Frank J. Sprague

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NOTICE.

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TRANSACTIONS

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AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

VOL. XI JANUARY TO DECEMBER, 1894.

New York, January 17, 1894.

The 83d meeting of the INSTITUTE was held this date at 12 West 31st Street, and was called to order at 8 p.m. by President Houston.

THE PRESIDENT:—The Secretary will read the minutes of the last stated meeting.

On motion of Mr. Wolcott it was voted that the reading of the minutes be dispensed with.

THE PRESIDENT:—Before calling for the paper of the evening, I would like to ask the Secretary to read the list of candidates for associate membership, and the names of members who were either elected at the Council meeting to-day, or transferred from associate to full membership.

THE SECRETARY:—The following candidates for associate membership have been proposed for consideration at the meeting of the Council, Feb. 21st:—

H H. Morehouse, James W. Crosby, E. Randolph Hix, Edward M. Gerry, E. W. Trafford, George E. Wendle, Charles Gesseaume, Charles T. Rittenhouse, Arthur Frantzen, Albert E. Richardson, S. D. Snook, Jas. A. Lighthipe, Albert L. Clough, A. T. Best, Harold Harrison, Clifford D. Babcock, Augustus Treadwell, Jr., Wm S. Barstow, Chas. Edwin Potts, George Forbes.

The following associate members were elected at the Council meeting this afternoon:

Name.	Address.	Endorsed by.
ADAMS, COMFORT A., JR.,	Instructor in Electrical Engineering, Harvard University, 21 Stoughton Hall, Cambridge, Mass.	E. H. Hall. C.F. Uebelacker. E. P. Roberts.
BETHELL, U. N.	Acting General Manager, The Metropolitan Telephone & Telegraph Co., 18 Cortlandt St., N.Y. City	A. E. Kennelly. H. L. Webb. Geo. M. Phelps.
BROADNOX, FRANCIS	Engineer, Safety Insulated Wire and Cable Co., 50 Broadway, New York City.	W. J. Jenks. L. Stieringer. T. C. Martin.
BROICH, JOSEPH	Superintendent and Electrician, with F. Pearce, 448 8th Ave., Brooklyn, N. Y.	Edw. Durant. James Hamblet. J. C. Chamberlain.

ENDE, SIGFRIED H.	Colonnade Hotel, 39 Layfayette Place, New York City.	C F. Chandler. Wm. J. Hammer. F. B. Crocker.
FLANAGAN, THOMAS FRANCIS	Supt. and Electrician, Portsmouth Gas Light Co, Portsmouth. N. H.	Herbert C. Wirt. C. D. Haskins. C. B. Burleigh.
FLINT, BERTRAM P.	Electrical and Mechanical Engineer, with Chas. H. Davis, 120 Broadway, New York City.	Chas. H. Davis. F. S. Holmes. Ralph W. Pope.
KNOX, JAMES MASON	Student in Electrical Engineering, Columbia College, School of Mines, New York City.	F. B. Crocker. Ralph W. Pope. W. H. Freedman.
MEREDITH, WYNN	Asst. Superintendent Operating, Electrical Dept. Midwinter Fair, San Francisco, Cal.	L. S. Boggs. R. H. Pierce. G. Sacco Albanese.
OSTERBERG, MAX	Student in Electrical Engineering, Columbia College, 232 East 62nd St., New York City.	F. B. Crocker. M. I. Pupin. W. H Freedman.
SELDEN, R. L., Jr.	Deep River, Conn.	Ralph W. Pope. T. C. Martin. Geo. H. Guy.
SEVER, GEORGE F.	Instructor in Electrical Engineering, Columbia College, 121 East 30th St., New York City.	M. I. Pupin F. B. Crocker W. H. Freedman
SMITH, CHARLES HENRY,	Assistant Electrician, South Eastern Tariff Association, Atlanta, Ga.	A. F. McKissick. A. M. Schoen. A. E. Worswick,
SPROUT, SIDNEY	Electrical Department, Midwinter Fair, San Francisco, Cal.	L. S. Boggs. R. H Pierce. O. G. Dodge.
WARDLAW, GEORGE A.	Assistant Engineer, People's Light and Power Co., Doolittle House, Oswego, N. Y.	Edw. L. Nichols. F. Bedell. Harris J. Ryan.

Total 15

The following associate members were transferred to full membership upon recommendation of the Board of Examiners, October 3d and December 7th, 1893.

EMMET, W. L. R.	Electrical Engineer, General Electric Co., New York City.
KEITH, NATHANIEL S.	Electrical Engineer, London, Eng.
ADAMS, ALTON D.	Electrician and Manager, Adams' Electric Co., Worcester, Mass.
McCLUER, C. E.	Supt. First District, Southern Bell Telephone and Telegraph Co., Richmond, Va.
JACKSON, J. P.	Assistant Professor of Electrical Engineering, Penn. State College, State College, Pa.

Total 5.

THE PRESIDENT:—The regular order of business is the paper for the evening. We are very fortunate this evening in having with us a gentleman who is in every respect able to handle the important subject on which he is announced to speak, viz.: "Practical Properties of Polyphase Apparatus." Dr. Louis Bell, of Boston, will now read the paper.

DR. BELL read the following paper:

A paper presented at the Eighty-third Meeting of the American Institute of Electrical Engineers, New York, January 17th, 1894, President Houston in the Chair.

PRACTICAL PROPERTIES OF POLYPHASE APPARATUS.

BY LOUIS BELL, PH.D.

During the past two years the use of polyphase currents for power transmission has been the subject of many papers and discussions. The writers engaged have been of two classes, those who have a practical concrete knowledge of the apparatus they comment upon, and those who have not. The former have for the most part merely interpolated casual remarks and made general recommendations. The latter have been prolific in researches upon paper, and experiments upon the electrical bric-a-brac they could build or borrow. The whole matter has been for the most part a sort of "Messiah dance" of *doctrinaires*, varied now and then by a skirmish between rival inventors who for commercial reasons could give the public very little detailed information. Meanwhile the art has been quietly advancing, and to-day, with half-a-dozen plants in operation abroad, and as many more in our own country, it is time to discuss more freely than has hitherto been possible, their nature and properties.

In the present paper I intend to devote myself entirely to experimental results, verified by repeated observation, and leave the mathematical investigation of the subject to those who prefer to conceal their more or less exact knowledge in that alluring guise. I have no lack of respect for mathematics, but it offers a painfully good opportunity for generalizations so broad as to be of no practical use.

In speaking of polyphase apparatus it should be clearly understood that I regard the number of phases as accidence rather than as substance of the matter. As the number of phases is changed, the quite interesting and not always unimportant differences to be noted are more often of degree than of kind, so that while most of my personal experience has been with triphase currents, much of what I have to say is quite applicable to systems having more or less phases, provided, of course, that such systems are symmetrical.

Of systems wherein this condition is not fulfilled I shall have something to say in discussing polyphase motors. I also pre-suppose that we are to deal with currents giving a tolerable approximation to sine waves, such as can without great difficulty be obtained. While it is theoretically true that departure from the sine curve causes a loss of energy due to the presence of higher harmonics, they must be larger than are found in any well designed machine before they produce any loss of efficiency that is of practical consequence. I have observed a measurable decrease of efficiency in an induction motor when used with a generator arranged to give a very wide departure from a sine wave, but even so, less than might easily be due to accidenta variations in the iron used for the laminated structure. Electrical power transmission must be developed into conditions beyond those that now seem to us extreme, before the current curves now readily attainable will need revision. On very long lines and under voltages already near the limit of insulation, we shall certainly have to consider the matter, but in the present state of the art we may generally pass it by. I shall refer later to cases in which it is really of decided importance.

Polyphase Generators.

While the general character of polyphase dynamos must closely resemble that of the ordinary alternator, the necessity of applying to the same armature two or more phase-windings leads to modification of the design so far as the winding is concerned, and quite generally in the direction of the direct current type. In fact the most convenient way of getting polyphase currents in the laboratory or for experimental use, is to tap the head of a continuous current armature in the requisite number of points, put on collecting rings and go ahead. A small multipolar machine of 220 or 500 volts lends itself most readily to this use, but generally will have to be run at a speed considerably higher than normal to give a sufficient number of cycles. The current wave given is apt to be flat-topped, and in small machines the windings cannot always be tapped symmetrically, but the method is often handy. It may sometimes be useful too, to derive polyphase currents from two or more ordinary alternators with their shafts coupled together, or from composite machines having phases on separature armature cores, but these devices are, I think, rather to be regarded as expedients convenient for divers reasons than as final types. This is on the broad general principle that one large machine is more economical than two or more small ones aggregating the same output, and that the more completely the armature core can be utilized, the better output can be obtained from the same structure. Therefore with similar machines of equal size and equal magnetic and electrical constants, a polyphase armature winding does give a better output than a single phase winding. Of course it is possible to construct a single phase machine that shall give a better output per pound of weight than a certain concrete polyphase machine, just as one might manage to double the voltage obtained from a given armature while retaining the same output, but other things being equal the more phases, the better output in a given structure. All this has been made so clear of late that it is only necessary to emphasize the fact, that while the difference in output between a single phase and a two-phase armature is quite considerable, the step from two, to three phases is somewhat less marked, and that from three to more, of relatively little importance.

It should also be noted that as various numbers of phases can be derived quite simply from two and three phases, it is quite unnecessary to consider the more complicated types of generator. The tendency in building polyphase generators is certainly toward the development of a rather better machine than we have become familiar with in the ordinary alternators, due mostly to the impetus given alternating current machinery in general by recent demands. This has led, first, to the adoption of a lower frequency than formerly, both to facilitate the use of motors and to avoid the serious difficulties due to inductance on the long lines that are now becoming more common. Second, it has caused more attention to be given to the production of generators able to take care of fluctuating and inductive loads, such as are produced by motors, without excessive over-compounding. With a properly designed machine, the popular idea of the difficulty of regulation under an inductive load is grossly exaggerated. A generator built as alternating generators should have been built long before now, with proper attention to the production of a good machine for all around practical work, will take care of an inductive load of the severest kind, more easily than the average alternator found to-day in central stations, will handle a load of incandescent lights.

I can point out the practical ease of regulating a polyphase generator under an inductive load in no way so effectively as by giving the results of some experiments recently tried with a triphase generator of 260 K. W. normal output. It was run at 600 revolutions per minute, and a uniform voltage of 2500 volts between lines, driving a synchronous motor of similar size, which was in turn belted to a direct current generator. This arrangement enables one to obtain any desired output with a very wide range in the lag of the current through the line. Accurate readings were taken of the currents necessary to excite the generator, with the following results, the voltage being preserved uniform at 2500:

Exciting current-output about 80 K. W.	No lag	19.05 amp.
Exciting current-output about 80 K. W.	Power Factor .2	21.5 amp
Exciting current-output 260 K. W.	No lag	22.8 amp.
Exciting current-output 260 K. W.	Power Factor .84	24.8 amp

No better test than this could be wished, for showing the excitation required under a lagging load. It should be noted that the power factor in the last case was similar to what would be found under ordinary circumstances, in running a load of inductive motors, and yet the increase in exciting current from a very light load with no lag, to a full load with very decided lag, was only 25 per cent. Experiments with other machines under an inductive load show results similar to this, so that I think we can safely say that a well-designed polyphase generator will take care of an inductive load easily and without excessive variation of the field strength. More than this, the regulation required can easily be made automatic in cases where this method is preferred to hand regulation, which, however, answers most purposes very well.

Of course the polyphase generator can be made self-compounding in a manner closely analogous to that followed with an ordinary alternating generator. The rectification of the necessary amount of current, makes this method somewhat inconvenient in large machines. In such cases, we may effect a very complete automatic regulation by the following means.

[Diagram of the apparatus is shown in Figure 1.]

The generator is excited from a rotary converter taking its polyphase current from the main machine of which it feeds the field magnets through its direct current side. In the lines between the rotary converter and the generator, are inserted inductive resistances which serve to cut down the voltage applied to the polyphase end of the rotary converter. On these choking coils are, however, reversely wound turns from the main circuit of the machine. The result of this arrangement is, that as the current in the main line fed by the generator rises, the inductance in the line which feeds the rotary converter is gradually removed,

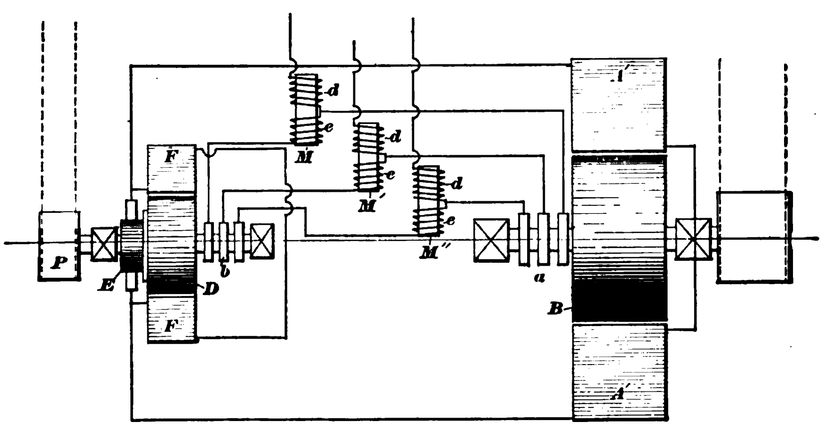

FIG. 1.

allowing the voltage to rise higher and higher, in proportion to the current in the main line.

Fig. 1 gives a clear idea of the arrangement. In this figure, A′ is the field magnet of the main machine of which B is the armature, *a* the three collector rings, the machine being of triphase type; F F are the field magnets of the rotary converter of which D is the armature, *b* the collector rings and E a commutator delivering direct current to the fields of the rotary converter F F and to the field magnets A′ A′ of the generator. The rotary converter may conveniently be brought up to speed by its pulley P unless another triphase machine be at hand by which to start it; M, M′, M″ are the three inductive resistances, *e* the windings connected with the rings *b*, *d* the reversely wound turns connected to the main line. Of course transformers may be inserted in the line between the inductances and the rotary converter, if the voltage of the main generator renders it desirable. In fact such converters wound with cumulative turns from the main circuit may replace inductive resistances. I have experimented, rather, more, however, with the form shown in Fig. 1. It may sometimes be convenient to employ the rotary converter merely to excite an auxiliary compound winding, in which case it can be started up from the generator already worked up nearly to normal voltage by its own exciter. This method of compounding polyphase generators, seems to promise ready applicability to the largest machines and very close automatic regulation of the voltage. The arrangement may be varied, but the underlying principle must remain the same—the automatic variation of voltage on the polyphase end of a rotary converter, in response to the varying output of the generator by means of varying inductances obtained from coils in the main circuit, and acting either directly or reversely to govern voltage applied to the rotary converter. In practice, the method works quite satisfactorily and effectively. It permits of very close regulation of the voltage for all loads at the machine, or of over-compounding at the distant end of the line and that even with severely inductive loads. I have tried a number of experiments to determine the best working conditions of this method of compounding, and the results are most striking.

A certain experimental generator of about 30 K.W. capacity, provided with triphase armature was fitted with this method of regulation, employing for the rotary converter a small 4-pole iron-clad machine fitted with triphase connections. The speed of the generator was kept constant at 600, the rotary converter running at 1500; at no load, the voltage was 119; at full load 120, the load consisting of banks of incandescent lamps operated on the secondaries of converters. In another experiment with the same machine at the same initial voltage, an over-compounding of 10 volts was easily obtained. Without regulation, the generator had so high an armature reaction that it would not regulate within forty per cent. On a standard triphase generator, very close automatic regulation can be obtained by this method, while even with the experimental machine just mentioned above, it is possible to reach quite satisfactory results.

