+ 9	−2
+4.3	+1.2	+2.8	+3.8	+4.0	+3.5	+5.2	+3.4	+ 7	−3
+3.2	−0.2	+1.6	+2.7	+3.3	+1.0	+4.1	+2.1	+ 4	+1
+3.5	−0.1	+1.9	+3.0	+4.1	+1.2	+4.6	+2.4	+ 5	−5
+1.0	−2.2	−0.6	+1.3	+2.0	−1.2	+2.1	+0.2	+ 0	−1
−0.8	−3.0	−0.5	+0.4	+1.1	−2.1	+1.0	−0.5	− 1	−2
−1.7	−3.1	−0.9	−0.5	+2.0	−3.0	−0.3	−1.0	− 2	−2
−4.0	−4.0	−1.4	−2.3	−0.6	−5.1	−2.5	−2.6	− 4	−3
−4.7	−4.0	−2.5	−2.9	−2.1	−5.4	−3.0	−3.3	− 7	−2
−5.8	−4.4	−4.0	−3.6	−3.0	−6.2	−4.0	−4.2	− 9	+2
−4.7	−3.3	−3.3	−2.7	−3.9	−4.7	−2.7	−3.5	− 7	−3
−6.7	−5.2	−5.2	−4.6	−3.4	−6.3	−4.6	−4.9	−10	−1
−7.7	−6.2	−6.1	−5.4	−4.0	−6.0	−5.4	−5.5	−11	+0
−7.6	−6 5	−6.1	−5.2	−3.9	−6.6	−5.5	−5.6	−11	+1
−7.0	−5.5	−5.2	−4.5	−3.6	−5.9	−5.0	−4.9	−10	+0
−7.7	−5.2	−5.6	−5.0	−4.4	−6.5	−5.8	−5.4	−10	+1
−6.7	−4.1	−4.2	−4.6	−3.7	−5.6	−4.8	−4.5	− 9	−2
−7.6	−4.8	−5.6	−6.1	−4.8	−6.4	−5.8	−5.6	−11	+1
−6.7	−4.4	−5.2	−5.7	−4.1	−5.4	−5.1	−5.0	−10	+2
−5.5	−3.8	−4.5	−5.2	−3.0	−4.2	−3.9	−4.1	− 8	+1
−4.6	−3.2	−3.4	−4.2	−2.0	−3.4	−3.6	−3.3	− 7	+0
−5.2	−3.9	−3.5	−4.5	−2.3	−3.8	−4.2	−3.7	− 7	−2
−6.4	−4.3	−4.5	−5.4	−3.3	−5.0	−5.3	−4.6	− 9	+0
−6.3	−4.0	−3.5	−5.1	−3.0	−5.0	−5.3	−4.8	− 9	+2
−5.7	−3.0	−2.1	−4.8	−2.2	−4.5	−4.8	−3.5	− 7	+0
−5.5	−3.0	−1.4	−3.9	−1.8	−4.3	−4.6	−3.3	− 7	+1
−5.6	−3.0	−1.6	−3.9	−1.5	−4.5	−4.9	−3.2	− 6	−1
−5.8	−2.9	−1.8	−3.8	−3.2	−4.7	−5.2	−3.6	− 7	+0
−5.0	−3.1	−1.0	−2.9	−3.1	−4.2	−4.6	−3.5	− 7	+4
−2.9	−1.9	−0.2	−1.4	−1.6	−2.1	−2.6	−1.6	− 3	+1
−1.8	−1.1	−0.3	−0.8	−0.9	−1.4	−1.5	−0.8	− 2	+2
+0.0	+0.0	+0.0	+0.0	+0.0	+0.0	+0.0	+0.0	+ 0	

The method of deriving the quantities given in the above table m the data given in the previous table will be obvious from the lowing illustrations:

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* Excluding (1).

FOR THE INCH SPACES.					FOR THE 3 INCH SPACES.				
From measures of ½ inch spaces.			From measures of inch spaces.		From measures of ½ inch spaces.			From measures of inch spaces.	
	Δ	Σ	°	sum.		Δ	Σ		sum
+ 2.5	+ 1.0	+ 1.0	+ 1.9	+ 2.9	+ 2.5	+ 2.4	+ 2.4	+ 0.6	+ 3.0
+ 0.5	− 1.0	+ .0	+ 3.9	+ 3.9	+ 0.5	+ 0.4	+ 2.8	+ 1.3	+ 4.1
					− 0.1	− 0.2	+ 2.6	+ 2.0	+ 4.6
+ 1.5					− 1.2	− 1.3	+ 1.3	+ 2.7	+ 4.0
					+ 0.4	+ .2	+ 1.5	+ 3.4	+ 4.9
					− 1.4	− 1.5	+ 0.0	+ 4.0	+ 4.0
					+ 0.12				

Since the greater part of the error of the screw is at the end from which the accumulated errors have been reckoned, the relative errors have been determined of the first ten revolutions of the screw, assuming the length of the first half inch to be correct. The results are given in hundred-thousandths of an inch.

Spaces.	Σ
1	+ 14
2	+ 10
3	+ 28
4	+ 6
5	+ 3
6	− 14
7	− 12
8	− 20
9	− 14
10	+ 0

The errors depending on single revolutions of the Waltham screw were found by measuring ten ruled spaces corresponding to even tenths of a revolution. The results for the Cornell screw were kindly communicated by Professor Anthony. The figures represent millionths of an inch.

Spaces.	Cornell Screw.	Waltham Screw.
	Σ	Σ
1	− 2	+ 10
2	− 2	+ 14
3	− 8	+ 10
4	− 9	− 2
5	− 8	− 17
6	− 12	− 33
7	− 14	− 34
8	− 11	− 33
9	− 2	− 21
10	+ 0	+ 0

The nature and magnitude of the errors of these screws will be more clearly seen from the curves upon the following plate, which have been described from the data given.

It will be noticed that there is a dotted curve in Fig. 55. The difference between the two curves represents the effect of a demand of my landlord for an increase of rent, in consequence of which the dividing engine was removed into new and, it may be added, more commodious quarters. After it was remounted, it was found that the old system of errors no longer held. The present discussion refers to data derived from observations made since the removal.

It will be seen that there is a pretty close correspondence between the curves for the English and the metric subdivisions. The former of course includes errors to a certain extent depending on single revolutions of the screw.

One more fact should be stated in order to complete the argument in favor of the new method, and it is a fact of supreme importance. At the time the line bar was graduated from which the Cornell screw was cut, the writer had not succeeded in completely eliminating the accumulated errors. It would be too much to expect that there should be a complete coincidence between the errors of the bar and the errors of the screw, but the coincidence is nevertheless so close that it would make but little difference whether the accumulated error of the bar at the middle point was derived from the screw, or that of the screw from the bar.

It has already been stated that the method has only been tried under great disadvantages, but the Ballou Manufacturing Company have now completed a screw machine having a direct capacity for screws 6 feet in length and an indirect capacity for screws having a length not much exceeding 18 feet. A smaller machine for screws less than 6 feet, and especially adapted for cutting all kinds of micrometer screws, is in process of construction. This paper is already by far too long to admit of a description of these machines, but Mr. Bishop, the president of the company, will gladly offer every facility to any member of the society who desires to make a personal examination either of the method or of its results. You will receive, I am sure, a hearty welcome and courteous attention at the office in Hartford.

DISCUSSION.

Mr. Oberlin Smith.—I want to ask a question of Professor Rogers. I understand from him that one of the great practical difficulties that would seem natural in all this work is the deflection by gravity of horizontal bars used for the guides, whether they be cylindrical or whether they be prismatic. My question is, whether experiments in such work have been made having the screws and slides all vertical, so that deflection would not come in as a factor at all, and having the nut and other moving parts attached to it balanced about the axis of the screw, so that as it traveled up and down there would be vertical stress only.

Prof. Rogers.—Experiments have been made in this direction, but not, as far as I am aware, in this country. Professor Wild, of the Central Physical Observatory of St. Petersburg, has investigated the problem. He found that the flexure—that is not quite the proper term in this connection—he found that the change which in some way takes place when a bar is supported vertically is nearly or quite as troublesome as when it is placed in a horizontal position. Simple flexure can always be neutralized by proper supports.

The required degree of precision must be secured under the ordinary conditions in which screws are used. When a stiff screw three feet in length is supported at the middle point the effect of flexure will be eliminated in ordinary practice.

But in the new screw machine embodying the methods which have been described, a very neat device has been adopted by which the flexure can be eliminated at any desired number of points, so that the element of length does not now enter as a disturbing cause.

Mr. Towne.—Mr. President, without doubt it may seem to some persons present that this discussion has reference to what may be termed mathematical theory rather than to ordinary machine shop practice, but a matter in my own experience shows that this is not so. In cutting some screws recently for use in "Emery" testing machines, it became a matter of importance to produce two screws of identical pitch, as nearly as the same could be reasonably done. The screws have a diameter of three and one-half inches and a pitch of two threads to the inch. A lathe for cutting such screws was made especially for us by the Pratt & Whitney Co. (who gave great care to the work throughout), having unusual solidity and stiffness, and having as a lead screw a copy of a Whitworth screw which Pratt & Whitney then had, and which had never previously been used, and of which they made as careful a copy as they could. After cutting the first pair of screws in this lathe, and chasing a pair of bronze nuts for use on them, and assembling the machine, an error in the screws was shown very apparently in this way: The two screws stand vertically in the machine, and carry a horizontal cross-head. On each screw is a pair of bronze nuts, one above and the other below this cross-head, the separation between the nuts on each screw being, approximately, nine inches. The work is all very carefully fitted, and, in the position when they were first assembled, the nuts, while in contact with the cross-head on each side, were still free, and there was no binding anywhere. On turning the screws, however, so as to raise the cross-head slightly, thereby bringing the nuts into a new longitudinal position on the screws, the nuts in one case were found to be separated—so much so that there was considerable freedom between the nut on the top of the cross-head and the cross-head itself; and in another position the distance was decreased, so that motion of the nuts was stopped. The total error, if I remember correctly, was something like six or seven thousandths of an inch—quite enough, with close work, to make binding. So that, in what may be termed ordinary machine shop practice, this matter of precision of screws comes in very frequently, and it is interesting to all of us to see how great precision is going to be obtained, not only in fine instruments, such as we see here, but I hope also in tools of more frequent use.

I wish to ask Professor Rogers one question, due, perhaps, to my not following him more closely. If I understand his process correctly, it differs from what has preceded it chiefly in the reproduction in the screw of the divisions of the plate or bar. The correction of each thread of a finished screw is not novel, as I understand, but it is the reproduction on the screw in cutting it of the divisions of the bar. Am I correct in that?

Prof. Rogers.—You are partly correct and partly not. The novelty of the process—I will not say novelty, the process itself—consists in a method of cutting single threads in such a way that we know just what is being done during every step of the operation.

If my screw has twenty threads to the inch, and my bar is subdivided to twentieths of a revolution of the screw, then for every entire thread cut I must maintain a coincidence between the line upon the bar and the fixed line of the microscope. If this coincidence is not maintained, the screw which is being cut will have an error of just that amount at that point. In the ordinary process, you really do not know whether you are cutting a single thread correctly until the entire operation is completed. We deal with single threads, and in such a way that no one thread can ever have an error greater than can be ground out by means of a lead nut. The screw exhibited was cut from a bar having half-inch graduations, but the subdivisions of the bar now in use with the new screw machine are tenths of inches. As the leading screw has five threads to the inch, the errors can be obtained for every half revolution.

Attention is called to the fact that as long as the screw which is being cut has the same temperature as the leading screw it will have the same absolute length as the graduations of the bar from which it was cut. Since in the new machine the entire shaft is immersed in oil, the trouble with temperature is reduced to a minimum.

Mr. Towne.—If I understand correctly, then, in tracing the first line of the thread, we will say, that is, the first revolution of the screw, and having made one exact revolution, you then compare the progress of your line on the work with the divisions of your bar. Am I correct?

Prof. Rogers.—Yes, sir.

Mr. Towne.—If you find an error in that thread, how is that error corrected? Perhaps you touched on that point in the paper.

Prof. Rogers.—Yes, I thought I had done so. It must be remembered that the cutter has two motions; a primary motion and a secondary motion. The primary motion is the movement of the carriage by means of the leading screw; the secondary motion is the movement of the slide to which the cutter is attached through the short and firm micrometer screw.

There is a certain amount of preliminary work, however, which must be done before the screw is cut. We must first ascertain how far the lines upon the bar are away from the fixed micrometer line of the microscope, for even revolutions when the leading screw runs free, *i. e.*, without doing any work, and secondly, we must ascertain whether the errors are substantially the same during every comparison; for unless this constancy is maintained it will be impossible to eliminate the errors of the leading screw. The tabular corrections which have been derived from observation cannot serve as a guide unless they are practically constant for every repetition of the observations.

I will repeat the various steps of the operation. The carriage is now at one end of the lathe bed, and the index of the leading screw is set at zero. Upon looking into the microscope, I notice that the first graduation upon the bar does not coincide with the fixed line of the eye-piece micrometer of the microscope. I slightly tap the bar at one end, and the coincidence between the two lines is perfect. A heavy bar can be moved into any required position by a series of light strokes with greater accuracy, and much more quickly than through the action of an abutting screw. I now allow the leading screw to make, *e. g.*, nearly five even revolutions, and by a hand movement I complete the fifth revolution. Again looking into the microscope, I observe that the line upon the bar is on one side of the micrometer line of the microscope, and I note how many divisions of the index of the secondary screw are passed over in bringing the secondary slide to which the microscope is attached into coincidence with the second line upon the bar. I record this number upon the slip of paper before me, which for convenience is pasted to the vertical face of the graduated bar. This operation is repeated for every five revolutions of the leading screw. I shall then have a system of corrections, some positive and some negative, which represent the errors of the leading screw for the conditions under which the observations were made. But these conditions are in many respects unlike those which hold while the cutting tool is doing its work. I therefore repeat the observations with the cutter at work upon a dummy shaft. In ordinary practice, however, it is not found necessary to take this precaution.

Having done this preliminary work we are now ready to cut the thread upon the cylinder which has been properly prepared in advance. The carriage is run back to the first position, and coincidence is made between the line upon the bar and the fixed line of the microscope after the cutting tool has been properly adjusted. The leading screw is thrown into feed, and during the movement from the first to the second line of the bar, the secondary micrometer screw is moved, by a continuous motion, the number of divisions of the index which corresponds with the previously determined error of the leading screw between these two points. To facilitate this movement the slip of paper upon the vertical face of the bar is divided into half-inch spaces, and a pointer is attached to the moving carriage. During the slow motion of the screw, the pointer will serve as a guide to the eye in making a uniform distribution of the total error over the entire space. Stopping the lathe at five even revolutions, I find that the coincidence between the lines is now maintained. Starting again, I apply the correction required for the second five revolutions, and so proceed till the first tracing of the thread is completed. Every subsequent operation is a repetition of the first. It is to be noted that I am describing the actual process employed in cutting the first screw upon a Blaisdell lathe, but in the new screw machine the secondary motion of the cutter is under the complete control of the workman.

Having cut the screw, what do we find? First, that there are here and there slight irregularities in pitch, due to the fact that the errors of the leading screw have not been quite properly distributed between the points at which the errors have been determined. Just here is the vital part of the operation. By the ordinary process long sweeps of errors are introduced. You may grind till doomsday without removing errors of this class, but these single errors, these minute irregularities will disappear after a few moments' grinding with a lead nut charged with fine emery. Secondly, I find that the entire length of the screw corresponds with the distance between the terminal graduations of the bar. It is true that there has been a slight forcing of single threads, but there has been no such forcing of a combination of threads as takes place in ordinary practice, when the attempt is made to change the general pitch of a screw.

Mr. Oberlin Smith.—Then, sir, as I understand, the practical process consists in a secondary screw which corrects the errors of the first screw, thread by thread, by being moved in some definite relation to it, that relation varying with each individual thread, each of which has a personality of its own, and you are in the habit of doing that by hand. Of course, the lathe must move very slowly at present, but you are going to do it automatically at some future time.

Prof. Rogers.—Yes, sir.

Mr. Oberlin Smith.—What kind of gear do you use to connect the secondary screw with the primary so as to get that variation of motion?

Prof. Rogers.—In a general way the secondary motion is obtained either by means of a worm gear which moves the carriage itself upon the screw, or by a combination of circular gears working about different centers. There was a patent by the Putnam Machine

Company which prevented our using a secondary movement of the tool itself, even if we had designed to do so. To get around the patent—which it is always necessary to do [laughter]—we really found a much better way, by simply moving the carriage upon the screw itself. There is an adjustable sleeve—but really I cannot describe the apparatus without the drawings, and perhaps not, even if I had them here [laughter]. Mr. Ballou, my colleague in this investigation, is the mechanician.

Mr. Oberlin Smith.—That is what we call castigating a certain old gentleman around the remains of a tree. It is very common with mechanical engineers, and I believe it is very good engineering [laughter].

Prof. Webb.—Mr. Chairman, I think that is one of the great advantages of our patent system, that it stimulates every one, in endeavoring to evade an existing patent, to make something better than was made before ; I know it has occurred in a great number of instances that something better and cheaper has been arrived at.

But it was another point that I wished to speak on : During the last two or three years, in looking around the machine shop of Cornell University, I have repeatedly met with a number of interesting mechanical appliances and ingenious mechanisms, and have inquired to whom they were due. I have always had as answer the name of a certain gentleman, an honored member of this society, who was formerly there. One of those things is a machine that will measure very accurately by ten-thousandths of an inch, from 12 inches down, and as that belongs to ordinary machine shop practice, I should like to hear from our president some remarks as to the applicability and importance of this new method of making screws in ordinary machine shop practice. I believe myself that it is a valuable thing for the machine shop. I have heard that certain firms have already expressed their intention, if this is a success, to turn out their old screws and put new ones into their lathes in order to have a systematic method of measuring. I cannot see that any further progress in accuracy of measurement is possible unless that is done, and I believe that the true method of measuring in a lathe is to have all the screws in that lathe graduated and to use them for the measurement of the work. I should like to hear upon this point from those acquainted with ordinary machine shop practice.

Mr. Hand.—Mr. President, if my memory serves me rightly, about six or eight years ago, you and I had something to do with a pair of Whitworth screws on a measuring machine. They were very much out of truth. While you are telling about what Prof. Webb referred to, I would like to ask you to tell about the device you put on the machine, to eliminate the errors of those screws. At that time we knew nothing about this grinding process, and when we got stuck in the making of gauges, for the want of a perfect screw to measure with, you kindly came forward and helped us out by applying the corrective device referred to.

President Sweet.—I hardly think it within the scope of this discussion to branch off into the question of measuring machines, unless it be the will of the meeting that we deviate from the title of the paper. It seems to me there is not much to say when we confine ourselves to the perfect screw. It has been made more perfect in the screw described than anything ever made before. But when we get the perfect screw we have got to use it, and it is desirable that we shall maintain its accuracy: for that is an important thing. If our screw is made perfect, and we go to use it and it soon wears out, we are pretty nearly as badly off as if we had never had it.

Professor Rogers referred to his hobby. My hobby has been to get equal length of wearing surface wherever I thought it was practicable. I applied that to the measuring machine referred to by Prof. Webb by making the screw and the nut of the same length, believing that if we ground them together in that form, we would maintain better results than if we put a short nut on a long screw. I was not present to hear that part of the paper, but I believe it was stated that when they came to grind their screw they were all right until they got pretty nearly to the end; then they met with difficulties that caused them more trouble and more time to eliminate than all the work done up to that point. To give a fair illustration, let us imagine we have an edge which in the main is perfectly straight, but throughout its entire length there are slight undulations. Let us take a short piece of like character and try to grind those slight undulations out. The result will be just as it was with the screw. It will be all right through the center, but when we come to the ends we meet with the same difficulties—that they do not disappear, or you grind the end off too much or not enough. Now, let us take two straight edges of the same length with the same undulations, but with the undulations in one of different pitch from those in the other. If these be ground together, what will be the result? We will soon fetch down our two straight edges, and they will be practically straight, as well on the ends as in the middle. That was the plan I adopted on the screw—to make the screw and the nut of the same length, and make them work back and forth. The screw and the nut were three inches long, and our measurement was only one inch. In our machine it made no difference to us whether we had sixteen threads to the inch or more or less than sixteen threads. We did not care whether it was 16 plus or 16 minus, because we had to read to a line, and that line could just as well be a spiral which would correct the error as to be a straight line parallel with the axis of the screw. We found our screw, if it continued 62 feet, would have one too many threads, so that by making the line a spiral that would make one complete turn in sixty-two feet, the error in the pitch was corrected.

The same principle was applied to the Richards machine, only in their case they have a short nut on a long screw. In that case, instead of making the line equivalent to a perfect spiral, the line had to be more or less curved, because those errors came in at the ends of the screw, when the short nut got up to the end, and the line had to be a curved line rather than a spiral.

Now, in applying this perfect screw to practical purposes, as I understand, they propose to make the lead screws of lathes.

In using a lathe for the ordinary purposes of a machine shop, you all know very well it will not be long before we have it worn down at the head-stock end, and not worn at all at the foot-stock end. We know we cannot make a screw and nut of the same length, and what shall we do? It is easy to do better than we do now, if we only had the courage. Cut away the threads at the foot-stock end—nine threads out of ten, eight out of nine, five out of six, etc., cutting out the most where the screw wears the least. This, cf course, could not be determined accurately, because we do not know how much the screw is to be used on long and how much on short work, but it would help the matter very materially. None of us have the courage to cut away the threads of a nice screw. I am having a lathe made, and putting in all the notions I have thought of—a good many more notions than improvements, possibly—and though I have a good deal of courage in mechanics, I have not enough to make a lead screw as it ought to be made [laughter].

Mr. Bond.—Mr. Chairman, I would say in regard to the correction of that screw referred to by Mr. Towne, which I had the honor of superintending, we found errors in it that varied all the way from minus .004 of an inch to plus .005. We determined to get these errors reduced as much as possible, and we ground three weeks on the screw in the attempt. It is 38 feet long and three inches in diameter, two threads to the inch, or one-half inch pitch, and a square thread. The sides being at right angles to the axis of the screw, made it very difficult to grind, but we managed it by taking two half nuts and lining them with a lead bushing. Having nuts about twelve or fourteen inches long, gave considerable length to the thread. We ran one nut back and forth the whole length of the screw to even up the thread. We then found there was very little difference in the tightness of the nut from one end of the screw to the other. Then we introduced a space between two nuts of about twelve inches, in order to separate them, and we found it went very easily in some places. We ran it along two or three days until we thought it was even, meanwhile measuring it. By introducing another length of two feet we still further separated these nuts to change the conditions, and then finally one of three feet was added, and we got it so that in all positions the nut apparently had the same resistance. We measured the screw, and found that the general errors had been reduced, as nearly as we could measure it, to about one-thousandth of an inch, either plus or minus. Of course, as the flexure of the screw came in, we measured it on all sides. In grinding this soft iron screw the sides of the thread were filled with emery. I think since the grinding there may have been a slight change in the pitch up to a certain time, but its use afterward always gave the same results. The screw that was cut by it and then measured had the same errors in it that the original screw had after grinding. We only use this long screw for screws that are nearly the length of the machine itself: We have shorter screws for doing short work, and we are in hopes of getting in time a perfect screw in this lathe. The use of this screw is only an expedient until that time comes.

Prof. Webb.—I suppose, to put it briefly, that Professor Rogers is introducing a new breed of screws. Now, we know that the agriculturists, when they have a fine animal, do not put him to hard work. They take care of him ; and yet they will have fine animals, and they pay the money for them, and I suppose that mechanical engineers, if they do not use a perfect screw where they need it, now that one is obtainable, will be worse off than the farmer is. An idea has occurred to me as to how they might be used. It is quite possible to have these screws so arranged that any part of them can be used at will. You would use your lathe for accurate work only, and would keep account of the screws you cut, making the record show as nearly as possible how much the screw was worn by each job. In this way the wear could be very evenly distributed. There are various ways of fixing it so that all the screw can be used, and used regularly.

Mr. Reese.—Professor Webb has said that this is like the introduction of a new breed of chickens. It just occurred to me that if Professor Rogers proposes to introduce a new breed of chickens, some attention ought to be paid to the rooster [laughter].

I have been surprised that in this discussion and in this paper, which is a very good paper indeed, no attention has been paid to the molecular physics of the bar, which I believe will have something to do with the generation of high-bred screws. It seems to me that it would be impossible to get a uniform series of screws accurate unless you had a uniform structure of metal to start with. I presume it is expected to make those screws out of steel. Now, we all know that it is a very difficult matter to get a uniform bar of steel—a bar in which the molecular structure is uniform at all points. It is very difficult to get a bar of steel in which the carbon is uniformly distributed throughout the bar. Variation of temperature varies the molecular action. It varies the expansion and contraction—not only in its entirety, but in any point of the bar.

Some experiments that I made some years ago that are not yet complete may be referred to. I am sorry that I have not had the means to continue them and make them complete. Still they are complete enough to make a reference to them. They go to show that the shape of the molecule in the molecular structure of steel, and, in fact, of all metals, is spherical, and that, in the act of rolling, the spheres are drawn out along the line of their long axes, and that this is not their normal position in a bar of metal; it is abnormal. The spheroids ought to have their long axes across the bar. Now, I have taken a bar that was rolled in the ordinary way, with its molecules having their long axis running parallel with the length of the bar, and found its tensile strength to be 109,486 to the square inch. This bar when annealed stood 107,486 lbs. to the square inch. The bar exhibited an elongation of 10 per cent. in 8 inches before annealing, and 16 per cent. after annealing.

Another bar of steel rolled in the ordinary manner to 1.012 diameter, exhibited a tensile strength of 109,486 lbs. to the square inch, and an elongation of 10 per cent. in 8 inches before annealing. This bar was passed through my machine, which caused the bar to rotate at high speed and under great pressure. The effect of this action was to twist the molecules so as to leave their long axes across the bar. This bar was rolled cold, its diameter was increased to 1.017, its tensile strength was reduced to 107,972, and its elongation increased to 20 per cent. in 8 inches. This bar was then annealed, but the annealing had no effect on its tensile strength, or its elongation before rupture. Annealing has no effect on a bar of steel in which the molecules are in their normal attitude. A perfect screw cannot be made from a bar of steel, in which the molecules do not exist in their normal attitude. The steel must possess a uniform physical structure, in which the molecules exist in their normal attitude, with their longer axes uniformly parallel to each other, before Professor Rogers can hope to secure the end he has in view.

Prof. Rogers.—Mr. President, if you will allow me—I do not like to take up so much of your time—but I have touched upon that problem. One of the most illustrious astronomers who ever lived, one who has made the largest contributions to our knowledge of the positions of the stars in the heavens, once said that "one is liable to fail of reaching practical results if the range of inquiry is extended too far." If we are compelled to determine the changes which take place in the molecular structure of steel before a perfect screw can be made, I fear it will be yet many years before such a screw can be made. This problem is an exceedingly interesting one, but fortunately it has but little bearing upon the ordinary behavior of steel under the changes of temperature, which ordinarily occur. But since this subject has been introduced I may say that I have during the past three years attempted to ascertain the effect of change of temperature upon the molecular structure of steel, copper and glass, by investigating the constancy of the coefficient of expansion of these metals.

I have two bars of glass having the dimensions $41 \times 1\frac{3}{4} \times 1\frac{3}{4}$ inches made for the Standards Department of the English Government by Chance & Sons in 1870. One of these bars was presented to me by Mr. Chaney, Warden of the Standards. I was allowed to bring with me from London a duplicate bar for the purpose of graduation. I have for two years studied the constancy of the coefficient of expansion of these bars. I cannot answer for the changes which may have taken place during the ten years in which they were allowed to assume a normal condition, but it is in my opinion very certain that these bars of glass have at the present time a constant co-efficient of expansion.

The observations which have now been continued for nearly three years with bars of copper, brass and steel of various grades, all point to the constancy of their co-efficients.

The argument for that change of length in metals which is a function of their age is, as far as I can learn, based upon the following observations. First, that the zero point of a thermometer always rises during the first one or two years of its life; and, second, that certain standards which have been compared at wide intervals of time give evidence of an absolute change in length. With regard to the argument from the recognized changes in thermometers, it may be said that a rise in the zero point does not by any means indicate a change in the structure of the glass tube. In this case we have simply the magnified effect of a thin shell of glass upon a comparatively large mass of mercury. With regard to the observed changes in absolute length, it is safe to say that the observations are not in a single instance conclusive. Take for illustration the various comparisons of the U. S. Standard Yard "Bronze 11" which have been made with the Imperial Yard. In 1855, "Bronze 11" was assumed to be about one ten-thousandth of an inch *too long*. In 1879, it appeared from the observations of Mr. Chaney and Professor Hilgard to be 88 millionths of an inch too short. But according to the last report of the Standard Department, the observations of Professor C. S. Peirce, made during the summer of 1883, gave the relation, "Bronze 11 + .000022 inch=Imperial Yard. If we can trust the comparisons, we have had therefore in this instance both a decrease and an increase of length during the last thirty years. I am very confident that no change at all has taken place. Probably we must refer the apparent change to the different kinds of illumination under which the defining lines were observed in the different series of observations.

With regard to the experiment upon steel to which the last speaker has referred, it must be said that the seven seconds of time mentioned ought to have been at least seven hours. Any change of temperature in the entire mass of a given bar of metal is a function of that mass and of the time of exposure to a given temperature. If a brass bar with the dimensions 41 x 1 x 1 inches and having the temperature, *e. g.*, 50° Fahr., is removed to a room in which a constant temperature of 70° is maintained, four or five hours must elapse before the bar will assume its normal condition in the latter temperature. Contrary to the general impression, the effect of this change of temperature will be very slow for the first five or ten minutes.

The effect of the presence of the observer in a comparing room upon a bar of this kind will not be perceptible for ten or fifteen minutes. I can even handle the bar with impunity during the first five minutes. But the effect of a similar change of temperature upon a bar of small section will appear at once. Hence in my practice I have everything in readiness to make the first comparison of the bar having the least mass within one or two minutes after entering the comparing room. I then compare the bars in the order of their masses, allowing the limit of fifteen minutes for bars having the dimensions just named. When the comparisons are completed I leave the room quickly, and do not enter it again for about five hours. During this time the slight increase of temperature due to the presence of the observer will have been taken up by the bar. It would seem that in the experiment which the last speaker described, the mass of the bar was not sufficiently considered, and that the length of time of exposure to a given temperature or rather the time during which compression was taking place was not noted with sufficient accuracy.

I have not yet seen any decisive evidence of a molecular change in the structure of metals under moderately slow changes of temperature. Certainly this is the result of my experiments thus far. I venture the prediction that the result of the discussion which will assuredly take place during the next ten years will be, that with the exception of a few composite metals, *e. g.* zinc, all metals assume a normal condition soon after they pass from a molten to a solid condition, and that we may safely dismiss the consideration of all changes which in any way depend upon the age of the metal. Certainly, we need not take account of this source of distufbance in the production of screws.

Mr. Woodbury.—Mr. President, at the first meeting of the Society in New York a paper was read criticising the metric system, and at that time a statement was made that there was not a single leading screw in the world graduated on the metric system, and I should like to ask if that is true to-day?

Prof. Rogers.—No, sir, it is not true. As I said before, Mr. Van Woerd constructed a screw for me on the metric system. A metric screw by Froment of Paris is now in the office of the Coast Survey at Washington.

Mr. Woodbury.—Do any French tool makers use the metric system on their lathes?

Prof. Rogers.—Always.

Mr. Kent.—Mr. Reese just told us that he has branched into the domain of molecular physics. I think he has made a slight mistake in the name. It is molecular metaphysics. Physics can carry us only about as far as Professor Rogers carries us when he says, "A piece of mechanism of the class which the French would call mechanism of precision may be termed perfect when it meets all the requirements of the purpose for which it was constructed." That is as far as physics, I think, to-day can carry mechanical engineers; but philosophers and scientists may be carried further when they trench on the domain of molecular metaphysics, with which, I think, this Society has nothing to do.

Mr. Towne.—Mr. President, I wish to add just one word relating to the Whitworth screw made by Pratt & Whitney. There is no doubt but what extreme care was taken in the production of that screw, as has been said by Mr. Bond, and I believe we have in that screw as perfect a large screw as there is in this country. I appreciate the force of the suggestion which the president has made that later work done with that screw will probably be better than its first work.

Mr. Bond made reference passingly to another feature in this lathe which may be mentioned a little more fully, and I may be pardoned for doing it, as it was, I believe, at my suggestion that it was incorporated. That is, providing for a *second lead screw* in lathes of this kind, for doing short work. In our lathe the main screw is 28 feet long, I think, and is on the front of the bed. Behind the bed is a shorter screw, about 10 feet in length, cut in the same way, and corrected, I believe, in the same manner, the purpose of which is to cut nuts of short screws, and in that way obviate unequal wear on the main lead screw, and keep that for cutting long and fine screws, using the other screw for less important work and for short nuts. I think in any lathe to be used in ordinary machine shop work, particularly for cutting long threads, it is exceedingly desirable to have that secondary screw for cutting the nuts which are always required for such work, and for doing other short and unimportant work.

Mr. Bond.—Another lathe which might be referred to is made specially for cutting leading screws for lathes. It has two lead screws, and it is intended to use one to cut a certain length of lathe screw, and the other for longer lengths, and in that way there is a tendency to make the wear uniform the whole length of both screws. It will take time to do this for all lengths of lathe screws, but that is what will be required.

Mr. S. W. Robinson.—Mr. President, I have two questions that I would like to have settled in my own mind. Possibly one of them has been settled already in the reading or in the discussion. If so, I did not notice it particularly. One is as to errors occurring in a single turn of the screw—a single thread—a single revolution. Professor Rogers speaks of using a microscope and micrometer screw to correct the position of the cutting tool, as I understand, for errors in the lead screw, as the work goes along. I heard reference once to divisions of a revolution into parts of a revolution—20ths, and so on. I did not quite catch whether a graduated standard bar was actually used or contemplated in making these screws, the graduation of which was fine enough, say, to divide the pitch of the screw being cut into 20 parts, and then stop at each graduation mark, or not to stop, either, but watch for the 20ths of the revolution and adjust the position of the tool at each mark. There might be a signal, for instance, giving notice of the approach of each 20th of a revolution. Every screw revolves so slowly in the cutting that there is time for a signal to be made and the observer to watch his opportunity and adjust the thread in each 20th of a revolution, if you please, or in any other number of divisions. It is evident that it can be carried to that extent, and I am not certain whether it was intended to be explained as if carried to that extent.