I have a record of experiments made after this paper was written, and illustrated in Fig. 2. The generator was first run with a non-inductive load at 114 volts initial. The output was driven up to 26½ kilowatts and the voltage at the end was 116.0 (A). It had varied slightly in the earlier portion of the curve, the extreme variation from the initial voltage being a couple of volts. That was with a poor machine, and lacked the good results that could have been obtained had the compounding arrangement had less work to do. We then tried the same machine under inductive load (B). The initial voltage was again 114 and it was arranged to over-compound so that at 30 K. W. we got 148 volts. The device over-compounds just as handily as it compounds for uniform voltage, and furthermore I may say that by proper adjustment of the inductive coils and the rotary con-

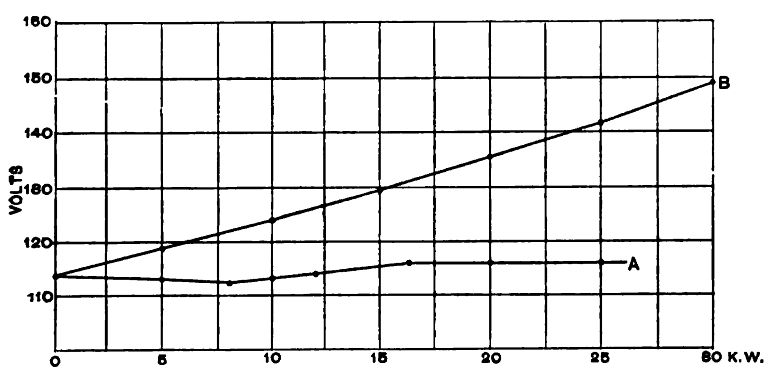

FIG. 2.

verter, it is possible to make the machine compound closely or over-compound, either for inductive load or non-inductive load, or for a load which is varying, sometimes non-inductive and sometimes inductive. The very act of the shifting of the phase due to running with an inductive load, may be made to help the rotary converter overcome the lagging current, as was the case in the example I have just given, where the voltage rose something like 25 volts on a heavy load consisting of induction motors running very light.

All this matter of regulation, is of particular moment in installations intended mainly for power purposes where the variations in load are apt to be considerable. Where, however, these variations are extreme I do not believe that any automatic apparatus can altogether obviate the necessity of watchful and intelligent hand regulation, especially when waterwheels are the prime movers. It should be remembered, however, that the variations in voltage which pass altogether unnoticed in ordinary service, and are practically of little account, are much greater than would be at first sight supposed. An evening passed in any station operated by water power teaches a useful lesson in this respect. Quite considerable occasional variations, particularly if slow, are far less objectionable than repeated or periodic ones of much less magnitude, such as are produced by "hunting" in a governor or overmuch fussing with the regulating devices. A governor should be "dead beat" even if something of sensitiveness is sacrificed to that end.

In passing from the subject of generators, I need only say that polyphase machines present no startling peculiarities and possess no mystical properties. All that is needful to produce a good polyphase dynamo is conscientious, straightforward, intelligent designing; fads and overmuch ingenuity generally do more harm than good.

Polyphase Generators as Motors.

Polyphase, like other alternating current generators make good synchronous motors. In fact, they are decidedly better than others for this purpose, in that they are self-starting with more or less facility, and do not go out of step quite so easily as the single phase machines. They are, as it were, more flexible. They start really as induction motors; that is, they may properly be regarded as induction motors having a revolving primary element and a non-laminated fixed secondary without windings, exerting torque simply by virtue of induced currents in the pole-pieces.

Naturally they cannot start very efficiently under these conditions and for motor purposes it is much better somewhat to modify the structure, so as to provide secondary windings or other devices to approximate the effect of a genuine induction motor. Nothing very complicated in this line is necessary, and it is quite easy to produce a synchronous motor that will be self-starting even with a considerable load of shafting. In the Redlands triphase plant for example, the motor of 150 H. P. is belted to a short countershaft mounted in a timber frame and carrying two large pulleys. The smaller of these, drives by a 36-inch belt a wooden wheel 18 feet in diameter mounted on a crank shaft that drives two large ammonia compressors. A pulley on this shaft is belted back to the framework aforementioned, and drives a force pump for the ice-making machinery, a circulating pump for the water jackets of the compressors, and a small ice elevator. All this machinery is brought promptly up to synchronism by the motor without the intervention of clutches. The starting torque is thus quite sufficient for most ordinary uses to which a large motor is likely to be put.

The synchronous type of polyphase motor is especially adapted to large units. Being a non-laminated structure it is easier to build and somewhat cheaper than an induction motor of the same size would be, and furthermore it possesses the advantage of introducing no lagging current whatever into the line, except at the moment of starting. While this lagging current is not of great moment on ordinary lines, it may as well be avoided where by so doing a machine equally as good and cheap can be obtained without any additional complications. With large units too, there is generally less need of a very great starting torque, and consequently induction motors are far less necessary in the large sizes than in the smaller ones.

There is no practical difficulty whatever in building or operating these large synchronous self-starting motors and they are likely to come into extensive use.

I can hardly do better in closing a description of polyphase generators and synchronous motors than to give some of the data obtained from tests of these machines.

A 260 K. W. machine showed at full load about 94 per cent. commercial efficiency. On a continuous run of 9½ hours under full load the heat developed was as follows, the figures given being rise in temperature of the several parts above the temperature of the surrounding air:

Field coils	18° C.
Yoke	7°.5 C.
Armature teeth	24° C.
Armature heads	20° C.

Taking these figures in connection with the data already given for exciting current, a pretty clear idea can be obtained of the general character of a modern polyphase generator or motor.

INDUCTION MOTORS.

Perhaps the most striking characteristic of the polyphase system is the use of the polyphase induction motor, obviating as it does necessity for moving contacts, and enabling the ready employment of forms of winding which give remarkable immunity from the ills to which continuous current motors are heir. In all that has been said about these motors there has been an unfortunate absence of exact data, particularly as regards their ability to start under heavy loads, the current required at starting, the current required when running light, the lag factors, light and loaded, the variations of speed under varying load, and other characteristics which are of direct practical importance in the use of such apparatus.

Of the various polyphase motors which have been heretofore described, some appear to have had certain of these above-mentioned properties well developed, and others very badly developed. None so far as I know, seem to have reached any carefully considered balance of the properties necessary to make a good commercial motor.

It is my purpose now to take up the induction motor as it can be, and is developed at the present time, and to give some plain facts concerning its actual properties; not determined from single experimental machines but from types which have been pretty thoroughly tried. I feel it especially desirable to do this in order to correct some of the errors into which those who are even well informed about polyphase machinery, have but too readily fallen.

The induction motor consists essentially of two laminated structures, one fixed and the other revolving, and each wound symmetrically for phases more or less in number according to the system on which the machine is to be employed. It may be considered as a transformer with its magnetic circuit imperfectly closed, and of which one member is free to revolve. For mechanical reasons I think it is generally preferable that the primary element which receives the higher voltage should be fixed, and the low voltage, secondary element, movable. By this arrangement too, all necessity for moving contacts is avoided. For certain specific purposes the reverse of this plan may now and then be advisable.

As to the general properties of these machines, I can hardly sum them up better, than to say that they behave in a manner strikingly like well designed shunt-wound continuous current motors, having, however, the advantage of being simpler and of having no commutator. Even their efficiency is closely similiar to that of continuous current motors of the same size, both as regards its maximum value, and its value under moderate loads. The hysteretic losses and those due to parasitic currents are, however, a source of inefficiency that has to be carefully considered in order to reach a satisfactory result.

These losses are located in large measure in the primary element of the motor, and would apparently indicate the desirability of making this the revolving, and naturally smaller part of the machine. The advantage, however, of using a very simple and substantial winding on the revolving element, is too great to forego for the sake of a little saving in hysteresis.

Induction motors can be made to start readily under load, yielding when properly designed for the purpose, a starting torque up to four or five times the full load running torque. Even more than this can be obtained if it be needed, but for most practical purposes, running torque is quite sufficient, and it is perhaps best generally, not to design motors with abnormal starting powers in view, regarding such rather as special types. As in continuous current work, a motor fitted for unusually heavy strains is not necessarily the best all around machine.

For everything except extraordinary requirements such as may be sometimes met in hoisting, a uniform design and rating may be conveniently kept, as it is very easy to regulate the torque and the current required to produce it within wide limits, by varying the resistance included at starting in the secondary circuit. I know of no motors without such a starting resistance that are capable of giving any considerable starting torque without an enormous initial rush of current, and even were one designed to secure a good torque without armature resistance, it would be almost certain to exhibit various other undesirable qualities that would more than offset the advantage.

The office of the starting resistance is two-fold : First. it limits the possible current in the armature so that it will not beat back the induction from the field ; that is, it sets a limit on the armature reaction. Second, it limits also the lagging in phase of the armature behind the field. There is a particular resistance best suited to each case, for which the torque is a maximum, at the given voltage. Any variation from this value will diminish the torque, the current meanwhile rising or falling according as the resistance is diminished or increased. This critical resistance should be found and used whenever it is necessary to start under abnormal loads. At a certain second value of the armature resistance, the torque per ampere will be a maximum, and this determines the best resistance to be used for cases where a large static torque is not necessary. Somewhere between these points will be found the best working resistance for practical purposes. The first point named does not call for impracticable current,

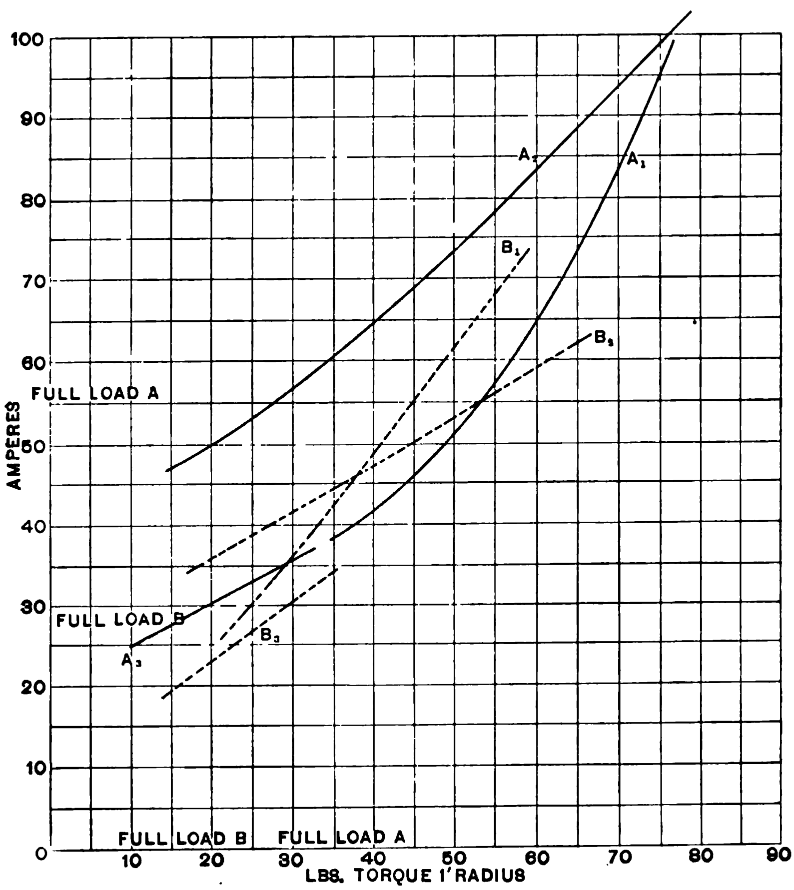

FIG. 3.

not the latter for inconvenient torque, if the motors be properly designed.

In using a starting resistance I prefer to fix it within the armature spider and so to avoid all need of collecting rings. Save for this device, moving contacts of some sort must be used either to lead the primary current into the revolving element, or to connect the resistance to the secondary, either of which procedures is objectionable, particularly the latter.

There is no subject connected with polyphase work which has been the theme of more hasty conclusions and ill-advised criticism than the current required by polyphase motors in starting. The popular verdict pronounces it enormous, an opinion generally derived from hearsay, or from experiments with poorly designed motors, often with armatures of the squirrel cage type, than which nothing is more ineffective in starting, if the motor be decent in other respects.

Nothing can be more grossly exaggerated than this common idea of immense starting currents. The best way to refute it permanently is to give exact experimental figures. The annexed curves (Fig. 3) will show very plainly the facts in the case. The torques given are genuinely static, being obtained by fitting a horizontal brake beam to the shaft of the motor and resting a stud on the beam upon a platform scale. The resistances used were of manganin strip. The currents were obtained from a current indicator standardized from a Siemens dynamometer, and the readings were made after the pointer had come to rest, a precaution very necessary, as the instrument was not at all dead beat. The current thus obtained is a true maximum for the given torque, the armature being permanently clamped at rest.

In Fig. 3 the relation between starting torque and current is very clearly shown, as well as the importance of the starting resistance. In the figure the curves A_1, A_2, A_3 belong to a 10 H. P. triphase induction motor, and B_1, B_2, B_3 to a similar machine of 5 H. P.

A_1 shows the effect of varying the resistance in the secondary on the relations between starting torque and current, the voltage being kept normal and constant. A_2 shows the variation of torque with current for a given fixed resistance, the voltage being varied, and the resistance being such as to give heavy torque. A_3 is the same as A_2 except that the resistance was such as to give very moderate starting currents. Full load torque was 35 lbs.

B_1, B_2, B_3 are similar curves from a 5 H. P. triphase motor, the full load torque being 17.5 lbs. Now examine the curves. Instead of abnormal currents being required in starting, each of the motors under examination will develop full running torque on considerably less than full load current. At full load current, in fact, each of them gives some fifty per cent. more than running torque. And this condition of things is not in the least exceptional—it will be true of any properly designed motor unless it be intentionally adjusted to have a very great starting

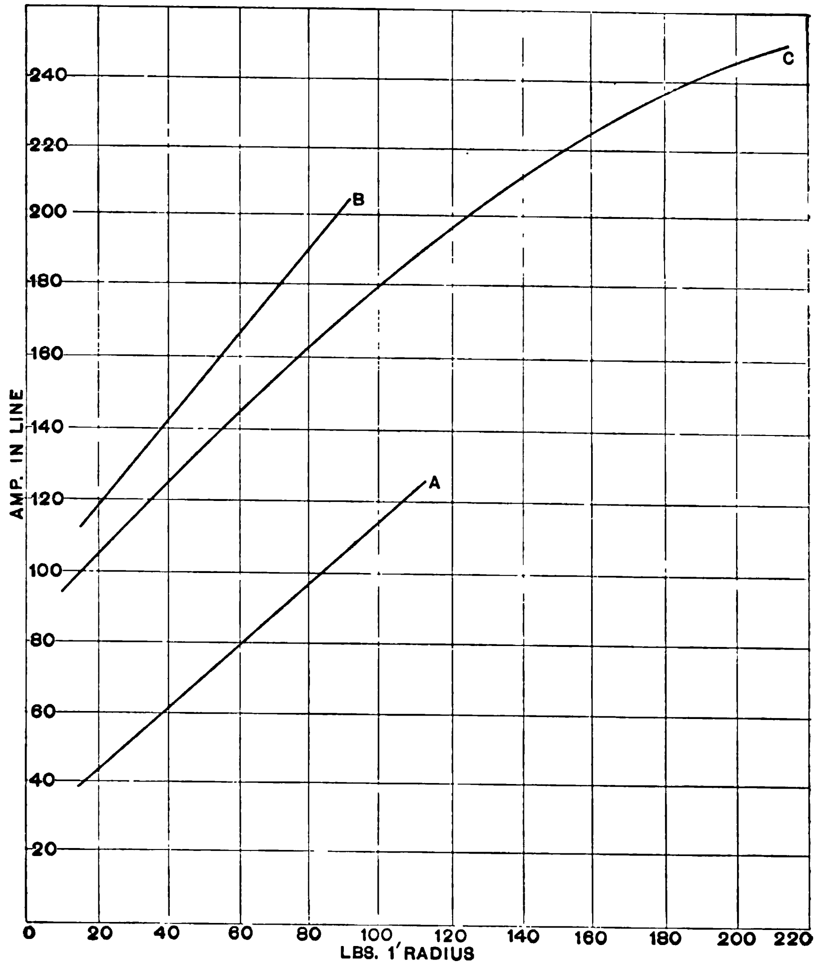

FIG. 4.

effort at normal voltage, as shown in Fig. 4 which is a curve from a 10 H. P. motor. Here curve A shows the relation between current and torque with a carefully adjusted resistance, and curve B the same relation without armature resistance. In this latter case we have reproduced just the state of affairs that is encountered when one attempts to start an induction motor by a rheostat in the primary circuit. A comparison of A and B tells its own story as to the advisability of this procedure. The results are never comparable with those obtained with a resistance in the secondary under similar conditions. Curve C gives the torque of a special 15 H.P. motor, full running torque being 52.5 lbs. It may be well here to note that an ohmic resistance cannot be replaced by an inductive resistance for the purpose under consideration, as the armature current is thrown thereby so much out of phase that no even tolerable results can be obtained. In leaving the subject, I need only say that there is no particular difficulty in constructing a polyphase motor that will give any torque that can reasonably be asked, and with a starting current by no means disproportionate to the result obtained.