Now, this subject touches, perhaps, a point of interest in regard to the permanence of metal; that is, with respect to the bar, the permanence of the bar itself. If we could easily obtain a bar which could be divided into 20ths of a thread for its whole length and be perfect, it strikes me that this method of cutting a screw would result in cutting a perfect screw, that is, perfect within our present means of measuring, or present notion of precision in such matters, to say the least.

As to the permanence of metals, that question, if it is touched upon, seems to be an important one here, not only to the standard bar, but to the screw which is to serve as a standard. A screw that has been made with great care, of course, should be permanent as to its constitution and dimensions. Dimensions are the leading point. If there is a change in the physical constitution in any way, such as in the relation of the molecules, there would be a change in the dimensions of the screw. I was once struck with a remark in a letter I received from a young man employed at the Elgin Watch Works. He said that there, in making mandrels that they desired to use in work of great precision, they were in the habit of getting out the blocks of steel nearly to size, tempering them, if they were to be tempered, then laying them away a year or two to season. Now, it seems to me that if there is anything in that, if it is necessary to season steel, it is quite necessary that these standard steel screws should be "seasoned," and also the standard graduated bars to be used in guiding the screw cutting, whether it be carried to the extent of divisions of the revolution or not. I would like to ascertain what has been done in regard to those two points.

Prof. Rogers.—The 6 foot screw in the new machine which has been referred to was cut in the way which Professor Robinson has described. It was not quite feasible to employ the principle of reversing the half nuts in a screw having this length. Since the provisional leading screw was cut five threads to the inch and the bar was graduated to tenths of inches, it is obvious that the periodic errors depending on half revolutions of the leading screw were eliminated, but this operation is rather tedious and requires a great deal of care. Hence, in the new machine, there is an automatic device for correcting the periodic errors which depend upon single revolutions of the screw. The larger share of the errors of this class are eliminated by means of a template. For the elimination of those which remain, dependence must be placed upon the principle of the reversal of a nut divided into sections. Professor Rowland has not yet published a description of his process of correcting a screw, but it is understood that he divides the nut into four sections. A short nut of this kind has recently been made for me by Mr. Ballou, and its action is very satisfactory. I suppose that Professor Rowland has anticipated me in the use of a nut having four sections. I may or may not have anticipated him in the use of one having two sections. When these sectional nuts are placed in position upon the screw, the faces of the threads of the screw and of the nut are everywhere nearly parallel. They may even touch at every point, but if there are periodic errors in the screw, the relation between the threads of the nut and of the screw will vary in every part of the revolution. By repeated reversals of the sectional nuts, therefore, the tendency will be to equalize the errors at every point.

Mr. Kent.—There has been a suggestion made about moving the leading screw along the lathe from end to end, and letting it project beyond the ends three or four feet, in order to have the nut move at all parts of the leading screw. I have a suggestion to make as to how that might be accomplished without moving the leading screw. Let the leading screw revolve in its own place all the time, and have a secondary bar or shaft above it, which may project beyond the end of the lathe, to which the tool post is attached. The nut of the leading screw is to be clamped to this shaft in any part of its length, and the position of the nut on the screw changed from time to time so as to wear the screw uniformly. The shaft slides between bearings so that it shall always pull in a straight line (illustrated on the blackboard).

Mr. Porter.—I would like to make one inquiry of Professor Rogers, and that is whether any attempt has ever been made to remove the periodic errors by a system of scraping instead of grinding. The reason I ask that question is this: It is obvious that when a perfect screw has been produced it is desirable that it be maintained as long as possible, that it shall have as long a lifetime and be capable of as much service in producing other perfect screws as possible. Now, any grinding material whatever finding its way into the metal makes it a permanent lap. If that can be avoided [illegible], it seems to me that a longer lifetime would be insured to the screw, and it has seemed to me, without having gone into the subject in my mind very carefully, that a system might be devised by which high points might be detected upon the face of the screw in the same manner as they are detected upon a plane surface and removed by scraping, which can be done in a manner exceedingly delicate. My inquiry is whether any attention has been given to that—whether any effort has been made in that direction.

Prof. Rogers.—Yes, the experiment was tried by Mr. Van Woerd, but without success. I do not think the plan would be possible. We do not deal with plane surfaces in cutting a screw. We deal with helices.

The objection to the use of emery in grinding is not a serious one in practice, especially if the steel is hard. We shall yet have hardened steel screws. Tempered steel behaves quite as well as annealed steel under variations of temperature. I regard the coefficient of my hardened steel standard as more securely determined than for any other metal except perhaps for copper.

Mr. Bond.—It may be hardened outside, and not in the center.

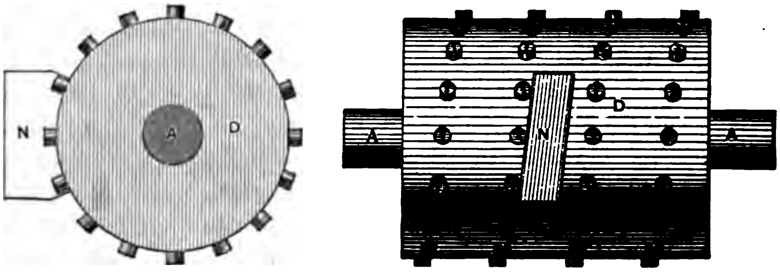
That is possibly the effect of a change of the co-efficient of expansion, because the main body of the bar is in a normal condition, and probably not very hard. We know that in standard cylindrical gauges that are made of tool steel, there are changes that come about that seriously affect the diameter of the ring which fits the gauge, but it is only in steel that is of very high carbon.

Prof. Rogers.—I shall still be obliged to say that I do not believe that the action which Mr. Bond describes is the result of a real change. All of my observations point to an absolute constancy in length soon after the steel is tempered. It cannot be possible that the effect of a change of two or three degrees in temperature at the surface of a steel plug two inches in diameter, will be to change the absolute diameter within a few seconds of time. Is it not more reasonable to suppose that the apparent increase of diameter may be due to the development of surface heat through surface friction in inserting the plug into the ring?

The principle of conservation of forces applies here as elsewhere. A change in the entire mass requires a certain amount of time, but there is some evidence that strictly surface changes may occur, and it is possible that the observations made by Mr. Bond may be explained in this way. The experience of Alvan Clark & Sons in applying the principle of local corrections in finishing an object-glass gives some color to this theory of local surface changes. Even in an object-glass of the largest diameter, two or three polishing strokes with the finger will change the curvature *at that point only*, by an amount which can be instantly detected by the eye in the optical tests employed. Are we to say that the slight change in temperature produced in the operation of polishing has affected the entire mass of glass? We might more reasonably go to the other extreme and say that in the operation of polishing the *direction* of the reflecting particles of glass which compose the surface is changed, and that the actual change in position is therefore enormously magnified by the change in the direction of the rays of light which reach the eye. It must, however, be clearly understood that this notion of surface changes, which are distinct from changes which occur in the whole mass, is only a tentative explanation of a very difficult problem. These slight changes are strictly local, and cannot in any sense be considered as affecting what we call the permanent "set" of metals. The observations of Mr. E. S. Wheeler show that in the case of zinc there may be a change of "set," but not in glass or steel. But in these experiments the change of temperature was very violent. The bar of zinc after having been packed in melting ice was quickly removed and plunged into boiling water. After remaining in boiling water for two or three hours the bar was re-packed in ice. Under the changes of temperature which occur in ordinary experience, the zinc bar should assume its normal length in melting ice within 15 or 20 minutes, but Mr. Wheeler found that after the bar had remained in the ice bath for 24 hours, it was still much too long, and it was only after an immersion of three days that it assumed its normal length. These experiments were very carefully made, and probably a repetition of them would give nearly the same results which Mr. Wheeler found. A small part of the amount of change of "set" may possibly be due to the fact that the bars were not wholly immersed in the bath. It is my experience that the packing of ice should cover the bar to the depth of three or four inches, but the error which may arise from shallow packing cannot much exceed five or six mikrons, or about $\frac{1}{5000}$ inch.

As far as I can learn, the positive evidence of "set" in any published record, is limited to the paper by Mr. Wheeler, and here the evidence is limited to a composite metal, viz. zinc.

Mr. Oberlin Smith.—Mr. President, I want to suggest a possible method that has just occurred to me—perhaps, though, it will all be knocked into a cocked hat by the experience of some of the gentlemen—of making a prime leading screw from which to cut other screws. Suppose we had a large cast-iron drum, D, Fig. 65,—so large that the flexure would not amount to anything. It could be made hollow for strength and to get less dead weight. Suppose it is a foot in diameter, and four or five feet long. Suppose that into that, in a spiral line as nearly as could be drawn, there were set a



Proposed Master-drum for making "Perfect-Screw".
Not drawn to Scale.

FIG. 65.

great number of round pegs, forming the thread of the master screw, the nut consisting of a piece of metal, N, resting against their sides. You could obtain any degree of accuracy you wished, by making the diameter of the drum large and by making a great number of pegs. The nut running against quite a number of them would probably give an accurate enough surface contact. Any individual errors occurring would extend through a very small angular part of the whole revolution of the drum. Suppose each of these pegs was adjusted in the direction of the axis, which could be very easily done by making the pin part which runs into the drum slightly eccentric with the part which projects out. By revolving the peg, and sighting one side of it with the microscope of the comparator, by a very easy process, without any grinding or cutting, the whole thing could be adjusted and a true spiral could be obtained, irrespective too of the pegs having an exact uniform diameter. One side of all these pegs would be a "*perfect*" screw. Now, it seems to me that that could be revolved, and another perfect screw cut from it.

Prof. Rogers.—I should say that the method proposed is new, but I apprehend that in a practical application of the method the experience of Hoe would be repeated. In an attempt to produce an original graduation of a circle, Mr. Hoe fitted 360 pieces to the periphery of a wheel, with the expectation that by making the parts so nearly alike that they could be interchanged at will, he could obtain an exact division into 360 equal parts. I have not had the pleasure of seeing the apparatus, but I should not expect success with it, on account of the large number of surfaces with which one would be compelled to deal, viz., with 720.

Mr. Oberlin Smith.—You need have no extreme accuracy in making such a screw, only in adjusting it, and we machinists do not pretend to adjust those pegs; we turn that over to you. You have a microscope and a comparator, you can turn it round and look at every peg, and adjust it to the one-millionth of an inch.

Prof. Rogers.—Without doubt you might get a pretty good screw in that way, since there would be no repetition of errors. Unless one can control the errors of a leading screw, a complete independence of a leading screw is to be desired. The members of the Society will recall the account in one of the journals of the method by which a screw was cut by the natives of India, without the help of a leading screw. It has been said that this screw is quite the equal of the best lathe screws of the present day.

Mr. Durfee.—I think the suggestion of Mr. Smith was carried out some years ago by a mechanician by the name of Andrew Ross, of London, for the purpose of making a dividing engine for dividing instruments for measuring angles. Instead of using an ordinary worm, gearing into a large worm wheel, he had what was equivalent to a worm constructed something in this way: We will suppose a cylinder, on a horizontal axis, and around that cylinder one and a small fraction of a turn of a very deep threaded screw.

Through this thread was a series of pins having hardened cast steel hemispherical ends; these were adjustable in a direction parallel with the axis of a cylinder, and each one could be adjusted independent of the others. As the cylinder was turned each of the hemispherical ended pins came in contact successively with the hardened flat cast steel ends of a series of adjustable pins projecting through short arms securely fastened to the rim of the great wheel upon which the instrument to be divided was placed. These last named pins were each placed parallel to a tangent to the great wheel. A full description of the invention of Mr. Ross will be found, accompanied with illustrative engravings, in the 48th volume of the Transactions of the Society of Arts.

Mr. Oberlin Smith.—I would recommend this over that for its simplicity, as it is a common thing that any machinist could make in "three or four days!' [laughter]. How much *longer* he would be at it I do not know, but I imagine that in Mr. Bond's workshop it would not be an extremely long job. The pegs being in duplicate, he could make them cheaply, and they need not be sized very accurately, as they could fit tight in the holes by being slightly tapered.

Mr. Porter.—I fancy, Mr. President, that each one of those pegs would need to be supplied with a worm wheel [laughter].

Mr. Oberlin Smith.—The eccentricity in those pegs need be so very slight—(I don't know how much, but probably a thousandth of an inch, or somewhere there, for the holes could be drilled within that limit easily enough)—the eccentricity would be so slight, and they would move so little, that they could be revolved with a long lever [laughter], and would answer the same purpose as Mr. Porter's worm gear exactly.

Prof. Webb.—Mr. Chairman, I should put a ball joint on the pins, so as to make them stand out [laughter].

Mr. Chas. E. Emery.—I must say that it seems to me impossible to consider the proposition of spirally arranged eccentric pins seriously. Mr. Porter's satirical suggestion is apropos. It was suggested that the pin device could be made in an hour or two, but there are few shops in the country which could do it accurately in a day or two or a month or two, if at all, as it involves that the hole for each pin be exactly radial and at right angles to the cylinder, that the pin fit the hole precisely, and that the face of the eccentric on the pin be exactly parallel. Otherwise the incline forming the nut will touch different pins at different distances from the centre of the cylinder. The difficulty is to work within one two-hundred-thousandth of an inch mentioned by Professor Rogers, when those of us who have tried know how difficult it is to find mechanics that can work within one-thousandth of an inch. The proposition as made is impracticable, but it is not improbable that the sides of fixed teeth spirally arranged could be ground separately to exact distances determined by a comparator. The teeth could be made either by driving in pins or by cutting a rough thread on a long pinion. An accurate series of faces being once determined, the interpolations would be made by making an accurate incline to form a nut.

The suggestion of Mr. Porter that the surfaces could be scraped involves the unusual difficulty of operating upon a warped surface. A grinding wheel mounted so that it would always approach the thread at a fixed angle could be manipulated to touch any point, and gradually work it back to proper position by a comparator.

Mr. Brashear (*by invitation*).—In reference to the screw made by Professor Rowland, which Professor Rogers has referred to several times, I would say that I had the privilege of examining it while on a recent visit to the Johns Hopkins University, and I may say that Professor Rogers is correct in regard to the division of the correcting or grinding nut into four longitudinal parts, and I would also say that the correcting nut is the same length as the screw, *i. e.*, the same as used by your president when making the screw at Cornell. The grinding was done under water, emery being used as the grinding material, and the water was kept at the same temperature throughout the grinding process. Professor Rowland has published a preliminary paper on the work done by this magnificent screw, whose error, if I remember rightly, is not greater than one-half the mean length of a wave of light. He has also promised a paper on the method of its construction, which, added to that of Professor Rogers, will certainly greatly enhance our knowledge of this difficult, yet important problem. Referring to the question of abrading metal surfaces by grinding, I think we have much to learn. We know that grinding with emery has been abandoned in nearly all mechanical work, and scraping has taken its place, but this plan would evidently be inadmissible in the correcting of errors in a screw. My own experience in using emery is, that no matter how carefully the work is done on ordinary steel or iron surfaces, the particles will imbed themselves in the surface, and many of them will remain there, the microscope always revealing them. I have made a goodly number of experiments in this line, and am convinced that we can find a grinding material for metal surfaces, which will possess all the good qualities of an abrasive powder without the *dangerous* qualities of emery, corundum, etc., which, becoming impacted in the wearing surfaces, must go on grinding as long as they are in use. In my experiments I have found elutriated or washed "Arkansas Oil Stone" powder to possess the same excellent qualities as emery without its dangerous qualities, save in a very limited degree. Superior to this is a material known as "Water of Ayr Stone" or "Scotch hone," which will cut glass very readily. To prepare these properly, they are crushed as fine as possible, then put into a glass jar with water enough to stand four or five inches above the powder. An ounce or two of mucilage is stirred in the water to assist in holding the finer particles in suspension. The powder and water are then thoroughly incorporated, the lighter matter that rises to the top skimmed off, and after the top water has stood undisturbed, say, for 30 minutes, it is drawn off with a syphon to within a half inch of the settled powder. The very fine particles still in suspension in the water that has been drawn off are allowed to settle, which for the finest powder will take a day or more. The clear water is now poured back, and again stirred, and the water drawn off after standing fifteen minutes. The operation is repeated until powder may be obtained of only half a minute suspension. These powders may now be put in cases where they can be kept clean, and are labelled for use. In this way very fine and excellent working abrasive powders can be made, which I have reason to believe will be especially suited to the work of grinding accurate screws and kindred parts of fine machinery. Professor Rogers succeeds best with some sort of slate. In England, France and Germany many different sorts of slate have been used, especially in the working of surfaces on speculum metal. I think this subject one of much importance and a fruitful field of research, and I trust that our American mechanics will fully investigate the action of abrasive materials upon metal surfaces, so as to arrive at the very best results. In reference to the question spoken of by my friend Mr. Reese, of the contraction and expansion of metals and other substances, I confess I have had to change materially my ideas of late. With Professor Rogers, I am convinced that the element of *time* plays an important part, and that rapid changes, such as can be readily seen by optical means, are, as a general thing, not changes of the whole mass by distribution of heat or cold throughout the whole substance, but simply changes of surface. Dr. Hastings has devised a beautiful test for optical surfaces by which the almost inconceivably small error of .00000002 of an inch can be detected. This test is called the color test, and is applicable particularly in testing glass surfaces. No human hand can possibly hope ever to work to such accuracy. Let two plates be placed together that we will say are optically flat. We see by angular vision a uniform tint distributed over the surfaces in contact or broad parallel bands. Let the finger or anything slightly different in temperature touch any of the surfaces, and "quick as a flash" the system of bands are disturbed, *i. e.*, in so inconceivably short time that it seems utterly impossible that the disturbance should permeate the mass, but, contracting or expanding the surface, as the case may be, the material is pulled out of shape by this contracting or expanding process. Take the case of a glass speculum, say 2 inches thick. By Foucault's method, errors of .00001 of an inch can be seen. Lay the finger on it in any spot for a moment, and a swelling is instantly seen that affects only the part touched, while the surrounding parts are disturbed very little indeed; gradually the swelling subsides. If, however, the speculum be polished immediately after this disturbance, and is afterwards examined when the speculum comes to its normal condition, a depression will be seen closely correlative to the elevation first formed. Can it then be other than a surface disturbance? These changes call up another question, *i. e.*, the idiosyncrasies of the molecular disturbances in the metal and glass surfaces we have to deal with. These are of such an unstable nature, owing to the difficulty of obtaining *homogeneous* materials, that they will perhaps be the one factor that will forever bar the way to the most perfect results. Thanking you, Mr. President and members of this association, for so kindly listening to my remarks, I will not detain you longer.

CXLVII.

RULES FOR CONDUCTING BOILER TESTS.

BY WILLIAM KENT, M. E., NEW YORK.

My object in bringing this paper before the members of the American Society of Mechanical Engineers, is not to add to their store of knowledge of a subject on which most of them are already well informed, but to open a discussion which may eventually lead to the adoption of a set of rules for conducting boiler tests which may be generally accepted among engineers as a standard code of practice.

The necessity for such standard rules is becoming apparent, as greater attention is being paid to economy of fuel, and as there is greater competition among builders of steam boilers and engines to supply the demand for such economy. Hitherto there have been no such rules, and every engineer who makes a boiler test makes a rule for himself, which may be varied from time to time to suit the convenience or interests of the party for whom the test is made.

As a result of the confusion of methods of making tests there is a great lack of concordance of results in tests of the same boilers when made by different engineers. Reports of tests are frequently made, and sometimes published, in which the evaporation of water per pound of fuel is greater than is theoretically possible in a perfect boiler. Communications often appear in the engineering and industrial weekly press which show that there exists a serious doubt in many minds of the accuracy of boiler tests which are made, even by eminent engineers.

The advisability of the adoption of a standard method of boiler testing has been felt abroad as well as in this country. Two societies in Germany, the Union of German Engineers and the Central Union of Associations for the Care of Steam Boilers, recently appointed a joint committee, which drew up a code for the testing of steam boilers and engines, an abstract of which is published in the "American Engineer," of August 24th and 31st, 1883. The German Code is scarcely such an one as is likely to find favor in this country, but it is desirable that some code be adopted here which would find general acceptance. The rules appended hereto are offered as a proposition for such a code, and the intelligent criticism of our members is requested, to the end that the rules may be amended and put into such form that they will be likely to be adopted in practice.

It is especially desirable that some standard method of starting and stopping the test should be adopted. I believe that the method preferred by the writer, and therefore included in the proposed set of rules, will be more generally criticised than any other of the rules, and, therefore, the reasons will here be given for the preference, mentioning some of the arguments for and against both this and the alternate methods.

METHOD OF STARTING AND STOPPING TESTS.

The conditions of the boiler and furnace should be in all respects the same at the end as at the beginning of a test. The steam pressure and the water level should be the same. The fire upon the grates should be the same in quantity and condition, and the walls, flues, etc., should be of the same temperature.

It is difficult to secure uniformity in all of these conditions. Several methods to secure a near approximation to uniformity may be practised, of which four methods will here be considered.

1st. Start the test when the boiler is in full working order with fires level on the grates and in ordinary working condition, and stop the test when fires are at the same height and in same condition as at the beginning.

The most serious objection to this method is that it introduces an element of guess-work at the beginning and at the end of the test, both as to depth of fuel on the grate and its condition. It is difficult to estimate within an inch or two the depth of fuel on the grate with a dull fire and hard coal, but still more difficult when soft or flaming coal is used, and even if possible to estimate closely the quantity of fuel, it is not possible to judge correctly of its condition as to the amount of ash it contains or as to the amount of available heating power remaining in it. For this reason I think this method of starting and stopping a test should be rejected.

2d. When the fires have burned rather low, as for cleaning, remove rapidly all the fire from the grate, and clean the ash pit, and as quickly as possible start a fresh fire with weighed wood and coal. noting the time of starting the test at the instant the fire is lighted. At the end, burn the fires low as at the beginning, and remove the whole fire, cleaning the grates, and noting the end of the test at the time when the grates are cleaned, taking note of the water level and steam pressure, which should be, as nearly as possible, the same as at the beginning.

In this method an error is introduced both at the beginning and at the end. While there is no error in the estimation of the quantity of fuel used, a serious portion is wasted both at the beginning and at the end of the test by radiation, and by the passing of cold currents of air through the boiler. During the operation of drawing the fire, the walls of the furnace become cooled, and during the first half hour at least of the test, while the fire is being lighted, the fires are not burning as under ordinary working conditions, and probably not with usual economy of the fuel. At the end of the test, before opening the doors to clean, the walls of the furnace are very much hotter than they were in the beginning of the test, and a large portion of the excess of heat is lost, not being absorbed by the boiler before the ending of the test, but radiated during the cleaning, absorbed by the cold air which rushes in at the time of cleaning, or remaining in the walls after the end of the test. The heat remaining in the hot fuel withdrawn is also lost. The errors in this method are all against the boiler.

3d. The fires are burned low, as in the second method, but the time of starting the test is taken to be the instant just before opening the doors to clean grates. The water level and steam pressure are noted, and all the water fed from this time to the end of the test is credited to the boiler. The fires are then rapidly cleaned and a fresh fire started with weighed wood and coal, which is charged to the boiler. At the end of the test the fires are burned low as at the beginning, and the end of the test is taken to be the time at which the doors are opened to remove the coal from the grates, the water level and the steam pressure being noted at the same time.

In this method the error is that due to the cooling of the walls of the furnace by radiation and by cold currents of air during the cleaning of the grates at the beginning of the test, and also that due to imperfect combustion during the time of lighting the fresh fire; the error being always against the boiler.

In both the second and third methods the fire removed from the grates contains a large portion of unburned coal. This is sometimes picked out and its weight deducted from that of the coal fired, but such picking can never be accurately done, and the result always shows a higher than the true percentage of ash. If the boiler test is made for the purpose of determining the quality of the coal, as well as the efficiency of the boiler with such coal, the second and third methods are thus unfavorable to the coal, since there is more unburned coal removed from the grates than there would be in ordinary working conditions.

In a test in which the capacity of the boiler is an essential feature to be determined, the second and third methods also give unfavorable results, since of the total time of the test, for at least half an hour,—while the fresh fire is being lighted, and again when the fires are being burned down at the close,—the boiler will not give its usual capacity.

4th. At the regular time for slicing and cleaning fires have them burnt rather low, as is usual before cleaning, and then thoroughly cleaned, note the amount of coal left on the grate as nearly as it can be estimated, note the pressure of steam and the height of the water level (which should be at the medium height to be carried throughout the test) at the same time, and note this time as the time of starting the test, and fresh coal which has been weighed should now be fired. The ash pits should be thoroughly cleaned at once after starting. Before the end of the test the fires should be burned low just as before the start, and the fires cleaned in such a manner as to leave the same amount of fire, and in the same condition, on the grates as at the start. The water level and steam pressure should be brought to the same point as at the start, and the time of the ending of the test should be noted just before fresh coal is fired.

The principal error in this method is that of estimation of the quantity and condition of the fire upon the grates. The condition of the fire is made as nearly uniform as possible by burning down and cleaning, and the error in estimation of quantity is lessened by the fact that the quantity on the grate after cleaning is less than at any other time.

On account of the various errors and inconveniences necessarily attending the first, second and third methods of making a test, the writer is inclined to favor the fourth method. Recognizing the existence of an error of uncertain quantity in the estimation of the quantity and condition of the fire upon the grate at the beginning and end of the test, it will always be less than the unavoidable error against the boiler due to the cleaning of the grates and lighting of fresh fires, as in the second and third methods, and less than the error in estimating the thickness and condition of fires as in the first method.

Where extreme accuracy is desirable, as in a competitive test between rival boiler-makers, the fourth method will be still preferred, but then a test should be made not less than twenty-four hours long, the working to be continuous during the whole time. The longer the test the less the percentage of error.

With these preliminary observations, the proposed code of rules will now be given, which are respectfully submitted to the Society for discussion.

RULES.

PRELIMINARIES TO A TEST.

I. *Establish the good condition of the boiler.*—Have heating surface clean inside and out, grate bars and sides of furnace free from clinkers, dust and ashes removed from back connections, leaks in masonry stopped, and all obstructions to draught removed. See that the damper will open to full extent, and that it may be closed when desired. Test for leaks in masonry by firing a little smoky fuel and immediately closing damper. The smoke will then escape through the leaks.

II. *See that the blow-off valve is perfectly tight,* and that there are no leaks of water from the boiler. During the test the blow-off pipe should remain exposed, and any water which escapes from it should be measured, or preferably it should be closed by a cap.

III. *See that there is no other feed pipe connected* with the boiler than the one which delivers the measured water, also that all connections with other boilers, either in water or steam spaces, are stopped with blind flanges instead of valves. If an injector is used it must receive steam directly from the boiler being tested, and not from a steam-pipe, or from any other boiler.

All connections to or from the boiler should be broken except those in use during the test. Thus if both pump and injector are attached to the boiler, the one or the other should be disconnected.

IV. *See that the steam-pipe* is so arranged that water of condensation cannot run back into the boiler. If the steam-pipe has such an inclination that the water of condensation from any portion of the steam-pipe system may run back into the boiler, it must be trapped so as to prevent this water getting into the boiler without being measured.

V. *Have an understanding with the parties* in whose interest the test is to be made as to the character of the coal to be used.

The coal must be dry, or if wet, a sample must be dried carefully and a determination of the amount of moisture in the coal made, and the calculation of the results of the test corrected accordingly.

Wherever possible the test should be made with standard coal of a known quality. For that portion of the country east of the Allegheny Mountains, anthracite egg coal, or Cumberland semi-bituminous coal should be taken as the standard for making tests. West of the Allegheny mountains and east of the Missouri River, Pittsburgh lump coal should be used.*

VI. *In all important tests* a sample of coal should be selected for chemical analysis.

VII. *Establish the correctness of all apparatus* used in the test for weighing and measuring. These are:

(1) Scales for weighing coal, ashes and water.
(2) Tanks, or water meters for measuring water.
(3) Thermometers and pyrometers for taking temperatures of air, steam, feed water, waste gases, etc.
(4) Pressure gauges, draught gauges, etc.

VIII. *Measure and record the dimensions,* position, etc., of grate and heating surfaces, flues, chimneys, etc.

IX. *Before beginning a test,* the boiler and chimney should be thoroughly heated to their usual working temperature. If the boiler is new, it should be in continuous use at least a week before testing, so as to dry the mortar thoroughly and heat the walls.

STARTING AND STOPPING A TEST.

A test should last at least ten hours of continuous running, and twenty-four hours whenever practicable. The conditions of the boiler and furnace in all respects should be, as nearly as possible, the same at the end as at the beginning of the test. The steam pressure should be the same, the water level the same, the fire upon the grates should be the same in quantity and condition, and the walls, flues, etc., should be of the same temperature. To secure as near an approximation to exact uniformity as possible in conditions of the fire and in temperatures of the walls and flues, the following method of starting and stopping a test should be adopted:

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* These coals are selected because they are about the only coals which contain the essentials of excellence of quality, adaptability to various kinds of furnaces, grates, boilers, and methods of firing, and wide distribution and general accessibility in the markets.

At the regular time for slicing and cleaning fires have them burnt rather low, as is usual before cleaning, and then thoroughly cleaned; note the amount of coal left on the grate as nearly as it can be estimated; note the pressure of steam and the height of the water level (which should be at the medium height to be carried throughout the test), at the same time; and note this time as the time of starting the test; and fresh coal which has been weighed should now be fired. The ash pits should be thoroughly cleaned at once after starting. Before the end of the test the fires should be burned low, just as before the start, and the fires cleaned in such a manner as to leave the same amount of fire, and in the same condition, on the grates as at the start. The water level and steam pressure should be brought to the same point as at the start, and the time of the ending of the test should be noted just before fresh coal is fired.

DURING THE TEST.

1. *Keep the conditions uniform.*—The boiler should be run continuously, without stopping for meal times or for rise of pressure of steam due to increased demand for steam. The draught being adjusted by means of the damper to the rate of coal combustion desired before the test is begun, it should not be changed during the test.

If the boiler is not connected to the same steam-pipe with other boilers, an extra outlet for steam should be provided, in case the pressure should rise to that at which the safety valve is set; and in case of such rise of pressure it should be reduced to the desired point by opening the extra outlet, without checking the fires.

If the boiler is connected to a main steam-pipe with other boilers, the safety valve on the boiler being tested should be set a few pounds higher than those of the other boilers, so that in case of a rise in pressure the other boilers may blow off, and the pressure be reduced by closing their dampers, allowing the damper of the boiler being tested to remain open, and firing as usual.

All the conditions should be kept as nearly uniform as possible, such as force of draft, pressure of steam, and height of water. The time of cleaning the fires will depend upon the character of the fuel, the rapidity of combustion, and the kind of grates. When very good coal is used, and the combustion not too rapid, a ten-hour test may be run without any cleaning of the grates, other than just before the beginning and just before the end of the test. But in case the grates have to be cleaned during the test, the intervals between one cleaning and another should be uniform.

2. *Keeping the records.*—The coal should be weighed and delivered to the firemen in equal portions, each sufficient for about one hour's run, and a fresh portion should not be delivered until the previous one has all been fired. The time required to consume each portion should be noted, the time being recorded at the instant of firing the first of each new portion. At the same time the amount of water fed into the boiler should be accurately noted and recorded, including the height of the water in the boiler, and the average temperature of feed and pressure of steam during the time. By thus recording the amount of water evaporated by successive portions of coal, the record of the test may be divided into several divisions, if desired, at the end of the test, to discover the degrees of uniformity of combustion, evaporation, and economy, at different stages of the test.

When the pressure of steam and temperature of feed are nearly constant, half-hourly observations of each will be sufficient; but when there is considerable variation, observations should be made more frequently, and the figures recorded should be the averages for each interval of time rather than the figures which are observed at the end of the interval.

3. *Priming tests.*—In all tests in which accuracy of results is important, calorimeter tests should be made of the percentage of moisture in the steam, or of the degreee of superheating. At least ten such tests should be made during the trial of the boiler, and the final records of the boiler test corrected according to the average results of the calorimeter tests.

On account of the difficulty of securing accuracy in these tests, the greatest care should be taken in the measurements of weights and temperatures. The thermometers should be accurate to within a tenth of a degree, the scales on which the condensed steam is weighed to within one-hundredth of a pound.

REPORTING THE TEST.

The final results should be recorded upon a properly prepared blank, and should contain the following items:

1. Heating surface		sq. ft.
2. Grate surface(ft. in. long x ft. in. wide).		sq. ft.
3. Ratio of heating to grate surface		
4. Kind of fuel used		
5. Duration of test		
6. Average steam pressure		lbs.
7. Average temperature of feed		deg.
8. Pounds of coal burned		lbs.
9. Pounds of refuse		lbs.
10. Pounds of combustible		lbs.
11. Per cent. of refuse		per cent.
12. Coal burned per sq. ft. grate per hour		lbs.
13. Total water evaporated		lbs.
14. Water evaporated per hour		lbs.
15. Water evaporated per sq. ft. heating surface per hour		lbs.
16. Water evaporated per lb. coal—actual conditions		lbs.
17. Water evaporated per lb. combustible—actual conditions		lbs.
18. Water evaporated per lb. coal—from and at 212°		lbs.
19. Water evaporated per lb. combustible from and at 212°		lbs.
20. Quality of steam. (Moisture or superheating).		
21. Rated horse-power. (Builder's rating)		H. P.
[*]22. Horse-power developed at 30 lbs. of water evaporated per hour from and at 212°		H. P.
23. Per cent. above (or below) rated capacity		per cent.
24. Temperature of boiler room		deg.
25. Temperature of flue gases		deg.
26. Force of draught in inches of water		inches.

DISCUSSION.

Mr. Le Van.—Mr. President, Mr. Kent prefers 212 degrees. The objection to that is that we have a great many condensing engines now, and the condensing water does not exceed 120 degrees, and very few feed-water heaters average over 180 degrees. I think 100 degrees would be a better standard, and the reduction could be made just as easily, of course.

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[*] The customary method of rating horse-power is 30 lbs. of water per horse-power per hour from a feed-water temperature of 212° into steam at 70 lbs. pressure above the atmosphere, which is equal to 30.985 lbs. from feed at 212° into steam of the same temperature. The writer prefers the calculations both of economy and horse-power to be made on the basis of evaporation from and at 212°, for the sake both of uniformity and of convenience in calculation.

Mr. Kent also says that he prefers averaging the fires at the commencement and end of the test. That is mere guess-work. If you want accuracy, you never guess at it. So I think we shall have to adopt some method of arriving at the amount of fuel on the grates other than that suggested.

I would say further that the pressure stated by him, 70 pounds per square inch, is not considered sufficient at the present day, with the introduction of electric lights, and small high speed engines. Higher pressure must be carried to produce the power. You will find the majority of boilers now are carrying 100 pounds, and, in fact, we would like to have 150 pounds pressure per square inch. Consequently we shall have to increase the pressure in any standard we may adopt.

Mr. N. W. Pratt.—(Presented in writing, and read by the secretary.) In regard to the method of starting and stopping boiler tests as proposed by Mr. Kent, a different plan would eliminate two of the possibilities for error as far as the fuel is concerned and the amount that is actually appropriated for the use of the boiler. I would make the following suggestions:

Given a boiler to be tested at, say, 75 pounds pressure. Arrange the safety valve to carry pressure up to 100 pounds. Start the fires, and get everything in good working condition, with the safety valve loaded to 100 pounds, and raise steam to that pressure. Clean the fire thoroughly, spread it evenly over the grates, estimate its thickness, remove the 25 pounds extra weight on the safety valve, allowing the steam to blow off, and call "Time" when 75 pounds is reached.

Just before the time for stopping the test, reload the safety valve to 100 pounds, raise steam to this pressure, clean the fires thoroughly, spread the coal, estimate its thickness, remove the 25 pounds extra weight on safety valve, and call " Time " when steam reaches 75 pounds pressure again. Meanwhile haul the fire and quench the coal.

By this method the error due to the amount of air which passes into the furnace both at the starting and stopping of the test is eliminated, and the amount of coal on the grate in practically the same condition, as far as combustion is concerned, has been guessed at, at the beginning and end of the test. By weighing the quenched coal which is hauled from the grate at the end of the test after the fire has been cleaned, spread and estimated, a check will be made on the two guesses, and a definite weight of coal can be assumed to have been on the grate in the same condition at the starting and stopping of the test. If the estimate on starting the test was that the cleaned fire was six inches thick, and the estimate at the end of the test was that the cleaned fire was four inches thick, by weighing the amount of quenched coal we have the amount of coal in the four inch fire, and by adding 50 per cent. will have the amount of coal in a six inch fire. It strikes the writer that this would, as far as possible, eliminate the question of admission of air into the furnace, while cleaning fires and the differences in temperature of furnaces and walls which occur if fresh fires are lit.

Mr. Charles E. Emery.—The rules proposed by the writer of the paper are, in my opinion, quite good in a general sense. They lack in detail, which the writer says was intentional, with a view of bringing out discussion. The lack of detail very often goes to the root of the matter. In looking over the portion of the paper which was not read, I find what I consider an error in the directions specified for the not uncommon method of raising steam to the required pressure before making test, then hauling fires and starting with a clean fire, and at the end again hauling fires. This is the second method mentioned in the paper, and the directions are, after hauling fires, etc., to "as quickly as possible start a fresh fire with weighed wood and coal, noting the time of starting the test at the instant the fire is lighted," and again provides for "noting the end of the test at the time the grates are cleaned." The time stated for starting and stopping the experiment are in my opinion wrong. The experiment should start when the boiler commences generating steam at a fixed pressure, and should be considered as ended when it ceases to generate steam at that pressure. To accomplish this I always arrange in starting and stopping a test of this kind that the boiler to be tested be disconnected entirely from other boilers, and the steam conducted away through an open pipe, regulated preferably by a safety valve. The operation then is, first to raise steam to the desired pressure, so that the safety valve previously adjusted, will blow freely; then the fires are to be hauled, new fires started, etc., and in the interim the water level is noted after the safety valve stops blowing, and the time of commencing test noted when the safety valve again blows on account of starting new fire. The ending of the test with position of water in gauge is to be noted when the safety valve closes after fires are hauled. The tank measurements would be those taken between these two intervals, and evidently all the water that leaves the boiler in that time is evaporated by the fuel used during the test. The actual time the fuel is burning may vary a little from the above interval; but this, it seems to me, is the only way to obtain the absolute quantities both of fuel and water.

The method of making a "flying" start and stop is much simpler, and, after some experience, gives satisfactory results. The third method in the Rules—really a modification of the first—is much the safer of the two, as it proposes to estimate the fuel on the grates immediately after cleaning fires, when the quantity present—and therefore the possible error—is small.

By either method it is very essential that the feed be steady for some time previous and immediately up to the time of starting and stopping the experiment. It is very easy to run the pump rapidly for a short time before making the record, and thereby introduce considerable more water than for regular conditions, from the fact that the water will become heated in a short time and expand, changing materially the level in the glass. The difficulty of preventing this, at least to some degree, makes it at times desirable to produce uniformity at the beginning and end by stopping the feed entirely a number of minutes before starting and stopping, and noting the time in both cases when the water is evaporated down to a certain mark. To do this requires, however, that the experiment stop, not at the actual moment when fires are lowest after cleaning, but at a certain time after the fire has burned through. There are errors in either method of starting and stopping the test, but they become unimportant when a long test is made.

The difficulties incident to making accurate measurements apply to testing boilers as well as to other matters. Too often boiler tests are undertaken with a poor equipment and too little assistance. Often a person will attempt to make a boiler trial alone. It is possible for one to dodge around to the water tank, watch the firing, see that the coal is weighed, and between times get a few temperatures; but at the end of a reasonably long run he will be so tired that he will not know whether the work was thoroughly done or not. Much the better plan is to have an assistant at each point of observation, particularly one to weigh the coal and another one to measure the water. If calorimetric tests are undertaken, still another should be provided. The principal is then left free to oversee the work of all.

It is not uncommon to be obliged to make boiler tests where parties are present antagonistic to a fair test. Some of us have had experience with steam gauges set fast, water escaping through unexpected openings, fuel brought in surreptitiously, and the like. While much difficulty can be avoided by blanking off all unused pipe connections, still methods are found to cause inaccuracies, and as it is not pleasant to accuse others of dishonesty, it is always better to have enough help to prevent the possibility of so-called accidents or mistakes. It is also necessary to have assistants with sufficient calibre that they cannot be persuaded by interested parties that they have tallied a barrow of coal or that they have not tallied the last barrel of water, or the like. This brings up another feature, the method in which tallies should be kept. It may seem very simple, in a dignified body like this, to speak of the matter; but it is surprising how few can keep an accurate tally. One can put down 1, 2, 3, 4, and a cross-mark, but when there are waits of ten or fifteen minutes between marks, confusion often arises as to whether the last tally has already been made. The only way to prevent this is to take a special line on the record paper for each tally, and note opposite it the time of a particular portion of the operation; for instance, when a wheelbarrow load leaves the scale, or when the load is dumped on the floor, or when a particular valve is opened to let the water down to the pump, or something of that kind. Then, by weighing coal and feeding with even approximate regularity, it will be impossible to make a mistake of a whole measure of coal or of a whole tank of water. Simple checks of this kind are far more important than reading all the thermometers to tenths of a degree.

Again, as to the apparatus for measuring the water. Measurements can be made with one tank by filling to one mark and pumping down to another, but it is not the proper way to stop feeding while tank is filling. The feed should be regular, so that two tanks may be called a necessity, and three are better—two measuring tanks, used alternately, and a third tank from which the feed is taken. The latter should have parallel sides, so that differences in height can be checked off at regular intervals.

In regard to the quality of the steam, I think that the feature which was first brought out in my own report on Centennial tests in regard to the steam pipes from boiler to calorimeter, should be well considered. I will not enlarge the discussion by expressing an opinion as to the kind of calorimeter that should be used. The subject is large enough for a special paper; but any good calorimeter will often show superheating for a boiler having no superheating surface, unless the connection to boiler be very large and reduced by a series of steps to the calorimeter, substantially as pointed out in the report previously referred to.* The reason is that if we withdraw steam rapidly through a small pipe tapped into a large one, the pressure and temperature of the steam in the small one will be reduced so that the entering steam will receive heat from the surrounding metal, the temperature of which will be kept up by the current of steam passing through the larger pipe. The consequence is that the steam in the small pipe shows more than its proper share of heat. By making the outlet leading to the calorimeter quite large, so that the steam flows into it without appreciable velocity, and then reducing at a distance from the main steam pipe, the small pipe can carry off no heat from a passing current. Even with this precaution, on ordinary barrel calorimeters a trifle of superheating will be shown at times with boilers having no superheating surface, but at other times a slight percentage of moisture will be found; so the average will show dry or nearly dry steam. The variations are due to small errors in observation, which, not being cumulative, offset each other in making the average.

Mr. Le Van.—Mr. President, I would like to say in regard to Mr. Pratt's observation about increasing the boiler pressure to 100 pounds, where the certificate calls for 75 pounds, that pressure cannot be carried, from the fact that most of our cities have rules for the inspection of boilers which would prevent it. If a boiler is tested to carry 75 pounds, the safety valve can only be set at 80, so that increasing the boiler pressure to more than five pounds would not be allowed.

In regard to checking the errors in coal, I would state that in a test we generally lay down coal enough for an hour's run—weigh it out. Each firing is weighed, and at the end of the hour we weigh the coal left over. All water is always weighed at the commencement of the test, at each charge of water to the boiler the difference is noted, and then what is left is weighed at the end. But I think you must find some other plan than guessing at the condition of the coal on the grate at the end of the trial. If anybody burns anthracite coal, and can tell about the condition of that coal by looking in the furnace, then I confess I do not know anything about it. Some anthracite coal will apparently be four inches thick when it is all ashes. Bituminous coal I have had no experience in

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* Report of Group XX. Judges of the Centennial Exhibition, page 82.

to any extent, but I am satisfied that no proper test can be made where you have got to guess at any part of it.

Mr. Woodbury.—Mr. President, the question of the amount of coal at the beginning and the end of a test is a matter of assumption or of guess-work whose accuracy is dependent on a personal equation of those who have the matter in charge, and the graphical method can sometimes be used in that matter. A few years ago, I was connected with a duty test of a city pumping works in New England where I used the graphical method in this manner. Taking several sheets of diagram paper, I laid off horizontally the time, something over a hundred hours to the test—and the vertical ordinates represent the aggregate amount of coal fed down in front of the boilers; the whole diagram being about ten feet long. As each delivery of coal was made, the time was taken, as well as the weight of the coal, and that was platted on this diagram paper, and the subsequent one with its time added to it also platted. And so, as the assistant kept up the aggregate amount of coal, we knew at all times how much coal had been delivered to the boiler room, and the whole result was something like this (illustrating by means of blackboard drawing, Fig. 66). The man in charge of those boilers was not handicapped by the ten commandments in his desire to make as good a show as possible. When the test began the consumption of coal was rather more rapid. For a while it was

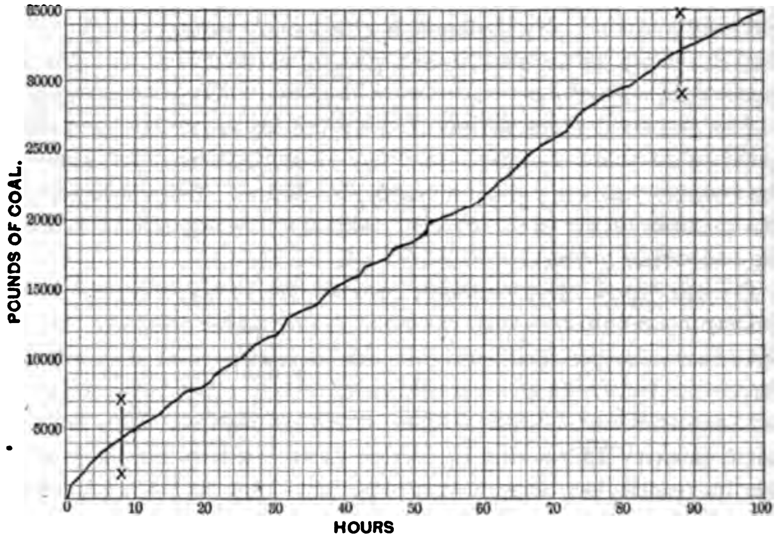

FIG. 66.

essentially the same, until a few hours previous to the time at which he supposed the test was to be brought to an end, and then he caused his men gradually to reduce the amount of coal in their barrows, and it fell off as shown at the right hand of the line. The time included between the lines ×, ×, was that taken for the duty trial.

This method would not apply to a test where the consumption of steam might be variable.

Mr. Nagle.—Mr. Woodbury's practice is also my own. If his rough sketch had been nearer to scale it would have shown the curve much steeper at the beginning and less steep at the end. There is no way in which the data of a boiler test can be so intelligently and satisfactorily studied that correct conclusions may be arrived at, as by the graphical method.

After the usual precautions have been taken and preparations made, let the steam pressure be at its ordinary working pressure, the water line at its usual point, clean out the furnace and ash-pan and make a new fire, noting the weight of fuel and *time*, which is the beginning of the test.

Prepare equal weight of fuel, say in 100 pound lots, and fire in the usual manner to keep up steam at its working pressure, but always noting the time the 100 pounds are thrown into the furnace.

As the water consumption is so much greater than the coal, it is not practicable to feed in like manner, but instead note the amount fed at regular intervals of time, say every 15 minutes. The water line can be, and should be, kept at the same point during the entire test. The steam pressure should be recorded every 15 minutes and maintained uniform throughout the test as near as may be, but it *must* be the same during the latter part of the test at all hazards.

When it is desirable to bring the test to a close, and no more fuel is fed, continue to burn all possible fire there can be got out of the fuel, until nothing but ash and clinker remain; keep up the water line to its normal point, but the steam pressure will necessarily fall. We have now data from which we can ascertain the truth of things.

The weight of all the fuel used and the weight of all ash and clinker, gives us the total net combustibles, or efficiency of the fuel.

Now make a diagram of the fuel, water, and steam pressure, to a scale that will permit the lines to run near each other, and it will be found that at the beginning of the test the coal line is far above the water line, then for the greater part of the test it is parallel with it, and at the end it is more or less below it. *Theoretically*, the area of the coal line above the water line should be nearly equal to (a little greater than) the area below it at the end, but according to the bias or skill of the arranger, these areas will vary very much, and it is with a view of exposing the facts to sight that the graphical method protects us against erroneous conclusions.

We shall find that from one-half to three-quarters of an hour after the test began that the coal, water, and steam lines will run together, and this is the *true* beginning. During the continuance these lines may fluctuate, but near the end the steam line should be kept up with conscientious care, and when it can no longer be kept up and the water and coal lines are yet together, the *end* of the test proper has arrived, and the coal and water between these two points furnish the true basis for calculating the ratio of coal to water.

Mr. Woodbury.—Mr. Nagle calls my attention to what was an omission of statement. We did plat the amount of water fed into the boilers, and platted it on the same paper, at a scale of one-tenth that of the coal. So the evaporation being, of course, nearer ten pounds than any other even decimal, the lines were at a tolerably uniform angle with each other.

Mr. Porter.—Mr. President, there seems to be an erroneous assumption. The graphical line represents the coal that is added to the fire, and the assumption is that it represents the coal that is burned, and all that we want to know is the latter. Probably the graphical line representing the evaporation, if it were parallel with the graphical line representing the coal added to the fire, would demonstrate that the combustion kept pace exactly with the addition. A man in adding coal to a fire all day may increase or diminish the quantity of coal upon his fire. The graphical line would in be one case more steep, in the other would approach the horizontal. But the graphical line representing the evaporation of water will represent also the combustion of coal; and if that should be parallel with the line representing the addition of the coal, then it would show not only the addition but the combustion. But it seems to me that really the only experiments which will command approval, which will carry on their face the evidence that they are correct, are experiments in which the trial is commenced on a clean grate, and ended with a clean grate, what is left in the grate being carefully weighed. The fires are very deceptive. I do not think Mr. Le

Van has overstated the case of the appearance of fires. It is not possible for the difficulty to be overstated.

I was told once by an engineer that if a test was limited to five hours, he would undertake on that basis to run without any coal at all, and to satisfy any intelligent man using the furnace that he had as much coal at the end of the test as he had at the beginning—no doubt of it.

Professor Webb.—Mr. Chairman, I think if a very accurate test was to be made, I should endeavor to have the grate made independent of the boiler, and so arranged with scales that the whole—grate, coal, and ashes—could be weighed at any time without interfering with the progress of the combustion. In this way it would be known how fast it was burning away. I should want to connect the lower end of the water gauge with the bottom of the boiler, so as to allow for the unknown density of the water and steam mixture, and thus obtain an indication of the actual amount of water in the boiler.

Mr. Le Van.—Mr. President, I think the only remedy is to put a track under the boiler and run your grates in on wheels, and when the trial is finished run out the grates. There will be no guess-work in that. Probably that would not be a bad idea if properly arranged.

Mr. Porter.—I remember a case in which the matter of time was very important, where an observer who was under my direction noted the time at which the water tank was filled, and an observer who was placed by other parties to check our examination merely put down a mark every time the tank was filled. We had one more tankful than he had, but the fact that the hour and minute of filling the tank had been noted against each one of our entries, and that those intervals of time were uniform all day long, was conclusive, and he finally concluded that the tank must have been filled once while he was gone to dinner [laughter].

Mr. Leavitt.—Mr. President, Mr. Woodbury has made an allusion to the Ten Commandments. The same Board that made the test to which he has referred afterwards made a test in which I was interested. They probably had profited by experience, and one of my assistants who represented my side of the case told me that he could not get up out of his chair to go for any purpose out of the fire-room without having two or three assistants of the Commission after him to see whether he had any coal in his pocket [laughter].

Now, I think it is necessary on occasions like this to have it set down as a fact that the party who is interested in the boiler should be assumed to be a person who will both steal coal and open the blow-off valve, and that whenever any of the Commissioners are absent the safety-valve may possibly be raised [laughter].

The only way to get at an accurate test is, as Mr. Porter has said, to start with a clean grate, to end with a clean grate, and to weigh the refuse. There are certain errors which obtain from the fact that the boiler is cooled in removing the original fire, and is also cooled at the end; but if the trial is of sufficient duration these amount to very little. In one case that came under my observation, in a test of seventeen hours' duration, the evaporation from and at 212° was 12.32 pounds per pound of combustible, while the duty of the engine, which was tested at the same time (being a pumping engine), was one hundred and eleven and one-half millions per hundred pounds of coal. One confirmed the other.

The matter of estimating the fires, as I know from personal observation, is very uncertain. I was caught on this same platted line at the time referred to. It should never be known long beforehand when the trial is to close. I had a suspicion that the trial was to close at a certain time. My assistant, who was a pretty sharp fellow, had instructed the fireman to fire light. I came into the fire-room with two of the Commissioners who estimated the condition of the fires. The doors were opened, and the Commissioners said that the fires were as good as they were at the commencement. There was about 150 pounds to 200 pounds of coal on the fire-room floor, and I told the fireman to put that in the furnace to make sure we should be on the right side. I thought I was very generous, because it was going against myself. But the chairman of the Board, when he came to look at his platted line, put three or four hundred pounds more on top of that. Now, if we had made the test with a clean grate when we started and when we left off, there could not have been an error of 400 pounds. That is all out of the question. The cooling of the walls at the beginning and end of the test, which would take place in fifteen or twenty minutes at the most, would not begin to be that. It would not be fifty pounds. For this reason I am in favor of the absolute method.

Mr. Le Van has objected to Mr. Kent's method of reckoning at and from 212°. I think if he will reflect a little he will conclude that is the best, because tests are liable to be made at different pressures and under different conditions. One engine may feed at

180°. Another engine may have a Green Economizer and feed at 300° or 400°, but we want to get at a fixed standard, and if we take 212°, we have a basis from which anybody who is familiar at all with boiler tests can make the computation, and know what he has got.

Mr. Kent.—The first point I was going to reply to was Mr. Le Van's objection to 212 degrees being taken as a standard. I think that has been sufficiently answered by Mr. Leavitt.

As to Mr. Pratt's communication concerning raising the safety-valve to 100 pounds, raising the steam to that pressure, and then lowering it to the working pressure, and starting and stopping the test in that manner, I think Mr. Le Van answered that very satisfactorily—that boiler inspection rules will have a great deal to say against it. The objection I would make to it, in addition, is that, so far as I know, no test has yet been made in that way, and we will have to know a little more about that method of starting and stopping by experience before it would be safe for engineers to establish it as the standard method.

There seems to be a difference of opinion, as already shown here to-night, among engineers, as to how to start and stop a boiler test. Mr. Emery seems to prefer the flying start and stop. Mr. Porter and Mr. Leavitt believe in clean grates. My paper seems to have been misapprehended in the discussion in regard to the method which I prefer, which is not starting with level fires and stopping with level fires, and guessing at the quantity on the grate, for that is mentioned in my first method in the introduction, and I say also in this introduction that I believe that method should be rejected on account of too much guess-work. The method preferred by Mr. Porter and Mr. Leavitt is also objected to, on account of the losses by radiation and imperfect combustion during the beginning of the test, when you are firing with wood, and during the first hour or two of the test, when you have, perhaps, a ton of coal on the grate and do not know how much is being burnt, and how much combustible gas is passing up the chimney. The method which I prefer, and which is perhaps not clearly enough stated in the paper, is: Suppose the boiler is running straight ahead for a week. Periodically the fires are cleaned, say, every eight or ten hours with good anthracite egg coal, every four hours with poorer coal. At each period the fireman pushes back all the good coal on to the back part of the grate, and pulls out all the ashes and clinkers. At that time the coal he leaves in there is a comparatively small quantity—not over one-half of the total amount in the grate. I have seen times when over a thousand pounds of ashes have been withdrawn from the fire, and the man would leave in, probably, from 600 to 800 pounds of bright coals. That amount when spread can be guessed at pretty nearly. The test is run through one, two, three or four of these periods between cleanings, or as many more as you want—a week, if you say so—and the test is stopped at one of those periodical times for cleaning. When the fire has been burned down to about the usual condition before cleaning, then the man cleans the fire as before, and he pushes back about the same quantity of coal, and you can see when it is pulled forward how much there is. By guessing on a smaller quantity, your error in guessing is less. I prefer in an accurate test to have the test run twenty-four hours, so that whatever error there is in the test will be smaller in proportion. The error in such a test would probably be not more than 100 pounds, which in a test where there is from 10,000 to 20,000 pounds of coal used is but a mere fraction.

I agree with all the speakers who spoke against starting and stopping with level fires, as guessed at ordinarily, because one man will look at a fire and say it is six inches, and another will say it is eight inches, or a foot, and twenty-four hours afterwards when they come to look at it they will not get within forty per cent. of a correct result, as regards the amount of combustible material in it, if it is not the same distance from a periodical cleaning time. Suppose a boiler to have 3,000 pounds of coal on the grate. Say half an hour after starting the fires, I suppose the value of combustible matter in that coal is at least eighty per cent. of the 3,000 pounds. Suppose, several hours after that, when the fire was in the same condition, as far as I could see, it might not contain over forty or fifty per cent. of combustible. So that it would be quite possible, as Mr. Porter said, to run a test without any coal at all.

Mr. Woodbury's graphical method is one which I have tried myself, and it is an exceedingly valuable method wherever the test is a long one. In a test that I made last week in which we started with clean grates, 3,000 pounds of coal were used in less than an hour. The total amount of coal used during twenty-four hours was only 16,000 pounds. In such a test the platted diagram would probably be of little service. In another case, where we started on a flying start with level fires, for a test of seventy-two hours, we had better results, and got some good information out of the diagram, because during that test we varied the condition of things, in regard to the draught, and in regard to thin fires and thick fires. We would run, say, for fifteen or twenty hours with thin fires, frequent firing and light draught, and again run with thick fires and heavy draught, and we could see on the diagram better than in any other way exactly what the results were as to economy and capacity. And in that test, I may mention, we found that thin fires, frequent firing and light draughts gave the best results, both as to economy and capacity. Exactly the opposite conditions in every respect gave the next best results, and very near to these. And the very worst results were found somewhere between the two. You can get very good results out of a boiler in several different ways. It requires experiment with each boiler to get the best results. I would advise the use of the graphical method in all long tests, and wherever practicable, because the graphical method will generally tell you more about the test than you will learn from mere inspection of the figures.

In regard to the differences of opinion which have been shown here to night, I think if the subject had been discussed more fully, and if it had been written upon by the engineers, without reference to this set of rules, we would find still greater difference of opinion, in regard, first, to general principles, and secondly, and far more largely, as to detail. It all shows that the engineers need to-day some set of rules for testing boilers, and I have not brought in this particular set of rules with the idea that they are perfect and that none others should be adopted, but I want some standard set of rules, so that when engineers make a test they can say it is according to an approved standard. I think it would be in order to move the appointment of a committee of the Society to report on a standard set of boiler testing rules, which could be reported to the Society at the next meeting, whenever that is to be held. I do not suppose the Society will take any action in the matter further than to receive the report of the committee, but I think it would be a valuable thing to have such a report signed by a committee known to be composed of engineers of high standing.

CXLVIII.

ESTIMATES FOR STEAM USERS.

BY CHAS. E. EMERY, PH. D., NEW YORK.

§ 1. A PORTION of the plant of the New York Steam Co. has been in operation for two years, but as many features are still in course of development, it would as yet cause some annoyance and embarrassment to publish the details of the engineering work. Bearing in mind, however, the duties of each member to contribute at least an occasional paper to the society, I present herewith in an appendix the substance of the rules on which most of the estimates of the company have been based, and propose to explain the manner in which they were developed and make some suggestions as to their adaptation for general use.

§ 2. The first problem was to fix a unit by which steam could be sold. A horse-power is definite, when considered simply as expressing the rate in which mechanical work is performed, but a horse-power of steam varies with the size and kind of engine used, and must be referred to an arbitrary standard. We adopted the customary standard of thirty pounds of feed water per horse-power per hour. Most of the engines supplied were small or without expansion gear and required more than thirty pounds, and had to be charged for more, and for miscellaneous uses, such as heating buildings, boiling soap, melting wax, ripening fruit, etc., the cost had to be finally estimated from the feed water or water of condensation, and as every use could be referred to this unit, its adoption seemed necessary. Confusion would, however, arise in the minds of consumers between pounds of water evaporated into steam and pounds of steam pressure, so a new unit seemed necessary which would be as distinctive as a ton of coal or a thousand feet of gas.

§ 3. We commenced selling steam in February, 1883, at a specified price *per thousand Kals*, explaining that a *Kal* was equivalent to a pound of water evaporated into steam, and thereby solved the problem for our special purposes. The term is employed regularly by our representatives and our consumers. Many of the latter understand it, and all use it. For instance, a dignified bank president has had a clause inserted in his contract that the price per thousand Kals to his corporation shall be as low as to our most favored consumer, and, without knowing anything technical about it, explains to his associates that *that is the way steam is sold.*

§ 4. The writer believes that such a term would be useful generally as well as specially, and would feel personally complimented if engineers and representatives of the different journals would use it in an alternative way to test its convenience and necessity. The

§ 6. The performances of boilers are now universally compared by the number of pounds of water evaporated from and at 212°. It may be considered as accepted then that the "unit of evaporation" is the evaporation of a pound of water from and at 212°, of which the thermal value is 965.7 thermal units. It would be desirable for simplicity to make the Kal equal to this, but there are serious practical objections to it. The Kal is to take the place of the unit in the expression "pounds of feed water" or of "water of condensation" required per hour for engines and other purposes. Its value should, therefore, be founded on the evaporation from a fair average steam pressure and from a fair average temperature of feed. In the report on a series of boiler tests at the Centennial Exhibition made by a committee of the Judges of Group XX, consisting of Mr. Charles T. Porter, member of the Society, Mr. J. Belknap, and the writer, who was the chairman, there was presented what was termed the "Commercial Evaporation,"—viz., the evaporation at pressure of seventy pounds from temperature of 100°, and the "Commercial Horse Power," based on thirty pounds of feed water per horse-power per hour, evaporated on same basis. The seventy pounds pressure had been accepted before, but the evaporation was generally assumed from 212°. In the report it was stated that 100° temperature of feed could always be obtained, even with condensing engines, and that frequently the temperature was little greater with non-condensing engines. Most engineers will agree that as a standard thirty pounds of feed water evaporated under *actual working conditions* per horse-power is small enough, though entirely fair when good and bad engines are considered together, and that although good heaters may raise the temperature of feed to 180° and occasionally higher, a temperature of 100° is nearer the average than 212°. The heat obtained in excess of 100° may properly be credited to surplus boiler power, which is so often deficient.

§ 7. The proposed standard of horse-power has already been accepted by several experts in suits as to power of boilers, and is known as the Centennial Standard of Horse Power, or the Centennial Horse Power. It is therefore proposed to make the value of a Kal the same as the unit of Commercial Evaporation above named.

§ 8. We have then based on the thermal or heat unit—

1. The accepted "unit of evaporation" (E) with a thermal value of 965.7 thermal units.*

2. The Kal (K), equal to one pound of water evaporated into saturated steam at seventy pounds pressure from temperature of 100°, with a thermal value of 1110.2 thermal units.

$$[K = .86984\ E;\ E = 1.1496\ K.]$$

3. The Centennial Horse Power ($C.H.P.$), which equals simply thirty Kals per hour.

§ 9. By a pleasing coincidence the volume of one Kal is almost precisely five cubic feet, hence the weight of a cubic foot, or the "heaviness" (D), as it is expressed in Coxe-Wiesbach, is two-tenths pound.

§ 10. We are now prepared to examine the rules for making estimates previously referred to, and the reader will first please examine §§ 32 and 33 in the Appendix.

§ 11. It will be observed that the charges for heating are based

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* The number of heat units here given would be varied slightly by consulting different formulæ and tables. The figures given are those adopted by the U. S. Navy as published by Isherwood and Nystrom, and agree with those in Mr. Charles T. Porter's work on the Richards Indicator.

on the ordinary steamfitter's method, using the *capacity* of the buildings, instead of the area and thickness of walls, area of windows and roof, extent of ventilation, etc. These rules are founded upon investigations of the cost of heating a large number of business buildings in the City of New York under actual practical conditions. The variation in cost between different buildings of similar construction and exposure was found to be very great, due doubtless to differences in apparatus and management, and made useless any elaborate system based on calculating the number of thermal units passing through window glass or walls of different kinds and thicknesses. These rules, when applied of course with good judgment, simply give prices for which business buildings of the kind described should be heated satisfactorily during the ordinary business hours of an average heating season, say ten hours per day for 200 days in the year. With care the work can be done more cheaply, but when the steam is used from a district system where pressure is maintained continuously, janitors and porters will use the same prodigally by leaving it on day and night, or by neglecting to shut off radiators so as properly to graduate the heat to the demand, thereby increasing the cost in some cases very materially.

§ 12. The reader will now please examine casually § 40 to § 48 Appendix, and afterward more carefully in connection with the following:

§ 13. The formulated rules for the power and the cost of the power (§ 43) are very simple and readily applied. The first step is to obtain the continued product of the square of the diameter of cylinder in inches (d^2), the stroke of piston in inches (s), and the revolutions per minute (r). This product is a factor in the equations for the power as well as for the cost of the power. In equation (4) § 43 the power P equals the product referred to, multiplied by a constant coefficient (.000004), and by the mean pressure (m). The coefficient is, of course, made up of the product of $\frac{\pi}{4}$ (required with d^2 to give the area), and $\frac{2}{12}$ (required with s to show double stroke in inches reduced to feet) divided by 33,000. A more exact result of the operation would give a coefficient of .000003966, but the simple value .000004 used is correct within less than one per cent., and was therefore satisfactory for the purposes in view.

§ 14. Eq. (5) is derived directly from (4).

§ 15. The cost of the power is estimated in all cases from the number of cubic feet capacity developed by the piston. It is at first assumed that the cylinder is at every stroke filled completely with steam of the initial pressure, but a multiplier is provided in the formula which is varied according to the known conditions to approximately give the corrected result. There are required to be known by this process: 1, the volume developed per unit of time; 2, the weight per cubic foot of the steam; and 3, the factor of correction above referred to. In formula (6) § 43, the expression for the number of Kals. per minute (k) is made up of the product ($d^2 s r$) referred to in connection with the power, and the coefficient is simply the joint product of factors to reduce the expression independent of factors a and n to cubic feet per minute, and of another factor, viz. .0023, belonging with the factors a and n yet to be explained.

§ 16. Complete formulæ representing the weight of a cubic foot of steam [viz., the heaviness (D)] at all practicable pressures are usually quite complex. The writer finds that the results shown by Rankine's elaborate formulæ are represented equally well between the limits of 1 and 1,000 lbs. pressure by the simple expression,

$$(1) \qquad D = .0030343\, p_1^{.94},$$

hence

$$(2) \quad log.\, D = \overline{3}\,.4820505 + .94\, log.\, p_1,$$

p_1 being the absolute pressure obtained by adding 14.7 lbs. to the pressure by gauge. It was found, however, that the following still simpler equation of a straight line was sufficiently accurate for practical purposes within customary limits, say from atmospheric to 80 lbs. gauge pressure, viz.,

$$(3) \quad D' = .0023\,(p + 17),$$

p being the gauge pressure. The $p + 17$ should not be confounded with $p + 14.7$ or $p + 15$ representing the absolute pressure. It is to be remembered simply that for absolute pressures varying as $p + 15$ nearly, the density and "heaviness" vary within ordinary limits as $p + 17$ nearly.

§ 17. By the method adopted the initial pressure is assumed to be the mean pressure plus the average back pressure, so it will be seen that the values of n in § 42 of Appendix simply provide for varying the initial pressure with the back pressure by adding the value of the latter above the atmosphere to $m + 17$. In formula (6) in appendix, the value of n corresponds to $p + 17$ in formula (3) above, and the coefficient of latter being already introduced in coefficient of (6), the heaviness (D) due to the initial pressure of steam is introduced, and the formula, independent of a, simply multiplies the number of cubic feet of steam per minute by the weight of a cubic foot at initial pressure.

§ 18. The factor a simply corrects the expression according to table, § 41, to approximate actual conditions, more particularly as to expansion, internal cylinder condensation and probable leaks. If the engine be operated without expansion, and neither of the losses mentioned be considered, the value of a should be unity. In some elevator engines there is no expansion whatever, but the losses are considered at 20 to 30 per cent., making a equal 1.2 to 1.3 as shown in table. The expansion obtained with a plain slide valve reduces the cost materially, so the value of a is fixed at 0.85 for large engines and 1.1 for very small ones in which various losses overbalance the gain due to expansion. For cut-off engines the value of a varies from 0.6 for large engines to 0.9 for the small ones.