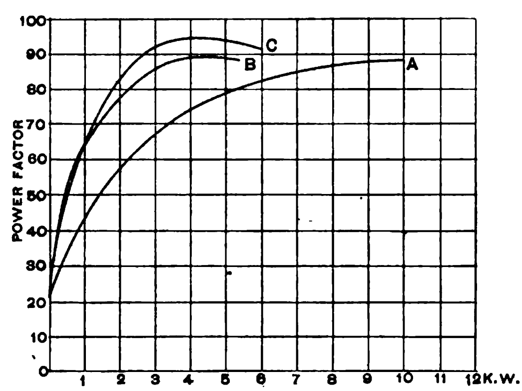

FIG. 5.

The next thing that comes up for consideration, is the power factor which may be expected in polyphase motors. By the power factor we mean the ratio between real and apparent watts which measures the angle of lag introduced by the motor. It goes without saying that a high power factor is desirable both on the ground of requiring less current capacity in the lines and generators and on account of less inductance in the circuit, and consequently less trouble in keeping up the proper voltage. In motors of various designs, the power factor is probably the point in which there is the greatest and most serious variation.

Fig. 5 gives three curves showing the variation of power factor with load, in three typical three phase motors. Curve A is the power factor of a 15 H. P. four-pole motor designed to run at 50 cycles. Curve B is from a 5 H. P. motor of closely similar design. Curve C is from another 5 H. P. motor which had specially valuable characteristics in the matter of power factor. The last curve shows what may be accomplished by designing with special care for a high value of power factor.

Now the points to be noticed in these curves are the following:

1st. That in all three motors at and near full load, the power factor is closely in the vicinity of 90 per cent.—in curve C fully 94 per cent.

2d. The power factor even at half load is still good. In the 15 H. P. motor it is 84 per cent.; in one of the 5 H. P. motors it is 75 per cent.: and in the other 79 per cent. In fact the half

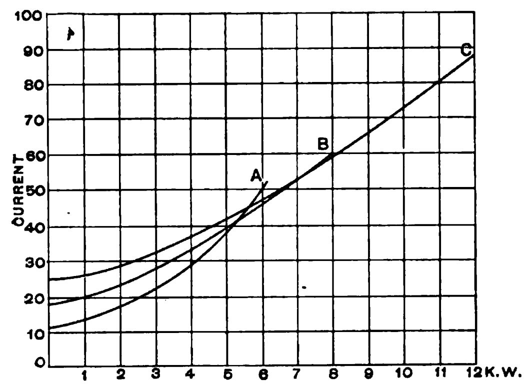

FIG. 6.

load values for the power factor, as here shown, are greater than the full load power factors of European three phase motors which have been described up to date. The Dobrowolsky 5 H. P. three phase motor[1] has a full load power factor of but .68, while the power factor of a small Oerlikon motor (as described by Mr. Kapp from actual tests) is but 74 per cent. at full load. We must not, however, attach too much practical importance to very high power factors, for the reason that those shown in Fig. 5 are already within the range of first-class commercial work. Perhaps we may better investigate the relation of this power factor in actual operation by reference to Fig. 6 which shows the relative currents required at different loads by three

1. *Electrical World*, April 22, 1893.

sizes of three phase motors. Curve A is for a 5 H. P. motor, curve B for a 10 H. P., and curve C for a 15 H. P. For convenience we will call the ordinates total amperes required.

Suppose now we have a dynamo running an exclusively motor load composed of these three sizes of machines and let us see what will be the actual conditions when the motors are running fully and partly loaded. Let us suppose the load to consist of 10, 15 H. P., 10, 10 H. P., and 20, 5 H. P. motors running at full load. The current demanded (as by the curves on Fig. 6 for this combination, is 1950 amperes, and the average lag factor will be at least 88 per cent., which could readily be raised to 90 per cent. if it were desirable to pay special attention to that feature of design.

At half load the aggregate current required would be 1125 amperes—57 per cent. of the full load amperes—and the average power factor 81 per cent. Even at one-third load the condition of things is by no means as serious as it might be, inasmuch as the aggregate current is 890 amperes—45 per cent. of the full load amperes—and the power factor is still 72 per cent. The generator can readily take care of any of these loads without serious trouble from the lag introduced on the line, and even supposing that the total generator capacity be taken at 200 H. P. instead of 350, the aggregate capacity of the motors, it would still be able to operate all the motors at half or one-third load, without unreasonable over-excitation on account of the lagging current, as may readily be perceived by reference of figures for excitation and the lagging current which I have previously given.

Of course it is possible to reduce the lag factor perceptibly by the employment of condensers, but it is certainly an open question whether so long as it is practicable to obtain power factors in the vicinity of 90 per cent., and even above it, without using condensers, the extra gain is worth the extra complication. There will certainly be some value of the power factor which it will not pay to increase by adding condensers—just what value it is hard to predict until condensers have come into more general use. Even if condensers be used they will not necessarily bring up the power factors, at moderate loads, to any very startling figures.

The place where it is most desirable to retain as high a value as possible for the power factor, is where the motors are to be used on long circuits, in which case it is important to keep the general inductance of the system low. In this connection, however, nearly all the long distance propositions which I have investigated—and their number is very great—require the use of mixed apparatus—induction motors, synchronous motors and lighting, in which case the general energy factor can be kept fairly high, particularly as the synchronous motors can even be made to compensate in large measure for the presence of the induction motors.

A word now with reference to efficiency. Fig. 7 gives a pair of efficiency curves, one of them, A, taken from a 5 H. P. motor, the other, B from a 20 H. P. machine. The 5 H. P. motor had four poles, the 20 H. P., six, and both were intended to run at 50

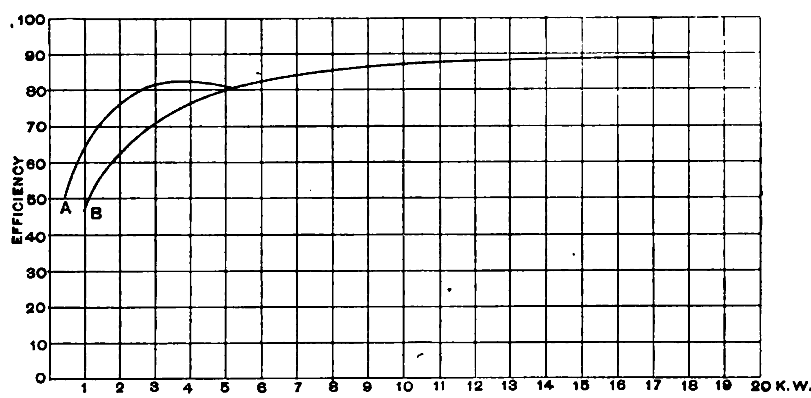

FIG. 7.

cycles. A glance at the curves will show that their full load efficiencies were respectively a trifle over 82 and 88 per cent., the larger motor in particular retaining a very uniform efficiency from half load up. These figures can be raised by paying the same attention to the iron as in the case of transformers which do not have a movable secondary.

The two in question had stampings of ordinarily good armature iron without extraordinary precautions is annealing. From a special 5 H. P. motor I have obtained a full load efficiency of very nearly 90 per cent. I can hardly refrain from comparing these efficiency tests with those of motors of similar size and of the synchronous alternating type, as made by Ganz and Co., and described in the report of the Frankfort Commission, the periodicity being 42. In this case the maximum efficiency of a 25 H. P.

motor was 88 per cent., the half load efficiency about 80, while the maximum efficiency of a 5 H. P. motor was but 80 per cent. All these are, of course, commercial efficiencies. Motors of other sizes will bear about the same relation to the figures given as in the case of ordinary continuous current machines, the two shown being thoroughly typical.

To conclude the matter of efficiency I annex the weight efficiencies of five sizes of induction motors:

H. P.	Weight per H. P.
5	108
10	66
15	68
20	78, 6-pole
100	66, 8-pole

These weights compare extremely well with those of any standard direct current machines, and were light weight a special object, could readily be considerably improved.

Much has been said regarding the effect of frequency on the properties of induction motors, and I am sorry to say, most generally from a purely theoretical standpoint. Judging from numerous experiments between 30 and 70 cycles, I am strongly of the opinion that within this range not much is gained or lost by varying the frequency, provided the motors are designed with reference to the particular frequency at which they are to be used, although as in static transformers, increased frequency tends to increased output. As reasons quite independent of the character of the motors which can be obtained, limit the frequency advisable in power transmission work to something like the working range just mentioned, I do not think the gain is one of sufficient importance to cause a choice to be made of one moderate frequency rather than another. For example, low cycles compel the use of uncomfortably large 2-pole induction motors, while at high cycles even very small motors must have 6 or 8 poles. At 10 cycles the highest possible motor speed is 600, at 100 8 poles are necessary to get any decently low speed.

I will now briefly pay my respects to what are really unsymmetrical polyphase motors, that is, the so-called non-synchronous single phase motors, which start by means of some phase differentiating device, whereby a derived phase is employed to produce a sort of rotary pole. The result is what may be characterized as an elliptical rotary magnetization, as distinguished from a circular one, and consequent loss of efficiency. In general they start and run like bad polyphase machines, have a less output for the same size, and a lower efficiency. These statements are amply borne out by figures which have been published on the Oerlikon three phase and single phase motors, showing that the latter have for the same output, greater weight and less efficiency. The power factors of such motors are decidedly below those of polyphase motors. I believe that it would be possible, however, to design a motor of this class which should have nearly as good properties as the polyphase motors I have described, but it would be costly, and would perform much better, if regularly wound for two or three phases. In other words the same structure that would give a good single phase motor would give a phenomenal polyphase one. With single phase induction motors, the use of condensers would be decidedly advantageous, and they thus might be made fairly operative even at rather high cycles.

Transformers.

The static transformers used for polyphase work being, in all essential particulars, like those with which the public is already familiar, little need really be said on this score except that in most cases the average efficiency of transformers in polyphase plants, will be found decidedly better than in the lighting plants now in operation, principally for the reason that larger units are, and will be, generally used.

The question of frequency is of more importance, however, and the general facts in the case are that the low frequency transformers, other things being equal, are the more bulky and expensive. though not in a very great degree. Within the working range for power transmission work, the difference is, as in the case of motors, not so considerable as entirely to overshadow other considerations which arise in specific cases, such as those of permissible inductance, size of units, permissible heating, loss of energy generally under the particular circumstance considered, and the like.

Rotary Converters.

The convenient and efficient use of rotary converters is a very characteristic advantage of polyphase systems. These machines have already been considerably discussed, and it is perhaps sufficient to say, as regards their general character, that they are, in all essential particulars, closely similar to standard direct current machines. They are self-starting, can readily be compounded for constant potential, and behave in all respects like direct current generators. It is possible to make a similar single phase machine of somewhat smaller output for the same size, and non-self-starting. In actual operation they are highly satisfactory and appear to give considerably better results than any methods which have yet been brought forward for converting alternating into direct current by any species of commutation.

As a good many of our friends who are not deeply versed in the electrical industry say, electricity is in its infancy, and from all I can learn, the art of directly commuting an alternating current and getting a decent direct current is very decidedly in its infancy.

In the use of rotary transformers, the frequency is a consideration of much more importance than in the case of transformers or of induction motors. It is a matter of some difficulty to build a large unit for high frequency, the difficulty being encountered, as might be expected, in the commutator. It is probably as easy or easier to build a 500 K. W. unit for 30 cycles, as it is to build a 100 K. W. unit for 60, so that where, for any cause, it becomes necessary to use a large number of rotary transformers, this necessity may quite control one's choice of the precise frequency to be employed.

In all practical cases of transmission of power, rotary converters require the use of transformers for reducing the line potential, inasmuch as if they are not used, the voltage on the main line will necessarily be so low that it would be quite as well or better to transmit the continuous current directly.

The System.

In planning a polyphase system, one of the first questions that naturally arises, is that of frequency in its relation to the machinery, the line and the service. So far as the first count is concerned, one can say that while in general the higher the frequency the less the bulk of the apparatus, each size of unit has a certain frequency which gives the greatest economy in material and labor. The larger the unit the less this frequency. The rate of change, however, is rather slow, and as an actual result of designing with economy in view, the largest units as yet proposed, 5,000 to 10,000 H. P., will show very little difference in economy between say 25 and 40 cycles. In discussing frequency, theorists have often fallen into the grievous error of considering only the generating units. The load of transformers and motors is of equal, in fact of greater importance, since the actual aggregate cost is far greater, and in nearly every practical case, will call for a frequency considerably higher than in the case of the generating units. One might, for instance, by ignoring this fact, save $10,000 in the generators and lose $50,000 in the transformers and motors.

So far as the line alone is concerned, the lower frequencies are the better, since they reduce inductance and static effects. Nevertheless too much importance should not be attached to this, since the vast majority of cases can be handled with perfect success at a frequency of 50 or 60 ~. For example, one can transmit 1,000 K. W. at 10,000 volts a distance of 20 miles or so on a single circuit (three phase) and still have the total impedance less than double the ohmic resistance. In practice, such a system would generally too, carry a considerable output in synchronous motors which can be, and should be, made to serve as gigantic capacities, reducing, or even annulling, the inductive drop. Finally we come to the service. There is a frequency below which incandescent lamps cannot be operated without perceptible flickering. This frequency certainly varies with different observers and perhaps also with the same observer at different times. Professor Mangarini and others have placed the limit as high as 40 or 50 ~. The average eye, I think, is less sensitive than this. Personally, the limit seems to be about 30 ~ certainly over 25 ~ and under 35 ~, and a number of other observers unite on about these same figures. It would be almost impossible not to notice variations at 25 ~, and they are most annoying, not to say intolerable: 30 ~ certainly leaves a small enough factor of safety.

Alternating arc lights are even more sensitive. The very best carbons begin to give considerable trouble at 40 ~, and with most carbons, flickering is perceptible at 50 or even at 60.

Inasmuch as most large systems find the arc lighting desirable, and practically all must use many incandescent lamps, the frequency should certainly be kept above 30 ~ and preferably above 40. To go below 30 ~ is wantonly to throw away the most characteristic advantages of the alternating system, and in the vast majority of cases is not worthy of serious consideration.

CAPACITY.

To my mind the most serious consideration in long overhead lines and all underground lines is static capacity. Particularly is this the case when the current waves are non-sinusoidal, for in such case we can get phenomena of resonance not only from the fundamental frequency which is generally low enough to keep out of the way, but from the higher harmonics as well. These are liable to produce discharges that will rupture almost any finite insulation. For this reason the sine wave is highly desirable, far more than from any considerations of efficiency. I am sure, however, that a sufficiently close approximation to it can generally be obtained from a properly designed dynamo, and the inductance that generally exists, helps to muffle the higher harmonics. Resonance is generally in evidence on long lines however, and makes itself felt by a tendency to spark and sputter beyond the capabilities of the nominal voltage. It may prove advisable in some cases when dealing with non-sinusoidal waves, to give the line artificially a capacity that will not readily respond to the most prominent harmonics. I remember once experimenting with a tuning fork with a sixth harmonic that quite drowned the fundamental, and it is quite possible to conceive of a dynamo with undesirable characteristics of a similar sort.

VOLTAGE.

The upper limit of practicable voltage is most uncertain: 5,000 and 10,000 volts have been experimentally shown to be available, and 15 to 25,000 possible If the conditions to be met render these voltages necessary, I think they can be handled well enough. The question really resolves itself into the commercial one of paying for the necessary precautions in insulation. A voltage will be reached, however, at which these precautions will cost more than the extra copper required for a lower voltage, and here commercial necessity will call a halt. This point, however, has yet to be experimentally found.

Whether these high voltages should be derived direct from the machine, or obtained from step-up transformers, is a question which has been often discussed. The high voltage dynamo in very large sizes has the advantage in first cost over one of low voltage plus transformers, but runs a far greater risk of serious injuries. Besides, what is of practical importance, is that with a high voltage dynamo, if anything happens, the dynamo is gone. Take a 10,000-volt dynamo, let a rupture once get started, and it is a case of ruined machine practically every time. When you have transformers and a burn-out, you may have lost one or more transformers to be sure, but probably you are running banked. Indeed it is very foolish to put all one's eggs in one basket in such a matter. So instead of losing a 5,000 or 10,000 H. P. dynamo you may lose only a transformer of 100 K. W. or something of that kind. Although we would all like to use high voltage dynamos on account of economy in first cost, until they have been practically proved to be sufficiently free from break-downs, I should say it would be very poor practice to use them extensively, although they are now being tried somewhat and I hope we shall find the results to be good.

I am now compelled to disturb a very much mooted question, that is, the amount of copper required for polyphase lines. It goes almost without saying that all polyphase systems using a complete circuit per phase will require the same amount of copper as an ordinary alternating system. Some statements have been going around recently as to the relative amount of copper required for the alternating system and for the direct current, which, I think, are very largely founded on a misconception. They seem to proceed on the principle that in all cases where alternating currents can be employed, we may consider direct current as a straightforward competitor. This is certainly not so. In a vast number of cases where we have to deal with voltages somewhere near the limit of insulation, the direct current is out of it from the start, and is not to be considered at all. In the second place it is a grave question whether the electrolytic strains of the direct current are not under some circumstances, perhaps many circumstances, fully as bad as the electrostatic stresses caused by the somewhat higher alternating voltage. That is a subject that will have to be studied very thoroughly before we shall know quite where we stand. Now as regards the copper required for polyphase circuits which are interlinked, I went to the trouble, for the sake of informing a few recalcitrant persons who do not want to believe a mathematical demonstration; of having an experiment actually made, setting up a triphase generator with a bank of lamps in the laboratory, and trying the relative conditions as between a single phase system and a triphase system with interconnected circuits. The experiment was performed in an exceedingly simple manner. Four non-inductive artificial lines were prepared, and using two of these in parallel, a certain amount of energy was transmitted to lamps banked at the other end. A single phase current was used, and the losses carefully measured. Then one of the lines, this experiment having been made, was removed, and the remaining three wires worked on the triphase system, transmitted the same energy at the same initial voltage, with the same loss, showing conclusively, as might be expected, that theory in the matter is quite correct and that the actual saving of copper in the interconnected triphase system is twenty-five per cent. The experiment came out within one per cent. of the calculated amount, and this must be true whatever assumption is made regarding the conditions under which we are to transmit power. Take a plain alternating system at any voltage you please, and compute the copper on any series of hypotheses that may suit your fancy, yet the interconnected three phase will do the same work, and do it just as well with three-fourths the outlay for copper. It is precisely equivalent in its net result on the economy of the system to raising the voltage about fifteen per cent. without, however, any added strain on the insulation, and I think the man would indeed stulify himself who would deny that raising the voltage affects the economy of the system. Aside from all other questions concerning it, an additional advantage of this particular type of circuit is the greatly reduced inductance for the same energy transmitted under similar conditions, amounting only to about 57 per cent. of that found on a single phase circuit. This reduced inductance, which after all is in part attributable to the less cross-section of copper required, or is rather interlinked with it, is perhaps the greatest advantage that this particular system has, and makes it of extreme value on all very long lines. There are not very many points which differentiate triphase from other polyphase systems, and so far as my knowledge goes, the two that I have mentioned are by far the most important, more especially that which relates to inductance and incidentally to capacity—getting around as it does the "bugs" which are most dreaded on very long distances. So much for some of the practical considerations regarding polyphase plants.

I may say that we now have in this country five polyphase plants running, not all of them very big, but perhaps averaging as large as the foreign ones. One of them, that of my friend

Stanley at Pittsfield, is a two phase plant. The others at Taftsville, Conn.; Concord, N. H.; Hartford, Conn., and Redlands, Cal., are triphase. Of those four triphase plants, one is particularly concerned with running induction motors. It is a small temporary generator installed in the present station, and taking care of several induction motors. Two others—those in Connecticut—are synchronous plants, the Taftsville plant consisting of a 300 H. P. generator and similar motor, and the Hartford plant being of like size but an older type of machine. The Taftsville plant I started only yesterday. The purpose of the plant is to drive a cotton mill. Yesterday afternoon a synchronous motor was started up—and it did start quite readily—at the Taftsville end of the line, the power station being four and a half miles distant and the voltage being 2,500. When the motor got up to speed, the motor clutch was thrown in so that the motor was running in parallel with the engine, and the load was then shifted from the engine to the motor without producing any noticeable disturbance at all. The shuttles moved sluggishly for a few strokes, and then went up to their normal speed, the service on the looms, which are occupied with weaving specially delicate cotton fabrics, being quite uninterrupted. In the course of the afternoon when it got quite dark at the power station, so that there was not sufficient light to attend to the long lines of shafting, the reverse process was put in play. The engine was started up in parallel with the motor, and then the motor was cut out by its clutch and the plant shut down, all without creating any disturbance.

The Redlands plant is rather the most interesting of the four three phase plants, inasmuch as it is a mixed plant running large synchronous induction motors and lights off the same generator. Redlands, Cal., is a small city not far from San Bernardino and at the head of the valley that sweeps down towards the Pacific. The power station is seven and one half miles from the centre of distribution, and nine and one half miles from the extreme end of the circuit. About four and one half miles from the power station is an artificial ice plant where is installed a 150 H. P. triphase synchronous motor. There are two or three small motors and a considerable number of lights running in the city of Redlands itself. I started up this plant the 7th of last September. That is the first triphase plant of any magnitude that we put in operation on this side of the water. My experience with it was very satisfactory. The power house is in a most inaccessible location, and getting the generators there was no small job, but about a week after they reached Redlands I had the plant running and turned on lights. The point in which I was particularly interested was the performance of the motor, it being of the synchronous type and having a very unpleasant load to start with. The ice plant was one of what are called the pipe variety, where the compressed ammonia is made to expand in great tiers of pipes on which the ice forms in gigantic icicles which grow together into great barriers of ice that are afterwards cut away. There are about twenty miles of pipe in the plant, and for three weeks, perhaps, we had amusement in starting up that motor, pumping up pressure on the receivers, shutting down, testing pipes, then starting up and doing it over again. So we had a fine opportunity to observe how the motor acted when it started, and it started extremely well. On one occasion it started altogether too well. It was belted to two large ammonia compressors, and the very practical men who were running the ice plant looked with something of contempt on the small size of the motor. It did not look nearly as big as an engine ought to, and I think they had a sneaking suspicion that it would not start, or even if it did start, it would not do so with any regularity. One day I caught the superintendent of the plant standing behind the cylinder of one of the compressors shutting off the main valve, while the motor was running in synchronism. I told him that he had better quit, because if he did not, he might go out through the side of the house with the end of the cylinder just behind him. He quit temporarily. But about a week later, when I was in the city of Redlands, they started up and forgot to open the by-pass valves which allowed the motor to come up smoothly without any excessive load—purely through accident, of course. The result was, that the motor made about ten turns. The big driving wheel, 18 feet in diameter, then proceeded to get in its work, pulled the pillow-blocks off both compressors, snapped the castings which supported them and linked them to the rest of the compressor, as you would snap a pipe stem, tore the end bearings completely off, dropped the wheel into the pit, and sprung the shaft. All that, was the result of about ten turns of the motor, starting absolutely from rest and starting as an induction machine. After that experience the proprietors of the ice plant looked on the motor as "heap big medicine." We had no trouble whatever in parallel running in the generating plant. The generators, 250 K. W. at 2,500 volts would go into step and run together perfectly well. No artificial load was used in throwing them in. If the load were on one, the other was simply brought up and thrown in with it. It was not often that we had occasion to run them any length of time in parallel—only for a few hours and usually simply in changing over. But there was not a particle of difficulty. They ran as smoothly as two railway generators would. So that with a machine which has a comparatively small armature reaction and a frequency of about 50, these triphase generators will run in parallel as nicely as if they were direct current.

We were also somewhat interested in seeing if there would be any trouble from unbalancing on the line. We had heard a good deal about it, and the Redlands company had been told a great deal about it by kind friends, so that we were much interested in seeing the effect. The practical result was, as it will be in every case where even ordinary intelligence is exercised in planning the plant, that it did balance. It is perfectly true that a triphase interconnected circuit, if very unequally loaded on the three branches is liable to get out of balance somewhat. It may, if conditions are unfavorable, get out of balance quite a little. If conditions are as favorable as they can readily be made, you will never know that there is such a thing as lack of balance. It does not begin to be as sensitive as a three-wire system. In fact all the abuse heaped on the lack of balance in the three phase system was poured on with double vigor years ago when the three wire system started. All the sore-heads and old fogies swore by all that was holy, that the three wire system would not balance, and to-day a very large proportion of all the incandescent central stations using continuous currents are running three wire. I suppose that we are to go through the same experience with the three phase. It is perfectly true that the system will be unbalanced in very unfavorable conditions, but if ordinary sense is used in arranging the plant, you will never hear of any trouble whatever from it.

Another thing that we were very much interested in, was the effect of the big synchronous motor on the lamps. It did not have any—which was rather a surprise. I should certainly have been prepared to find some trouble from that big motor—as big as the entire load of lights. But except at the moment of starting it gave no trouble whatever, and inasmuch as there was never any need of starting the motor when the lights were on, as we had two generators, it practically worked with entire success.

I regard that experience in Redlands as most satisfactory, because the machines were thoroughly modern, the plant was laid out for a three phase plant and was operated under ordinarily favorable conditions. There is one thing I should mention with respect to it, which I regard as of great importance, and that is the matter of governing water wheels. That is the *bête noir* of every electrical engineer. Waterwheel governors mostly do not govern, at least with anything like accuracy. When the Redlands plant was first started, the governor could not be depended on to hold the voltage constant within fifteen per cent., and it would hunt in the most vicious manner. Afterwards a change was made in the governor. The double cone friction arrangement which had been used to work the exciter shaft which drove the constant speed side of a Pelton differential governor, was thrown out after many attempts to make it work, and a small Pelton differential governor was put in its place, together with a moderate sized fly-wheel. That arrangement is holding the voltage to-day perfectly well. It is the first waterwheel governor I have known to be actually operating with results entirely satisfactory to the electric company that is running the plant. That, after all, is the crucial test of a governor, not that it shall operate well before a committee of experts, or when it is being nursed by its inventor, but when it is in service twenty-four hours a day, and under such circumstances gives satisfaction.

In concluding I can only say, I am convinced that polyphase work in one form or another has come to stay. It may not be in the form of two or three phase work just as we know it now; but the principle is pretty sure to stay by us. I do not have many fears that the polyphase plants now installed will be scrapped in a few months by reason of some invention that will entirely supersede them. We have threshed over pretty thoroughly the possibilities of the ordinary alternating current, and by far the most practical thing we have as yet, is the polyphase in one form or another. You "pays your money and you takes your choice." In some form it is going to stay by us long enough to make it worth while to develop it a little.

Discussion.

The President:—Discussion on Dr. Bell's paper is now in order.

Dr. Bell:—I wish Mr. Stanley, whom I see seated over there, would tell us a little about his polyphase plant. He has the only two phase plant in the country.

The President:—We would be pleased to hear from Mr. Stanley, not only on Dr. Bell's invitation but on our own.

Mr. William Stanley:—I have been a very attentive listener to the very interesting paper Dr. Bell has given us, and I am sure I can appreciate a great deal that he has said. Those of us who have devoted a little time to this work and have, or think we have, got it to the point it has now reached, have met some obstacles that Dr. Bell has not mentioned. He passed them by very nicely, but I know that he has had now and again to stop in his work, and perhaps to reconsider his designs and so revamp his old ideas. While I agree with what the author has to say in general, I also differ in a great many points. I would like to speak of one or two. I do not believe in a system of power distribution that has a power factor of about four or five-tenths under commercial conditions. The average load of a large power station varies from 30 to 40 per cent. of the maximum load of all motors. At that load the power factor as given by Dr. Bell, if I correctly understand him, would be somewhere from 45 to 55 per cent. In other words he would be sending out of his station as much magnetizing current for his motors, as he was sending out for doing work. Now I do agree with the Doctor that if it were possible always to keep our motors loaded, we could neglect the lagging currents in the system. It is possible —and Dr. Bell has clearly shown us how—to regulate a multiphase generator, but when the current in the generator lags 45 degrees, the armature reaction cannot be very small, and although the generator may be regulated—and it can be as Dr. Bell has shown—yet the variation of potential on the system outside of the generator is almost fatal where we desire to operate lights and motors together. About fifteen or sixteen months ago we started in Pittsfield a 80 H. P. two phase motor and light plant. We are operating a saw mill, and part of a woolen mill. We have a 15 H. P. motor operating a printing press. We have three or four machine shops and some other small shops, and we are also distributing lights from the same circuit, the lights and motors being sprinkled about without any regard to whether the circuits are balanced or not. I have had a voltmeter on my desk day after day and watched the change of voltage as the lights and motors changed, went on and off in the afternoon, and the average maximum change that I have been able to discover on the system has been a little over 2 per cent. It is the best, I am sorry to say, the most constant potential circuit that we have in Pittsfield to-day.

This constant potential is entirely due to the fact that we have condensers on the motors, supplying the lagging currents to the motor magnets.

I am surprised to hear Dr. Bell advocate synchronous motors. I thought we had got by synchronous motors Mr. Kelly, my associate, has developed a very clever device for taking care of the lag of the magnetizing current on very large motors, which is this—if you take a synchronous motor, run it up to synchronous speed, either by an induction motor or any other means, and then over-excite its field so that the back electromotive force from the synchronous motor will be in excess of the applied electromotive force to it, the current in the synchronous motor will lag in respect to the motor, and lead in respect to the line, and by using a small synchronous motor in this way, we can replace the condensers and furnish the lagging current for large induction machinery. And as it is possible to build large induction motors economically and to have them start—as has been shown—with great torque, I cannot for the life of me see the use of synchronous motors. I think we have got by them.

But there is a point which Dr. Bell passed over which to me is very important. He spoke of the question of frequency. He says that we ought to use any frequency practically between 30 periods and 70 periods. Surely the Doctor knows, and we all know, that the torque of an induction motor is directly dependent on its frequency, and with your permission, I will put the formula on the board.

$$\text{Work} = \frac{N\, 2\,\pi\, n\,(2\,\pi\, n - 2\,\pi\, n_1)\,\rho\, M^2 A^2}{\rho^2 + (2\,\pi\, n - 2\,\pi\, n_1)\, L^2}.$$

FORMULA.

N = Number of pairs of poles.
n = Generator frequency.
n_1 = Motor armature frequency.
ρ = Resistance of armature.
M = Coef. of mutual induction.
A = Primary current in motor field.
L = Coef. of self-induction in armature.

This formula gives the work done by an induction motor expressed in terms of the primary current. The work varies directly as $2\,\pi$ times the frequency of the motor, multiplied into the motor slip times the resistance of the armature, times the mutual induction squared, into the primary current squared, and the work of the armature varies inversely as the impedance squared. This is the regular formula for the work that an induction motor can do.

Now if we divide this equation by the frequency of the motor, we get this formula for the torque—

$$\text{for torque} = \frac{(2\,\pi\,n - 2\,\pi\,n_1)\,\rho\,M^2\,A^2}{\rho^2 + (2\pi\,n - 2\,\pi\,n_1)\,L^2}.$$

Now this is a very interesting equation to me, if I understand it. If you double the frequency applied to a motor you do not change the numerator of this formula. But how about the denominator? Your impedance is less, because L is one-fourth ($\frac{1}{4}$) at double frequency, and as a matter of fact in all high frequency motors, the armature resistance should be lower with the same material used, because the chords across the armature ends are shorter, and you have the impedance term on the whole less; you also have the ratio of R to L in the armature less, and the current in the armature lags less. So if you double the frequency in an induction motor, the lag of the armature current goes down very greatly. Now look at it once more. If you use the same material in two motors, one designed for double the frequency of the other, and combine the material in a number of magnetic circuits for constant speed, you can then determine the relative armature reaction for the two frequencies. Considering the motor as a transformer for the low frequency case, we have the magnetizing power on the field A times t ampere turns, and on the armature the back magnetizing power is $A\ T \sin\theta$ amp. turns. Now if you double the frequency of the motor, you have $2\ A$ over $\frac{T}{2}$ amp. turns for your primary amp. turns, and for the back magnetizing power on the armature $A \times \frac{t}{2}$ amp. turns. In other words, the armature turns per magnetic circuit, are one-half for the higher frequency case; consequently we do not have in high frequency motors the "blowing out" effect which is the most serious obstacle to motor construction. So I do not believe in low frequency motors. I believe in motors of 130 periods, if possible. We are operating our plant at Pittsfield at 130 periods. We are running a cotton mill at Housatonic at 60 periods, and we are using condensers to take care of our magnetizing currents.