§ 19. It is so natural to suppose that there should be some consideration of the expansion curve in an equation proposing to deal with the economy of an expansion engine that this method of using a simple coefficient of correction will at first sight seem strange to all. If many here present were jointly developing an equation, there is every probability that a discussion would soon be in progress whether the hyperbolic or the adiabatic curve of expansion should be considered. The formulæ under discussion stand on higher grounds. Though approximate only, they include the more important conditions. Independent of large engines, and exceptionally good small ones, all that the wisest of us know about cost of steam power is that the average costs for average engines of certain kinds, under average conditions are certain figures, the values of which would not be changed greatly by individual opinions. Here, then, is a basis superior to all minor details as a means of arriving at the result. For the purposes of this case the values of a are varied to produce the corresponding values of k_p, or kals. per horse power per hour in last column of table § 41, and if there be differences of opinion as to the proper valves of k_p the values of a can be varied to suit. To a mathematician it is plain that if we know the result to be obtained, a factor in an equation which is constant for particular conditions can be changed to suit other conditions. Unfortunately it is often thought that the value of many formulæ lies in the constants. The natural inquiry comes, Why not use the results direct without any formula?

§ 20. In this case there are good reasons. With non condensing engines the most important variations in the cost of the power are caused by simple variations in the ratio of the back pressures to the effective or, better, the total pressures. If this fact be considered, other variations in result from engines of the same general size and design, using steam of the same quality, will be almost entirely due to differences of mechanical condition. The relative effective pressure is provided for perfectly in the formula. The basis in all cases is the cost of the power absolutely without expansion—the initial pressure being assumed equal to the mean and back pressures. Expansion simply furnishes an indicator diagram of the *same* area, with one portion made higher in order that the other can be lower and reduce the cost, though not proportionally to the lowering of the pressure.

§ 21. The multipliers a in table § 41 of Appendix are calculated for the values of k_p in same table with the mean pressure m equal to about 30 pounds for the cut-off engines, and about 40 pounds for the others. The comparative results given by formula for a fair-sized engine which would be customarily rated at 30 to 40 H.P. considered first as a slide-valve engine, and second as as a cut-off engine, for different mean pressures and constant back pressure, above atmosphere of 2 pounds ($n=m+19$) are shown in the following table:

MEAN PRESSURE (m) AND INDICATED HORSE POWER (P).	5	20	40	80
Slide valve engine $a = .85$				
Kals. per horse power per hour k_p......	128.52	52.21	39.49	33.13
Kals. per hour k_1....................	642.6	1044.2	1579.6	2650.4
Cut-off engines $a = .60$:				
Kals. per horse power per hour k_p......	90.72	36.5	27.87	23.4
Kals. per hour k_1....................	453.6	730.0	1114.8	1872.0

§ 22. The table shows very forcibly the influence of the back pressure; the cost of the power rising rapidly as the mean pressure is decreased and reducing materially for high mean pressure, without any consideration of expansion whatever except that the percentage of saving between the slide valve and expansion engines is assumed to be the same for the same power and development. This may not be true precisely, but is approximately true within ordinary limits. An exception was thought necessary for engines running very light, for the rules provide—see § 49 Appendix—that minimum charges shall be based on $a = 1$, etc., which practically charges the cut-off engines with slide valve minimum rates as is evidently nearly correct. For case given in § 21 above, the minimum charge by this modification would be 661.5 kals per hour, the indicated power being that due to the friction pressure and the useful power zero. It is in general approximately correct to state that a high-pressure engine cannot be run for less than half the steam required to drive it when developing its rated power.

§ 23. It will be seen that a slide-valve engine run with high mean pressure can readily give better results than a cut-off engine with low mean pressure. Very simple reasons may explain wide differences in result obtained under different conditions.

§ 24. It is well known that large cut-off engines of approved make in some cases operate more economically than shown in table, but the formulæ were intended to give results approximately correct, though on the safe side for small engines of average construction, and have in practice been found to answer the purpose very satisfactorily.

§ 25. Formula (7), § 43, derived from (6), gives the number of kals. required per thousand revolutions of an engine. When the work is substantially uniform, the simplest way to account with a consumer is to apply a counter and charge by the thousand revolutions, basing the price on a mean pressure sufficient to perform the maximum work.

§ 26. Formula (8), § 43, is a modification of the others to give directly, if desired, the kals per horse power per hour. Note that the factors $d^2\ sr$ disappear from this formula, as they should, as they occur in both divisor and dividend in making the combinations.

§ 27. The remainder of the rules will, I think, be understood without detailed explanation, except, perhaps, equation (12), § 55, Appendix, showing the number of kals per trip of ordinary steam elevator engines. This formula gives what are called high costs, but it has proved insufficient for this class of engines when constructed with piston valves, many of which seem to offer very slight resistance to the passage of the steam from the boiler to the exhaust pipe. For slide-valve elevator engines the formula gives approximately accurate results. These engines are generally made absolutely without expansion, with small ports, and are run at high speeds. The small ports generally require cylinders altogether too large for the work, so that the mean pressures are low and the costs correspondingly high.

§ 28. An examination of the notation § 54 will show that the revolutions r_2 apply to a trip in *one* direction. The return trip is provided for in the quantity $m_{11} + 46$, in equation (12), § 55, which is made up of $m_{11} + 21$, corresponding to n_2, § 42, plus $4 + 21$, four pounds in excess of back pressure being considered necessary to lower the elevator. If $m_{11} = 10$ pounds, as it frequently does, the cost in ascending is proportioned to $10 + 21 = 31$, and in descending to $4 + 21 = 25$, which do not differ at all proportionally to the useful power exerted. In fact, it is difficult to tell whether such an elevator is ascending or descending by watching the exhaust pipe.

§ 29. The following calculations apply to a specimen elevator, with double 8 x 10 cylinders, in regular use in New York:

$W =$ say 1200 pounds. $T = 200$
$L =$ 75 feet. $b = 2.5$
$r_2 =$ 100 revs.

Then

(11) $m_{11} = 13.125$
(12) $k_t = 19.1$
(13) $k_2 = 3820.$

A trip is usually made in $\frac{7}{12}$ minutes, so in § 41, $r = 171.4$.

(4) $P = 11.5$ ascending.
3.5 descending.

7.5 average.

k_t being 19.1, $k = \frac{12}{7}$, $k_t = 32.7$, $k_1 = 98.1$, $k_p = 130.6$.

§ 30. Appendix to Paper of Chas. E. Emery, Ph.D., entitled "Estimates for Steam Users."

ENGINEER'S DEPARTMENT

The New York Steam Company.

Rules to be observed in making estimates by the Division of Steam Supply.

§ 31. Offers, estimates and contracts may be made for a stated price, but must in every case contain a reservation of the right to apply meters, as given in Regulations. Regular meter rate to be 60 cents per M. Kal. In contracts for considerable amounts 50 cents per M. Kal will answer when the consumption can be accurately ascertained.

HEATING.

§ 32. Estimates for heating will be based on the capacity of the rooms heated, and the following rates, varied by judgment, according to exposure, temperature and use of the rooms and the amount of ventilation.

§ 33. Minimum rate, $2.50 per thousand cubic feet per season for deep buildings with minimum exposure, or fairly well-lighted rooms used as workshops for manual labor, when the heating surface is limited or so divided that it can be regulated to use small portions at once. Office buildings, well lighted, generally require $3 per M. cubic feet; buildings with large windows about $4 per M. cubic feet, and those with unusual exposure and good ventilation $4.50 to $5 per M. cubic feet.

Cost of heating estimated from heating surface:

§ 34.	Steam Pressure.	20 lbs.	.6	Kals per sq. foot of heating surface per hour.
		40 lbs.	.7	
		80 lbs.	.8	

§ 35. For steam tables, etc., augment actual surface of table and uncovered pipes reasonably to allow for food, cover, moisture carried off, etc.

§ 36. Charge for uncovered pipes in basements full time, day and night.

§ 37. Ordinary heating season, 200 days of ten hours. Allow about five days per season steam on one-fifth surface all night.

§ 38. With ordinary heating apparatus it is supposed the radiators will, on the average, be shut off at least half the time allowed for heating.

§ 39. For dry rooms special calculations are necessary, founded on the conditions of the particular case. The steam required will vary principally with the quantity of air circulated and the weight of moisture carried off. A steam meter or some arrangement to measure the water of condensation to be applied in every case of this kind.

POWER.

§ 40. All power will be estimated for in units to suit the customer, but the estimates are to be based on calculations for the costs in Kals, which are to be spread on the reports of surveys for the information of the engineer.

§ 41. The following notation and formulæ will be used as required:

Let k = Kals per minute.
k_1 = " " hour = 60 k.
k_2 = " " day.
k_m = " " 1,000 revolutions.
k_p = " " Indicated Horse Power per hour.
P = Indicated horse power.
d = diameter of cylinder in inches.
s = stroke of piston in inches.
r = revolutions or double strokes per minute.
p = pressure above atmosphere.
m = mean pressure in cylinder.
a = a multiplier varied under conditions given in table to allow for expansion and provide for cylinder condensation, clearance and other losses.

§ 41.	HP.	a.	k_p.
Cut-off engines	40 and up.	.6	30
	20	.7	35
	10	.8	40
	5	.9	45
Slide valve engines	30 and up.	.85	40
	15	1.00	45
	5	1.10	5[illegible]
Elevator engines, new, good order		1.1	55
" " small and old		1.2	60
	DIAM. STEAM CYL.		
Compound pumps	20" and up.	.75	85
	16"	.85	40
	5"	1.00	45
Ordinary steam pumps	20"	1.0	50
	15"	1.1	[illegible]
	12"	1.2	[illegible]
	8"	1.3	[illegible]
	5"	1.4	[illegible]

[illegible]

§ 42. [illegible]

(To [illegible]

§ 43. [illegible]

(8) [illegible]

[illegible]

1st Case.— [illegible]

§ 44. Ascertain [illegible] (8) as required.

§ 45. (*A.*) The [illegible] with the indicator, [illegible] with a fixed cut-off.

[illegible] *m*, [illegible] tain [illegible] rs per [illegible] n other [illegible] number [illegible] of tank, S [illegible] ay, and then

§ 46. (*B.*) When [illegible] mate may be made [illegible] and a price [illegible] (7) with $m =$ [illegible] exceed that given [illegible]

§ 47. (*C.*) The [illegible] in steam chest. [illegible] pressure in [illegible] particular [illegible] cal purposes, [illegible] ng point. stroke, or [illegible] ds × $\frac{1}{8}$ nearly. be taken [illegible] and n_3. [illegible] of a, and [illegible]

[illegible] against atmospheric

[illegible] diamete [illegible]

the loss of pressure entering cylinder is balanced by using a lower value of a.

2d *Case.*—WHEN THE POWER IS GIVEN WHICH IS TO BE DEVELOPED IN AN ENGINE OF A GIVEN SIZE.

§ 48. (*A.*) Find m from (5) and cost from (6), (7), or (8) as required.

§ 49. *Note.* All contracts for H. P. actually developed will name the minimum charge for operating the engine. This will be stated on surveys and ascertained from (6), (7) or (8) as required by making $a = 1$ and $m = 0$ in value of n used in the other calculations, and adding also 1 to 3 pounds for friction pressure.

HYDRAULIC ELEVATORS.

§ 50. If D = dia. of hydraulic cylinder in inches,
d = as before dia. of steam cylinder in inches,
d_1 = dia. of pump cylinder in inches,
S = stroke of hydraulic piston in feet,
s_1 = stroke of pump in inches,
T = number of trips of elevator per day,
l = height in feet between water tanks,
r_D = number of double strokes of pump per day,
f = multiplier = 1.25 for large pumps, say steam piston 20″ dia., increased to 2.0 as steam piston decreases to 10″ dia.

§ 51. Then: (9) $m = 434 \frac{d_1^2}{d^2} f\, l$

$$(10)\ r_D = \frac{6\, D^2\, S\, T}{d_1^2\, s_1},$$

from which k_2 may be obtained from (6) by substituting k_2 for k and r_D for r.

§ 52. *Note.* In this calculation only one pump is considered. If the pump be duplex the actual strokes will be half those calculated by formula but the steam used will be the same. [N. B. In calculating power or cost for duplex pump the reading of a counter on one pump should be doubled.]

§ 53. For compound pumps refer m only to the large cylinder. The size of the small one does not enter the calculation.

STEAM ELEVATORS.

§ 54. Let W = unbalanced weight of car and load.

L = lift.

m_{11} = mean pressure in each cylinder of double engines.

r_2 = number of revolutions of engines required to run car from bottom to top of shaft, or *vice versa*, (to be counted by Solicitor).

k_t = Kals per trip.

T = number of trips per day.

$$b = \begin{cases} 1.75 \text{ for geared elevator engines.} \\ 2.50 \text{ for screw elevators and old geared elevator} \\ \quad \text{engines with piston valves.} \end{cases}$$

§ 55. Then for double engines—

$$(11) \quad m_{11} = \frac{3.82\, b\, W L}{d^2\, s\, r_2}$$

Multiply by 2 for single engine.

$$(12) \quad k_t = .000\,00504\ (d^2\, s\, r_2)\ (m_{11} + 46.)$$

Divide by 2 for single engine.

$$(13) \quad k_2 = T\, k_t.$$

STEAM PUMPS.

§ 56. For ordinary steam pumps raising water in a building, *m*, may be found from (9), and the pump considered as run a certain number of strokes per minute for a certain number of hours per day, as reported by engineer and confirmed by the practice in other buildings. If a certain sized circular tank is filled a certain number of times per day, (10) applies by making D = diameter of tank, S its depth, and T the number of times it is filled per day, and then proceeding as described in connection with (10).

HEATING WATER.

§ 57. For water heated from 60° to boiling point.
Kals. = No. gallons × 1⅓ = No. pounds × ⅙ nearly.

STEAM JETS.

§ 58. Charge for open steam jets discharging against atmospheric pressure.

d_0 = diameter of jet.

15 lbs. steam pressure kals. per hour, $k_1 = 1216\, d_0^2$
45 " " " $k_1 = 1804\, d_0^2$
60 " " " $k_1 = 1978\, d_0^2$

Calculate for pressure of 60 pounds except when jet works off a heating system at lower pressure, or when the full opening of valve would drive employees from the room.

DISCUSSION.

Mr. Kent.—I do not believe our code of rules describes any punishment for a member that does a wrong action. Mr. Emery has done a very bad thing, and he deserves our condemnation for it. He has invented a new unit—a "Kal." The punishment I think he will get, however, will be that the next time Mr. Nystrom writes a book and puts in that list of engineering terms which ought not to be used, he will put in the word "Kal." It seems this term is to be defined as a pound of water evaporated into steam. We have had enough trouble about the units of weight and measurement, and here we introduce a new word to take the place of pounds. A man says, "For how much will you sell me a boiler that will give seventy pounds of steam?" That is his way of rating the power of a boiler—and to get over the difficulty that such customers have of mixing up pounds pressure and pounds weight or quantity, it is proposed to introduce this term "Kal." If you want to sell steam, and do not want to sell it by the pound, why not sell it by the thousand feet or by the gallon or cubic foot of water evaporated, or by some other known unit, instead of coining a new term?

Mr. Emery says I am mistaken about this Centennial standard. No doubt I am, since he says so, for I have not looked at the Judges' Report for a couple of years; but my impression was that it was from 212 degrees feed water into seventy pounds pressure, and I have the impression that I got that figure from some of Mr. Emery's own writings, if I did not get it from the Judges' Report. At the bottom of the second page of his paper Mr. Emery says the seventy pounds pressure had been accepted before the Judges made this Report, but the evaporation was generally assumed from 212.

Mr. Emery says in regard to the objection to the use of this term "Kal" to mean a pound of water evaporated from and at 212, that it should be founded on the evaporation from a fair average steam pressure, and from a fair average temperature of feed. He used the same argument concerning the amount of water that should be evaporated for a horse-power—that it should be from 100 degrees, and not from 212, because 100 degrees was more nearly a fair average. The only fair way of determining the horse-power in steam boilers, as in everything else, is to get some definite standard of measurement. He says here, "The unit of evaporation is now accepted to be the evaporation of a pound of water from and at 212 degrees." That is generally accepted by engineers as a fair unit, and it is all right; but it is not an average. If it was necessary to get an average, we would make some other unit of evaporation, such as the evaporation of a pound of water from 100 degrees temperature, at 70 pounds pressure. There is no more reason for calculating the economy in a boiler test from and at 212 than there is for the use of 30 pounds of feed water from and at 212° to indicate a horse-power. We ought to refer to one standard, and not have three or four. If 30 pounds is too little, make it 40 or 35. I think that 30 pounds of water from and at 212° per horse-power is a fair standard, when we consider that some engines use 18 or 19 pounds, while others use a hundred, and that in non-condensing engines from 25 to 30 pounds of water from 212° is reached, and I do not see why 30 pounds is not large enough as a standard. But I protest against the use of too many units. I believe we should start with the British thermal unit as a basis. That is a standard definitely known and understood. The unit of evaporation should be just what Mr. Emery puts it here—965.7 thermal units. It is the evaporation of one pound of water from and at 212 degrees. A horse-power should be a definite number of units of evaporation—say 30. If 30 is too small a number, let the committee of this Society on Rules for Boiler Tests fix it at 35 or 40. Let it be 965.7 thermal units to a unit of evaporation, and 30, 35 or 40 units of evaporation to a horse-power, and wipe out the term "Kal" and all other new units for measuring steam. [Applause.]

Mr. Leavitt.—Mr. President, I think the simplest proposition is to come down to thermal units. I once had a great deal of trouble with a report where I attempted to adopt an arbitrary standard. It seemed to be a very just and fair one, as I assumed a standard pound to represent the water, which contained 966 units of heat, but the interested parties told me that there was not any such thing, that it was not ponderable, and I of course saw the force of that, and have always since adopted the thermal unit. It is just as easy to say you sell a man a hundred thousand thermal units as it is to

§ 20. In this case there are good reasons. With non condensing engines the most important variations in the cost of the power are caused by simple variations in the ratio of the back pressures to the effective or, better, the total pressures. If this fact be considered, other variations in result from engines of the same general size and design, using steam of the same quality, will be almost entirely due to differences of mechanical condition. The relative effective pressure is provided for perfectly in the formula. The basis in all cases is the cost of the power absolutely without expansion—the initial pressure being assumed equal to the mean and back pressures. Expansion simply furnishes an indicator diagram of the *same* area, with one portion made higher in order that the other can be lower and reduce the cost, though not proportionally to the lowering of the pressure.

§ 21. The multipliers *a* in table § 41 of Appendix are calculated for the values of k_p in same table with the mean pressure *m* equal to about 30 pounds for the cut-off engines, and about 40 pounds for the others. The comparative results given by formula for a fair-sized engine which would be customarily rated at 30 to 40 H.P. considered first as a slide-valve engine, and second as as a cut-off engine, for different mean pressures and constant back pressure, above atmosphere of 2 pounds ($n=m+19$) are shown in the following table:

MEAN PRESSURE (*m*) AND INDICATED HORSE POWER (P).	5	20	40	80
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§ 29. The following calculations apply to a specimen elevator, with double 8 × 10 cylinders, in regular use in New York:

$W =$ say 1200 pounds. $T = 200$
$L =$ 75 feet. $b = 2.5$
$r_2 =$ 100 revs.

Then

(11) $m_{11} = 13.125$
(12) $k_t = 19.1$
(13) $k_2 = 3820.$

A trip is usually made in $\frac{7}{12}$ minutes, so in § 41, $r = 171.4$.

(4) $P = 11.5$ ascending.
3.5 descending.

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k_t being 19.1, $k = \frac{12}{7}$, $k_t = 32.7$, $k_1 = 98.1$, $k_p = 130.6$.

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§ 34.

Steam Pressure.	Kals per sq. foot of heating surface per hour.
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§ 41.	*HP.*	*a.*	k_p.
Cut-off engines..................	40 and up. 20 10 5	.6 .7 .8 .9	30 35 40 45
Slide valve engines...............	30 and up. 15 5	.85 1.00 1.10	40 45 50
Elevator engines, new, good order.........................		1.1	55
" " small and old..........................		1.2	60
	DIAM. STEAM CYL.		
Compound pumps................	20″ and up. 16″ 5″	.75 .85 1.00	35 40 45
Ordinary steam pumps............	20″ 15″ 12″ 8″ 5″	1.0 1.1 1.2 1.3 1.4	50 55 60 65 70

n = multiplier varied from $m + 17$ up under conditions given in table, to allow for probable back pressure.

§ 42. $n_2 = m + 21$ for quick elevator engines.
$n_3 = m + 20$ for small fast engines and small pumps.
$n_4 = m + 19$ for engines with free exhaust.
$n_5 = m + 17$ or $p + 17 =$ minimum; useful only in calculations where back pressure is not considered.

(To distinguish $p + 17$ from $p + 14.7$ see § 16.)

§ 43. (4) $P = .000\,004\,(d^2 s r)\,m.$

$$(5)\quad m = 250\,000\,\frac{P}{d^2 s r}.$$

$$(6)\quad k = .000\,0021\,(d^2 s r)\,a\,n.$$

$$(7)\quad k_m = .0021\,(d^2 s)\,a\,n.$$

$$(8)\quad k_p = 31.5\,\frac{a\,n}{m}.$$

APPLICATION OF THE FORMULA.

1st Case.— *When the mean pressure* (m) *is known.*

§ 44. Ascertain the power from (4) and its cost from (6), (7) or (8) as required.

§ 45. (*A.*) The mean pressure can be accurately ascertained with the indicator, and this is the only method when engine is fitted with a fixed cut-off.

§ 46. (*B.*) When the engine operates intermittently the estimate may be made on an assumed mean pressure of say 40 pounds and a price given per thousand revolutions based on results from (7) with $m = 40$, the contract stipulating that the power is not to exceed that given by (4) with $m = 40$.

§ 47. (*C.*) The mean pressure may be derived from the pressure in steam chest. For accuracy, the ratio of the mean pressure to pressure in chest should be ascertained by the indicator for each particular engine, and this ratio used as a factor of a. For practical purposes, with slow engines—like pumps—using steam full stroke, or nearly so, with cylinder ports of ordinary size, m is to be taken equal to pressure in chest, using value of a, as per table, and n_5. For quick engines, deduct one-tenth from tabulated value of a, and use n_5 with $m =$ pressure in steam chest, which will apply to slide-valve engines at all times; to automatic cut-off engines, when regulated by throttle with governor belt thrown off, but not to engines with fixed cut-offs. The last rule is on the basis that the loss of pressure entering cylinder is balanced by using a lower value of *a*.

2d *Case.*—WHEN THE POWER IS GIVEN WHICH IS TO BE DEVELOPED IN AN ENGINE OF A GIVEN SIZE.

§ 48. (*A.*) Find *m* from (5) and cost from (6), (7), or (8) as required.

§ 49. *Note.* All contracts for H. P. actually developed will name the minimum charge for operating the engine. This will be stated on surveys and ascertained from (6), (7) or (8) as required by making $a = 1$ and $m = 0$ in value of *n* used in the other calculations, and adding also 1 to 3 pounds for friction pressure.

HYDRAULIC ELEVATORS.

§ 50. If D = dia. of hydraulic cylinder in inches,
d = as before dia. of steam cylinder in inches,
d_1 = dia. of pump cylinder in inches,
S = stroke of hydraulic piston in feet,
s_1 = stroke of pump in inches,
T = number of trips of elevator per day,
l = height in feet between water tanks,
r_D = number of double strokes of pump per day,
f = multiplier = 1.25 for large pumps, say steam piston 20″ dia., increased to 2.0 as steam piston decreases to 10″ dia.

§ 51. Then: (9) $m = 434\frac{d_1^{\,2}}{d^2} f\, l$

$$(10)\ r_D = \frac{6\, D^2\, S\, T}{d_1^{\,2}\, s_1},$$

from which k_2 may be obtained from (6) by substituting k_2 for k and r_D for r.

§ 52. *Note.* In this calculation only one pump is considered. If the pump be duplex the actual strokes will be half those calculated by formula but the steam used will be the same. [N. B. In calculating power or cost for duplex pump the reading of a counter on one pump should be doubled.]

§ 53. For compound pumps refer *m* only to the large cylinder. The size of the small one does not enter the calculation.

STEAM ELEVATORS.

§ 54. Let W = unbalanced weight of car and load.

L = lift.

m_{11} = mean pressure in each cylinder of double engines.

r_2 = number of revolutions of engines required to run car from bottom to top of shaft, or *vice versa*, (to be counted by Solicitor).

k_t = Kals per trip.

T = number of trips per day.

$$b = \begin{cases} 1.75 \text{ for geared elevator engines.} \\ 2.50 \text{ for screw elevators and old geared elevator} \\ \quad \text{engines with piston valves.} \end{cases}$$

§ 55. Then for double engines—

$$(11) \quad m_{11} = \frac{3.82\, b\, W\, L}{d^2\, s\, r_2}$$

Multiply by 2 for single engine.

$$(12) \quad k_t = .000\,00504\, (d^2\, s\, r_2)\, (m_{11} + 46.)$$

Divide by 2 for single engine.

$$(13) \quad k_2 = T\, k_t.$$

STEAM PUMPS.

§ 56. For ordinary steam pumps raising water in a building, m, may be found from (9), and the pump considered as run a certain number of strokes per minute for a certain number of hours per day, as reported by engineer and confirmed by the practice in other buildings. If a certain sized circular tank is filled a certain number of times per day, (10) applies by making D = diameter of tank, S its depth, and T the number of times it is filled per day, and then proceeding as described in connection with (10).

HEATING WATER.

§ 57. For water heated from 60° to boiling point.
Kals. = No. gallons × $1\frac{1}{3}$ = No. pounds × $\frac{1}{6}$ nearly.

STEAM JETS.

§ 58. Charge for open steam jets discharging against atmospheric pressure.

d_0 = diameter of jet.

15 lbs. steam pressure kals. per hour, $k_1 = 1216\, d_0^2$
45 " " " $k_1 = 1804\, d_0^2$
60 " " " $k_1 = 1978\, d_0^2$

Calculate for pressure of 60 pounds except when jet works off a heating system at lower pressure, or when the full opening of valve would drive employees from the room.

DISCUSSION.

Mr. Kent.—I do not believe our code of rules describes any punishment for a member that does a wrong action. Mr. Emery has done a very bad thing, and he deserves our condemnation for it. He has invented a new unit—a "Kal." The punishment I think he will get, however, will be that the next time Mr. Nystrom writes a book and puts in that list of engineering terms which ought not to be used, he will put in the word "Kal." It seems this term is to be defined as a pound of water evaporated into steam. We have had enough trouble about the units of weight and measurement, and here we introduce a new word to take the place of pounds. A man says, "For how much will you sell me a boiler that will give seventy pounds of steam?" That is his way of rating the power of a boiler—and to get over the difficulty that such customers have of mixing up pounds pressure and pounds weight or quantity, it is proposed to introduce this term "Kal." If you want to sell steam, and do not want to sell it by the pound, why not sell it by the thousand feet or by the gallon or cubic foot of water evaporated, or by some other known unit, instead of coining a new term?

Mr. Emery says I am mistaken about this Centennial standard. No doubt I am, since he says so, for I have not looked at the Judges' Report for a couple of years; but my impression was that it was from 212 degrees feed water into seventy pounds pressure, and I have the impression that I got that figure from some of Mr. Emery's own writings, if I did not get it from the Judges' Report. At the bottom of the second page of his paper Mr. Emery says the seventy pounds pressure had been accepted before the Judges made this Report, but the evaporation was generally assumed from 212.

Mr. Emery says in regard to the objection to the use of this term "Kal" to mean a pound of water evaporated from and at 212, that it should be founded on the evaporation from a fair average steam pressure, and from a fair average temperature of feed. He used the same argument concerning the amount of water that should be evaporated for a horse-power—that it should be from 100 degrees, and not from 212, because 100 degrees was more nearly a fair average. The only fair way of determining the horse-power in steam boilers, as in everything else, is to get some definite standard of measurement. He says here, "The unit of evaporation is now accepted to be the evaporation of a pound of water from and at 212 degrees." That is generally accepted by engineers as a fair unit, and it is all right; but it is not an average. If it was necessary to get an average, we would make some other unit of evaporation, such as the evaporation of a pound of water from 100 degrees temperature, at 70 pounds pressure. There is no more reason for calculating the economy in a boiler test from and at 212 than there is for the use of 30 pounds of feed water from and at 212° to indicate a horse-power. We ought to refer to one standard, and not have three or four. If 30 pounds is too little, make it 40 or 35. I think that 30 pounds of water from and at 212° per horse-power is a fair standard, when we consider that some engines use 18 or 19 pounds, while others use a hundred, and that in non-condensing engines from 25 to 30 pounds of water from 212° is reached, and I do not see why 30 pounds is not large enough as a standard. But I protest against the use of too many units. I believe we should start with the British thermal unit as a basis. That is a standard definitely known and understood. The unit of evaporation should be just what Mr. Emery puts it here—965.7 thermal units. It is the evaporation of one pound of water from and at 212 degrees. A horse-power should be a definite number of units of evaporation—say 30. If 30 is too small a number, let the committee of this Society on Rules for Boiler Tests fix it at 35 or 40. Let it be 965.7 thermal units to a unit of evaporation, and 30, 35 or 40 units of evaporation to a horse-power, and wipe out the term "Kal" and all other new units for measuring steam. [Applause.]

Mr. Leavitt.—Mr. President, I think the simplest proposition is to come down to thermal units. I once had a great deal of trouble with a report where I attempted to adopt an arbitrary standard. It seemed to be a very just and fair one, as I assumed a standard pound to represent the water, which contained 966 units of heat, but the interested parties told me that there was not any such thing, that it was not ponderable, and I of course saw the force of that, and have always since adopted the thermal unit. It is just as easy to say you sell a man a hundred thousand thermal units as it is to say you sell him a hundred "Kals," and personally I should prefer the expression "Thermal Units."

Mr. Chas. E. Emery.—Any unit proposed must be based on the "heat unit." It would of course be correct to sell steam direct by that unit, but it is so small a subdivision that many places of figures would be required, and, being a scientific term, few would understand it.

Mr. Kent.—I do not wish to be understood as objecting to the term "unit of evaporation." I said distinctly, let us have a table based upon the thermal unit as a standard. The thermal unit is the basis to which we all must come. Then, as a convenient larger unit, based on the thermal unit, have the "unit of evaporation" equal 965.7 thermal units; then the next larger unit is the horse-power, that shall be some definite number of units of evaporation, say 30, and not a certain definite number of something else. Mr. Leavitt, Mr. Emery and I will all agree in this thing. It is simply a question of what we shall make the horse-power—how many units of evaporation.

Mr. Emery struck the key-note in this business in saying that the "Kal" was invented so that people would not understand it. It is a very convenient term, so that they will not know how many tons of coal make a "Kal" when coal is five dollars a ton. [Laughter.]

Mr. Chas. E. Emery.—True, and in selling steam to some people it is desirable to have something they do not understand. [Laughter.] The true sentiment was expressed by the Vice-President of the Mutual Life Insurance Co., who said, "I don't know what a 'kal' is, but I suppose there are others who do know." [Laughter.] In short, a distinctive term is necessary, the meaning of which can be definitely ascertained.

Mr. Cole.—Mr. President, speaking of this term "Kal," while we may as engineers weigh and consider, and even decline to accept it, I would say that, after some experience years ago with the class of persons that Mr. Emery is dealing with as consumers, it probably saves a good deal of engineer's and office time to have something these parties cannot argue against. You say thermal units, and they will talk with some friend, and come back the next day and say that you have made a mistake; that that thing is not right; that it ought to be a different price, and they do not like it. But when they take the word "Kal" home, that is something that they look in vain through the encyclopædia and the dictionary for, and they explain it as the President of the Insurance Co. did to his friend, by saying that that is the way steam is sold. A similar term was invented years ago—"Tanite." The party that invented it said it was designed so that people would not know what it meant. But here the customer will certainly criticise, and think he knows more about the price of steam than the engineer of any company, unless you use some term which he does not know how to shoot at. [Laughter.]

Mr. Le Van.—Suppose the customer is an engineer?

Mr. Cole.—Then he can take the data which Mr. Emery furnishes.

Mr. Chas. E. Emery.—I will reply briefly to the various criticisms. The term "Kal" has been in use in New York City for over a year and one-half. It is thoroughly understood there, and probably no change will be made. It is to be expected that the introduction of a new term will meet with opposition, but it is not proposed here in a dictatorial way. All I ask is that it be used alternatively or referred to from time to time, no matter how lightly, when in the long run it will gradually be adopted or fall into disuse, and thus its convenience and value will be tested by actual practice.

I should like to see the Society settle the question as to what should be considered as a horse-power in terms of the steam required. If 30 pounds of feed water are to be used as a horse-power, then 30 pounds should actually produce a horse-power in average practice, and it will so far as the engine is concerned. The only difficulty is that the boiler in order to be of the same power must evaporate 30 pounds of feed water *under actual conditions.* For a lower temperature of feed more heat units are required from the fuel, and the boiler must do more work. A horse-power of 30 pounds of feed water evaporated at 70 pounds pressure from a temperature of 100 degrees is equivalent to 33.4 pounds of feed water evaporated from 70 pounds and 212 degrees, or 34.5 pounds from atmospheric and 212 degrees. The evaporation of a boiler may be expressed in units of evaporation, viz.: In multiples of 965.7 heat units [representing the heat absorbed from the fuel by the evaporation of a pound of water at atmospheric pressure and temperature of 212 degrees], but as engines do not work at atmospheric pressure and the temperature of feed water is rarely 212 degrees, we must select another unit for the power of the engines to approximate actual conditions. In the paper 1110.2 heat units are suggested, as this number represents the heat required from fuel for a pound of water evaporated at 70 pounds pressure from a temperature of 100 degrees. This is the unit which was termed in the Centennial Report the "unit of commercial evaporation," and now proposed to be called a "Kal." In this connection it is important to consider the unit to be used, not the name. If the evaporation be considered from 70 pounds and 212 degrees, when the feed is 100 degrees, the boiler would have to supply 33.4 such units—called pounds of water evaporated per horse-power—while the engine and boiler would be actually using by tank measurement only 30 pounds of water per horse-power. Could confusion be worse confounded? This illustration shows why in the "Centennial Report" and in the paper I have proposed a separate unit.

CXLIX.

NEW YORK TO CHICAGO IN SEVENTEEN HOURS.

BY W. BARNET LE VAN, PHILADELPHIA, PA.

In this paper it is proposed to show how the distance between New York and Chicago can be covered in seventeen hours via the Pennsylvania, and Pittsburgh, Ft. Wayne, and Chicago railroads. The distance by this route is nine hundred and eight miles, as follows:—

	Miles.		Minutes.
New York City to Philadelphia, Mantua Station....	88.26	in	100
Philadelphia to Harrisburg........................	103.07	in	114
Harrisburg to Altoona........................	131.6	in	144
Altoona to Pittsburgh...........................	116.7	in	128
Pittsburgh to Alliance............................	83	in	91
Alliance to Crestline..............................	106	in	117
Crestline to Ft. Wayne...........................	131.39	in	144
Ft. Wayne to Chicago...........................	148	in	162
Total miles..................................	907.91		——
Total time in minutes..................................			1,000

Crossings at grade via in Ohio......................	16
" " " " Indiana....................	10
" " " " Illinois....................	8
Total number of crossings at grade.................	34
Time lost by slowing down according to law.................	20
Total number of minutes consumed.........................	1,020
Total time of run to Chicago in hours......................	17.00

The Pennsylvania Railroad has been selected, because that company controls the best equipped, most direct route between the two cities mentioned, and possesses the further advantage of having tenders fitted with a "pick up" apparatus for supplying them while running with water from troughs placed between the rails.

To accomplish the distance in the time named, is with the company only a question of additional safety-gates, so as to keep the track clear through the large towns and cities scattered along the route.

The route is divided into eight sections, necessitating the use of eight locomotives. (This is, however, on account of the partition of the road into Superintendents' Divisions, and not from the necessity of changing by reason of wear on the locomotives.) On the Western Division of Pennsylvania Railroad, however, two locomotives will be needed in crossing the Alleghany Mountain.

At Philadelphia, instead of running into Broad Street Station, a locomotive and passenger car will be in waiting at Mantua, and take the place of the locomotive and car-passengers for Philadelphia only. At the other stations on the route, the passengers can be changed in the time occupied in changing locomotives.

The ability of the locomotives of the Pennsylvania Railroad Company to perform this journey will be seen by the following indicator diagrams:

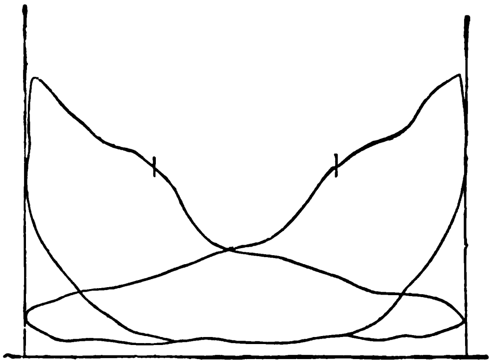


FIG. 58.—CUTTING OFF AT 7 INCHES, BOILER PRESSURE 135, 55 MILES PER HOUR.

Diagram figure 58 was taken when running at the rate of *fifty-five miles an hour*, cutting off after the piston had traveled seven inches, with a boiler pressure of one hundred and thirty-five pounds per square inch, an average initial pressure of one hundred and twenty-one and one-half pounds at commencement of the stroke, and eighty-four and one-half pounds at point of cut-off, and eight pounds average back pressure.

Diagram figure 59 was taken when running *sixty miles an hour*, cutting off at seven inches, initial pressure one hundred and nineteen and one-half, and eighty-one pounds pressure at point of cut off, averaging eight and one-half pounds back pressure.

Diagram figure 60 was taken when running *sixty-four miles an hour*, boiler pressure as before one hundred and thirty-five pounds per square inch, initial pressure one hundred and twenty-seven and one-half, and eighty-four pounds at point of cut-off, averaging six and one-quarter pounds back pressure.

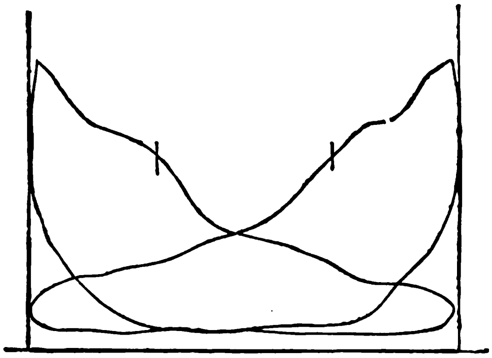


FIG. 59.—CUTTING OFF AT 7 INCHES, 60 MILES PER HOUR.

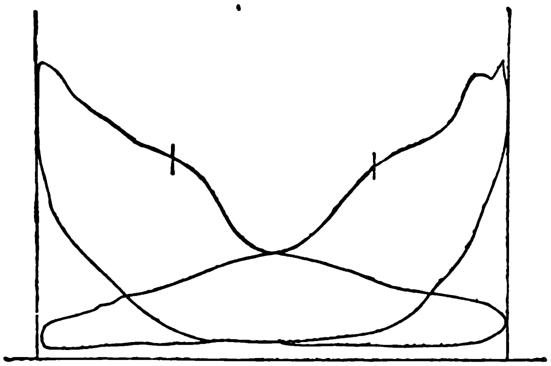


FIG. 60.—CUTTING OFF AT 7 INCHES 64 MILES PER HOUR.]

The dimensions of these locomotives are as follows:

Diameter of cylinder in inches	18
Diameter of piston rod in inches	3
Area of piston less one-half arc of piston rod in square inches	251
Length of stroke in inches	24
Diameter of drivers in inches	78
Capacity of tank in gallons	1,920
Capacity of coal box in pounds	12,000
Weight of tender loaded in pounds	56,800

Weight of locomotive in working order:

On truck in pounds	27,400	
On first pair of drivers in pounds	33,600	
On second pair of drivers in pounds	31,700	
Total weight in pounds		92,700

The tractive force exerted for each pound of effective pressure per square inch on the piston is:

$$\frac{18^2 \times 24}{78} = \frac{324 \times 24}{78} = 99.7 \text{ pounds.}$$

The water tank, as before stated, is fitted with Ramsbottom's water-lifting apparatus for taking in a supply of water while running.

To accomplish 908 miles in seventeen hours, the average miles run per hour must be *fifty-five miles.* Therefore, as the locomotive must be able to exceed the average number of miles per hour required for this purpose, we will take diagram Figure 3, whose average mean pressure is 40.3 pounds per square inch, and 276 revolutions per minute, averaging 677 horse-power.

$$HP = \frac{18^2 \times 0.7854 \times 1104 \times 40.3}{33000} \times 2 = 677 \text{ horse-power.}$$

It is an every-day occurrence at intervals on the Pennsylvania and Bound Brook route to average, for short distances, *seventy miles* an hour, in fact often a mile in *forty-five seconds,* or at the rate of *eighty miles* an hour. Therefore it is not a question of capacity of either the boilers or engines, it is simply a clear track and a disposition of the company to order it done.

It is evident from the indicator diagrams shown that the boilers are superior to the engines. The diagrams show only an average of *sixty-five ver cent.* of the theoretical diagrams, while diagrams from stationary engines of similar capacity with automatic cut-off show an average of *ninety per cent.* This difference is due to the use of the link motion in locomotive engines. The scant opening which it gives when cutting off at *six* to *eight* inches is one of its most prominent defects, as a great part of the actual boiler power is expended in forcing the steam through the narrow openings, but partially uncovered by the valve, whereby a loss of over *thirty per cent.* of effective motive power is the result.

By the substitution of a separate cut-off valve, similar to that adopted by Mr. A. J. Stevens of the Central Pacific Railroad, this great loss could be overcome and there would be a great saving of fuel. This substitution would cost about $300 for each engine, and about *thirty-three per cent.* additional working power would be gained by it.

DISCUSSION.

Mr. Chas. E. Emery.—As the paper does not appear to excite discussion, I will say that I am much obliged to Mr. Le Van for showing that locomotives now in use can be run regularly from New York to Chicago in 17 hours, simply by keeping the road clear, but I predict that no advantage would be obtained by the application of a separate cut-off to these engines, as suggested in the last clause of the paper. The cylinders of a locomotive being proportioned for starting a train are altogether too large to operate with economy at the higher speeds. A mere inspection of the diagrams will show that if the initial pressure had been held to a point of cut-off which would have continued back the same expansion curves, and represented therefore the same theoretical steam consumption, such point of cut-off would have been short for best economy. Moreover, the shorter cut-off would produce more cylinder condensation; this would be intensified if cushioning were partially omitted, as for the same actual steam consumption the cut-off would be altogether too short for economy, and evidently the claim that there would be a gain of about *thirty-three per cent.* additional working power does not appear to have been well considered. The fact is, that the application of the much abused link motion and lap-valve to locomotives has effected important savings in two directions. 1st, in reducing wear and tear by extreme cushioning, and 2d, by practically reducing the size of the cylinders at high speed by wire-drawing, early release and cushioning. Many master mechanics are even increasing the wire-drawing by reducing the ports, claiming a saving thereby. This has been done for years on the Boston and Albany road, and the only explanation of the result when false valve faces are applied to old engines, so that no saving in clearance spaces is made, is that it is a device to bring down the engine power to that of the boiler, more efficient than the throttle, simply because it is nearer the cylinders. I recommend reducing the size of the cylinder instead of the ports, thus making engines a little more difficult to start but much more economical at high speeds. On the freight engines of the Central Pacific road independent cut-offs have produced economy, simply because the speed is so low that the supply of steam required for the cylinder development is not greater than can be supplied by the boilers. A well-known builder of stationary engines at the East held the same views as Mr. Le Van about the locomotive, and after I had advised him to abandon the subject, for reasons previously stated, he acknowledged that he had some time previously got permission to apply independent cut-off valves in upper separate chests to a passenger engine, though he was not allowed to meddle with the main valves, and had found to his great surprise that he could do no better with his attachments in use than when cut-off was produced with the link motion.

Mr. Leavitt.—Mr. President, some of the members may remember Mr. Holley's speech at the Hartford meeting on the Jigger. The Jigger was a locomotive built by Mr. Corliss, which had all the elements of success in the way of economy of fuel and distribution of steam. She would go over the road and take a great train, probably, quicker than anything they had, but the trouble was that it took a corps of machinists all night to put the machine in order for her work the next day. [Laughter.]

Mr. Le Van.—My object in reading the paper was to show that the present locomotives, such as are in general use on the Pennsylvania Railroad, could make the run to Chicago in *seventeen hours* over their present road-bed.

By the addition of a separate cut-off valve in connection with the shifting link motion, the present locomotives could be further improved as to capacity and economy. In a locomotive with sixteen-inch cylinders fitted with an independent cut-off valve arrangement, with our present size of boilers, the same amount of power could be produced which is now developed with eighteen-inch cylinders fitted with shifting link motion only.

The average consumption of coal per train mile on our best roads is over fifty pounds; in England thirty pounds is the average; an independent cut-off valve arrangement would reduce this between twenty and thirty per cent. independently of the gain of power.

I do not believe there is an engineer in the room who would like to design and build an engine for stationary use which would produce indicator diagrams such as those illustrated in this paper, and would admit that he admired such a performance.

Mr. Leavitt, in referring to Mr. Holley's criticism of the locomotive built by Geo. H. Corliss, was correct, but due consideration must be given to the time at which it was built. We must recollect that this engine was entirely a new departure, both in stationary and locomotive practice. He employed four valves, all operated independently of each other, and a liberating valve motion for his steam valves, which for high speeds does not answer; a positive movement similar to the Buckeye, Porter-Allen, and others, complies with the requirements. Independent cut-off valves are not new in locomotives, having been used many years back with great success, but on account of their complication, as it was thought, under the known state of art they were discarded.

But in the present state of the art, what engineer, manufacturer, or corporation would object to a Leavitt compound pumping engine, or a Worthington duplex pump, a Buckeye, Porter-Allen, or Corliss engine, or a stem-winding watch with a fifth second hand, on account of a complication of parts? or the additional care necessary to keep them up to their standard performance? All these machines were objected to on account of complex arrangement, but they have for years worked economically, satisfactorily, and without giving trouble.

The satisfactory running, and the economy in the use of fuel, of the Buckeye, Corliss, and other cut-off valve arrangements on locomotives have never been questioned.

Mr. Emery's argument is that the use of a separate cut-off valve, in bringing about a sharp cut-off, would be the reverse of economical unless smaller cylinders could be used, and that this was impracticable because the large cylinders were required in starting the train. He forgets that my proposition is not to dispense with the link, but to retain the link and add a variable independent cut-off, as I consider the link motion the readiest and simplest means of reversing the locomotive, and its functions of cushioning can be retained.

Mr. Kent.—Mr. Le Van states in his paper that the running of such trains depends simply on a clear track and a disposition of the Company to order them run. A clear track can easily be obtained, if needed. The disposition of the Company not to order it done must be based upon some reasons. I wish Mr. Le Van would explain what those reasons are. I thought also there was some question about extra friction on locomotives, and wear—a question of repairs.

Mr. Le Van.—No, sir.

It would necessitate, as I have stated, the erection of additional safety-gates at all crossings at grade, especially in the large towns, and to be always prepared to have their roadway clear and their locomotives promptly on hand, so as to lose as little time as possible in making the changes. To do this it would cost more money to maintain such a train, and the question arises whether or not a sufficient number of passengers would be willing to pay the additional charges which would be demanded to maintain such a train. My opinion is that there are, and always will be, a sufficient number of passengers, to whom *time* is of *more* importance than the extra charges which would be made in money. One of the best evidences is the paying success of the present "New York and Chicago limited" trains, which make the time in twenty-six and one-half hours. The Pennsylvania Railroad have the ability. It is only a question of dollars and cents; the friction is reduced to those terms, and the merchantable question of "Will it pay?" is the real question.

Added since the meeting.

Mr. Emery has referred to instances in which the ports of locomotives have been reduced beyond the ordinary practice; so far as I am able to learn, the results of such reduction in the ports have been unsatisfactory.

The matter of theory as to expansion of steam *vs.* throttling of steam has been conclusively settled in marine and stationary engines in favor of expansion, as demonstrated by the *practice* of intelligent engineers of to-day, although the theory in favor of throttling is as good to-day as ever.

The question is not as to whether *in the abstract* expansion or throttling upon locomotives is best, but is not expansion beneficial in a large part of the operations now controlled by throttling; the point is the best practical means of achieving desirable results, and a comparison of the cost of such means with the value of the results, and not whether any scientist's theory is perfect or at fault.

I contend that there is at command in the present practice of the generation of steam on railways the means of getting better results, both in time and extent of load carried, by more economical applications of steam, and that such results are attainable with a change of valve and construction of the engine, so that more of the force of the steam may pass to the pistons and cranks instead of being wasted in friction by passing through constricted openings between the valve and ports, and I am not assuming too much when I declare that this view of the case is deserving of most careful and serious consideration.

CL.

COMPARISON OF THREE TYPES OF MODERN INDICATORS.

BY GEO. H. BARRUS, BOSTON, MASS.

The investigations on three types of modern indicators, recently conducted by the writer, and forming the subject of this paper, consist of a brief analysis of the principal errors to which indicators are liable, and more especially an examination of the effect of the varying strain on the driving cord produced by the action of the paper drum mechanism. The writer was incited to these experiments by the publication of some diagrams taken with the "Brown

FIG. 67 INDICATOR A.

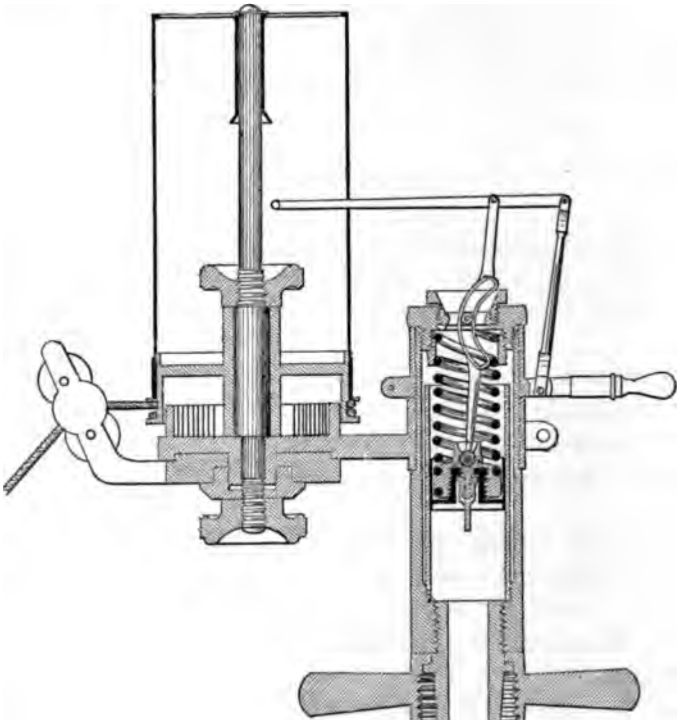


FIG. 68 INDICATOR B.

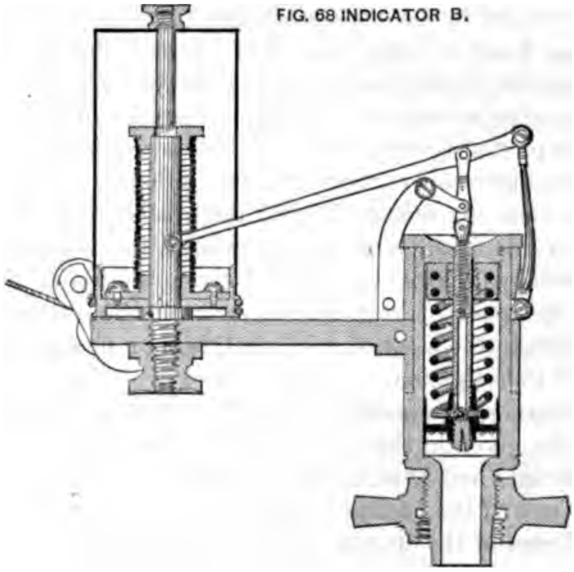
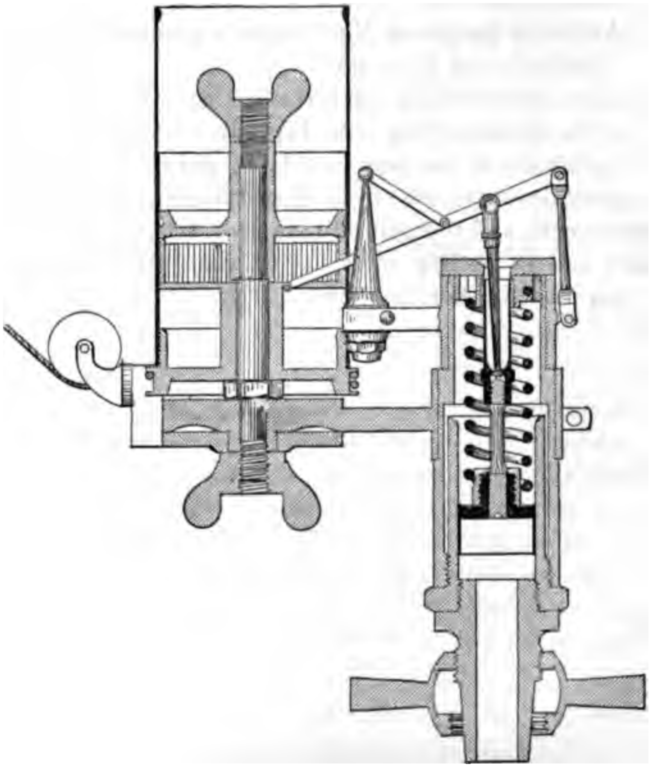


FIG. 69 INDICATOR C.



Device," similar to the one used in this investigation, and from the same three kinds of indicators. The inference drawn from the diagrams was, that the mechanism of the paper drum and drum spring of an indicator had an important effect, and the statement was made that the errors produced in the form of indicator diagrams, by defects in the drum mechanism, were the most serious to be found in indicator practice. It seemed highly improbable that several years' practice in this field, on engines of widely varying sizes, and running at widely different speeds, had given the writer no suspicion as to the existence of such serious errors in the instruments which he had been in the habit of using, but, being a new problem, it was worthy of investigation.

The several instruments are treated under the various heads, and in the order given in the following list of subjects:

1. Weight and Dimensions of Parts.
2. Form of the Indicator Diagram.
3. Errors of the 50-pound Indicator Spring.
4. Parallelism of the Pencil Movement.
5. Lost motion in the Pencil Movement.
6. Action of the Drum Mechanism in producing Distortions in the Indicator Diagram.

The indicators on which the investigations were made are represented in the accompanying cuts, Figs. 67, 68 and 69, and designated by the letters of the alphabet A, B, and C. The cuts are sectional views, drawn to scale from measurements taken directly from the instruments, and reproduced here half size. The instruments were new and in perfect order at the time of the investigation, which was made in April and May, 1884.

1. Weight and Dimensions of Parts.

Table No. 1 gives the weight of the essential parts of each indicator, expressed in grammes, together with the essential dimensions, expressed in centimeters. [1 gramme = 0.0353 oz. avoirdupois; 1 centimeter = 0.394 in.]

TABLE NO. 1.—WEIGHTS AND DIMENSIONS.

	A.*	B.	C.
1. Weight of piston	10.735	10.155	20.75
2. Weight of all the rods to which the piston is attached, and constituting the pencil movement collectively	10.685	12.925	11.162
3. Weight of pencil arm, including metallic point	1.928	1.980	2.900
4. Weight of metallic point	.122	.130	.170
5. Weight of indicator spring	23.584	14.030	29.971
6. Weight of back link	2.748	3.550	2.804
7. Weight of paper drum	80.900	41.540	87.140
8. Weight of drum carriage	100.450	35.031	205.800
9. Weight of drum spring	25.790	34.783 (including fittings.)	44.100
10. Weight of the complete instrument	1075.00	658.50	1402.00
11. Diameter of drum	5.15	3.86	5.08
12. Height of drum	10.15	6.98	9.52
13. Number of times the piston motion is multiplied	4	6	4
14. Diameter of wire in drum spring of Indicator B (Helical form)	—	.124	—
15. Cross-section of steel in drum spring of Indicators A and C (spiral form)	.954 x .053	—	.962 x .046
16. Number of coils in drum spring	8	22	13
17. Length of drum spring	79.95	116.49	145.18
18. Total length of indicator spring	5.25	4.19	6.01
19. Diameter of wire of indicator spring	.20	.20	.28
20. Number of coils of indicator spring between supports	6.6	7.0	7.4

Comparing these figures, it appears that the paper drum of Indicator B is much smaller than those of A and C. It is similar in size, however, to the small drum, A, referred to in the foot-note. The relatively large multiplication of the piston motion is noticeable in the case of Indicator B. The various weights are in some instances very different. For example, the collective weight of the paper drum, drum carriage and drum spring in Indicator C, is 3.03 times that in B, and the collective weight of the piston and indi-

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* The weight of the paper drum in the Indicator A having a small drum, referred to in Table No. 4, is 51.5 grammes. Its diameter is 3.68 c. m.; its height 6.60 c. m.; and the weight of the complete instrument 824 grammes.

Fig. 70.

DIAGRAM FROM INDICATOR A.

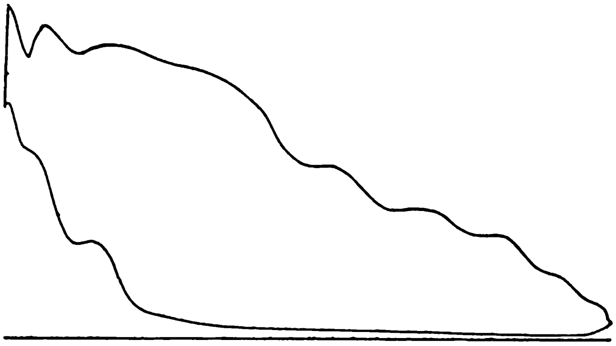


Fig. 71.

DIAGRAM FROM INDICATOR B.

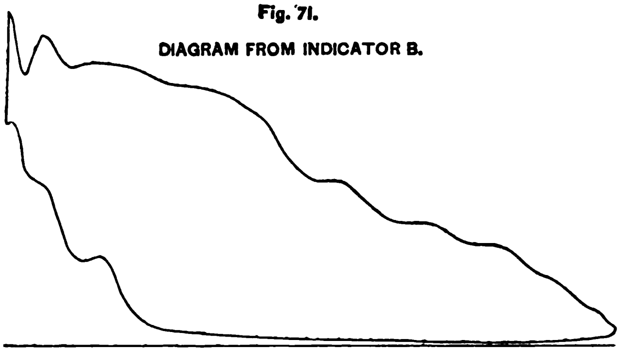
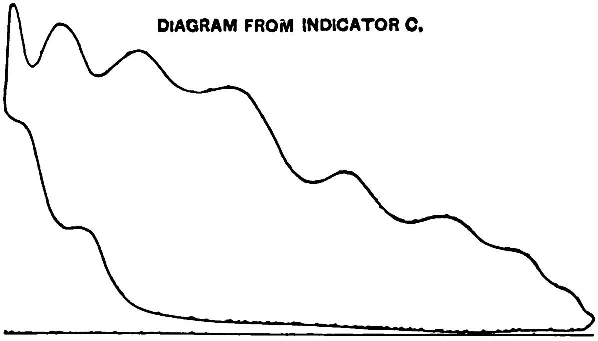


Fig. 72.

DIAGRAM FROM INDICATOR C.



cator spring in C is 2.1 times that in B. Indicator B takes the lead among the three instruments in having the least collective weight of the parts connected with the paper drum, and in having the least weight of piston and indicator spring. Indicator A takes the lead among the three in having the lightest pencil arm, and the smallest collective weight of pencil movement.

2. Form of the Indicator Diagram.

The instruments were applied, one at a time in rapid succession, to one end of a high-speed Armington and Sims engine, making 310 turns per minute. The test was made at the central station of the Brush Electric Lighting Company of Boston. The size of the cylinder of the engine is 9½ inches diameter by 12 inches stroke. During the tests it was employed in driving dynamo machines carrying a steady number of lamps under a steady pressure of steam of 85 pounds per square inch. The mechanism for driving the indicators consisted of a reducing lever and segment attached by a connecting rod to the cross-head. The indicators were placed on the end of the cylinder furthest from the crank. The driving cord was of braided linen, about $\frac{3}{32}$-inches diameter, having a length between paper drum and segment of 36 inches. Prepared paper and a metallic marking point were employed. Sixteen diagrams were taken in each case with a 50-pound indicator spring. One of each set representing a fair sample is reproduced full size in the accompanying illustrations, Figures 70, 71, and 72.

As far as the eye can judge from these diagrams, there appears to be little difference in the operation of the Indicators A and B. There are the same number of wavy lines in these two cases, occurring at the same points of the diagram, and having practically the same amount of curvature. The diagram from Indicator C has similar features, but the curvature of the wavy lines has more amplitude. Here may be seen the evident effect produced by the comparatively heavy weight of the pencil movement.

3. Errors of the 50-pound Indicator Spring.

Each indicator was tested for errors of the spring, under various steam pressures, the instruments being applied, one by one, to a testing apparatus, and compared with the same standard gauge. The indications on these tests measured with a 50 scale, are given in Table No. 2.

TABLE NO. 2.—ERRORS OF SPRINGS.

GAUGE INDICATION.	A.	B.	C.
10	9.4	9.5	9.4
30	29.6	29.1	30.1
50	50.4	49.1	50 .9
70	71.6	69.6	71.1

From these measurements the average error in the mean effective pressure of the diagrams represented by Figures 70, 71, and 72 have been found to be as follows:

A.	B.	C.
.86 lbs. too high.	.75 lbs. too low.	.57 lbs. too high.

4. Parallelism of the Pencil Movement.

The springs were removed from the indicators, and the pencil movement operated in each case by holding the instrument to the mouth, and alternately blowing in and drawing out the air under the piston. At the same time the pencil point was applied to a blank card on the paper drum, and a line traced with the drum at rest. The results are reproduced full size, in the accompanying Figures 73, 74 and 75. A straight dotted line is given in each case, side by side, with the full line traced by the instrument, to show more readily to the eye the amount of departure. According to the maker's rating, the vertical range of A is 64% of the length here shown, that of B 55%, and that of C 74%. Within the specified range, the lines are all practically straight.

5. Lost Motion in the Pencil Movement.

In continuation of the tests of parallelism, each instrument was held so that the pencil arm stood nearly vertical. A weight of four ounces was suspended from the pencil point, and a line traced upon a blank card by gently moving the pencil along with the finger. The instrument was then inverted and held so that the weight might take up the lost motion in the opposite direction, and another line traced as before. The distance by which the two lines are separated represents the amount of lost motion. The results are reproduced, full size, in the accompanying Figures 76, 77 and 78. Indicator C shows the least defect in this respect. The other two show defects of about the same extent, one being due principally to lost motion in the curved slot, and the other to yielding of the numerous joints situated in the line of the piston rod, and to excessive multiplication.

6. Action of the Drum Mechanism.

DIAGRAMS SHOWING PARALLELISM.

Fig. 73 A.

Fig. 74 B.

Fig. 75 C.

In order to show the fact that differences in the type of drum spring, and in the design and weight of parts connected with the drum, effect the strain on the driving cord of the indicator, each drum was tried with the Brown drum-testing device, an instrument represented one-fourth size in Figure 79.

It consists essentially of a spring, *a*, of the type employed in Indicator B, having a nominal scale of 20, which is interposed between the driving cord of the drum and the cross-head and guide, *b*, which gives it motion. The motion of the free end of the spring

DIAGRAMS SHOWING LOST MOTION.

Fig. 76 A.

Fig. 77 B.

Fig. 78 C.

is communicated to a pencil point, *c*, through a multiplying bell crank lever, *d*. The pencil point marks on a piece of paper placed on the board, *e*, which for this purpose is swung into the required position. The distance from the centre of the paper drum of the indicator, which is attached by the clamp at the right-hand end to the hook on the spring, when the cross-head is at the centre of its range of motion, is 19 inches. This was tried in each case under two different tensions of the drum spring, and two different speeds. The number of turns of the cap which holds the end of the spring, starting with a position of no tension in each case, is given in Table No. 3. This table also gives the pull in pounds required to move the drum at each end of its range of motion, which is the true indication of the tension of the spring.

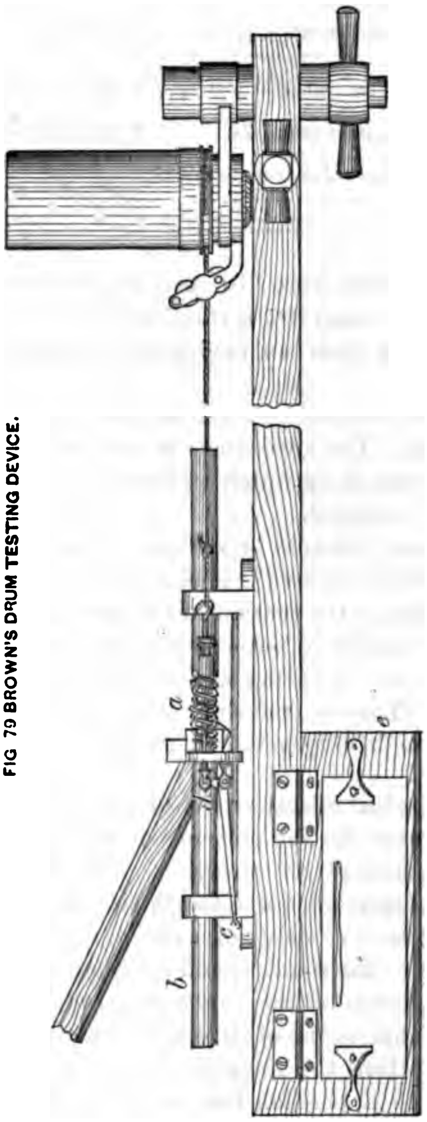


FIG 79 BROWN'S DRUM TESTING DEVICE.

TABLE NO. 3.—TENSION OF DRUM SPRINGS.

	A.		B.		C.		
AMOUNT OF TENSION.	LOW	HIGH	LOW	HIGH	LOW	ME'M	HIGH
1. Tension in turns of cap, when at rest.....	0.25	1.25	0.50	1.00	1.30	2.60	3.60
2. Weight required to start drum, lbs.......	1.12	2.75	0.62	1.25	2.37	2.50	3.12
3. Weight required to revolve drum to end of its motion, lbs......................	2.87	3.50	2.00	2.50	3.12	3.50	4.25

The speeds selected were 310 revolutions per minute, the same as the engine speed, and 395 revolutions per minute. The driving motion was taken from an engine-lathe properly rigged for the purpose.

The diagrams obtained on these tests are reproduced, full size, in Figures 80 to 90. The lower lines in each case were traced when the instrument was in operation without connection with the driving cord of the indicator.

These diagrams, it should be noticed, show the *fact* that a difference in the stress on the cord is produced by variation in the kind of drum mechanism, in the tension of the spring, and in the speed of working; but it should be borne in mind that the effect is increased by the presence of the spring and multiplied by the lever of the Brown device. They do not show the actual change produced in the length of the driving cord, and they give no indication of its amount.

To show the actual effects produced, each paper drum was tried on a device planned for the purposes of this investigation, by Mr. E. H. Gowing, assistant to the writer. The instrument, in essential features, is represented one-fourth size in Figure. 91.

The Gowing device consists of a rod, *a*, working through suitable guides attached to the stand, *e*, and having a reciprocating motion, transmitted by means of the connecting rod, *c*, and cross-head, *b*. The rod, *a*, extends to the other end of the stand, and there carries the pencil, *d*. Here the indicator is clamped to the stand, with the drum in such a position that it lies parallel to the rod, and directly beneath the point of the pencil. The screw, *f*, adjusts the vertical position of the pencil, and enables it to be brought into contact with the drum, as is done in the act of working the instru-

DIAGRAMS TAKEN WITH BROWN'S DEVICE.
SPEED 310 REV. PER MIN.
INDICATORS VERTICAL.