I greatly appreciate the paper Dr. Bell has given us and trust he will continue as successfully as he has shown that he has proceeded thus far.

MR. CHARLES P. STEINMETZ:—Having had some experience myself with polyphase motors, I may add a few short remarks:

First, with regard to this whole system of rotary field motors, quite generally the opinion is expressed that this way of producing motion by a rotary magnetic field is a very new thing. But if you will look back into the records of science to some years before the oldest of us here were born—three-quarters of a century back—you will find a complete mathematical investigation and correct explanation, by Arago, of the experimental fact, old already at that early time, that a disk or a short-circuited conductor is set in rotation in a revolving magnetic field. You will find there the mathematical proof and everything. These were the earliest rotary field motors.

About fifteen years ago a further step in advance was made. You find mathematical and experimental proof of how a revolving magnetic field can be produced by stationary electromagnets. That was in 1879. So far with regard to the history of the polyphase motor

It may be of interest, perhaps, to the members, since the

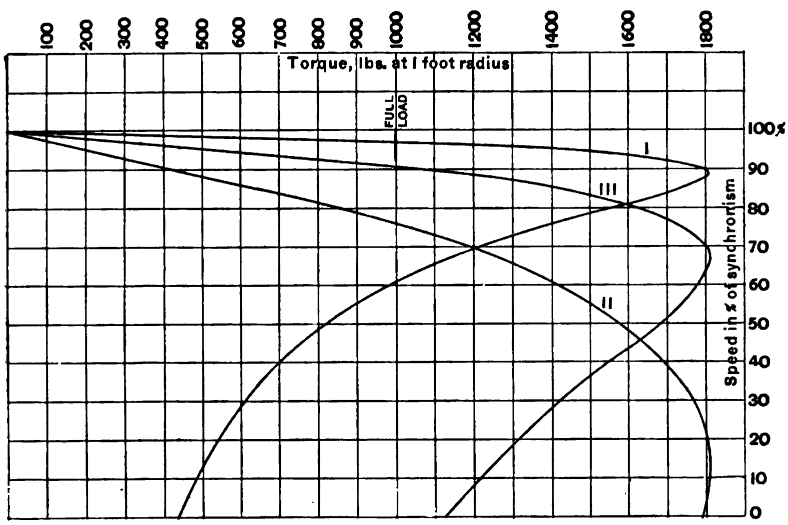

FIG. 1.—100 H. P. Three Phase Induction Motor, 38 Cycles.

lecturer has so ably told us about the behavior of polyphase motors in general, to get some data of a 100 H. P. three phase motor, which I designed some time ago, and of which quite a number have been built and are now in successful operation. I have here plotted a curve giving the torque of the motor at varying speeds. As abscissæ are given the torque in lbs. at one foot radius—1,000 lbs. corresponds very nearly to 100 H. P. As ordinates are plotted the speeds in per cent. of the synchronous speed. (See Fig. 1.)

You see that when the motor is running light, the speed is practically synchronous. With increasing load, that is, increasing torque, the speed drops, though very slowly, by only 3 per cent. at full load, or 1,000 lbs. torque. With still increasing torque, the speed drops faster and faster until a point is reached, at 1,800 lbs., where the torque curve bends around, that is, the torque as a function of the speed reaches the maximum, and if the speed decreases still farther, the torque decreases again more and more, the motor losing its torque, until at last only 440 lbs. torque are left at standstill.

This is the running condition: very constant speed at all loads up to the maximum load, which can be carried by the motor, and lesser starting torque.

Now, as Dr. Bell has told us, we can put resistance into the armature to increase the torque at low speed. Then we get a different torque curve. The speed decreases faster with increasing torque and has dropped of already by 24 per cent. at the full load torque of 1,000 lbs; but the torque constantly increasing with decreasing speed reaches the maximum of 1,800 lbs.

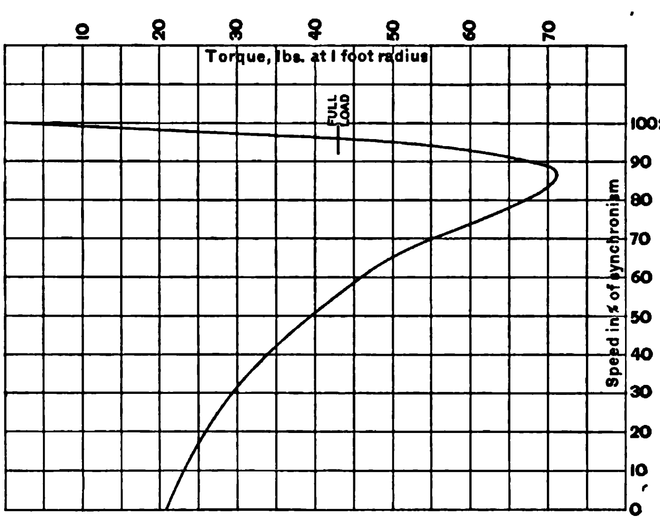

FIG. 2.—15 H. P. Three Phase Induction Motor, 125 Cycles.

about at rest, that is, the torque curve II intersects the zero lines at about 1,800 lbs.

These are the two extreme cases: in one case the armature resistance is as small as possible, very steady speed, 3 per cent. drop at full load, and even at 80 per cent. overload only 9 per cent. drop of speed, but only small starting torque.

The other extreme case is: very large starting torque, but greater drop in speed.

In the first case the motor will keep very constant speed up to the maximum point, at which it can give 1,800 lbs., and loaded still further, it will come to rest; in the latter case, with increasing load, the motor speed will steadily decrease until the motor comes gradually to rest at its maximum torque. Obviously any intermediate condition between these two curves, the curve of maximum starting torque and the curve of constant speed, can be reached by putting a lesser amount of resistance in the armature; and in reality in the 100 H. P. motor which I am speaking of here, an intermediate step is provided giving the torque curve III.

But one statement I want to emphasize here: while the speed corresponding to a given torque can be varied by suitable resistance in the armature circuit to anything between curve I and standstill, no matter what the speed is, the same torque always corresponds to the same current, that is, the current depends upon the torque only, and not upon the speed of the motor, and the speed of the motor is independent of the torque or of the current, and merely depends upon the resistance in the armature circuit, while the torque of the motor is independent of the armature resistance.

This indeed is the case only in a properly designed motor. Obviously there is no difficulty in designing, or rather mis-designing, a motor which does not fulfill these conditions. But I only refer to motors which are properly designed.

Then, at full speed or at a standstill, the torque is a function of the current only; that is, the current is the same for the same torque, although the speed may be different, and can be anything from this maximum curve I to zero. I may add here that these curves were calculated theoretically originally, but after the motor was built and tested I had no reason to change the curve, because the observed values fell into this line I, etc. The drop was exactly 3 per cent. and it gave this torque. I may add here in fairness, that in reality the pre-determination of a polyphase induction motor characteristic can be done with far greater exactness than that of any continuous current machinery—the behavior under any conditions of load, speed and anything else.

The motor whose speed characteristic is given here in Fig. 1 is a 100 H. P. motor running at 33 cycles. The frequency, however, has no influence whatever upon the behavior of the motor, upon its output, torque, etc. That is, obviously, one and the same motor when run at a frequency for which it is not designed, will, in general, work poorly, but what I mean is, that a motor can be designed for any frequency whatever, within reasonable limits, to give the same characteristics, that is, work at the same variation of speed, torque, current, etc., for 125 cycles as well as 33 cycles, as you can see by comparing the diagram in Fig. 1 with Fig. 2, which gives the speed characteristic of a 15 H. P. motor, which I built for 125 cycles, that is, nearly four times the frequency of the motor given in Fig. 1.

Hence, a motor designed for a frequency of 125 cycles behaves at this frequency exactly as a motor designed for 33 cycles behaves at 33 cycles, supposing always, you know, that the motor is properly designed.

Obviously, if you try to run a high frequency motor at low frequency, it will not give any decent result, and if you try to run a low frequency motor at high frequency, it will probably fly to pieces.

But as long as you maintain the same correct magnetic and electrical design, you can get the same good features at any frequency.

Indeed one restriction has to be made here: if you go very high in the frequency, you have either to choose a very high speed, which is mechanically objectionable, or you cannot maintain the same magnetic disposition.

Take for instance, a 10 H. P. motor. Now you do not want to imitate the steam turbine, but want to run at a decently low speed, say 900 revolutions. This will at 60 cycles per second give an eight polar motor of very good magnetic design. But at 125 cycles, you have a 16 polar motor for 10 H. P. This and the excessive drop of potential in longer feeders, due to their self-induction, and probably also the high speed required in the generators, are the foremost reasons which make a reduction of the frequency desirable.

In balancing all the advantages and disadvantages carefully, I found that 60 cycles per second will be about the all around best frequency for standard alternate current work.

With regard to static transformers and the frequency effect, I have investigated and published a few things on this question before,[1] and have shown that for a given size of transformer the output is in inverse proportion to the three-eighths power of the frequency, if the transformer is worked at its maximum output as determined by the heating of the transformer. that is, if the loss of energy in the transformer, which, as known, is the limiting factor of the output in a properly designed transformer, is kept the same. But then the efficiency for lower frequencies gets lower, and the magnetizing current larger, and if you take this into account, you will get as an approximation, that the output of a given transformer is about proportional to the square root of the frequency.

But this rule only holds goods under the supposition that by reducing the frequency you can run the magnetization higher. Hence it holds only down to that frequency where the iron of the transformer approaches such a high saturation, that you can not decently run the magnetization higher without getting a distortion of the wave of the electromotive force and an excessive magnetizing current.

The progress made in the selection of the iron for transformer work, due to chemical analysis and careful testing of the iron for permeability and hysteresis, have made it possible to secure iron whose saturation curve makes a very sharp bend between

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1. *Electrical Engineer*, 1893. *Electrical World*, 1893.

14,000 and 16,000, so that you can run up to as high a density as 12,000 without fear of distortion or excessive magnetizing current.

This point of saturation of 12,000 is, as practical experience has shown, reached at 30 to 33 cycles, in larger units of transformers. Hence if you go still lower than that in the frequency, the output of the transformer decreases directly proportional to the decrease of the cycles, and the efficiency decreases in the same proportion. That is, the transformer becomes very rapidly bulkier and it becomes impossible to build transformers of decent efficiency at any other but very large units. Probably in the future with the improvement in the production of good iron etc, this limit may go higher. Perhaps four years ago the limit was at lower frequency, because it was not possible then to secure the iron that we get now. As an instance, I may mention that while two years ago Ewing's value of the coefficient of hysteresis $\eta = .002$ was unrivalled, and the best Norway iron I could secure gave only $\eta = .0023$, now the standard transformer iron runs from $\eta = .0020$ up to $\eta = .0025$, and is rejected if η is found to exceed .0025, while some time ago I accidentally even got a sample of iron whose hysteretic coefficient was as low as $\eta = .00124$, 38 per cent. lower than Ewing's value. I did not believe this value at first, and had the test repeated three times, but with the same result. I have never got such iron again.

With regard to another point that came up in the discussion, the behavior of incandescent lamps at various frequencies, I had occasion to make a set of tests some two years ago. I had an incandescent lamp fed by an alternator driven by a continuous current motor, whose field strength I varied to vary the frequency of the alternator, and varied its excitations to keep the potential the same, and I found that at 15 cycles and up to 20 cycles the flickering was so abominable that you could not look at the lamp without making your eyes ache. The brighter the lamp burns, the higher is the frequency where you notice the flickering. If you burn the lamp dull, at half intensity or so, then the flickering is still unnoticeable at 25 cycles, but becomes noticeable just below this. But if you run the brightness of the light up to or beyond the normal c. p., then you notice the flickering plainly at 25 cycles, and it disappears only just below 30 cycles. I did not make any tests with arc lamps, but since the temperature of the arc is considerably higher still than that of the incandescent lamp filament, probably the flickering is noticeable at still higher frequencies. Obviously the frequencies where flickering becomes noticeable, depend probably somewhat upon the personal equation of the observer.

MR. O. T. CROSBY:—As there is considerable personal equation in the matter of flickering I should like to learn if others were present and agreed with the speaker in his estimate as to the flickering?

MR. STEINMETZ:—I think the sensations of the gentlemen present at these tests agreed with me pretty well in that. Mr. Eickemeyer was there, and I believe Professor Forbes was there also, and some other men.

With regard to the parallel running of alternators, that is quite an interesting point, because that has been brought up quite frequently as a disadvantage of the alternating system that alternators cannot run well in parallel.

To settle this question I carried out a lengthy investigation, covering frequencies from 25 cycles up to 125 cycles, and alternators and three-phasers of very low armature self-induction, as smooth core machines with very few turns on the armature, and iron-clad armatures of high self-induction, and found in no case any difficulty in parallel running, even under very extreme conditions, with properly designed alternators.

Frequency and self-induction have directly nothing whatever to do with the ability of the alternators to run in parallel, and I found that it was a very difficult matter indeed, if two alternators are running once in parallel, to make them drop out of synchronism.

Even equality of potential is by no means necessary in the machines which shall be synchronized, although desirable for equal distribution of load.

It is known that Mordey has succeeded in running a 2,000 volt and a 1,000 volt alternator in parallel.

I went even a step further and took two 1,000-volt alternators, high frequency machines (since many people still share the superstition that high frequency machines do not run as well in parallel), excited one machine up to 2,000 volts and left the other machine unexcited, at zero volts, and then closed the synchronizing switch, thereby throwing a 2,000 and a zero volt alternator into parallelism. Both machines dropped into step directly and ran in synchronism at a potential of 1,075 volts at the bus bars.[1]

In the notion that for successful parallel running self-induction

1. It may be of interest here to give the results of a set of tests I made on the parallel running of iron-clad high-frequency alternators at very different voltages.

Two 60 K. W. 1,000-volt alternators, iron-clad high-frequency machines of the General Electric Company's make running at their normal speed of 125 cycles per second, were thrown in parallel at different excitations.

Columns 1 and 2 give the electromotive force at the terminals of the two machines before synchronizing.

Column 3 gives the difference between one and two, that is, the difference of electromotive force between the two machines in the moment of throwing them in parallel.

Column 4 gives the voltage at the common terminals of the machines after synchronizing.

Column 5, the cross current flowing between the machines.

The machines fell into step and kept in synchronism without any difficulty even under these extreme conditions, except that in the case where the one machine was entirely unexcited, a certain fluctuation was noticeable in the re-is necessary, there is something true and something not true. The maximum synchronizing power will be exerted between the two alternators, if in the cross circuit formed by the armatures of the alternators, the resistance equals the ohmic inductance (not as it is frequently stated erroneously, if in the external circuit this condition is fulfilled), but it is out of the question to use that, because if the inductance is so low as that, and you throw them together and they are not at absolutely the same phase, the synchronizing power will be so great, that they will probably be torn to pieces. But fortunately there exists no alternator that fulfills this condition, since in good alternators the armature inductance is far in excess of the resistance. The larger the inductance, the weaker the synchronizing tendency, that is, the less you need to be careful to get the same phase and voltage in throwing them together. But you cannot in any way with any decent machine get such a large self-induction that they will not run in synchronism.

With regard to the unbalancing of the three phase circuits I may refer to the paper I read before the Chicago Congress where I gave a mathematical investigation of this question, showing that according to the conditions of the circuit, the resistance and inductance, you can get it to zero or as high as you want or not want. The avoidance of the unbalancing of a three phase system depends entirely upon its proper design.

With regard to the synchronous motor, I have very fully in-

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sultant voltage, which periodically varied by ± 70 volts, showing the approach to the limits of synchronizing power.

SYNCHRONOUS RUNNING OF TWO HIGH FREQUENCY IRON-CLAD ALTERNATORS, GENERAL ELECTRIC CO. STANDARD.
60 K. W. 125 CYCLES.