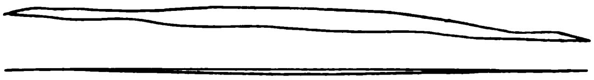


Fig. 80. A.
Low Tension

Fig. 81. A.
High Tension

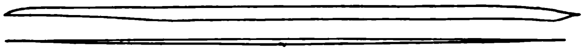


Fig. 82. B.
Low Tension

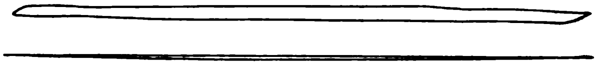


Fig. 83. B.
High Tension

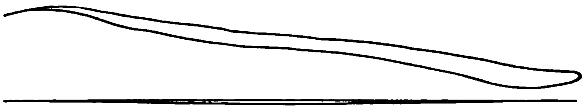


Fig. 84. C.
Low Tension

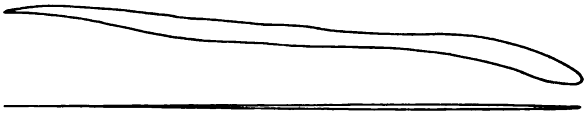


Fig. 85. C.
Medium Tension

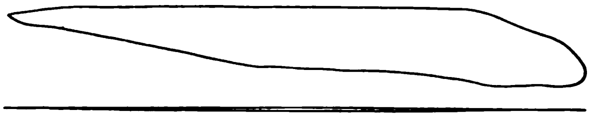


Fig. 86. C.
High Tension

ment. The driving cord of the indicator, after passing over the carrier pulley, is attached to a hook on the cross-head, and thus receives its reciprocating motion.

It will be readily understood that the line traced on the surface of the drum, when the apparatus is at work, will assume, in general, a diagonal course across the rectangular card on which it is drawn. If the motion of the cross-head is accurately transferred through the cord to the drum, the line is straight, and it has

DIAGRAMS TAKEN WITH BROWN'S DEVICE
SPEED 395 REV. PER MIN.
INDICATORS VERTICAL

Fig. 87 A.
High Tension

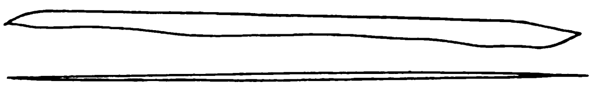


Fig. 88 B.
High Tension

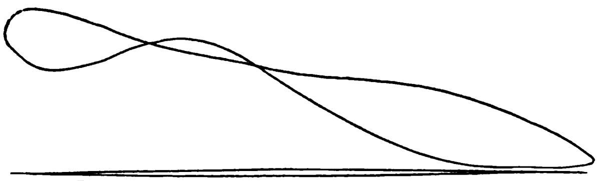


Fig. 89 C.
High Tension

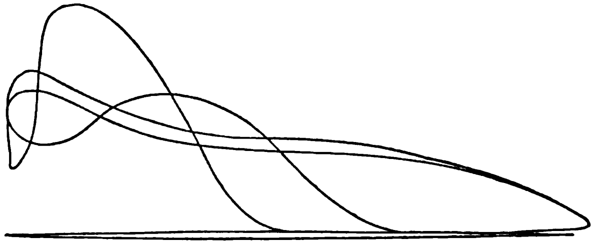


Fig. 90 C.
Medium Tension

the same location whether the motion be forward or backward. The effect of variation of strain on the cord, is to lengthen or shorten it, thereby retarding or accelerating the motion of the drum and displacing the position of the line. The displacement occurs in the direction of the motion of the drum, and its amount

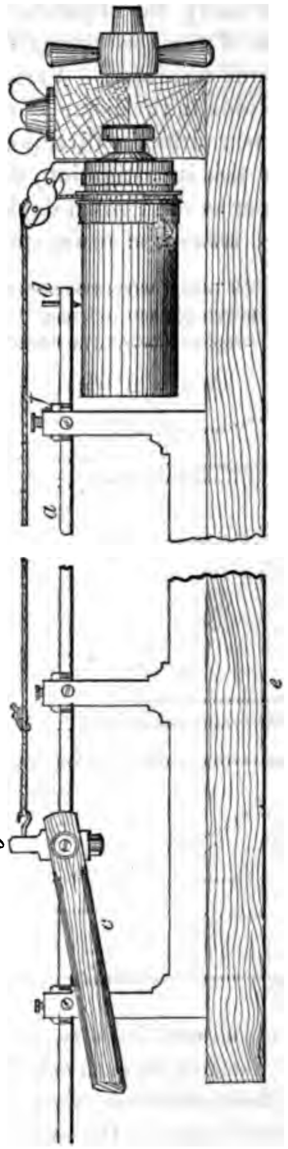


FIG. 91 GOWING'S DRUM TESTING DEVICE.

any point represents the actual error at that point produced by fects in the drum mechanism.

In working the apparatus, a base line is drawn with pencil at st, and drum in motion. Then a diagram is drawn with both in otion, at slow speed, or under conditions which give the least variation of strain. Finally, the apparatus is worked at the speed to be tested, the length of the cord being first slightly changed, so as to obtain the diagrams separated. Variations in the strain are indicated by the variations in the distance of the slow diagram from the speed diagram measured in the direction of the base line.

For the purposes of this investigation, the instrument was operated in the same manner as the Brown device, and under the same conditions of speed and tension of drum springs.

DIAGRAMS TAKEN WITH BROWN'S DEVICE
SPEED 395 REV. PER MIN.
HIGH TENSION - INDICATORS HORIZONTAL

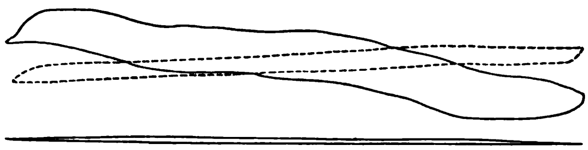


Fig. 92 A.

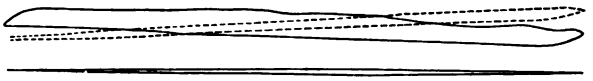


Fig. 93 B.

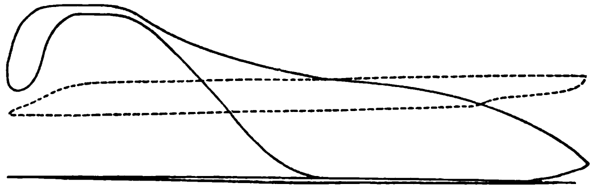


Fig. 94 C.

The diagrams are reproduced full size, in the accompanying Figures 95–104, the slow diagram in each case being beneath the speed diagram. Preceding them are some taken by Brown's device, Figures 92–94, with the indicators in the same horizontal position, as that occupied during the tests with the Gowing device. In the tests with the Brown device already considered, the cord passed directly to the drum, 36 inches distant. In the remaining tests it passed at right angles over the carrier pulley of the indicator, and the distance was 24 inches. Preliminary trials showed a marked effect produced by friction of the carrier pulley, when not sufficiently

DIAGRAMS TAKEN WITH GOWING'S DEVICE.
SPEED 310 REV. PER MIN.
HIGH TENSION · INDICATORS HORIZONTAL.

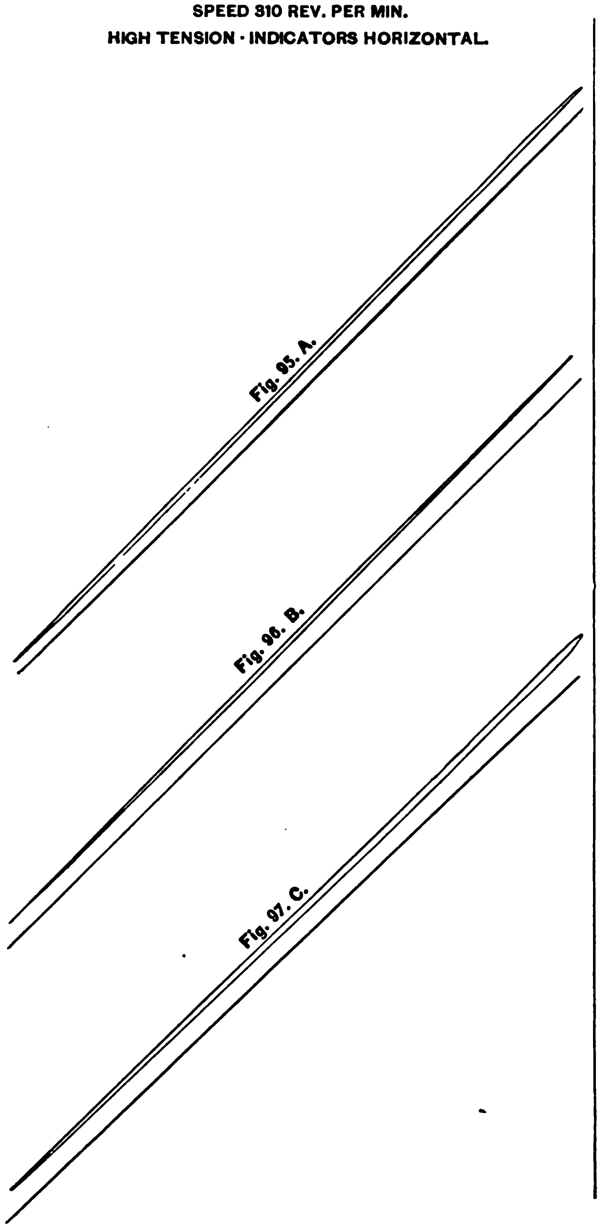


Fig. 95. A.

Fig. 96. B.

Fig. 97. C.

DIAGRAMS TAKEN WITH GOWING'S DEVICE.
SPEED 310 REV. PER MIN.
LOW TENSION - INDICATORS HORIZONTAL.

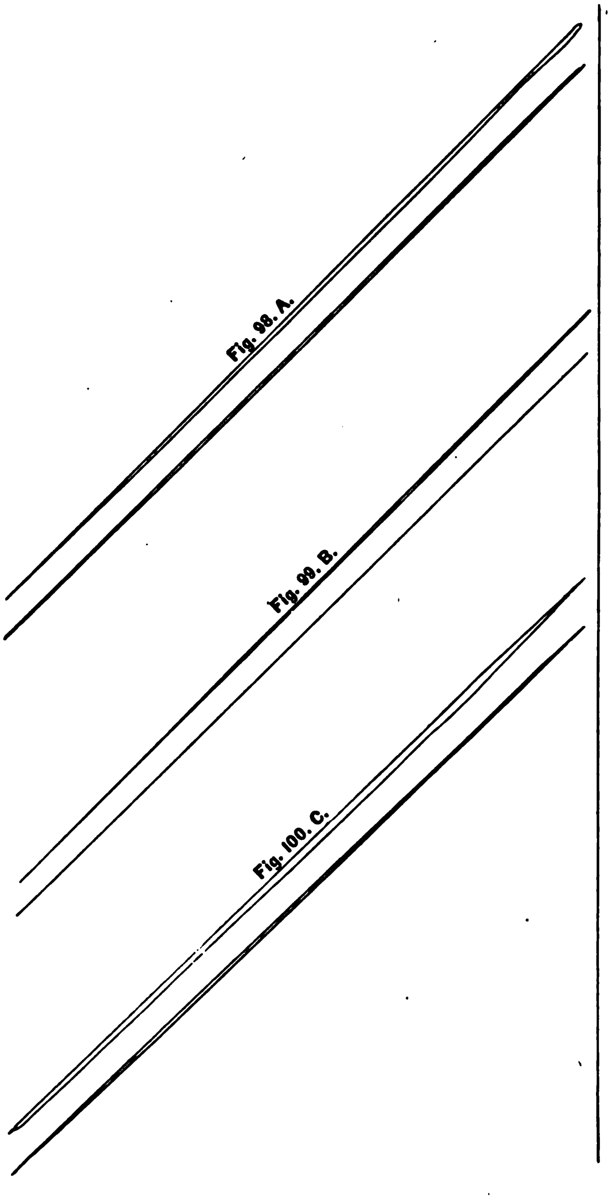


Fig. 98. A.

Fig. 99. B.

Fig. 100. C.

DIAGRAMS TAKEN WITH GOWING'S DEVICE.
SPEED 395 REV. PER MIN.
HIGH TENSION - INDICATORS HORIZONTAL.

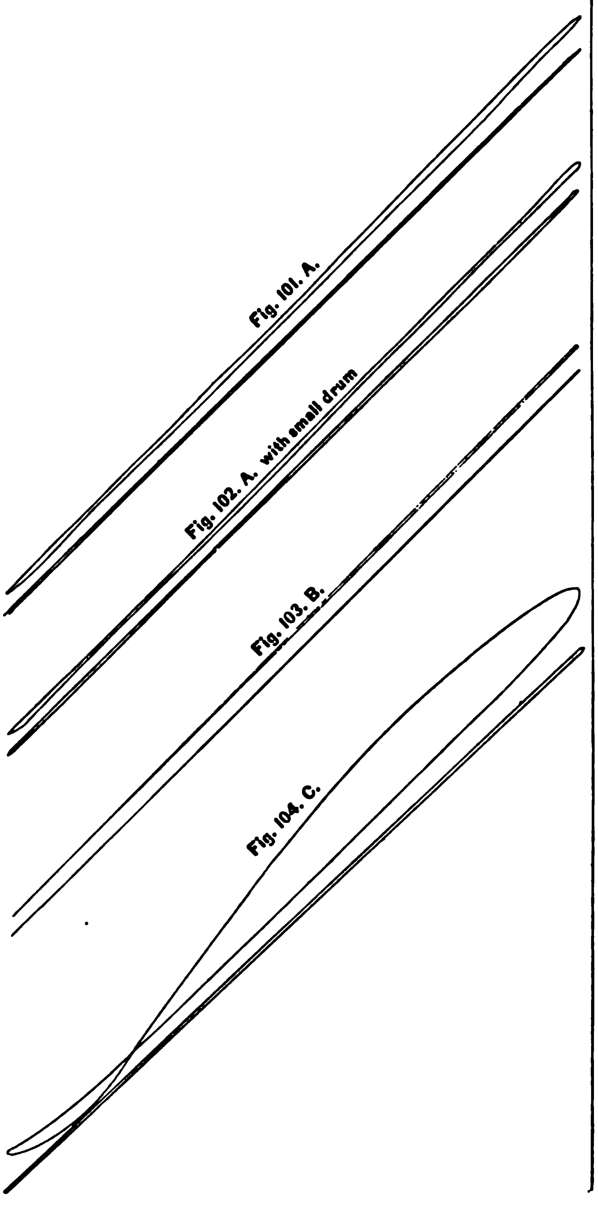


Fig. 101. A.

Fig. 102. A. with small drum

Fig. 103. B.

Fig. 104. C.

lubricated. On all the tests the same kind of cord was used as that employed in taking the engine diagrams.

The results of the trials with Gowing's device may be best understood by referring to the following measurements, given in Table No. 4, expressed in inches.

1st. The length of the *slow* diagram in the direction of the drum motion.

2d. The difference between the length of the *slow* diagram, and that of the *speed* diagram, representing the total distortion or stretch, produced by defects in the drum mechanism.

3d. The greatest distance by which the lines of the *speed* diagram are separated, measured in the direction of the drum motion, representing the friction of the drum mechanism under speed.

4th. The greatest distance by which the lines of the *slow* diagram are separated, measured in the direction of the drum motion, representing the friction of the drum mechanism at slow speed.

TABLE NO. 4.

ACTUAL DISTORTION PRODUCED BY DEFECTS OF DRUM MECHANISM.

	A.	B.	C.
LOW TENSION, LOW SPEED.	In.	In.	In.
1. Total length, slow	2.96	2.96	2.84
2. Total stretch produced at speed	.06	.04	.08
3. Distortion by friction at speed	.025	.015	.035
4. Distortion by friction, slow	.01	.005	.01
HIGH TENSION, LOW SPEED.			
1. Total length, slow	2.96	2.96	2.84
2. Total stretch at speed	.04	.03	.07
3. Distortion by friction at speed	.03	.02	.05
4. Distortion by friction, slow	.015	.005	.02
HIGH TENSION, HIGH SPEED.			
1. Total length, slow	2.96	2.96	2.84
2. Total stretch at speed	.06	.03	.18
3. Distortion by friction at speed	.05	.015	.39
4. Distortion by friction, slow	.02	.005	.02
INDICATOR A WITH SMALL DRUM. HIGH TENSION, HIGH SPEED.			
1. Total length, slow	2.96		
2. Total stretch at speed	.04		
3. Distortion by friction at speed	.03		
4. Distortion by friction, slow	.02		

These measurements show that among the three standard indicators, the least total distortion is produced in the case of indicator B, and the greatest in that of indicator C. Expressed in percentage of the total length of the slow diagram, the total stretch ranges in A, from 1⅓% to 2%; in B, from 1% to 1⅓%; and in C, from 2½% to 6%. In the case of C at high speed, it was apparent on the trials that the chosen speed was beyond the limit within which the drum mechanism in this case was adapted. It does not properly compare, therefore, with the other cases.

A noticeable feature in the results obtained from indicators A and C, is the excessive friction shown by the *slow* diagrams. It appears to be caused by the rubbing of contiguous coils of the drum spring upon each other. A considerable length of the spring is coiled closely upon itself, with no space separating one coil from the next. The results given at the bottom of the table, obtained from a second indicator A, having a drum of similar size to that in B, shows the same defect, but otherwise slightly better action. In indicator B the contiguous coils do not touch, and the friction shown by the *slow* diagram is reduced to a minimum.

There remains to be considered the effect of these errors of drum motion on the engine diagram. The examination is confined to the effect produced on the mean effective pressure. The diagrams, of which Figures 70, 71 and 72 are samples, were carefully measured with a planimeter, and their lengths taken, with the results given in Table No. 5. The first eight diagrams in each case were taken with low tension on the drum spring, and the last eight with high tension. The measurements of all the diagrams are given to show that the load on the engine was practically constant, during the time any one indicator was in use.

TABLE NO. 5.

MEASUREMENTS OF ENGINE DIAGRAMS.

Number.	Area enclosed in Diagram. sq. in.	Length of Diagram. in.
Indicator A.		
1	2.51	3.13
2	2.53	3.14
3	2.50	3.13
4	2.50	3.13
5	2.51	3.13
6	2.52	3.14
7	2.54	3.13
8	2.51 Av. 2.515	3.13 Av. 3.133
9	2.50	3.14
10	2.51	3.13
11	2.52	3.14
12	2.51	3.13
13	2.47	3.13
14	2.51	3.13
15	2.51	3.13
16	2.52 Av. 2.505	3.13 Av. 3.133
Indicator B.		
1	2.48	3.18
2	2.48	3.18
3	2.51	3.18
4	2.50	3.19
5	2.48	3.18
6	2.48	3.19
7	2.51	3.18
8	2.52 Av. 2.495	3.18 Av. 3.183
9	2.45	3.18
10	2.46	3.18
11	2.49	3.18
12	2.51	3.18
13	2.44	3.18
14	2.48	3.18
15	2.50	3.18
16	2.49. Av. 2.477	3.18 Av. 3.18
Indicator C.		
1	2.43	3.09
2	2.39	3.08
3	2.43	3.08
4	2.43	3.08
5	2.45	3.09
6	2.41	3.08
7	2.41	3.08
8	2.43 Av. 2.423	3.08 Av. 3.085
9	2.42	3.07
10	2.39	3.07
11	2.42	3.07
12	2.44	3.07
13	2.43	3.07
14	2.40	3.06
15	2.37	3.06
16	2.42 Av. 2.411	3.06 Av. 3.066

The mean effective pressures in pounds per square inch, computed from the averages of these results, and corrected for error of indicator spring given in Table No. 2, are as follows:

	Low Tension.	High Tension.
Indicator A.	39.78	39.62
Indicator B.	39.95	39.89
Indicator C.	38.73	38.75

These quantities cannot be analyzed without allowance for the possibility of a change of load on the engine during the period while the tests were being made. The record in Table No. 5 shows no material change while any one indicator was under test, and this is a strong indication that the load remained virtually constant from one test to another. Taking the figures as they stand, it appears that the indicators A and B give nearly identical results. Indicator C shows some 2½% variation. This variation may be due in part to the distortion in the drum motion, but it is no doubt due quite as much to the effect of the momentum of the comparatively heavy pencil movement. The difference produced by the change of tension on the drum spring appears to be immaterial.

The results which have been tabulated show for themselves, but it may be said in review that the *serious* faults claimed to be produced by drum mechanism appear *wanting.* In careful and intelligent hands, any one of the three indicators may be relied upon for substantial accuracy. Competition in the manufacture of indicators has been active in this country, and it has had the usual tendency to induce improvements. If the results of this investigation prove an incentive to still greater advances for securing more accurate instruments, its object will have been realized.

DISCUSSION.

Mr. Porter.—Mr. President, my experience inclines me to think that undue importance is attached to the stretching of the diagram at excessive speeds. I have repeatedly taken diagrams on the same sheet—first at a very slow motion, and then at a very rapid motion, with the same instrument, without moving anything, and compared them. The diagrams taken at a very rapid motion will be considerably longer at each end—elongated each way—than the one taken at a very slow motion. But a careful examination of the two diagrams, setting them out, computing the power, shows that the real distortion in their shape amounts to very little. So far as I have been able to judge, I think these very trifling percentages of distortion are really practically of no account whatever That is the way it strikes me. These reciprocating movements of the drum are very rapid in those cases, almost 400 revolutions of the engine being reached in one case—400 reciprocations each way. The parts are made as light as possible, and it is a matter of degree. We must have some strength in the drum. The drum must hold the paper with some firmness, and it must be capable of maintaining its position upon the arbor with precision, and I fancy that care has been exercised in the construction of all these instruments in the highest degree, and the utmost lightness consistent with first-rate work has already been attained, and I do not think we need give any consideration to that matter. It will always be found as we increase the speed of the engine the diagram is a little elongated, with any instrument; but I do not think it is distorted in any degree.

It is very pleasant to see such a point as this raised, however. It shows the degree of attention that the subject is receiving, and the real thoroughness of the thought that is given to it, and extending to such a point as that which years ago would have passed without any notice whatever.

The really serious points with respect to the instruments, however, are their accuracy as instruments of precision in actually recording the pressures and indicating the pressure at every point in the stroke, the accuracy of the spring, the correctness of the area of the piston, and their permanent correctness, so that year after year it shall be the same thing, and the freedom from friction in the action of the indicator that should check its action at the top or bottom of the scale, or anywhere on the scale. Those are really the vital points in the construction of the indicator, and, as I understand the experiments of Mr. Barrus, the errors observed in the instruments in this respect are very trifling indeed.

It often happens that an instrument tried for friction when the piston is not moving, being set in the steam-chest or on the end of the cylinder, and the steam turned on without the piston and the engine moving, throwing the pencil of the indicator up, then drawing a line by hand with the spring, that that will not be accurate, because the piston stands motionless for quite a while, and a very little thing will check its movement to the precise point. But when the engine is in motion, the piston moving freely and rapidly, then there will be no such hindrance to its action. I have observed that to be the case many times. So that it is very wrong to form a judgment against an indicator because its action is a little impeded when the piston is in this stationary condition.

Mr. Hand.—I would like to ask if there have ever been any instruments made to determine how much and in what way the diagram was distorted by the friction of the pencil on the paper.

Mr. Porter.—I think I can answer that question, sir. The effect of friction is always found to be to limit the range of motion in both directions, upward and downward, so that less than the boiler pressure will be shown, the cut-off later than it was, and a greater back pressure than really exists, or a less perfect vacuum than really exists. For example, those lines on the expansion curves represented are perfect lines. If there is an angle in the line, that is produced by friction ; but when, as in these cases, the line is a flowing line, perfectly free from angles, that shows the action to be entirely frictionless, and the mean of the vibrations will always be the true, real expansion curve of the steam.

Mr. Hand.—I know that the wavy lines are caused by friction, but I want to know whether any one has ever separated the friction caused by the rubbing of the piston in the cylinder from the friction of the pencil on the paper. I want to get the two separated.

Mr. Porter.—I can answer that question. It is a very common practice, so far as I know, among engineers, after taking the diagram to remove the pencil from the paper a little way, and watch it and see if it retraces the line.

Mr. Hand.—That would depend something on the angle at which a man looked at the pencil [laughter].

Mr. Porter.—Oh, yes, sir. It all depends, you know—everything depends [laughter]. But that is the way to tell whether the line has been affected by the pressure of the pencil on the paper—be sure your point does not touch the paper, keep it so close to it that you can see that it is retracing the line precisely, and then you know. It is very satisfactory to know that the pressure of the pencil on the paper has not affected the diagram.

Prof. Robinson.—In regard to that question, I think that we might get an approximate result, to say the least, by pressing the pencil quite hard upon the paper and getting one diagram, then pressing very lightly upon the paper, assuming that the engine would make the same diagram if it had the chance.

I think a true or perfect indicator is one which has no mass of reciprocating parts, or no weight of reciprocating parts. I think that is fully as evident as that a perfect screw can be made after the manner described in the paper read by Professor Rogers.

It seems to be impossible, however, to employ reciprocating parts without weight, and this will involve certain dynamic force which will cause a modification of form of the diagram taken. To free the indicator from dynamic forces it will be necessary to reduce the reciprocating parts of the indicator to absolutely no weight. It is evident that it is impossible to do this with the ordinary scheme of an indicator, that of having a piston, a spring, and connections for carrying the pencil. We can greatly reduce the weight of the piston and other parts, but the spring, however, has absolutely to have some certain weight, and according to the statements in the discussion on spiral or helical springs, it would seem that the weight of the spring would have some relation to its stiffness. In other words, for the indicator to have a spring that shall do its work, and that will stand the pressure which is brought to bear upon the piston, there must be a considerable weight of spring. If we reduce the area of the piston, we reduce the weight of spring, because we reduce the acting force, so that there seems to be a relation between the force acting upon the piston and the mass of matter in the spring to be overcome by the action of the pressure upon the piston, thus necessarily involving dynamic force. So that it is impossible to approximate even to a near result in eliminating dynamic force, because if we reduce the pressure upon the piston one half, we may reduce the weight of the spring—I am not quite certain whether the law is for that to be one half, but it would be reduced. I am not prepared to state what the law would be, whether the law runs parallel or not. According to the formulæ given, however, by the reader of the spring paper, the volume is to be in definite relation to the work performed in compressing the spring. If that be true, we can then predicate that the weight of the spring would be proportionate to the area of the piston. The stroke would perhaps modify that somewhat, although the longer the stroke the lighter the spring might be. I am not prepared to carry that point out fully.

If Professor Webb were here, I think he would be able to describe an indicator in which the effect of mass would be very nearly totally eliminated.

I would allude to one point in regard to the effect of the weight of the barrel in modifying the length of the diagram. I would like to put the question whether in obtaining those figures that are given us in the paper, these figures were obtained from a given horizontal scale of stroke, or from the length of diagram as unit of horizontal scale. I think the true way would be to measure the diagram in each case; that is, for obtaining the nearest approximation to the true value of indicator area, take the length of the diagram produced by the indicator to represent the stroke of the engine. It strikes me that if this has not been done, if it should be done, these figures would come nearer to an agreement. I presume, however, Mr. Barrus has made that clear in his paper.

Mr. Le Van.—I think the Society ought to thank Mr. Barrus for the care and pains he has taken in making the trials. But he has not gone far enough. The difficulty, I think, is, he has taken these diagrams at different intervals of time, whereas they should have been taken simultaneously. There is a great chance for an error to creep in which no one man can prevent.

Mr. Hand.—I have no hesitation in saying that I never expect to know as much about the indicator as these gentlemen here, but, at the same time, I am very much interested in it, and for the past year have been experimenting to find a way to photograph the lines on the paper, and thus make a diagram instead of using the pencil. As the members here seem to have some feeling for those who do not always meet with success, I will not hesitate to make an example of myself by saying that I think I have bitten off a larger piece than I am able to masticate [laughter].

My object was to get a diagram without pencil friction, and find the distortion due to such friction. This is what led me to ask the first question.

Mr. Porter.—Before the Richards indicator had been adapted to use at higher speeds, I was often in the habit of taking an observation that will be interesting as a better answer than I thought of before with respect to this matter of the effect of the friction of the pencil point upon the form of the diagram. Running the Richards indicator at a pretty high speed, cutting off very short and very sharp on the expansion curve, the pencil would be thrown, when it did not touch the paper at all, into a state of extreme vibration. If we touch the paper very lightly indeed, we get a delicate line showing these vibrations. Bear a little harder, and those vibrations are reduced. Bear harder still, and they are reduced still more, and with a little more pressure we get a line with very slight waves in it, which line approximates very nearly to the mean of the most ample vibrations, showing that the latter line was nearly correct, and showing also that the friction of the pencil on the paper did not make the line untrue, because we could compare it with the mean of the large vibrations drawn through both lines.

Mr. Hand.—Taking a light line and then taking a heavy line would of course show the comparative friction. Am I right in supposing that you are to take the mean between the two?

Mr. Porter.—No, sir, the true curve, which is the mean of the most ample vibration, will also be the mean of the smaller vibrations.

Mr. Hand—I will state further that I have tried that on an indicator, making as light lines as the pencil would make on the paper, using the special pencil that is sent with one form of indicator, and I have also tried heavy lines. I had also two or three fine camel's-hair brushes, which I dipped into India ink, and then put in the pencil-holder. I can assure you there was a wide discrepancy developed between the diagram made by the pencil and the one made by the brush. I do not know anything about this thing, but I would like somebody to try it and tell me what it is. I am asking for information on this point. I am here to learn something.

Mr. Kent.—How would it do, when you are indicating engines, to have at each end of the engine two indicators, of different makes? Take the diagrams simultaneously. Then place the two diagrams one on top of the other. If they agree, they are right.

Mr. Le Van.—Mr. President, I have taken quite a number of diagrams in that way, but I did not like to speak of it, because I had not the indicators nor diagrams here to show; but it has been my usual custom in indicating engines to use two indicators, often of different makes. An assistant would be placed at each instrument, and all diagrams are taken simultaneously.

Mr. J. F. Holloway.—Mr. President, I desire to protest against this idea of putting any more than one indicator on an engine. The fact is, that people who put one indicator on an engine can tell nothing at all about what the engine is doing. What will they be able to do when they put a half a dozen on? [Laughter.]

Mr. Chas. E. Emery.—I wish to thank Mr. Barrus for the interesting paper he has presented. The errors pointed out were known before, but he has made a full line of experiments to ascertain their exact value for different instruments under different conditions, and the published results render improvement possible without extra labor in the same direction. A brief statement of my experience with indicators may be of interest.

Ten to fifteen years ago I had probably the privilege of taking more indicator diagrams than any one in the country. The Government experiments at the Novelty Iron Works, New York, terminating in 1868, required the continuous use of the instrument for several years; the subsequent experiments for that firm continued its use, as well as the well-known experiments on the U. S. Revenue Marine and U. S. Coast Survey steamers. The "McNaught" form, with piston and pencil having the same movement, was in general use in the U. S. Navy at the beginning of the civil war in 1861, and the first one I used had no paper drum, but instead a flat board traversed in both directions by a positive connection through levers with the main piston. This antiquated arrangement therefore did not have the errors due to the stretching of a string referred to in the paper. The more common form of the McNaught instrument had the well-known paper drum, but could not be used satisfactorily above fifty revolutions per minute, though I got fair diagrams from gunboat engines at eighty revolutions by letting the top of the piston rod of the instrument strike a shoulder on a stick adjusted to control the extreme initial impulse, which, more than anything, caused a series of waves throughout the whole diagram. My principal experience afterward was with the Richards instruments, which operated very satisfactorily when well made, at least up to 150 revolutions per minute. Mr. Porter had by this time shown the practicability of high-speed engines, and I made a series of the Novelty Iron Works experiments which showed that an increase of speed produced economy by a reduction of cylinder condensation, as was published in the Novelty Iron Works circular of 1869, and reproduced by Prof. Trowbridge.*

The three indicators here discussed have all been developed since that time. The indicator lettered C, was the immediate successor of the Richards, and was the one ordered for Revenue steamers when the manufacture of the old Richards indicator ceased. About a year ago I carefully examined and tested instruments of the type A, and was so much pleased that I ordered a pair for a client. The work and finish of the A, and apparently also of the B instruments are very good,—in fact, a credit to the mechanical skill of our

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* Tables and Diagrams relating to Non-Condensing Engines and Boilers, by W. P. Trowbridge, 1872.

countrymen. The work on the C indicators cannot be so well spoken of, nor that of those so-called Richards indicators formerly made in this country by the same firm. I used their instruments of course for government steamers when there was nothing else to be had without importation, and was asked professionally about ten years ago to overlook the manufacture, but never got time to do it. The several indicators probably have each advantages for special uses. I would prefer to put the C instrument in unskilled hands, as it is a less delicate piece of apparatus. Any good mechanic who takes an interest in the matter can rebuild one so as to do good work within its limits in a short time. The A indicator will need no rebuilding, and will always give satisfaction to any one who can appreciate it. The indicator B is a mechanical gem, but some features, such as the leverage and size of drum, have been carried to extremes to overcome the known difficulties, and being beyond the mean, I cannot find that this instrument does any better work than the type lettered A, even at the highest speeds. I judge principally from results obtained by one of my assistants, who uses all three kinds in our business. He has made a lighter drum with modified spring arrangement for his C instrument, thereby adapting it for much higher speeds than formerly.

Referring to the discussion, our earnest friend, who acknowledged that he had attempted more than he could carry out in photographing the movement of an indicator pencil, made the statement that he knew the vibrations in the lines were due to friction. I do *not* know it. Mr. Porter probably did not notice the statement, as he did not correct it. The effect of friction is to *reduce* the vibrations, and also to change the form and reduce the area of the diagram. Extreme friction will modify the smooth curves and cause abrupt changes of direction readily detected by the practised eye, and easily learned by the simple device of pressing the pencil lightly at one time and again with considerable force on the papers. Every spring connected with a given mass has a definite period of vibration, which explains the vibrations of an indicator pencil. The less the mass and extent of movement and the stiffer the spring, the less the amplitude of the vibrations, and this is the direction in which improvements on the indicator have been made. To keep friction at a minimum, I recommend discarding the special points and the paper with the roughened surface, and using simply a fairly hard bit of drawing pencil on smooth paper adapted to receive the mark. By the latter method slight undulations can be detected when using the most modern indicators at moderate speeds, and they show the instrument to be in good condition. The areas of such diagrams are more nearly correct than when a smooth diagram is obtained, though the difference is trifling when comparison is made of careful work by each method.

CLI.

APPENDIX.