E. M. F. of First Alternator Before Synchronizing.	E. M. F. of Second Alternator Before Synchronizing.	Difference of the E. M. F.'s of the Two Machines Before Synchronizing.	E. M. F. at the Common Terminals of Both Machines After Synchronizing.	Interchange of Currents Between Machines After Synchronizing.	Excess of Momentary Current at the instant of Synchronizing over the Cross-Current.
Volts.	Volts.	Volts.	Volts.	Amperes.	Amperes.
1,000	1,000		996	4.0	2.0
1,100	900	200	1,000	6.5	.5
1,200	800	400	1,000	13.0	3.0
1,300	700	600	1,000	18.0	4.0
1,400	600	800	1,026	24.0	6.0
1,500	500	1,000	1,010	28.0	6.0
1,600	400	1,200	1,040	39.0	3.0
1,700	300	1,400	1,046	44.0	5.0
1,800	200	1,600	1,060	50.0	6.0
1,900	100	1,800	1,066	56.5	5.5
2,000		2,000	1,075 ± 70	62.0	10.0

Full load current = 52.0 amperes.

vestigated that also. Since I have built quite a number of induction motors and they are running successfully, nobody can say that I am prejudiced against the induction motor. But I believe and am fully convinced that wherever a synchronous motor can be used, it is a serious fault of engineering to use an induction motor, because the synchronous motor is so far superior to the induction motor, even with condensers, in the reaction upon the line and in the efficiency and the absolute constancy of speed, that wherever the disadvantage of a lower starting torque is not of very serious consideration, especially for larger units, the only proper motor to be used is the synchronous motor, and only where a very large starting torque is required and, therefore, the synchronous motor cannot be used, or for smaller motors, where simplicity of construction and of handling is of foremost consideration, the use of induction motors is advisable.

The condenser effect of the synchronous motor that Mr. Stanley spoke of is nothing new to me, nor probably to the members of the INSTITUTE, because if you look back in the TRANSACTIONS to the discussion of Mr. Kennelly's paper on impedance at the April meeting of last year, you will find the statement made by me in the discussion "That a synchronous motor at certain conditions of excitation acts like a condenser of very large capacity." That is all I think I can say at present.

DR. BELL:—Simply referring to something Mr. Stanley said; being in the back part of the room and being a little hoarse myself, I think he misunderstood the values which I gave for the power factor at light loads. I picked out a commercial case to illustrate it. Supposing that, as Mr. Stanley stated, the load is somewhere between thirty and forty per cent. of the rated load of the motor; taking the three sizes of induction motors from which I gave the figures the average power factor at one-third load is just about 72 per cent., so that the case is by no means as bad as it might be supposed at first. The induction motors at very light loads have a bad power factor, even the best of them, when it comes down to extremely light load, one-tenth, one-eighth, or something of that kind. Anywhere over quarter load they are really quite good as to power factor.

I should say that I fully agree with Mr. Steinmetz that the synchronous motor is the advantageous thing to use when it can be used. As a matter of fact the synchronous motors do not work so nicely in the small sizes as the big ones. I should say that for motors of 100 H. P. or above, it would in nearly every case be practical to use a synchronous motor, unless we come to railway work or something of that kind. Then we may have to use the induction motor in order to get the big torque necessary. Most of the smaller work I believe will have to be done by the induction motors, and most of the larger work by the synchronous.

In respect to the question of frequency, it was perhaps sufficiently discussed by Mr. Steinmetz. It simply falls in with my own experience, except that I rather confined it to the limits within which most of the transmission work will have to be done. That is, a little above that which enables you to run incandescent lamps, and below that it gets you into serious difficulty with induction.

MR. STANLEY:—I think perhaps we are talking on a little different point with regard to frequency. My point was that with the same material in two motors designed for two frequencies the lag of current in the armature at low frequency must be greater than at high frequency. The power factor actually measured at the Pittsfield station—the motor plant at the Pittsfield station, is 94 to 96 per cent., by actual tests, with the condensers in.

MR. C. O. MAILLOUX:—As we have here this evening many authorities on the subject of alternating currents, I think it would be well to have the facts brought out regarding the limit of voltage at present imposed where it is necessary to use cables. I quite understand that it may be possible to use very high voltages where the wires run overhead. But there are many cases involving or suggesting the use of alternating currents where it is, nevertheless, necessary to use cables. Now the experience abroad has shown that they were not able to use successfully the extreme voltage which had been contemplated; furthermore the question must be influenced, as was very clearly set forth by Dr. Bell, by the cost of the extra insulation and precautions required for the rise in voltage. It all really comes back to the question of cost. It would be interesting to know what the extra cost is of the extra precautions, as the voltage is increased, and especially to know what at present is the commercial limit, so to speak of alternating currents used in underground cables, for two phase and three phase. I apprehend that for three phase the cost would be a little higher for the cable, because the mechanical difficulties of placing three conductors in one cable, if used concentrically, which I imagine would be best on account of its better elimination of the impedance; and if there is any experience which has been had in this country at present, I think it would be very interesting to the fraternity to have the result of it. I have been given to understand, from having casually had occasion to look into the subject in connection with a practical case, that about 2,000 volts was as far as it was expedient to go at present in using underground cables.

THE PRESIDENT:—I will call on Mr. Frederick Darlington to make some remarks on this subject.

MR. FREDERICK DARLINGTON:—If I understand the gentleman's question, what he asks is the limitation of the conditions that you get when you go underground to the use of high voltages and high frequencies also.

MR. MAILLOUX:—Yes, frequencies also.

MR. DARLINGTON:—Dr. Bell touched on that question when he said in his paper that the effects most likely to trouble you were the resonance of the circuits and the capacity of the circuits. I cannot give you any information based on practical experience as far as very high voltages are concerned. I can say in reference to the cost of insulation that it is usually customary for various reasons, under practical conditions, in putting down a cable for say 1,000 volts, to have margin of safety enough to run two, three, four or five thousand volts on the cable. One important reason for this (especially where you have aerial connection) is that if there is any possibility of lightning discharges on your line and you are working either 1,000 volts or 5,000 volts you want to have sufficient insulation on the line to enable static discharges to be taken off safely. Except on very long lines you will not find the charge or discharge currents of cables enough to injuriously affect the working of the system at 3,000 volts or less at the ordinary frequencies—anything less than 130 cycles. The matter of resonance I do not think will cut any figure at all at 3,000 volts or 5,000 volts, unless it may be, possibly, on some very irregularly shaped current wave. The capacity of ordinary electric light and power cables varying in sizes from No. 3 and No. 4 B. & S. up to No. 0000 B. & S. is about one-third to one-half a microfarad per mile of conductor. When you are working a circuit you usually have two insulated conductors; and this capacity given is the total capacity measured to earth for both conductors. Suppose you have four-tenths of a microfarad per mile as the capacity of your cable. The capacity of each side of the circuit will be half the total capacity, as each side of the circuit contains but half the cable, and since you have two dielectrics in series, making two condensers each having half the capacity of the whole cable, the capacity between the conductors will be one-quarter of the total capacity of the cable in the circuit. If you have a little over two miles of circuit you have a little over four miles of conductor and that would give a capacity measured to earth of about two microfarads. Measured to earth for one conductor it would give a capacity of about one microfarad and give the capacity between conductors in actual working of one-half microfarad. That would make one-half microfarad capacity being charged and discharged all the time, and that gives for 1,000 volts on 135 cycles about one-half ampere charge and discharge current. It is evident that in the assumed case the static capacity does not make the false current in that circuit very great. If you are working on a circuit with more or less retardation, the capacity may come in as an actual advantage by very nearly neutralizing the retardation. To sum up the commercial feature of it, I find that up to 5,000 volts, using such cables as are readily obtainable in the market, you can buy any one of half-a-dozen good makes of cable that are safe for 5,000 volts. I do not find it economical on 1,000-volt circuits to use a thin insulation such as would be unsafe for 3,000 or 5,000 volts

MR. STEINMETZ :—First I want to say one or two words on this power factor. If you use the same magnetic disposition you get the same power factor and same torque, current and everything, whether you have a high or low frequency, proper design supposed indeed. But by going down to lower frequency you are enabled to use a more favorable design. Consequently you can reduce the lag, increase the power factor and get a better efficiency and better output per lbs. of weight; hence a low frequency is preferable—within certain limits indeed; obviously nothing is gained, but much lost at least in weight efficiency, and in flexibility, if you go down too far, for instance, below 30 cycles.

With regard to the cable, it is indeed true that in circuits with lagging current the capacity of the cable will, by taking a leading current, counteract and supply the lagging current in the circuit, so that occasionally more current comes out of the cable than is sent in.

A simple analytical solution of the problem of a circuit containing distributed capacity, self-induction, resistance and leakage, I have given in my paper read before the Chicago Congress, with curves showing the periodical increase and decrease of current and of electromotive force along the line.

But I would rather prefer to keep the lag and not introduce this very dangerous compensation, because this capacity is not only in shunt to the self-induction of the receiving circuit, but it is in series to the self induction between the capacity and the generator, and in the generator, and in this case, if capacity and self-induction are in series, as soon as they balance each other they annul each other and you get a current as large as corresponds to the resistance only. Say you have a generator and a line which normally consumes 2 per cent. of the voltage. Then at open circuit, if the capacity of the line is just balanced by the self-induction, the current will increase to the value which it would have by short-circuiting the generator by the line with no self-induction, that is, to fifty times the normal value, and across the self-induction and across the capacity, that is, from line to line, you get the voltage corresponding to this abnormal current, that is, fifty times the normal voltage. That means resonance, and that means destruction.

Usually you do not get resonance with the normal wave of current, but may quite likely get resonance with one of its higher harmonics, which is of lesser amplitude and, therefore, causes a lesser rise, but in high potential circuits, it may probably be high enough to destroy.

MR. DARLINGTON :—In speaking of the results from capacity and resonance effects, I spoke more from experience than from theory, and the conditions under which I have had experience in operating circuits underground have been very varied. In many instances I have put a very short underground circuit on a dynamo and had a small capacity in series with the self-induction of the armatures. In other cases I have had a large static capacity made up from several underground circuits on one armature. I watched very carefully under both those circumstances for any effect of resonance or rise of potential or anything that would tend to injure the cable and I have never seen it to any degree at all.

MR. STEINMETZ:—I may give you some data on an experiment I made some time ago, on resonance at very low potential and very low frequency. That was on a 100-volt alternating circuit of 25 cycles, where the line had a large self-induction, but small resistance. The line was feeding a bank of incandescent lamps, and across the terminals of the lighting circuit I put a condenser or rather an apparatus that was equivalent to a condenser of about 7,000 microfarads. Then I found that with an electromotive force of some 40 volts at the generator, at the end of the line I had something over 100 volts, when a current of 100 amperes was passing. That was resonance.

DR. BELL:—I am inclined to think that resonance of the higher harmonics is the rule rather than the exception. I think there are comparatively few long lines of any kind where one is not likely to find a great deal more tendency to sparking and sputtering around the switchboard and on the line than would be accounted for by the normal voltage of the machine, even where the atmospheric conditions are not such as can account for it. There is quite frequently enough resonance to make it noticeable —a tendency to get some sort of abnormal quasi-static effect on a line of any considerable length. I am inclined to attribute it in many cases to the actual existence of this resonance mostly of higher harmonics of the E. M. F.

DR. M. I. PUPIN:—I have observed peculiar resonance effects on short lines—only about six feet in length, that is to say, the alternator was only about six feet from the transformer. They seem to me to bear on the matter of sparking on long lines. The resonance effects which I refer to, never occur when there is a big load on the transformer. It always occurs when there is no load at all, or a small load. It looks as if there was an oscillation between the alternator and the transformer, which did not extend into the alternator or into the transformer. Of course on this short line containing a small coil without iron there was a condenser in shunt with the primary of the transformer. Now it is quite possible to have resonance on a long line when there is some distributed capacity there—and there always is—provided the self-induction of the line be such as to give the length of the line the periodicity of some of the upper harmonics. The oscillation on the line may, and according to my investigations, will occur extending into the motor, or into the transformer or into the alternator. It is entirely local. I have every reason to believe, although I have no conclusive proof as yet, tha· since this resonance effect, whenever it occurs, occurs always when there is a big self-induction in the transformer or motor, that it is due to some sort of a reflection; that is to say, the high frequency wave belonging to some upper harmonics strikes, as it were, a solid wall when it encounters the large self-induction of the motor or transformer and is reflected back. These interferences between the direct and the reflected waves may quite easily produce the sparking observed on long lines, especially when these long lines are worked by an impressed E. M. F. of several thousand volts.[1]

[COMMUNICATED AFTER ADJOURNMENT BY CHAS. P. STEINMETZ.]

The analytical proof, that the frequency has no direct influence upon the action of the polyphase induction motor, is the following:

Leaving aside secondary phenomena, which can be neglected in a properly designed motor, the maximum output which an induction motor can furnish is given by the equation:

$$W = \frac{p\,E^2}{2\,r + 2\,u}$$

where
p = number of phases,
E = electromotive force per phase,
r = total effective resistance,
u = total impedance of the motor circuit, per phase, that of the secondary circuit being reduced to the primary by the ratio of transformation.[2]

The impedance:

$$u = \sqrt{r^2 + (2\,\pi\,N\,L)^2}$$

where
N = frequency,

1 Dr. Bell calls this phenomenon sputtering and not sparking. I have studied this phenomenon at some length and called it *flaky discharge*, because it consists of minute silent sparks looking like small snow flakes, which, when a large conducting surface is brought near the wire may take the form of a faint brush discharge. This brush discharge will take place even at considerable distance, so through half inch of fibre, hard rubber or dry wood, even if the potentials employed are only a little over a thousand volts, provided that the frequency is high enough, say over 300 periods per second. I intend to discuss this phenomenon at some length in a paper which I am now preparing for the INSTITUTE.—M. I. P.

2. It may be mentioned here that this formula is the fundamental equation of the output of an alternating current circuit containing resistance and inductance (the latter being positive as magnetic inductance or negative as capacity inductance, while the resistance is the "effective resistance" in the sense as explained in my paper read before the Chicago Congress). It applies to the maximum output of transformers as well as of generators, synchronous motors, or the whole system in general.

L = coefficient of *self*-induction (that is, coefficient referring to the magnetic flux interlinked with one, but not with the other circuit).

With varying frequencies, if the same magnetic disposition is maintained, the coefficient of self-induction L is inverse proportional to the frequency.

Hence $2 \pi N L$, and therefore u and W are constant and independent of the frequency.

Thus the maximum output of an induction motor of given speed and size, and consequently its torque, is independent of the frequency, if the same magnetic disposition is maintained.

The same holds for the angle of lag of the motor, which at the moment of maximum output is given by

$$\cos \varphi = \sqrt{\frac{r + u}{2 u}},$$

hence is independent of the frequency also.

In reality, as secondary phenomenon, a slight variation takes place with varying frequency, and r gets a little larger, and u a little smaller at low frequency; that is, while the maximum output remains the same, the angle of lag gets slightly less at lower cycles, and the power factor a little higher consequently.

However, if the same favorable magnetic design is maintained, at a frequency of from 70 to 80 cycles the peripheral speed of the motor becomes higher than is desirable for mechanical reasons. This is one of the reasons which induced me to recommend 60 cycles as standard frequency for alternate current circuits for light and power distribution.

Schenectady, N. Y., February, 1894.

THE PRESIDENT:—If there is no further discussion I will call for the report of the Committee on Units and Standards.

REPORT OF COMMITTEE ON UNITS AND STANDARDS.

NEW YORK, Nov. 15th, 1893.

To the President and Council, of the
American Institute of Electrical Engineers.

GENTLEMEN:—Your committee on "Units and Standards" begs to recommend to the Institute the provisional adoption of:—

The term "gilbert" for the C.G.S. unit of magnetomotive force, the same being produced by 0.7958 ampere-turn approximately.

The term "weber" for the C.G.S. magnetic unit of flux, sometimes described as the C.G.S. line of flux.

The term "oersted" for the C.G.S. unit of reluctance.

The term "gauss" for the C.G.S. unit of flux density, or one weber per normal square centimetre.

The committee, it will be remembered, in its previous report, dated June 20th, 1891, advocated that the above terms should be accorded to magnitudes in conformity with the "practical" electromagnetic system, and therefore following in natural order and extension from the volt, ohm, ampere, and other units in universal use.