REPORT ON NATURAL GAS FOR INDUSTRIAL PURPOSES.

Presented by a Committee of the Engineers' Society of Western Pennsylvania, and read in joint session with the A. S. M. E. at Pittsburgh, May 20, 1884.

To the President and Members of the Engineers' Society of Western Pennsylvania:

GENTLEMEN—Your committee appointed in January to examine into and report upon the utilization of natural gas, beg leave respectfully to submit the following report:

After prompt organization, we have held a number of meetings, and have visited several prominent manufacturing establishments where natural gas is being used. We have examined into the methods of distributing and regulating the pressure in mains, and the question of municipal control.

We have from the outset taken counsel with the Board of Insurance Underwriters of Allegheny County.

We have to express our grateful acknowledgments to the following gentlemen for courtesy in affording us facilities for the prosecution of our work: President Ford, of the Pittsburgh Plate Glass Works, Creighton Station, Pa.; Messrs. Atwood & McCaffrey, Messrs. Carnegie Bros. & Co., Mr. Howard Morton, Forward Avenue, East Liberty; Mr. Pew, of the Penn Fuel Company; Messrs. Spang, Chalfant & Co., Fuel Gas Co., all of Pittsburgh.

It is now nearly twenty-five years since the first wells drilled into the sand and rocks of Venango County gave origin to the great and steadily increasing petroleum industry, but we have only recently begun to realize that with the petroleum is associated an invisible fuel, which, by reason of its calorific power and the variety of its possible applications, may yet assume a degree of commercial importance comparable to that of petroleum.

I. Natural gas from Western Pennsylvania is, in most cases, a mixture of more or less complex character. The few investigations published during the past few years tend to show that it is essentially composed of the hydrocarbons of the series in chemistry know as paraffins. In an accompanying table are enumerated some of the leading members of this series.

From the table it is evident that the members differ in their relative proportions of carbon and hydrogen. The vapors of these hydrocarbons are heavier as the proportion of carbon is greater. The calorific valnes show the superiority of marsh gas, weight for weight, over all the others. The first three are odorless; among the others the odor is stronger in proportion as the amount of carbon is greater. A remarkable similarity of chemical properties is exhibited by all, and by reason of the strong attraction existing between them, the boiling point of a mixture is always found to be considerably higher than that of its most volatile constituent. They are theoretically the point of departure for the formation of a great number of useful compounds, such as alcohol, chloroform, acetic acid, and glycerine, but, on account of serious technical difficulties, due chiefly to their remarkable resistance to ordinary chemical reagents (paraffin, parum and affinis), they have never yet been turned to practical account. They are not actively poisonous. It should be stated that many of these paraffins are known to exist in several different modifications, differing especially in boiling points. Hence the list of boiling points above given must be understood as merely including the temperatures at which the typical members of the paraffin series pass from the liquid to the vapor state. It will serve to show that what is a gas or vapor in summer may become a liquid in the winter.

In the lower sand rocks of the oil regions occur probably all the members of the series, the less volatile flowing as petroleum, and the more volatile existing in a state of compression ready to escape through every opening.

Natural gas is then a mixture of the most volatile of these hydrocarbons, carrying various quantities of the vapor of the less volatile compounds. The lightest member, marsh gas (so called from its constant occurrence among the products of vegetable decay), is the chief element of the gas likely to be supplied to Pittsburgh. In addition to these, hydrogen, carbonic acid, carbonic oxide, oxygen, and nitrogen are found.

It is stated that C_2, H_4, ethelyne, and other hydrocarbons of the series known as olefines, occur; but positive evidence upon this point is wanting, except in the extreme north end of the oil field. An accompanying table gives a general view of the composition of gas from a number of wells. (See page 357.)

The table illustrates the predominance of marsh gas. Natural gas is usually a little more than one half as heavy as air. The gas from Sheffield, Warren County, has a specific gravity of 0.45, while that from Pioneer Run, if the analysis of Fouqué is correct, must be about 1.5.

As the gas and oil sands all have a slight dip toward the southwest the gas in the southern part of the region is drawn from rock strata which are higher in the geological series than those yielding the gas in Northern Pennsylvania and New York State. If any attempt at a generalization may be made with the few data at disposal, it appears, therefore, that the deeper strata yield in general a gas of higher specific gravity and illuminating power.

Analytical data covering a greater area of gas-producing territory may in the future throw important light upon the interesting question of the origin of gas and oil. The theory which traces both to the sea-weeds of the ancient Devonian sea, which once covered Western Pennsylvania, has been very generally popular. Exhalations of combustible gas have been frequently met with in other countries, although nowhere in quantity comparable with the prodigious outflow from the gas wells of Western Pennsylvania.

In the district Tsien-Luon-Tsing, in China, gas is obtained in large quantity from salt-well borings, and is used in boiling down the brine, and also for illuminating and heating purposes. (*Comptes Rendus*, Vol. XII., page 667.) Some of these borings are 3,000 feet deep, and penetrate carboniferous strata, yielding gas under great pressure. Many openings have been made with the special view to utilizing the gas.

The escape of gas bubbles, which readily take fire and burn, is a common occurrence in strongly saline mineral springs. In the salt mines at Slatina, in Hungary, natural gas escaping from fissures has been utilized for illuminating the mines. This is an unusual instance in which the active component of the terrible enemy of the mines—fire-damp—has been made into a useful servant. Considerable volumes of combustible gas frequently issue from fissures in the well-known "mud-lumps," which form at the mouth of the Mississippi.

II. Wells drilled for natural gas, outside of the oil regions, are of recent date, with a few exceptions. The wells of New Cumberland, W. Va., have supplied gas for more than twenty years for the manufacture of bricks. The East Liverpool wells have been burning twenty-five years, and are still productive. At Beaver Falls, natural gas has been used for six years in a cutlery works, but lately the gas has failed, presumably on account of the wells becoming filled up with either paraffin wax in the pores of the rock, or with an incrustation of salts of lime and magnesia, as it is said they have never been cleaned out since they were drilled. At Erie so many wells have been drilled to the strata of gas rock that it has become partially exhausted. In the oil regions a gas well was looked on rather as a curse than a blessing, and, as most of the wells produce gas as well as oil, and so many were drilled to the same sand or rock, it soon exhausted the supply.

Our city has the advantage of being able to tap three or four prolific gas belts or fields. The Butler County field, which supplies Spang, Chalfant & Co.; the Bull Creek, or Tarentum field, which struck gas at 1,147 feet depth, and supplies the Pittsburgh Plate Glass Company, Pennsylvania Salt Manufacturing Company, and will supply Richards & Hartly's and Chalinor & Taylor's new glass houses, and Godfrey & Clark's new paper mill. The Murraysville, or Turtle Creek and Lyons' Run field, which tapped the gas at 1,337 feet depth, and supplies the gas for the Acme Gas Company, used by the Edgar Thompson Steel Works; the Fuel Gas Company, who furnish the gas to the several mills and glass houses on the South Side; the Penn Fuel Company, who furnish the Union Iron Mills, Park Brother & Co. Limited, Wilson, Walker & Co., Hussey, Howe & Co., Shoenberger & Co., and many other works in the same neighborhood on the Alleghany River. The belt or field in Washington County is the one in which the celebrated McGuigan well is, the gas from which is being piped to the South Side. No doubt other prolific fields will be found to produce gas in the near future.

We have records of depths of different wells in different districts which we thought not best to include in this report.

If small wells are struck on the same belt as large ones, and are not sufficiently productive to be utilized, they should be plugged, as they drain the belt to no purpose. The more durable wells tap the gas-productive strata generally at a greater depth than one thousand feet.

It is a common opinion among those versed in the management of gas wells that the outflow is subject to a gradual diminution tending ultimately to total extinction. Evidence of this is to be found in all parts of the gas territory, where gas wells have been long in use. In many localities, however, there is reason to think that the gradual falling off of the supply of a well is due to the choking up of the pipe by a deposit of salt or paraffin, rather than to the failure of the original source. This is notably the case with the Freeport gas wells.

The following historical facts in regard to the wells drilled by Spang, Chalfant & Co. are of interest in this connection:

No. 1. Has been in use nine years, and is still a good well.

No. 2. Four years in use, still blowing, though with diminished force. Its location is three miles distant from any other gas belt.

No. 3. Yield insignificant.

No. 4. Pressure diminished from 1¼ to 0 in one week.

No. 5. Failed after four years' use.

No. 6. In use six years, gradually failing.

No. 7. Failed after five years' use.

No. 8. Good yet; drilled in 1883.

No. 9. Dry hole on Anderson farm; struck quicksand at depth of over 1,100 feet

No. 10. Was a small well.

No. 11. A good well; gas struck within the past few days.

These wells being all in Butler County, their partial failure may be due to close contiguity to the numerous oil wells of that district by which they have been drained.

These wells have been supplying the mills of Spang, Chalfant & Co. some years with varying success, being able to supply the entire plant at times, and then as the wells failed, and before others could be drilled, the gas supply was sometimes insufficient, and it was therefore either necessary to stop part of the machinery, or return to the use of coal.

III. The number of companies chartered to supply natural gas in Pennsylvania up to Feb. 5, 1884, was 150, representing a capital stock of $2,160,580. Since that date, a large number of new charters have been granted.

IV. Natural gas, next to hydrogen, is the most powerful of the gaseous fuels, and if properly applied, one of the most economical, as very nearly its theoretical heating power can be utilized in evaporating water.

It is used for almost all the purposes to which coal is applied, with one notable exception, viz.: for smelting ores in blast furnaces, and it is our belief that at no distant day it will be used for this, but not in the present style of furnace.

Being so free from all deleterious elements, notably sulphur, it makes better iron, steel and glass than coal fuel. It makes steam more regularly, as there is no opening of doors, and no blank spaces are left on the grate-bars to let cold air in, and when properly arranged, regulates the steam pressure, leaving the man in charge nothing to do but to look after the water; and even that regulation could be accomplished by the gas if one cared to trust to such a volatile water tender. Boilers will last longer, and there will be fewer explosions from unequal expansion and contraction, due to cold draughts of air being let in on hot plates.

Gas engines of large size can be built to be driven by natural gas, as in the case of the Otto and other styles.

For domestic purposes a beautiful fire can be made, dust, ashes and coal carriage avoided; smoke, and the smoked ceilings and walls of Pittsburgh may become things of the past, yet if sold at prices now charged, *i. e.*, 50 cents per thousand cubic feet, it is much more costly than coal, especially if used in grates and stoves constructed for coal. The invention of burners for its more economical consumption in stoves must follow its general introduction.

As the introduction of natural gas has been of such recent date in this city, most of its users consume it in such a crude manner that they fail to get its best results, the difficulty being the expense of making the necessary changes in the burning. There is, however, one notable exception among the large consumers, namely, the Union Iron Mills of Messrs. Carnegie Bros. & Co., where it is being used with economy in Siemens' regenerative furnaces.

An experiment was made to ascertain the value of gas as a fuel in comparison with coal in generating steam, using a tubular boiler of 42 inches diameter, 10 feet long, with 4-inch tubes. It was first fired with selected Youghiogheny coal, broken to about 4-inch cubes, and the furnace was charged in a manner to obtain the best results possible with the stack which was attached to the boiler. Nine pounds of water evaporated to the pound of coal consumed was the best result obtained. The water was measured by two meters, one on the suction, the other on the discharge. The water was fed into a heater at a temperature of from 60° to 62°. The heater was placed in the flue leading from the boiler to the stack in both gas and coal experiments. In making the calculations the standard 76-pound bushel of the Pittsburgh district was used; 684 pounds of water was evaporated per bushel, which was 60.90 per cent. of the theoretical value of the coal. When gas was burned under the same boiler, but with a different furnace, and taking a pound of gas to be 23.5 cubic feet, the amount of water evaporated was found to be 20.31 pounds, or 83.40 of the theoretical heat units were utilized. The steam was under the atmospheric pressure, there being a large enough opening to prevent any back pressure; the combustion of both gas and coal was not hurried. It was found that the lower row of tubes could be plugged and the same amount of water could be evaporated with the coal, but with gas, by closing all the tubes on end next to stack, except enough to get rid of the products of combustion, when the pressure on walls of furnace was three ounces and the fire forced to its best, it was found that very nearly the same results could be obtained. Hence it was concluded that the most of the work was done on the shell of the boiler. Another experiment was made with the tubes plugged entirely, and a very small opening leading to stack, and with an increased pressure on the furnace and of course a different style of burner; the results were nearly the same; but the rivets and seams began to suffer, although only the same amount of gas was burned, but not in the same time. The gas required much more air to accomplish complete combustion per pound of fuel than coal. One singular fact was noticed, that is, when the products of combustion showed the smallest amount of carbonic oxide, the best results were not obtained. This was probably due to the fact that the increased heat, due to the burning of the carbonic oxide to carbonic acid, did not compensate for the loss occasioned by the amount of air that had to be let in to burn it, and which air had to be heated to about 1500°. As the air, gas and water were all accurately measured, the results were considered very nearly correct. Analyses of the gas in the escaping products of combustion were made quite often, only carbonic oxide and carbonic acid being determined.

Natural gas is being extensively used in heating boilers; in most cases by introducing a gas pipe with a row of small holes on its side, the fire space being closed up partly to check excessive draught.

No other data as to evaporative power are at the disposal of the committee, but it is apparent that in none of the boilers seen by us is the method of heating to be regarded as economical. A portion of gas taken from the flue of a 42 4′ two-flue boiler consuming natural gas was found to contain nitrogen, 85.88, carbonic acid 6.16, and oxygen 7.96, showing that a great excess of air was passing up the chimney, notwithstanding that in this instance more than usual care was taken in the regulation of the draught.

So long as meters are not employed in measuring the volume of gas consumed in manufacturing establishments, it is scarcely probable that owners will study economy in its use; but with an increased demand for natural gas, particularly when its superior heating qualities and low price as compared with coal are understood, the officials of the supplying companies will doubtless take such action as will prevent the reckless waste of this valuable natural product. If, for instance, as it might be shown by cheap contrivances, easily applied, a factory could be better supplied with only one third the present consumption of gas, the owners would certainly deem it no hardship if a meter was placed at their establishment, provided rates were not increased. In fact, it is most probable that a perfect system of supply will reach many more consumers, and with rates much lower than have heretofore been charged. At present the want of method by the companies forbids as rapid a development of the gas supply as the public wants really require. Heretofore it seems that contracts have been made to supply the gas at rates only a trifle less than the cost of coal, but in the haste to declare dividends the companies seem to forget that by permitting its reckless waste by a few large consumers, they are crippling a resource which would yield better financial results through a more general distribution at more reasonable rates.

ILLUMINATING POWER.

V. The composition of the gas now being brought to Pittsburgh renders it improbable that it will compete with coal gas as an illuminant, until some specially suitable form of burner has been contrived. Pure marsh gas yields about one half the light produced by coal gas.

Experiments made with a view to charging natural gas with the vapor of heavy hydrocarbons, have thus far been unsuccessful, the mixture thus far tending to separate in the gas-holder into layers of different composition.

USES.

VI. It has been attempted to apply natural gas to the conversion of iron into steel.

Experiments having in view the dephosphorization of iron through the agency of the hydrogen of natural gas have been made, but thus far the results have been very unsatisfactory. Imperfectly burned at a high temperature the gas deposits carbon in a form having a remarkable density. Upon this principle the manufacture of electric light carbons is now becoming an extensive industry in the hands of the McTighe Electric Light Company.

The tendency of the gas when under pressure is to absorb and carry off oil and grease, and leads to its being used for the cleansing of delicate fabrics.

The powerful reducing action of the gas upon metallic oxides at high temperatures may lead to its application to the smelting of metals upon a large scale.

The application of gas to glass making, on account of the purity of the fuel, has led to the production of superior glass, more rapid fusion is possible, and covered pots are found unnecessary.

VII. Pipes of various sizes and strength have been tried and with different kinds of sockets or couplings. Standard weight wrought-iron pipes with fine and coarse threads, tapering threads and sockets, light pipe with the Converse joint (which is a cast-iron socket calked with lead, the same as ordinary cast-iron water pipe).

Lead rings have been used between the beveled ends of the pipe, in the regular socket. Pipes have been screwed together with fine threads, and the sockets calked with copper wire. Cast-iron gas pipe with calked lead joints has been used at Wellsburgh, West Virginia, but, on account of the high pressure of the gas, proved a failure, the gas leaking not only through the joints, but through the pores of the iron in many places.

The tapering socket with pipe cut to match seems to have the best record.

If standard wrought-iron pipe be used and laid in ditches below the frost line and care taken in laying, no allowance need be made for expansion, for the flow of gas will keep the pipes at a fairly even temperature of not much over 45° Fahr., and no trouble from expansion or contraction need be feared. This statement, of course, does not apply to lines laid in cinder banks or where they are exposed to extreme changes in temperature due to proximity to furnaces, etc.

Light oil-well casing should not be used for pipe lines, because, first, it is only .1885 inch thick at its thickest part, and the thread (14 to the inch) reduces it .061 inch, leaving, therefore, only .1165 inch thickness, which is not sufficient. Again, some soils, and more especially cinder banks, will rapidly corrode such thin pipes.

The Acme Gas Company uses an 8-inch pipe of somewhat less than the standard weight, but still heavy enough to resist all ordinary pressure and strains.

The Fuel Gas Company has two lines of 5⅝ light casing, but they will never repeat this mistake, as they are about to lay two lines of standard weight pipes; all their city connections are made with standard weight pipes.

The Penn Fuel Company laid one line of 5⅝ *casing*, and one line of 8-inch *pipe*. Connections to mills are made with standard pipes, but it is to be regretted that this company laid any casing inside the city limits.

The varying requirements of a large iron works will render it desirable to be able at all times to control an unlimited volume of gas supported by high pressure.

In private dwellings the danger from explosions due to leaks in the pipes would be enormously increased by a pressure much exceeding that of ordinary coal gas in the service mains. In the opinion of the committee a pressure of over 6″ water pressure should be forbidden by law in pipes leading to dwellings.

The importance of having the high pressure mains, as they enter the city suburbs, subjected to careful tests, and the mode of laying such pipes under municipal control, cannot be over-estimated.

We are convinced that most scrupulous care is being bestowed both in the construction of pipes and valves by prominent manufacturers, and in the selection of material by some of the gas companies.

The necessity for a reduction of the pressure, which is often 75 or 100 pounds per square inch as the gas comes from the well, to an amount not exceeding five inches water pressure in the street mains, renders the selection of regulating valves for accomplishing this purpose of great importance. The regulators proposed are of two classes:

1. *Valves.*—Among the best known is the Luther valve, by which it is proposed to reduce the high pressure in the mains leading from the well to an amount suited to the purpose to which the gas is to be applied, and to preserve constantly this lower pressure.

From what the committee have seen in the use of valves, we believe we are justified in the statement that not one has yet been suggested which will satisfactorily answer the purpose.

2. *Tank Governor.*— his is undoubtedly the best form of regulator which the committee has seen tested. It is similar in princi-

ple to the gasometers or holders employed at the large gas works in the country.

EXPLOSIBILITY.

VIII. The fact that natural gas if mixed with air will explode on contact with fire, and is in effect the dreaded fire-damp of the coal mines, is no argument against its introduction and general use under due precautions. To those who understand its character, it is wholly unnecessary to state that the qualities which render it explosive when mixed with excess of air are the very ones which render it valuable as a producer of light and heat.

Taking the gas from Creighton Station, Western Pennsylvania, as approximately representing in composition the gas now being used in the city, the following trials were made with a view to ascertaining the limits of its inflammability.

Different mixtures of measured quantities of natural gas and air were prepared, and also mixtures in the same proportions of coal gas and air. The effect was noted when a coal gas flame was plunged into each. From these results we concluded that in a room filled with air containing $\frac{1}{10}$ to $\frac{1}{12}$ gas the danger would be one of explosions; above or below these limits, there would be danger of fire, but not of explosion.

A natural gas charged with the higher members of the series of paraffin (see Table A) would flash or explode when diluted with a still larger proportion of air. On the other hand, if the air in a room contains $\frac{1}{6}$ or $\frac{1}{7}$ coal gas contact with flame would cause explosion, while with an admixture of $\frac{1}{10}$ or $\frac{1}{11}$ of coal gas, there would be danger of fire, but not of explosion. As will be seen, as regards safety, there is a difference in favor of coal gas.

As coal gas is richer in free hydrogen, the most easily inflammable of all gases, its temperature of ignition may be assumed to be somewhat lower. The well-known property of coal gas of rendering incandescent a mass of spongy platinum or lead is found to be generally wanting in natural gas.

Accurate experiments (*Bulletin de la Société Chimique*, 1883, page 2, Mallard & LeChatelier) have shown that a mixture of O and H ignites at 552° C., while a mixture of marsh gas and O ignites at a temperature between 600° and 660° C. A calculation shows that a cubic foot of natural gas mixed with 9.2 cubic feet of air, and fired, will produce an expansion to 91 feet. A cubic foot of coal gas mixed with 6½ cubic feet of air will, on explosion, expand to 73.3 cubic feet. It has been found that a flash travels in an explosive mixture of natural gas and air at a rate considerably exceeding 18′ per second in a 2″ pipe.

Natural gas brings with it from the well the minute quantities of heavier liquid or solid hydrocarbons, which are carried along in the form of vapor or spray by the force and velocity of the gas under high pressure, and impart to it a strong and characteristic smell.

A peculiar substance resembling butter is often taken from the mains bringing the Murraysville gas to the city. A specimen of this substance was found to contain common salt, water, small quantities of lime and magnesia salts, coarse sand and a considerable quantity of solid paraffin all blown into a kind of light froth.

The odor of the gas in the mains appears to be dependent upon these traces of condensible hydrocarbons, for if kept in a closed vessel for a few days, the gas becomes absolutely odorless. The odor will therefore in all probability diminish more and more as it is carried away from the wells, or from the high pressure mains. This may explain the contradictory statements upon this point which have found circulation.

It has been found that air containing per cent. of Murraysville gas (fresh from the high pressure mains) has a decided odor; this is also true of Freeport and Creighton gas, but the same gas after standing in an air-tight glass for 24 hours had lost every trace of odor. Owing to their minute quantities and rapid condensation, these heavier hydrocarbons are not easily accounted for in an analysis.

Air containing 2 per cent. of Allegheny City coal gas has been found to possess a decided odor.

IX. The velocity of the gas depends largely on the amount of friction it has to overcome, as well as the initial pressure it has in coming from the well. A well which with its conducting pipes indicated pressure of 3¼ inches of water at the mouth, took just 4¼ minutes for the gas to traverse the 16,000 feet of pipe, which was then connected on, the pressure running up to 15 pounds at the well, due to the increased resistance in the friction of the pipe. The following experiment was also tried. Gas was turned into the pipe with an initial pressure of 90 pounds per square inch. It took just 2¼ minutes for it to traverse the 16,000 feet of pipe.

Natural gas pipes should be laid without any right-angled elbows, or other fittings of the kind; changed direction in the line should be made by bending the pipes, and no bend should have a radius of less than 48 inches for a 6-inch pipe, or eight times the diameter of the pipe.

Gas from a well having a pressure of 20 ounces had a velocity of 23,400 feet per minute; a rubber ball was driven through three miles of a 5⅝ casing pipe in 2½ minutes.

When gas is blowing freely from the mouth of a well, the pressure has not been found in any case to reach 2 pounds per square inch.

Statements in regard to higher pressures than this are probably in error.

The gas as it issues from the wells has a temperature of 42° to 45° Fahr.

At the moment of release from the well the volume no doubt undergoes a very considerable expansion, resulting in a lowering of the temperature.

This absorption of heat due to expansion may perhaps explain the fact that blocks of ice are often seen to be thrown from the stand-pipes while the gas is burning with a powerful flame.

The temperature has been found to be 45° in several of the mains in Pittsburgh.

X. At the time of the appointment of this committee, the prominent legal question of interest to natural gas companies was the definition of their rights to lay their pipes in the city streets, and as corollary to this, the responsibility resulting from explosions or other accidents due to the use or presence of the gas. But recently a more important question has been brought forward by the decision of the local court that but one natural gas company, under the law of 1874, is authorized to supply gas to consumers in this city.

As the law of 1874 is so frequently referred to in the newspapers, it may be useful to introduce it here for the benefit of readers who may not have ready access to a law library.

LAWS OF PENNSYLVANIA.

Act approved 29th of April, 1874, relating to water, gas, light and heat companies.*

1. Companies incorporated under the provisions of this statute for the supply of water to the public, or for the manufacture and

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* 1. Act 29th April, 1874, 34 P. L. 93. 2. Id.

3. In Bloomfield, etc., Gas Light Company vs. Calkins, 62 N. Y. 386.

supply of gas, or the supply of light or heat to the public by any other means, shall, unless otherwise provided by this act, from the date of the letters patent creating the same, have the powers and be governed, managed and controlled as follows:

Clause 1. *Gas, heat, light—powers of companies.*

2. Where any such company shall be incorporated as a gas company, or company for the supply of heat and light to the public, it shall have authority to supply with gaslight the borough, town, city or district where it may be located, and such persons, partnerships and corporations residing therein, or adjacent thereto, as may desire the same, at such price as may be agreed upon, and also to make, erect and maintain therein the necessary buildings, machinery and apparatus for manufacturing gas, heat or light from coal or other material, and distributing the same, with the right to enter upon any public street, lane, alley, or highway, for the purpose of laying down pipes, altering, inspecting and repairing the same, doing as little damage to said streets, lanes, alleys and highways.*

3. And impairing the free use thereof as little as possible, and subject to such regulations as the councils of said borough, town, city, or district may adopt in regard to grades, or for the protection and convenience of public travel over the same.

When the act of 1874 was passed, electric lighting was in its infancy, and it has been suggested that in deference to the promises of scientific men regarding its then future, that the terms of the act were purposely made so diffuse as to cover its possibilities—as for instance, "the supply of gas, or the supply of light or heat to the public *by any other means.*"

This vagueness and want of supplementing legislation is a source of evil in various ways, and it is the opinion of some well informed, that as the knowledge of electric lighting and of natural gas is *now* better understood, there is an urgent demand for additional legislation.

There can be no doubt about the fact that the intention of the law-givers was to encourage both the electrician and the gas man, but the wants of the two interests are so different that only confusion and trouble will attend their affairs until their rights and privileges are separately and distinctly defined, and this it is reasonable to believe could be much more effectually accomplished by

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* 1. Thomps & Co., 541; it was held that gas pipes could not be laid under country highways without compensating the owners of the fee. See supra, p. 56, N. 2, Dillon vs. Gas Light Co., 1 McArthur, 626.

new legislation, than by awaiting decisions based upon such a fundamentally defective law as that of 1874.

Under the law understood by this committee, natural gas companies have the right to lay their pipes in the streets of towns and cities, with precisely the same privileges as coal gas companies, so far as such rights extend. But this does not prevent cities from passing laws based upon "reasonable grounds" ordaining the mode of laying the pipes as far as the language of the act cited allows, as regards grades or for the protection and convenience of public travel over the same.

In regard to the question of the right of the city to restrict the gas companies as to pressure in, and size and strength of pipes, etc., a difference of opinion exists; but under the head of "police regulations" it is believed by the committee that against any insufficiency of strength in the pipes, or defective workmanship in the joints which might cause explosion, a city can in its charter find grounds for interference, and protect itself by the enforcement of appropriate legislation. But from the extent to which even this apparently wise and proper proposition has been debated, it is made clear that further action is demanded by the Legislature. It does not, of course, comport with the character of this paper, even if the committee felt able to do so, to attempt to criticise the recent decision of the court, which is generally construed to grant one company, to the exclusion of all others, the privilege of supplying the city of Pittsburgh with natural gas.

Natural gas had been used under the boilers of engines at wells in the northern oil fields before the passage of the act of 1874, and to some extent it had been introduced into dwellings for domestic consumption, but it was then considered a waste product, which any one could dispose of by sale or gift, as could be done with any natural product, such as coal, limestone, or other minerals. It simply flows from the ground, with no process of manufacture involved in its formation, and no patentable form of composition, force or capability. It would, therefore, be strange indeed if our legislators intended natural gas to come within the scope of statutes governing the organization and specifying the privileges of manufacturing companies.

If the Engineers' Society of Western Pennsylvania were called upon to suggest on what points, in connection with the subject of natural gas, legislation was desired, this committee would not hesitate to recommend for consideration as follows:

First.—That a State Inspector of gas and artesian wells, and natural gas companies, be appointed by the Governor, among whose duties we would specify: To report to the proper authorities, after due notice, any waste or extravagant use of natural gas, either at the wells or elsewhere, and that all natural gas companies be required to report to him the details of their operations, force and flow of the gas, and that all arrangements for its control at the wells, or in the pipes to a city or borough line, be under his general supervision. His office being compelled to keep up the records, and to report annually, with the aid of statistics and plans, the state of the natural gas supply and demand.

Second.—That in cities or towns, companies desiring to supply either manufacturers or domestic consumers with natural gas, may, with the consent of the councils thereof, and under the direction of the city or borough engineer, lay their pipes through or along any street, lane or alley, but no city or borough to have the right to give any exclusive privileges to any one gas company.

Awaiting the action of the State Legislature on the general matters which it might be expedient for that honorable body to consider, such as we have outlined above, your committee feels that they might be considered derelict in duty if they did not advise and propose at least some measures which might be exercised by individual consumers of natural gas, tending to both security, as regards the dangers of fires and explosions, as well as to economy in its use.

We therefore would propose for the consideration of companies and individuals interested, the following:

First.—That the distributing mains for domestic consumption of natural gas be of size amply sufficient to conduct gas to dwellings with at no time or place a pressure exceeding 5½ inches (water pressure). This pressure can be guaranteed to be uniform, certainly never in excess, by a properly constructed form of tank governor.

Second.—Every domestic consumer of natural gas should see to it that an automatic cut-off valve be placed on his service pipe, so arranged in case the supply from the tank governor should from any cause fail, that his valve would immediately close the pipe conducting to his premises, and requiring personal attention to restore the pressure when it again returns through the main. The committee believes that such automatic valves can be provided.

Third.—No cast-iron fittings, or parts of fittings of cast, or any coarse-grained or inferior metal, should be allowed in private houses. Carefully selected wrought-iron pipes, with brass or malleable iron fittings, should alone be adopted. The great object of care being to prevent the possibility of leaks.

Fourth.—Special care should be taken to see that the street cut-off, or valve, is so boxed or tubed as to permit free outlet to the air of any gas escaping from a leaky main which may follow along the branch pipe from the street. The work of trenching to and tapping gas mains should be done when the frost is out of the ground, and the soil next the pipes in such trenches should be clay, well wetted and puddled.

Fifth.—In the case of fires under boilers, in ranges, fire-places, stoves, etc., consumers of natural gas have, for the most part, entirely disregarded its laws of combustion in not using the proper appliances for the admixture of air to the gas jets. In many instances, even among wholesale consumers of this fuel, it can be demonstrated that with the use of improved mixers the same quantity of heat can be produced as is now developed, from one-third the volume of gas.

Sixth.—The feed valves to fires, and all burners where natural gas is used in private houses, should be securely placed so far above the floors as to be out of the way of children and where they could not be accidentally turned on. To this end valve stems with movable socket handles are of advantage.

Seventh.—All gas pipes laid from service mains should be thoroughly tested for leakage before being accepted as in working order.

Eighth.—Where natural gas is sold either entirely or as a component part of illuminating gas, its candle power should be guaranteed to be of a satisfactory amount.

Respectfully submitted,
T. P. Roberts,
F. C. Phillips,
A. E. Hunt,
W. S. Jarboe,
N. M. McDowell,
Committee on Natural Gas.

TABLE SHOWING THE PROPERTIES OF THE CHIEF GASEOUS ELEMENTS OF NATURAL GAS AND INCLUDING SOME OF THE HEAVIER HYDROCARBONS.

Paraffins.	Condition.	Composition.		Specific Gravity of Vapor.	Heat Units Yielded by 1 lb. in burning.	Cubic feet of air theoretically needed to burn 1 cubic foot of vapor.
		Per Cent. Hydrogen.	Per Cent. Carbon.			
Marsh Gas..	Gas...........	25.04	74.96	0.5576	13,370	9.56
Ethane.....	Gas...........	20.05	79.95	1.043	12,469	16.74
Propane....	Gas...........	18.22	81.78	1.522	12,145	23.92
Butane.....	Gas, liquefies at 34° Fahr.	17.28	82.72	2.007	Not yet experimentally determined for the higher members.	31.10
Pentane....	Liquid, boils at 100° Fahr.	16.71	83.29	2.49		38.28
Hexane....	Liquid, boils at 158° Fahr.	16.32	83.68	2.97		45.45
Heptane....	Liquid, boils at 210° Fahr.	16.04	83.96	3.46		52.63
Octane......	Liquid, boils at 255° Fahr.	15.83	84.17	3.94		59.80

TABLE OF ANALYSIS OF NATURAL GAS—FROM VARIOUS SOURCES.