As, however, so important a series of new unit magnitudes could only meet with general recognition and favor under the authorization of an International Electrical Congress, which authorization has been withheld at the recent Congress at Chicago, and since objections have been raised to those magnitudes, your committee considers that the urgent need for names specifying the principal quantities dealt with in magnetic circuits can best be met with general favor, by adopting for those names the fundamental unit magnitudes of the international C.G.S. system after the precedents already established in the cases of the C.G.S. units of force and work, entitled respectively the "dyne" and "erg."

Yours very respectfully,

COMMITTEE ON UNITS AND STANDARDS.

F. B. CROCKER,
W. E. GEYER,
G. A. HAMILTON,
A. E. KENNELLY, *Chairman.*

THE PRESIDENT:—Gentlemen, you have heard the report. What action will you take on it?

MR. TOWNSEND WOLCOTT:—It does not strike me that those names follow the precedents already established. The names of individuals are applied to C. G. S. units in no other case.

MR. MAILLOUX:—There is another fact which I think ought to be brought to the attention of the committee, and that is that the term "weber" is already preëmpted. That was done at the International Congress of 1881. I think there was some discussion as to the substitution of the word "ampere" for "weber" which was then the accepted term for the unit of current, and it was agreed, and, I think, entered on the record at the time, that the word "weber" should be used for the absolute unit of current which is 10 amperes, and, I think, the term is still in vogue or in force as the name for the absolute unit of current (10 amperes). Hence before we could appropriate that term for the unit of magnetic flux it would be necessary to have some action rescinding its use for the unit of absolute current. The unit of absolute current is in reality seldom, if ever, referred to by name. It was a very good way to relegate that term to obscurity to use it for this unit, because we always speak of amperes and not tens of amperes or webers. At the same time, unless the way is made clear by undoing the work of that congress, I think we should hesitate to make use of the term for another unit until we have a clear title to it. I have no objection at all to the term "weber" for that purpose, if we can have the right to use it.

MR. STEINMETZ:—I do not consider the proposal of our committee a happy one, and should not recommend the adoption of these new names, for the reason that, when we introduced the name "henry," we were aided greatly by the fact, that the name "quadrant" proposed before, was not in conformity with the established system of denotation, which applies to the practical units the names of eminent scientists, and denotes the absolute units with Greek names. Here, now, we fall in the same error and propose the names of scientists, not for practical units, but for absolute units. Therefore I fear no congress would adopt these names since they do not agree with the established practical units "ampere." "ohm," etc.

Furthermore, I really cannot see any urgent need for these new terms. For instance, the first one—"gilbert"—the centimetre-gramme unit of magnetomotive force will probably never be used in practice, because everybody calculates with ampere-turns, which is more convenient than (c. g. s.) units. As to the term "weber," you simply replace one name by another name, but the term "line of force" is generally used and cannot be mistaken for the practical unit, and it serves the purpose just as well. Then with regard to the term "gauss" you can just as well speak of lines of force, of kilolines or megalines. For "oersted" the term of magnetic reluctance, the same holds as for "gilbert." Reluctance is not generally used in engineering practice, and if it is used, it is applied to: $\frac{\text{flux}}{\text{ampere turns}}$. Therefore I should not recommend proposing such names, the more, as just after our success with the name "henry," we ought to be very slow and careful not to lose the advantage that we gained by successfully introducing a new name, in proposing without full consideration and investigation new names, which have little, if any, chance to be adopted finally, and should rather leave the matter as it stands c. g. s. units, lines, kilolines, and megalines.

Mr. A. E. Kennelly:—What Mr. Steinmetz has said in regard to the absence of occasion for the use of the quantities now under consideration for names, has been up to the present time unfortunately only too true; for the reason that we have not had names to call them by. That has been the experience not only in the magnetic system but also in the preceding electric system, that there was no active general development of the ideas connected with the quantities of each science until we had names upon which to build those ideas. It is only when we are able to express our thoughts in clear, simple terms, and only when we possess such terms that these ideas can extend and generalize. If we did not have the names "volt," "ohm," and "ampere," we should not have electrical science and arts in the condition they are in to-day. There is surely a very urgent need for names of some kind in connection with a magnetic circuit, because we are constantly seeing the strenuous efforts people now make to avoid expressing their ideas relating to magnetic circuits in a quantitative manner. If I take up this book in my hand and want to express the weight of it, isn't it far better to say, this book weighs three hundred grammes, than three hundred units of weight in the C. G. S. system. Contrast the conciseness and definiteness of these two statements.

In regard to the objection as to names taken from eminent electricians or names taken from Greek roots, it is true we have not a sufficient number of great Greek electricians upon whom we can fall back for suitable names. If any one will suggest suitable Greek names that are likely to meet with support and favor, I am sure we would only be too pleased to hear them. But as we do not possess the facility for creating Greek names in this country that will meet with general apprehension and support, how can we do better than select these well known and honored names that have been such stars on the pathway of the development of this science. Surely there can be no question concerning the relative advantages of Greek derivations that not one speaker in a hundred might understand, compared with such names as those we advocate, which are household words among us all. We cannot at the present time expect to create new unit magnitudes. That is the province of an international electrical congress. But we can, without inflicting any disadvantage upon our neighbors, provisionally adopt names of this kind for units already established by electrical congresses—the C. G. S. units that we constantly employ—and we believe that by so doing, we can greatly aid the science and art of electrical engineering.

MR. MAILLOUX :—It seems to me that the objection might be to some extent obviated by regarding these terms as applying to practical units instead of C. G. S. units. They might, if we chose, be looked upon as being coincident with the C. G. S. value. At the same time in calling them practical units we would not be departing from the established precedent in that respect. We have a perfect right to confer such names as these on *practical* units. I think myself that the objection against invading the prerogatives of pure science is somewhat valid. I think that there is a tendency, in our science especially, to a certain jumbling of terms by not giving careful attention to what might be called the "proprieties" of lexicography, and I think that certainly it would be a step which would command the approval of a greater proportion of the members and of scientific men generally, to consider these things as practical units, rather than as C. G. S. units, even though they might have the same value. For instance, it was a mere accident that we were using one ampere, and that we gave it a special name which was one-tenth the value of the absolute electro-magnetic unit of current. In the case of the proposed new units it may be a coincidence that we happen to use the same value as the absolute unit for the practical unit.

Another point that I would like to call attention to, is the fact that the expression .7958 ampere-turn is a very awkward one to handle. That is one objection in fact to the adoption of the term "gilbert" as now defined by the committee. It would not be used probably as much as the term ampere-turns to which we are now accustomed, for the reason that the conversion involves a rather awkward fraction. There is also a more or less definite objection to the use of any term for reluctance, because the term reluctance itself does not express any concrete value unless we take into consideration the particular facts and circumstances surrounding the particular case.

THE PRESIDENT:—What action will you take on this report, gentlemen?

MR. C. S. BRADLEY:—There seem to be two objectors and there were four members of the committee. I think the majority is in favor of the report. I move that it be provisionally adopted. [Seconded.]

MR. MAILLOUX:—I move that the matter be laid on the table. [The motion was seconded and carried.]

THE PRESIDENT:—I call for the report of the Committee on Revision of the Rules—Dr. Herzog, Chairman.

THE SECRETARY:—This report was handed in by the Chairman, Dr. Herzog, who could not remain to present in person.

THE PRESIDENT:—It contains a notice of certain proposed changes in the by-laws. The Secretary will please read it.

The Secretary read the following report:

REPORT OF COMMITTEE ON REVISION OF RULES.

At a meeting of the Committee, duly held, it was unanimously resolved to advise that the rules relating to elections be immediately changed as set forth below.

In pursuance of this resolution and of the provision in Section VIII., controlling the manner in which amendments may be made, the prescribed written notice is hereby given by the Chairman on behalf of the Committee.

At the next or at some subsequent regular meeting of the INSTITUTE the following separate amendments of the rules will be brought up:

RESOLVED, that Section V. be amended as follows:

1st change—After "a" in line 13, add the words "second list headed."

2d change—Line 18, after "choice" add "opposite the name of each nominee in each list shall be printed a number indicating the number of nominations received by him, and a suitable explanation of these numerals shall be placed on the sheet."

3d change—Lines 33 to 36, shall be changed to read "sealed, unmarked and unidentified 'Inner envelope' of any suitable character, to be in its turn enclosed either in the 'Voting envelope' (received from the Secretary) or in any other envelope marked on its face 'Non-official Voting Envelope-Enclosing a ballot only.' The outer envelope of either class must be identified by the signature of the member on its face, and must be sealed and mailed to the Secretary."

Respectfully submitted,

Jan. 17, 1893. Signed, { F. BENEDICT HERZOG, Chairman. / T. C. MARTIN. / F. R. UPTON, Assenting. }

[Adjourned.]

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

New York, February 21, 1894.

The eighty-fourth meeting of the INSTITUTE was held this date, at 12 West 31st street, and was called to order by President Houston at 8 P. M.

The Secretary read the minutes of the last meeting and on motion they were approved.

The Secretary read the following list of associate members elected, and of associate members transferred to full membership at the Council meeting held February 21st.

Name.	Address.	Endorsed by.
BABCOCK, CLIFFORD D.,	203 East 87th Street, New York City.	Chas. A. Doremus. Wm. J. Jenks. L. Stieringer.
BARSTOW, WILLIAM S.,	General Supt., Edison Electric Illuminating Co., 360 Pearl St., Brooklyn, N. Y.	Chas. Hewitt. James Hamblet. J. P. Wintringham.
CLOUGH, ALBERT L.,	Box 114. Manchester, N. H.	Wm. L. Puffer. Geo. F. Curtiss. Caryl D. Haskins.
CROSBY, JAMES W.,	Electrical Engineer. Hix, Crosby & Co., 128 Broadway.	Wm. J. Hammer. Joseph Wetzler. Wm. J. Jenks.
FORBES, GEORGE,	Electrical Engineer, 34 Great George St., London, England.	T. C. Martin. Joseph Wetzler. Horatio A. Foster.
FRANTZEN, ARTHUR	Inspector, Electrical Engineering Dept., World's Columbian Exposition, 662 Shober St. Chicago, Ill.	R. H. Pierce. Lemuel S. Boggs. G. Sacco Albanese.
GERRY, EDWARD M.,	Weston Electrical Instrument Co., 114 William St., Newark, N. J.	James Hamblet. R. O. Heinrich. Edward Weston.
GESSEAUME, CHARLES	Chief Draughtsman, Newark Factory, Westinghouse Electric and Mfg. Co., Newark, N. J.	L. A. Osborne. Philip A. Lange. F. N. Waterman.
HARRISON, HAROLD	New York Representative, Slater Engine Co., Montclair, N. J.	Wm. E. Geyer. F. A. Pickernell. James H. Bates.

HIX, E. RANDOLPH	Hix, Crosby & Co., Electrical Engineers and Contractors, 128 Broadway, New York City.	Wm. J. Hammer Jos. Wetzler. Wm. J. Jenks.
LIGHTHIPE, JAMES A.,	District Engineer, General Electric Co., 15 First Street, San Francisco, Cal.	F. F. Barbour. Louis Bell. Elihu Thomson.
MOREHOUSE, H. H.,	General Manager and Electrician, Alumbrado Electrice de Quezaltenango, Apartado, Quezaltenango, Guatemala, C. A.	C. O. Mailloux. H. A. Sinclair. Jos. Wetzler.
POTTS, CHAS. EDWIN	Student in Electricity, 1248 Dean Street, Brooklyn, N. Y.	Samuel Sheldon. O. R. Roberson. James Hamblet.
RICHARDSON, ALBERT E.	Lecturer in Science, County of Surrey, 55 Coleridge Road, Crouch End, London, Eng.	R. W. Pope. T. C. Martin. Jos. Wetzler.
RITTENHOUSE, CHAS. T.,	Post-Graduate Student, Columbia College, 247 W. 138th Street, New York City.	M. I. Pupin. F. B. Crocker. W. H. Freedman.
SNOOK, S. D.,	Manager, Williamsburg Exchange, N.Y. and N. J. Telephone Co., 14 Boerum St., Brooklyn. N.Y.	W. D. Sargent. J. C. Reilly. R. W. Pope.
TRAFFORD, E. W.	Electrical Engineer, Richmond Railway and Electric Co,, 104 N. 7th St., Richmond, Va.	A. M. Schoen. C. E. McCluer. M. B. Leonard.
TREADWELL, AUGUSTUS, JR.,	Private Assistant, Polytechnic Institute, 448 3d St., Brooklyn, N. Y.	Samuel Sheldon. Chas. Hewitt. G. W. Gardanier.
WENDLE, GEORGE E.,	Instructor in Electrical Engineering, Lehigh University, 705 Dakota St., So. Bethlehem, Pa.	E. J. Houston. F. B. Crocker. R. W. Pope.

Total 19.

TRANSFERRED FROM ASSOCIATE TO FULL MEMBERSHIP.

Approved by Board of Examiners, December 7th, 1893.

WOLVERTON, BYRON C.	Electrician, New York and Pennsylvania Telephone and Telegraph Co., Elmira, N. Y.
VAN TRUMP, C. REGINALD	Engineer and Manager, Wilmington City Electric Co., Wilmington, Del.
SARGENT, WM. D.	General Manager New York and New Jersey Telephone Co., Brooklyn, N. Y.
GIFFORD, CLARENCE E.	Assistant Electrical Engineer, Buffalo Railway Co., Buffalo, N. Y.
LOVEJOY, JESSE R.	Electrical Engineer, General Electric Co., Boston, Mass.

Total 5.

THE PRESIDENT:—The next order of business will be the report of the Committee on Revision of the Rules governing Elections. Is it your desire to take this up now, or after the paper? If there is no desire expressed, the Chair will decide to take it up after the paper.

I believe there is also a report of the Committee on Units and Standards, Mr. Kennelly Chairman.

MR. KENNELLY:—I beg, sir, to give notice of a motion to be made at the next meeting of the INSTITUTE, that the report of the committee be taken from the table at that time.

THE PRESIDENT:—It has been suggested that Mr. Mauro will read a paper concerning a Change of Policy in the Administra- of the Patent Office, prior to Mr. Leonard's paper, as Mr. Leonard's paper will be illustrated by experiments. I take pleasure in introducing to you Mr. Philip Mauro, of Washington.

MR. MAURO:—Mr. President and Gentlemen. It may not be amiss, I hope, before coming directly to my theme, to express the gratification I feel in meeting for the first time, my fellow members of this INSTITUTE. Although, as I say, this is the first time I have enjoyed that pleasure, I have, nevertheless, through the medium of your transactions noted with a great deal of satisfaction the progress of your body in keeping pace with the marvellous advance in the branch of human activity to which most of your members are devoted.

I understand that the rules and practices governing your procedure here, permit the author of a paper, which has presumably been read and to some extent digested in advance, to present briefly the points and propositions that he desires to advance, in order to leave larger time for discussion. I shall the more gladly avail myself of this privilege, because, there is another paper to be submitted, which will probably be of more interest to the body than the paper I present, and because I am particularly interested in having those persons who are concerned with the subject that brings me here this evening, express their views in the fullest manner.

[Mr. Mauro's paper appears in full on the following pages.]

A paper presented at the 84th meeting of the American Institute of Electrical Engineers, New York, February 21st, 1894. President Houston in the Chair.

CONCERNING A CHANGE OF POLICY IN THE ADMINISTRATION OF THE PATENT OFFICE.

BY PHILIP MAURO.

The views presented in these pages were called forth by the announcement or rumor that the present Commissioner of Patents had decided to inaugurate a radical change of policy in his office, in the treatment of applications for patents where the margin of novelty is small, or the exercise of invention doubtful. The old rule, unwritten but tacitly recognized, has been; when a substantial doubt exists to give the applicant the benefit of it. This rule, it is said, has been reversed.

The policy and purposes of the head of this important bureau are matters of deep interest to the public in general, and particularly to the industrial portion thereof; and while the present incumbent has not seen fit to make any public declaration of his policy in the treatment of applications, the impression that very generally exists, to the effect that radical changes are in contemplation, furnishes a suitable occasion for examining and considering the relations of the Patent Office to the industrial arts, and the influence it exercises upon the development of the country's resources. It is the natural desire of every citizen to see that influence increased, and the affairs of the Patent Office so administered as to produce the greatest possible benefit to the public. It may, therefore, be taken for granted that any changes which the Commissioner of Patents may contemplate in the administration of his office, will have that object in view; but the consequences of changes of this sort can never be wholly foreseen, and frequently are of a nature quite unexpected.