CONSTITUENTS.	1 Petrolia, Canada.	2 West Bloomfield, N. Y.	3 Olean, N. Y.	4 Fredonia, N. Y.	5 Pioneer Run, Venango Co., Pa.	6 Burns' Well, near St. Joe., Butler Co., Pa.	7 Harvey Well, Butler Co., Pa.	8 Cherry Tree, Indiana Co., Pa.	9 Leechburg.	10 Creighton.	11 Penn Fuel Co.'s Well, Murraysville.	12 Fuel Gas Co.'s Well, Murraysville.	13 Roger's Gulch, Wirt Co., W. Va.	14 Gas from Marsh Ground.	15 Baku, on the Caspian Sea.	16 Gas occluded in Wigan Cannel Coal.	17 Blower in Coal Mine, South Wales.
Hydrogen	Chiefly marsh gas, with ethane and some carbonic acid.	......	......	A mixture of marsh gas, ethane and butane.	Chiefly propane, with small quantities of carbonic acid and nitrogen.	6.10	13.50	22.50	4.79	......	Marsh gas, with a little carbonic acid.	19.56	Chiefly marsh gas, with small quantities of nitrogen and 15.86 per cent. carbonic acid.	......	0.98	......	......
Marsh Gas		82.41	96.50			75.44	80.11	60.27	89.65	96.34		78.24		47.37	93.09	80.69	95.42
Ethane		......	......			18.12	5.72	6.80	4.39	......		......		......	......	4.75	......
Propane		......	......			trace.	......	......	trace.	......		......		......	......	......	......
Carbonic Acid		10.11	......			0.34	0.66	2.28	0.35	3.64		......		3.10	2.18	6.44	0.60
Carbonic Oxide		......	50.			trace.	trace.	trace.	0.26	Trace of heavy hydrocarbons.		......		......	......	......	......
Nitrogen		4.31	......			......	......	7 32	......			......		49.39	0.49	8 12	3.98
Oxygen		0.23	2.00			......	......	0.83	......			2.20		0.17	......	......	......
"Illuminating Hydrocarbons"		2.94	1.00			......	......	......	0.56			......		......	3.26	......	......
		100.00	100.00			100.00	99.99	100.00	100.00			100 00		100.08	100.00	100.00	100.00
Specific gravity		0.698	0.692			0.6148	0.5119		0.5590	0.5923	0.56						

1. Fouqué, "Comptes Rendus," lxvii., p. 1045.
1. H Wurtz, "Am. Jour. Arts and Sci.," (2) xlix., p. 336.
3. Robert Young.
4. Fouqué, "Comptes Rendus," lxvii., p. 1045.
5. " " " " "
6. S. P. Sadtler, R rt L, 2d Geol. Sur. Pa., p. 153.
7. S. P. Sadtler, Report L, 2d Geol. Sur. Pa., p. 152.
8. " " " " " " p. 153.
9. " " " " " " ".
10. F. C. Phillips.
11. Robert Young.
12. Rogers.
13. Fouqué, "Comptes Rendus," lxvii., p. 1045.
14. Bischof's "Chemical Geology," i., p. 730.
15. " " " " "
16. J. W. Thomas, London "Chem. Society's Journal," 1876, p. 793.
17. Same, 1875, p. 793.

TABLE SHOWING COMPARATIVE INFLAMMABILITY OF NATURAL GAS AND COAL GAS.

Mixture of Natural Gas and Air.		Effects.	Mixture of Allegheny City Coal Gas and Air.		Effects.
Gas.	Air.		Gas.	Air.	
1 volume.	4 volumes.	Burns feebly.	1 volume.	4 volumes.	Burns feebly.
1 "	6 "	Burns slowly.	1 "	6 "	*Explodes.*
1 "	9 "	Burns slowly.	1 "	7 "	Burns explosively.
1 "	8 "	Burns rapidly.	1 "	8 "	" "
1 "	9 "	Burns explosively.	1 "	9 "	" "
1 "	10 "	*Explodes.*	1 "	10 "	Burns explosively —less rapid.
1 "	12 "	Burns somewhat explosively.	1 "	12 '	Flashes.
1 "	13½ "	Burns quietly.	1 "	13½ "	No flash.
1 "	15 "	Flashes, but flame dies out.	1 "	15 "	"
1 "	16 "	Very feeble flash.	1 "	16 "	"

TABLE SHOWING COMPARATIVE EFFECTS OF DIFFERENT GAS FUELS.

	Heat Units Yielded by 1 Cubic Foot.	Number of Cubic Feet Needed to Evaporate 100 Pounds Water at 212 deg. F.
Hydrogen	183.1	293
Water Gas (from coke)	158.1	351
Blast Furnace Gas	51.8	1038
Carbonic Oxide	178.3	313
Marsh Gas	571.0	93.8

DISCUSSION.

President Sweet.—This has been a very interesting paper, and is now open to discussion. The members of both Societies are expected to take part.

Mr. Kent.—It seems that the engineers of Western Pennsylvania and the Mechanical Engineers of the United States are a little afraid of each other to-night, since no one seems willing to open the discussion. When I was an active member of the Engineers' Society of Western Pennsylvania, our then President, Mr. Metcalf, used always to call on me to start the debate on any subject, as the other members were too timid.

Mr. Metcalf, perhaps, is best capable of any of us of talking about gas. But I may say a few words regarding what was read in the paper to-night about the economy of gas in firing steam boilers. The consumers of gas want to know the best method of securing economy in its use. There are a few very simple principles according to which they should work in order to secure the best economy. First, secure the highest possible temperature in the front end of the boiler, and, secondly, secure the lowest possible temperature in the back end of the boiler. The highest possible temperature can be secured in the front end of the boiler by mixing with the gas just the amount of air needed to secure its perfect combustion, and no more. It may be that amount of air is much larger than the amount theoretically necessary, as we find in burning coal that nearly double the amount of air which is theoretically necessary to burn the coal must be passed through the coal in order to burn it thoroughly, and consequently there is twice as much air going up the chimney as there ought to be theoretically. It may be the same with natural gas. It will require chemical analysis to determine whether or not that is true. But in any case, just so much air should be supplied to the front end as will generate the highest possible temperature. After having obtained the highest possible temperature in the front end of the boiler in order to obtain the lowest possible temperature in the back end, all that is necessary is to supply sufficient heating surface in the boilers.

Some months ago I visited a place where they were using natural gas, and was told that whereas, before the introduction of natural gas the number of boilers they had was rather too small, after they used natural gas they threw off several boilers, and got along with less. As the boilers they had before were all over-driven, and the products of combustion were going up the chimney at too high a temperature, reasoning from the temperature found in the breeching of these boilers, it was natural to suppose that there was a great waste of fuel, and that this waste was still greater when natural gas was used. The high temperature in the furnace chamber can no doubt be best secured by burners which intimately mix the gas and air, and by a combustion chamber which is thoroughly surrounded by fire-brick, or some other non-conducting material, to make an intensely hot chamber. The heat allowable is limited only by the melting point of the fire-brick. I can give this rule, therefore, to consumers of natural gas: Burn the gas or the other fuel with the smallest possible quantity of air which will burn it thoroughly (although that may be more than the theoretical amount), and by means of fire-brick combustion chambers, or other means, heat the furnace chamber hot, and then put enough heating surface in the boilers to absorb the heat, and let the gas go off at the lowest possible temperature.

Mr. Jarboe.—I differ from Mr. Kent as to burning all the gas in the front part of the boiler. After a number of experiments, I find the most economical and the best evaporative results are obtained from burning the gas equally along the length of the shell, distributing the supply, and putting the flame against the boiler. In one boiler I had the gas fired sideways across the boiler. It gave me the very best results I had, but it was good for the boiler makers. It makes the rivets melt and the shell open.

With a furnace running at the present time the boiler is fed with gas from a series of jets, along the whole length of the boiler, the flame from each and every individual jet touching the shell, with no combustion chamber in it. The pipe is placed about 8″ from the boiler and the jets running up into the bed. That boiler has an air-tight furnace, so to speak. Instead of having a 36″ stack, it has about a 6″ opening to let the gas out. That boiler is doing splendidly. It has given a high result, and the result is not obtained from theory, but it is given from data taken on the spot. The gas is taken through two meters, of different makes; the air is metered in to get the proper quantity so that I can get proper data from that. The water was metered into the boiler also, and that boiler is running to-day, with jets the whole length of the boiler and no combustion chamber at all in the whole place.

Mr. Kent.—Will you state how many pounds of water were metered in per pound of gas?

Mr. Jarboe.—20.30 pounds. That is high.

Mr. Kent.—What is the theoretical limit?

Mr. Jarboe.—About 27.8 pounds.

Mr. Kent.—But I understand from the report only one third of the heat was utilized?

Mr. Jarboe.—This boiler has a stack placed 20 odd feet from the boiler, and the heat from the boiler is almost nil. There is a piece of tin in the stack, and that piece of tin has never melted yet.

That one-third business you heard of was where they had two-flue boilers, five in a battery, 14" flues, a 36" stack, 60' high. No measurement of the air mixed with the gas has ever been taken. The parties said they paid $3 per ton for the iron finished, and the engineer of the gas company had to look out for the economy of fuel.

Mr. Metcalf.—I came here to hear something about natural gas. I came here a complete ignoramus, not knowing anything whatever about it, and I feel very much the same way now [laughter]. I hardly know what we are going to talk about. Almost all of us knew that natural gas was mainly marsh gas, that it had high caloric powers, that it did not smell and it did smell, and all that kind of thing [laughter]. Of course, these are all matters that are important and necessary in a record of this kind. But these gentlemen have not told us yet anything about the value of this gas. There is no comparison given between the value of this gas and the value of solid fuel. There is no comparison made between the value of this gas and the ordinary mode of burning coke, or the ordinary mode of burning producer gases in the system of regenerative furnaces. The gas companies come along and say, "We will supply you with gas for your fuel bill," and of course that is such a nice arrangement at present that a few have taken it up. But the time will very soon come when so many will want this gas that it will be absolutely necessary to run it at a very low pressure; it will be necessary to expand it in volume, and burn it in a more reasonable, rational way, as we do our producer gas.

Our friends state that the only places where the gas is utilized now in the Siemens furnace, are the Union Iron Mills and Black Diamond Steel Works. It is largely used in regenerator furnaces for melting open hearth steel. What security is there in utilizing or attempting to use this gas now, under these enormous pressures, when the records of the wells show that they will peter out in from two or three days to a year? I would like to know what the value of this gas is, and how it can be used without pressure—what it is worth in money per thousand cubic feet. I will state to the committee what coal fuel is worth in the regenerative gas furnaces, and then, if they will give us an idea of the value of this fuel, when we come to dicker with these gas companies we will be able to talk intelligently to them. We are now puddling iron with common slack with .75 pound of coal to a pound of muck iron—three-quarters of a pound of common slack to a pound of muck iron. That is done every day, all the year through. We also melt steel in the crucibles with one pound of slack to a pound of steel. These are figures we can very easily get at. Now, how much can we pay for a thousand feet of gas to justify us in throwing away these enormously expensive furnaces, or in readapting our plant to the use of natural gas?

With all due respect to the committee, if they had told us more about this and less about the law, I would have felt better informed.

Mr. Roberts.—It occurs to me that I recollect the very first paper that was read before the Society of the Engineers of Western Pennsylvania. It was dedicated to the subject, "Why Steel Hardens." That paper I understood was the result of many months of labor and a considerable expenditure of brains, but I think the result was, "if any person knows any more on the question why steel hardens" than the author of that paper, he would like to hear about it.

Now, gentlemen, steel is something which has been before the public for, well I do not know how long. I have heard of the Damascus blade and the Toledo blade, and it has been dignified as a metal I do not know how long. But this natural gas question is something entirely new. We are dealing entirely with an invisible fluid, and we have presented statistics about it as far as we could obtain them to date for the information of these two honorable societies. Possibly some of the members may have been asleep when this information was given, because this is an invisible fluid, and will leak through a man unless his joints are tight. It differs from steel. You can see something in steel when you hammer it out, but this gas has no odor. It is not a tangible material, and I think you have got to study over it very carefully to know just what has been said. Candidly, however, the committee recognize the shortcomings of their report, and they know that many further examinations will be required to exhaust the subject.

Mr. Hunt.—I would like to make another correction. It has been stated that the report says that natural gas has not been successfully used in the making of steel. What the report actually did mean to say was that it had not been successfully injected into or otherwise used in the way that the melters call as a "medicine" for steel. We all know in several of the largest steel manufactories of the city it has been successfully and well used as a fuel both in Siemens gas and other forms of furnaces, and we have so stated in the report.

Mr. Jarboe.—The only experiment I know of where the value of gas and coal has been compared is in the shape of the comparative test of fuels. We took coal, Pittsburgh selected lump coal, at five cents per bushel, and used the gas with the best arrangement of burners. I found the gas was 7.8 cents per thousand cubic feet. That is by actual measurement, burning it as well as we could burn it, with the smallest trace of carbonic oxide. That was in the same furnace I mentioned. The air is mixed with the gas outside of the furnace and injected into it, and no other air can possibly get in.

Mr. Reese.—I would like to say that Mr. Beale, of Leechburgh, has been using natural gas in his open hearth furnace for making open hearth steel for some time. He has discovered an incident connected with the use of natural gas that has not been mentioned, and I will take the opportunity of mentioning it. Natural gas, as our friend has said, is a new thing. The committee has informed us that its specific gravity differs from that of air about one half;—probably the specific gravity of the Leechburgh gas is .58 or .60. Now, it has been discovered that, to get the best results, these two gaseous elements should be brought together at the same velocity. When the specific gravity varies, the velocity will vary, and it has been found, where the experiments were made, that if the gas and the air were sent through the regenerators together, that they had nothing like the most economic or the best calorific result, nor as good results as when the air was sent through the regenerators, and the gas was put in cold. The gas being so much lighter its velocity was greater, and being put in at a high pressure, the two came together in such a way that it was impossible to secure perfect combustion. You know that when you consume anything there is a relative velocity of combustion. Now, to secure this relative velocity and economic combustion, it is necessary that the air and the gas be admitted at the same velocity. This could not be secured by passing both the gas and the air through the regenerators, as the gas was so light that it moved with greater rapidity than air. So that it was found necessary to heat the air alone in order to reduce its specific gravity to about that of the gas, if the latter was about .60. And by thus uniting the hot air with the cold gas, the relative velocity and perfect combustion was secured.

A friend from abroad has asked me something about the continuity of the gas—whether it will last. I go down to Beaver County occasionally, and down at the mouth of Raccoon Creek, three miles below Phillipsburg, on the south side, there is a salt well, and they have a pipe coming up from the salt well and running into a big hogshead or tank. The gas squirts the salt water up, and the gas and water are separated here, and the old fellow who runs the well just sits down on his dignity and lets it boil. [Laughter.] I asked him the question, "How long will this thing last?" He said he did not know; he has been letting it boil for 21 years now, and it may boil 21 years more, but he thought he would not live that long. [Laughter].

This is 1884. I think it was in 1860 that I went up with another party, and bought a large tract of land of 2,000 acres in Venango and Clarion counties, and we were going to bore right away for oil. But as soon as we located and paid for the ground there was another party went in and bought alongside of us, and we thought that probably they would want to bore too, and so we would let them bore first. We did let them bore [laughter], and they struck gas. A lot of us from Pittsburgh—John Scott, President of the Allegheny Valley Road, was one of us—went up there, and by that time they had five wells, and were getting some oil and a good deal of gas, one well altogether gas and the others considerable oil. Some of us suggested that they had better turn the gas under the boilers, and they did so. It worked so nicely as a calorific power they did not see but what they might as well put it into the cylinders at once, and use it as a dynamic power. They put it directly into the cylinders, and that is the way they have run the machine up there, I believe, for twenty-three years. That is in what they call Gas City. There is more gas there to-day than there was at that time, and there will be more if they bore another hole, I guess. [Laughter.]

My friend Hunt, who read the paper, says he thinks that it will dephosphorize iron, and that it can be used in smelting, in the present blast furnace. Now, I do not see how 96 per cent. of marsh gas, with about 30 per cent. of hydrogen, can be used in a blast furnace. I think you will find about 33 or 34 per cent. of hydrogen in the Pittsburgh natural gas. I cannot see how you can put an element into a blast furnace or into any apparatus to deoxodize metals that will have 30 per cent. of hydrogen. The gas resulting from the combustion of hydrogen will be water. I have gone all through that, you know. I took out some patents on that. [Laughter.] If you will look in 1866 and 1867, you will find about forty claims there on the use of hydrocarbon vapor. I went up there and saw it, and I thought it was a big thing, and I patented it. [Laughter.] I built an apparatus that cost me over $10,000, and tried dephosphorizing with this gas, and I did dephosphorize occasionally, and sometimes I did not. [Laughter.]

Mr. Jarboe.—In regard to Mr. Beale's furnace, in the first place, if this marsh gas is passed through regenerators, the gas is decomposed, the carbon is deposited on the checker bricks, and the hydrogen is set free and takes the shortest and quickest road to the chimney before its heating properties are extracted.

Secondly, this gas requires much more air than carbonic oxide, and the air checker-work in regenerative furnaces is not large enough. The proof of this was seen at Carnegie's mill, there they tried to use the gas, admitting air only through one set of chambers —the air regenerators. It was not successful; but after closing the valve from the producer, and with an outside opening, letting the air pass through the gas checker-work as well as the original air regenerators, good results were obtained.

Mr. Jones.—I think there is a mistake in this report. I think they should have embodied in the report a few instructions how to use the gas. In giving an account of introducing the gas at an establishment on the south side, the newspaper reporters put it in about this shape: "The foreman of the works turned the gas into the furnace. He then lit a long pole, and lit the gas, and then he lit out himself." [Laughter.] I think it would be well to embody in the report instructions to get the fire started before you put the gas on.

Another question is suggested by Mr. Metcalf's inquiring into the commercial value of the gas. If Mr. Metcalf had been working, like myself, among eighty-two coal heavers, one half of whom belonged to the Amalgamated Association, and the other half to the Knights of Labor, each striving to show how much better labor agitators they were than the other gang, he would appreciate the value of the gas. In fact, whenever I approached the boiler house

I felt as if I was going into the walls of a penitentiary. Every one looked at me as much as to say, "Have you got permission to come in here?" Ever since I used natural gas and got rid of those eighty-two coal heavers I felt like saying, "God bless the discoverer of natural gas."

In regard to the use of it, I can say there is very little difficulty in using it. I think probably the greatest difficulty in using it is to get a sufficient quantity of air for combustion. That is one thing I would lay down as a rule. See that you get plenty of air. There seems to be the greatest difficulty in getting a sufficient quantity of air for proper combustion.

Mr. Durfee.—If, as has been said, this natural gas is delivered at the mills under a pressure of from 60 to 70 pounds, it is quite possible to use it in the cylinders of the engines as a motive power; and the exhaust gas can be turned into a storage reservoir, or used directly for heating. By such a method the greatest possible economy in the use of the gas can be obtained:—first, using its pressure as a motive power; and then using the exhaust gas as a source of heat.

Mr. Jones.—The statement of the last speaker about using the gas first as a motive power and then as a source of heat should not go forth unmodified as the sense of this society. We have an eight-inch pipe at our steel works which is heating all the steam for probably 4,000 horse-power every 24 hours, and doing the heating for 600 tons of rails. In other words, an eight-inch pipe will represent somewhere about 400 tons of coal consumed every 24 hours at our works. The eight-inch pipe carrying gas to act by pressure will only drive one of our ordinary engines, while the eight-inch pipe will supply enough gas at 65 pounds pressure, to be equivalent to 800 tons of coal. So that the use of the gas as a motive power, I think, is out of the question. I think the most economical way is to use it in generating steam.

Mr. Painter.—Mr. Chairman, I came here for facts. In a few weeks I expect to use a McKean well, and I want to know how to use it. We have got to get something cheap. I have seen every gas furnace in the city, and they are all expensive with one exception. I want to have something by which I can change my furnace at a cost not to exceed four or five thousand dollars.

Mr. Jones.—I think the statement should go forth that the Siemens furnace is well adapted to the use of natural gas, requiring very little expense. Those who have seen the furnaces say they can change them with comparatively little expense. I have not a particle of doubt that Mr. Painter will find that the principle of using regenerators in some shape or form is the true one. It is very essential to get the temperature in the rear as high as possible. That adds to the economy of combustion, and is one of the main questions; another, and as I before stated, is to get plenty of air into the furnaces.

Mr. Painter.—They have checker-work in the furnaces at Carnegie Bros., and they have a reversible valve. At one time they had the valve at the very end of the furnace. They found that did not work very well. Then they put the chambers underneath, which made it so hot a man could not stand there. But now they have put the valve up next to the stack. I put my hand there when a man was working on the stack, and there was no heat there at all. He took me in and showed me his figures. It had cost $470 to change the furnace. Now, that is not what is true economy. We do not want that. I acknowledge, from what little experience I had with it, that you have got to heat your air to burn your gas.

Mr. Jones.—Not necessarily, but to get the best economy.

Mr. Painter.—I intend to use 60 pounds pressure.

Mr. Roberts.—I will call Mr. Jones' attention to that table at the end of the report, which gives the result of different mixtures of natural gas with certain volumes of air. He might get some information from it.

Mr. Metcalf.—Mr. President, the question of the first cost amounts to very little in changing from one system to another, provided you get good results in the end. I know of a very small concern that spent not less than $75,000 in throwing away old-fashioned furnaces and putting in gas regenerative furnaces, and have got it all back every two or three years since. That is the proper way to look at it.

I think Mr. Jones misunderstood Mr. Durfee in regard to utilizing this gas for motive power. The gentleman suggested the use of the gas in the steam cylinder and the use of the waste gas for heating. I know that he was right, because it certainly is not right to try and burn this gas under an enormous pressure. I cannot see why it is, when you want five volumes of air to one of gas, at the very least. If the gas pipe ever comes to our place, the first thing I shall devote my attention to will be to get rid of the pressure before I try to burn that gas economically.

In regard to the committee, I will say I knew very well what was coming. I told you to-night that if you heard Mr. Roberts, you would be reminded of his great ancestor; but I think that all will agree that that stirring up was what was needed. Every one of the committee was bursting with information, and we wanted to get it.

Mr. Jones.—I think Mr. Metcalf misunderstood me. What I wanted to say is this: that we have an eight-inch pipe supplying us with gas for heating and steaming, which is saving us at least 400 tons of fuel a day. That eight-inch pipe would not run our rail mill alone. I say that an eight-inch pipe would be equivalent to the consumption of 800 tons of coal. I think that the true economy would be to use your steam engines, and generate steam by means of natural gas, instead of using the natural gas as a motive power.

Mr. Henning.—I would like to ask Mr. Jones, if the pressure in his main is just sufficient to deliver the gas to his boilers? Would not another engine be required to force the gas there, if the pressure was used to do other work? In the gas main there must be sufficient pressure to deliver the number of cubic feet of gas required, in order to supply enough to the furnaces. If you absorbed pressure by doing work with it, would you not require some driving power back of it to supply sufficient gas to heat the boilers? The pressure in the main is the driving power necessary to be able to utilize the gas as a fuel in ample quantity.

Mr. Jones.—The only practical view of the matter will be this. Suppose we take an engine representing 800 horse-power. In the first place, an eight-inch pipe was not sufficient to run the engine; we find it takes a ten-inch pipe at 70 pounds to run the engine. It is hardly fair to suppose that an eight-inch gas pipe with 60 pounds pressure will do the work. It enters, I think, largely into the question of the first cost.

Mr. Strong.—I would like to ask if the ejector principle has been used to feed the air into the furnace?

Mr. Jones.—It is a generally settled fact that its use is possible, and I think it is just as good as any. The ejector principle can be used to inject the necessary amount of air in, which can be fairly regulated, but for heating purpose, furnaces, etc., it is also a pretty well settled conclusion that it is necessary to come down by using the regulators to a gas pressure of two and a half pounds. The members of this Society will have full opportunity to see the gas working in all shapes and forms, and I have not a particle of hesitation in saying that the mechanical genius of Allegheny County will soon have he matter tested in every way, shape and form, for the gas can be used in almost every furnace that is now built—just the ordinary draught furnaces that are generally used here in Pittsburgh. Just put a blower in them and blow your air in. You can find plenty of blowers around Pittsburgh. [Laughter.]

Mr. Jarboe.—About this injecting principle—I have an analysis of chimney gases: of 24 or 26 (I think) different boilers being used with injectors, and have an analysis of several used with a blower to put the air in. The analyses show less waste by a large majority with a blower putting the air in, and there is also a great deal less gas burned and a great deal more steam made by the same style of boilers. It is a very difficult thing to get an injector that will work under a variation of a few inches of pressure.

I have made a number of experiments with different injectors and different blowers, and in no case have I found an injector that will do satisfactory work. I have a little furnace, a little bit of a baby, that is burning with flameless combustion, by using gas with blast. That little furnace will melt down Benzet brick, and it is heating rivets. Major Munroe will tell you there that very few of those rivets have been oxydized.

My idea of a furnace is to put a blast on it, when you can get it. You have perfect control of your air. You have perfect control of your gas valves, and you can mix the two just as you please.

Mr. Jones.—What pressure have you made it?

Mr. Jarboe.—I have tested it by inches up to 20 inches, then by ounces up to two pounds, and then by pounds up to 175 pounds pressure, every pound from two pounds up. Remember, gentlemen, I used a gas-holder. I metered the gas that went in, and then I metered the resulting gas in the gas-holder, so I should know exactly what proportion I had. I never found an injector yet that mixed it right.

Mr. Reese.—While I agree that the greatest difficulty to be encountered is in getting enough fresh air to mix with the gas in use, I only want to say one thing. In conversation with some stockholders of the Citizens' Passenger Railway the other day, it was stated that some parties had struck a very large gas well not far away, and they were considering the feasibility of bringing it in to drive their road by cable, that is, to drive the machinery by which the cable would be operated by the gas as a motive power, and selling their excess for heating and light.

Mr. Towne.—Mr. President, it seems to me that the point just touched upon can be stated in this way: That the gas flowing through these mains has two kinds of potentiality; one dynamic, and the other chemical, and that it has heretofore been used only in its chemical capacity, and certainly, if there is the flow that an eight-inch pipe will give, with a pressure of 65 pounds to the inch, those of us who do not live in Pittsburgh would think that it is worth saving, and that if it can be utilized dynamically, without at all impairing the efficiency of it afterward as a combustible gas, it would be worth doing. That, I take it, is the point or suggestion that was made by Mr. Durfee.

Mr. Jarboe.—I will answer this point. Sometimes this gas we talk so much about is a minus quantity. Shoenberger & Co., and Hussey, Howe & Co., had to shut down to-day on account of not having gas enough, so it is a minus quantity. At the wells the pressures rise up very high. You take a well flowing with a pressure of 175 pounds to the square inch per minute, and that pressure will be reduced down to 105 pounds by friction in the pipe, and by the time you get it way down town it is not worth much. It is used in the oil country largely where they put it into the engines, but this gas brings up with it so much sand that it is very hard on the engines.

Mr. Roberts.—The question before the public in Pittsburgh turns a good deal upon the domestic consumption of natural gas, and the various means looking to the safety in use of this new fuel. It has been in use for a number of years, as stated in the report, in smaller villages, but when it comes to introducing it over a large city like Pittsburgh it is a different matter. People do not know all the conditions of pressure, etc., etc., and it will be of importance to the companies to post consumers as to the way to handle it in such a community as this, and it is looking toward that point, that we illustrated here so particularly the forms of tank governors to hold this pressure down to the minimum of safety; it was suggested during our examination, that these are questions the public take more interest in than its utility for manufacturing purposes.

I will state for the benefit of our friends that here are specimens of the various forms of joints referred to in the report.

Mr. Jarboe.—(Exhibiting the various styles of joints.) Here is a light casing or light wrought-iron pipe, and here is heavy pipe, 28 pounds to the foot, 8-inch pipe. Here is 6-inch heavy and here 6-inch light casing with taper joint of the National Tube Works. Here is one with taper thread and taper socket to match; here is an ordinary straight socket joint, a Converse lock joint; that light pipe is put together with the lead being poured in here, just the same as cast-iron pipe. That is the one that has been used in Oil City.

Here is a little joint that is made with ordinary standard pipe and standard thread. The ends of the pipe are beveled. One end has a lead rim put in, and the other end of the pipe is put in here and screwed together. They force the lead into the thread and into the thread of the socket and also between the two ends of the pipe, and it makes a very good joint. Here is a taper socket. The socket is tapped from both ends, and the pipe is cut to suit. I do not know that every person here is aware that the pipe sockets are tapped with a straight tap right straight through. The thread of the tap is cut at right angles to the face, and therefore all the threads are at right angles to the face of the socket.

These samples here are used by the Fuel Gas Co. (indicating certain ones). The others are ordinary samples.

Mr. Towne.—On this question of pipe joints it is pertinent, I think, to say that a joint similar to one of those that has just been described, with a lead ring interposed between the two abutting surfaces, has been found very satisfactory in joints connected with Emery testing machines. In that case, however, the ordinary thread is used merely to force together the abutting surfaces which form the joint. That is, not to form a joint on the thread, but to press the abutting faces together on to a lead joint, and to prevent the flowing of the lead joint into the bore of the pipe and its escape in that way, and the resulting formation of a ridge on the interior of the pipe. For this purpose one of the abutting surfaces is grooved, the other is left either flat or with a corresponding tongue which enters the groove, and a ring of lead is laid in the groove and the two surfaces screwed together. In that way the free surface of the lead is reduced to a very small amount, its flow otherwise is prevented, and a tight joint is formed. We have found them tight under very high pressures.

Mr. Jarboe.—Did you ever take one of your pipes apart, screwed together with pipe joints like that, and examine it?

Mr. Towne.—Not in that identical way, but we have made other joints in the same way.

Mr. Jarboe.—That, sir, was tried in England a good many years ago, and they found the same trouble with it that I have found. If the grooves were turned in a perfectly automatic lathe, it was all right; otherwise, when they would face each other that way they would cut each other. When one was used and the other left blank, it just forced the lead right out of the pipe. To a very considerable extent the English avoid that by the use of copper.

Mr. Towne.—The most perfect way in making such a pipe coupling is to form a ring on the interior of the coupling, and to then make on the end of each pipe a square recess to receive the lead ring. The two surfaces are thus forced together, and without the trouble you speak of, because there is no tongue required.

Mr. Kent.—One of the most important questions connected with this natural gas business which has not been fully touched on to-night is, what is to be the future of it? It is very important to gas companies, the owners of wells, and the people of Pittsburgh. Mr. Jarboe gave us some figures to-night in regard to the value of this gas as fuel. If I understood him rightly, he said that gas was worth 7.8 cents for a thousand cubic feet. In order, then, to make fuel gas economical for the manufacturers of Pittsburgh, who use it by the wholesale, they must reduce the price to 7.8 cents for a thousand cubic feet. I do not think it will be practicable to sell such a valuable fuel so cheap for any great length of time, and I may venture the opinion, subject to revision, that the future of the natural gas business in Pittsburgh will be that the gas will not be used on the wholesale plan by the large manufacturers, but they will stick to the cheaper plan of using coal slack with regenerative furnaces. But there is a much larger future, I think, for its use for domestic purposes, and that it will pay to use this gas at a much higher price, even, possibly, fifty cents per one thousand cubic feet for domestic purposes. I think the gas companies will find that it will pay them better to look forward to domestic consumption than for wholesale consumption and its attendant waste.

Mr. Roberts.—Mr. Kent thinks exactly right. I want to give notice that if any of our friends desire to see the gas used in the best form and by a number of ingenious contrivances in a private house, they will find it at the residence of Mr. Howard Morton, in the Twenty-second Ward, Pittsburgh. After getting there they will have a fine view overlooking friend Jones' place, the Monongahela River, and the most superb view in the city of Pittsburgh. I think it would repay those who desire to visit the place. Mr.

Morton requested me to mention this to the Society. He will pilot any gentleman who will go up.

Mr. Jones.—This fine view he speaks of is looking *away* from Pittsburgh.

Mr. Kent.—Mr. President, one of the things people have been looking for, too, in connection with this natural gas business, is the diminishing of the smoke of Pittsburgh. I made the prediction a few years ago that inside of twenty years Pittsburgh would be a clean city. It will be a clean city, probably, through the introduction of natural gas for domestic purposes, the introduction of the Siemens' regenerative furnaces for iron works, and the introduction of smoke-consuming furnaces for boilers. I repeat the prediction made several years ago that Pittsburgh will yet be a comparatively smokeless city.

Prof. Webb.—I would ask, first, what is the pressure of the gas when the price is 7$\frac{8}{10}$ cents per one thousand cubic feet. The second point is whether this gas has been used in gas engines, and, if so, whether the natural pressure was sufficient to do the work required; and third, could not the question of pressure be regulated by using larger pipe?

Mr. Jarboe.—Figures have been taken at five inches pressure and then reduced to a still lower pressure, and it is found that the gas would still be worth 7$\frac{8}{10}$ cents. This question of large or small pipe has been figured down very fine, and in consequence the Fuel Gas Company are laying 8″ pipes where before they laid down casing. The gas engine question is answered in this way:

The American makers of this engine will not furnish an engine to burn the gas, but the English makers say, send on your orders and we will send you engines.

Mr. Miller.—This gas is used in another form by the Carbon Black Company in making ink for printing purposes.

Mr. Painter.—I have figured this question about the cost very thoroughly, and I think in making iron, natural gas is not worth over $1.50 per ton to the finished ton of iron. That is all it is worth, and perhaps less.

Mr. Morton.—In regard to the cost of this gas for domestic purposes, it is my belief that for the laborer who cannot indulge in luxuries, he could afford to use it if it cost more than coal. We have used it at our home in the Twenty-second Ward since the first of December, and when you take into consideration the saving on carpets, wall paper, furniture, labor, and the magnificent satisfaction it gives in every respect, you don't stop to consider the difference between its cost and that of coal. We burn it there in range, furnace, and grates. We get the very highest results from it, and the satisfaction is perfect in every respect. There is no fault to be found with it; it's so clean, so prompt. Breakfast can be cooked by it in a wonderfully short time, and the gas is always ready. No stuffing of chimneys in summer. Often you want just a little fire when it is too warm to burn coal and not warm enough to do without fire at all, and then this gas comes in nicely. We would not be without it now.

Mr. Dempster.—I would ask the committee if they have made any attempts to odorize this gas for domestic purposes? How are you to know where there is one part of gas to certain volumes of air? Have you made any experiments relative to odorizing the gas by which it can be detected when it is escaping?

Mr. Roberts.—Experiments in odorizing this gas have been very unsatisfactory so far. It has been tried in a number of instances. It was suggested in one of the morning papers that it be passed through some of the Legislative bodies of the country. [Laughter.]

Mr. Kirchhoff.—I would like to ask if the committee has any record of any actual explosions.

Mr. Jarboe.—Yes, sir. I was blown about twenty feet once with it. I had my clothes torn, and as fine a boiler setting blown down as you ever saw built.

Mr. Kirchhoff.—Mining engineers are rather afraid of this marsh gas, and you would make their hair stand on end if you propose bringing it into their houses. This is the most dangerous gas of all.

Mr. Jarboe.—We have tested this gas in several different connections. My explosion was due to pure carelessness, although I have been burned several times. My man turned on the gas and then lit it, whereas a fire should be built first, and then the gas turned on. The gas has been used and can be used so that there is no danger in handling it.

As to odorizing it, if you will use a very small quantity of bisulphide of carbon, so arranged that it can be blown into the pipes when the gas is moving through the pipes, it will produce an odor as loud as ordinary carbon oil. But you must use a very small quantity, as any large quantity will make it very dangerous, and therefore it has been thrown aside as a kind of dangerous experiment to try.

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OF THE

TRANSACTIONS OF THE AMERICAN SOCIETY OF MECHANICAL ENGINEERS.

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