This is my excuse for putting before the INSTITUTE OF ELEC-

TRICAL ENGINEERS my views as to the probable consequences of such change of policy in the Patent Office as is supposed now to be going into effect.

The particular point of inquiry is, whether the examining corps of the Patent Office has been so lavish, lax and imprudent in the issue of patents, particularly where the novel improvement sought to be covered was of a trifling character, that the public interests have been detrimentally affected. If so, what are the particular evils that have resulted from this undue liberality, and how far should the Patent Office shift its ground in the other direction in order to avoid them?

Upon the mere statement of these questions it is apparent that they will not be easy to answer in a conclusive and satisfactory manner; yet it is my hope and belief that, after a fair consideration of them, the conclusions will be that there is no ground whatever for apprehension of damage to the public or to individuals by reason of undue liberality on the part of the patent examiners, but that, on the contrary, the only damage that has been, or will be caused. proceeds from those members of the corps who by nature and habit are disposed to take the narrow and illiberal view of inventions.

In so large a body of men as the examining corps, there is, of course, great diversity of character, disposition, and mode of action. In the exercise of judgment upon applications for patents, we find the two extremes of undue liberality on the one hand, and excessive strictness on the other, and this will always be so; but no one competent to judge will deny that, up to the present time, the work of the bureau *as a whole* has been characterized by fairness, just discrimination, and due appreciation of the rights of inventors, with a leaning rather in the direction of the more illiberal and narrow decisions which have in recent years emanated from judges of small experience in patent matters, and of slight acquaintance with the actual steps of the process whereby the development of the useful arts is effected. The patent system of the United States owes its present position of usefulness and importance to the happy circumstance that its principles received their first interpretation, and its purpose their first judicial declaration, from broad-minded and far-seeing men; and so long as the development of the system follows the impetus and direction given it by the earlier decisions of the Federal courts, its beneficent influence upon national prosperity will extend on every side.

Unless the actions of the examining corps *as a whole* have been lax, careless, and unduly liberal (which certainly is not the case), it is clear that the sum of all the effects of a change in the direction of greater stringency must be detrimental and injurious. The easy-going and indulgent examiner (how many such are there?) may be restrained from improvident grants, but the man of fair mind and sound judgment will feel impelled to refuse patents which, in the untrammelled exercise of his discretion, he would ordinarily allow; while the strict constructionist, whose dominant motive appears to be hostility to inventors, will be confirmed and encouraged in his disposition to perceive an antagonist in every applicant for a patent, and to dispute and place obstacles in the way of every claim that is submitted for allowance.

The proposition at this point is simply that the policy of the Patent Office *as a whole* in the treatment of applications has not heretofore been liberal to the point of laxity or improvidence. The only basis that I am aware of for any opinion to the contrary, is the fact that many patents have been held by the courts to be void or illegal grants, on the ground that the subject-matter was not patentable, or did not, in view of the evidence and character of the results achieved, rise to the dignity of an invention, or involved merely the exercise of mechanical skill. But it would be hasty and illogical to conclude from this fact that the Patent Office has been too liberal in deciding upon applications, and that it would be proper to inaugurate a less liberal policy. In the first place, assuming (which is rarely the case) that the court in annulling the patent has before it the *same evidence* upon which the decision of the examiner was made, we venture to assert that, in the large majority of such cases, the examiner was right and the court wrong. Without disparaging the qualifications of judges, appointed to the bench for their learning and experience in other branches of the law, to deal with what has been aptly termed the metaphysics of the science, we would maintain with great confidence that the majority of the examining corps are better qualified by experience and educated judgment to pass upon the patentability of inventions than the average Federal judge.

That this is not a rash or ill-considered statement must be evident upon reflection. The patent examiners, as a rule, are men of scientific attainments, many having enjoyed in addition a legal education. Each one has been devoted, for a longer or shorter time, to the study of a particular art or class of invention, and in the course of each day makes more decisions upon questions of patentability than will ordinarily be presented to any one judge in the course of a year. On the other hand, the judiciary is composed almost entirely of lawyers who before their elevation to the bench, if charged with similar patent cases to those they are then called upon to decide, would have considered them selves incompetent to conduct or argue them, and have been obliged to employ a specialist in that branch of the law.

Furthermore, it must be remembered that we are not now considering decisions that expound the meaning of the statutes or declare legal principles, as to which, the voice of the court is the only authority that can be recognized. The question whether, in view of the state of the prior art, a given improvement was an inventive act, or differed from mere mechanical skill, is a question of *fact*, which in all trials at law, is decided by the twelve men whom the chance of lot may have placed in the jury box. In the nature of things, a decision that a particular mechanical expedient or departure did not involve invention, can have little or no effect or value beyond the settling of the question for that particular case. For example, the decision that to put a rubber tip on the end of a lead pencil did not involve invention, or produce a patentable article (Reckendorfer *v.* Faber, 92, U. S.) gives no aid in determining whether it was an inventive act to put a torsional spring on a telegraph sounder, there to behave in the manner usual with torsional springs. If the first case has *any* bearing upon the second, it would, of course, lead to the conclusion that the latter involved no patentable change. Yet the Supreme Court held otherwise (Western Electric Co., *v.* La Rue, 139, U. S.).

Finally I would ask those who desire to find and follow court precedent in the class of cases now under consideration, to remember that patents, involving changes apparently as slight as any that ever issued from the Patent Office, have been sustained by courts and judges of the highest reputation and authority, so that, if precedent be sought, it can be found in abundance and to the liking of the seeker. The example given above is a sufficient illustration.

In drawing deductions from decided cases, however, it should, in justice both to the judges and to the examiners be said that in the case of most of the patents which have been found invalid by the former, new evidence not accessible to, or perhaps overlooked by the latter, has been presented; so that in but a small number of cases can it be said that the decision of the examiner has been reversed by the court. The conclusion, therefore, is that decisions of this sort constitute no reflection upon the work or judgment of the Patent Office and no justification for any change of policy in the direction indicated.

But admitting the full force of the fact that certain examiners, in certain instances, have erred on the side of excess of liberality, what are the consequent evils *as compared with those of errors in the other direction?* The grant of a patent is, in ninety-nine cases out of a hundred, an act without *any* consequences whatever. But so potent for good is the hundredth invention—the one that contains the germ of vitality and usefulness—to such an extent does it stimulate the exertions of other inventors that it more than pays for all the failures. The chances, then, of issuing one patent too many, are infinitely small as compared with the chances of prematurely stifling and suppressing what might be productive of benefit; so that the greatest care in conducting the work of the Patent Office is needed to guard against actions which both work injustice to meritorious inventors, and at the same time injure the public by depriving them of the advantage which inevitably accrues from the grant of a patent for a useful novelty, however trivial.

As to the ultimate career of an invention, the judgment of the most experienced persons is ordinarily worthless. Frequently it is the things that promised least, from which the best results have followed, and *vice versa.* It appears strange at first, and yet entirely explicable upon reflection, that the novelties which contain the greatest amount of "invention" and ingenuity are often of the least practical benefit. Machines which are marvelous products of inventive skill, and full of the most intricate and complex mechanism, for which a patent will be granted with enthusiasm, become frequently but curious exhibits of misdirected inventive imagination; while on the other hand the inventor who aims to effect but a *slight* departure or simplification of *what already exists* is the one who really benefits himself and the community. It is by the accumulation of small changes of this nature that the industrial arts advance, step by step, in ever-increasing usefulness.

It is in partial appreciation and recognition of this fact that the accepted policy of the Patent Office has heretofore been to give the inventor the benefit of the doubt in marginal and doubtful cases. Experience shows this to be the safe and wise policy. The examiner who knows something of the history of patentable inventions will, in cases like this, be influenced by the consideration that the *results* of the grant alone can determine whether the device in question has the quality of invention or not; that if it have that quality the inventor deserves his patent, and that if it lacks it, no material harm can result from its grant. He will also be influenced by the consideration that the grant confers after all only a *prima facie* right; and in proportion as the courts are the *more* ready to try anew, without regard to the decision of the examiner, the question of patentability, the *more* freely will the latter exercise his discretion in doubtful cases.

But we have of late heard the reverse of this policy termed "giving the benefit of the doubt *to the public*." This expression thinly conceals the fallacious idea that, in rejecting a patent for a new but slight improvement, it is thereby *given* to the public. Nothing could be more delusive or contrary to actual experience. It is the *grant* of the patent, not its *refusal*, that gives the invention, great or small, to the public; and even the grant is but *a step* in that direction. After that, it requires the utmost persistence, the most favorable conditions, the enlistment of capital and enterprise, to make the blind and heedless public see that the change will be beneficial, and to force the stolid and reluctant public to adopt it. The notion that an improvement comes into possession of the public when the discriminating examiner had decided that it is too trivial for a patent, is one that cannot exist in any mind after a most superficial consideration of the facts. The very contrary is the case, namely, that the most effectual way to prevent its *ever* coming into the possession of the public is to thwart the inventor's efforts to secure a patent for it.

Take any case of actual occurrence that will illustrate the point, as, for example, Butler *v.* Steckel (137 U. S., 21), which involved an improvement consisting simply in producing, by *machinery*, a bretzel having the appearance of a *hand-made* bretzel. For some reason, there is a prejudice on the part of those who are partial to that delicacy, in favor of the *looks* of a hand-made bretzel; and the inventor in this case had the idea of satisfying that prejudice and yet effecting the great economy of machine production. The Supreme Court, which has also judicially decided (McLean *v.* Ortmayer, 141 U. S., 419), that "invention" *cannot be defined* so as to determine in any given case whether the "inventive faculty" has been exercised or not, decided that there was no "invention" in making a mould to produce an apparently hand-made bretzel.

We may assume that the Supreme Court, while not knowing what "the inventive faculty is," but indeed declaring it to be unknowable, yet is gifted with some means of knowing when it has been exercised, and was right in this case, and consequently that the examiner who granted the patent was wrong. The question that concerns us is, what consequences resulted from his action in granting the patent? It is easy to see that the results of this error of judgment, if it were one, were distinctly beneficial. The grant and publication of the patent made the improvement known, and stimulated the patentee and his associates to introduce it to the public and demonstrate its merits. These were recognized, so far as to encourage others to take up the manufacture, thus accomplishing the great purpose of the patent law in spreading the benefits of the improvement, and incidentally bringing about a suit for infringement. Then, the case coming before the Supreme Court for application of the divining rod, or intuitive faculty, or whatever means that august tribunal employs to detect the presence or absence of "invention," the subtle quality was not perceived, the patent was declared void, and the whole matter—fully developed to the manufacturing stage, thanks to the examiner—was thrown into the public domain.

This is, and must be, the result in every similar case; and it is quite safe to say that if the examiner had assumed to pronounce that the "inventive faculty" had not been exercised in this matter, the public would never have known anything about it, and a useful idea, even if not a patentable one in the opinion of the judges who decided the case, would have been strangled in its incipiency.

If, therefore, the judges have sometimes differed from the examiners as to what constitutes a patentable invention, I can see in that, no reason for hesitation in the granting of patents for fear the courts may find an occasion for such difference of opinion. The chances are that the judges were mistaken in many of these cases; and if they have corrected errors in others, they have simply discharged one of the purposes for which courts are established, and were certainly, with the evidence on both sides before them, in a better position to pass the final judgment than the examiner could be.

Let the Patent Office, then, pursue its course courageously, leaving to the courts their proper functions, and not risking, in the attempt to avoid a harmless error, the perpetration of a cruel injustice to the individual and a serious damage to the public.

Why refuse a patent because the invention is a "little" one? Why take away from the humble inventor his *small* invention on the ground that it is so trivial that the public ought to have it gratis, especially when it is thereby buried in the musty pigeonholes of the Patent Office beyond the influence even of the resurrection trump? Why not give him the "little" patent that he seeks for his "little" invention, and encourage him to cultivate and tend and promote his idea, on the chance that it may be nurtured into something useful to mankind, even in a "small' degree? The Patent Office need not fear the consequences of making a mistake in so doing, for there is every ground for assurance, that if the inventor succeeds commercially, the court will be ready enough to take away his patent if the invention has not the magnitude to bring it within the mental perception of the judge who tries the case.

After all, the patent has but the dimensions of the invention which it covers. If they be limited, the patent is correspondingly narrow. Therefore, the Commissioner and his examiners are not called upon to exercise the same discrimination as if a patent were an award or premium of definite value, only to be granted when an invention is found to posses a certain degree of excellence.

But I cannot turn from the discussion of the relations between the courts and the Patent Office, without briefly referring to the other side of the subject, and endeavoring to point out where the real damage has been inflicted, and where the real danger lies.

The trend of judicial decisions furnishes no occasion to narrow the field of patentable invention and of inventive enterprise. On the other hand, there is every reason to broaden the spirit in which the laws are administered in the Patent Office.

The wrong that has been done in adherence to the policy of trimming down each patent to the narrowest dimensions, is incalculable. The courts, while ever ready to *restrict* patents by the state of the prior art, never *broaden* them by interpretation beyond the language of the claims, which bind the patentee, but not the infringer, nor anyone else. There was a time when correction of mistakes by enlargement of claims unduly limited, was permitted through reissue of the patent on the payment of another tax, and when the refusal of this privilege, even without an express statute, was denounced by the great Chief-Justice Marshall as an act that would be "disreputable in an individual" and which "a court of equity might interpose to restrain" (Grant *v.* Raymond, 6 Peters). But the "courts of equity" have since swept that privilege away, and the inventor's only chance now is, with perhaps incompetent solicitors, and in the face of an alert and often hostile examiner, to secure claims sufficiently broad in the first instance.

If we ask where a material injury has been done by an excess of liberality in the decision of an examiner, it would be difficult to find an instance. If we ask in how many cases have patents for meritorious inventions failed, because of the persistent and successful efforts of examiners to narrow the terms of the claims, it would be impossible to determine the enormous total.

The catalogue of the reissue decisions contains the history of grievous wrongs and injustice, due in many instances to the inability of the inventor, through lack of means or of competent solicitors, to combat successfully the opposition of an examiner.

The grant of a patent to an applicant for more, or other than he can sustain before the courts profits him nothing, and deprives the community of no right. The failure of any inventor, who has communicated to the public his discovery, of whatever magnitude, to secure a grant to the full extent of his right, is occasion for profound concern, against which the officials of the Patent Bureau should be constantly on the alert. This part of an examiner's duty we have reason to think, is one that sits lightly upon his mind; and the exercise of official vigilance to which we are most accustomed has quite a different motive and object. Theoretically, the obligation above referred to is uniformly recognized. Practically, the examiner's duty is usually discharged to his entire satisfaction when he has made a *rejection.*

If, by the exercise of unusual watchfulness and careful scrutiny, the issue of a few hundred worthless patents be arrested, the result may be creditable to the administration, but can hardly be regarded with satisfaction for any practical benefit accomplished.

The discovery and remedy of a *single case* of injustice, where the inventor has paid, perhaps, his last dollar for the preliminary fees, and is helpless to contest the too ready letter of rejection, would afford, to my mind, a far better basis for a claim upon public gratitude; and who can dispute that the opportunities for the head of the Patent Office, are much richer in this direction than in the other?

I have noted with much concern the growing disposition of the patent examiners to follow the lead of some of the Federal judges in rejecting applications, where novelty was conceded, on the ground that there was "no invention in doing this in view of that," or as sometimes expressed, because the matter "does not involve the exercise of the inventive faculty." This tendency has developed quite recently to an alarming extent, and being a serious matter, which hitherto has received but scant attention on the part of those whose interests are involved, it may not be amiss to devote a brief time to its consideration.

If, in addition to the exhaustive and fatiguing labors of novelty searches, the thirty-six examiners are to determine, in the case of every novelty submitted to them, whether the new thing specifically pointed out and distinctly claimed, is an "invention" or not, it is manifestly necessary that they should be provided with a clear and comprehensive definition of what an "invention" is, and that each and every examiner should recognize the same definition. The authorities do not, and confessedly cannot, furnish such a definition. Professor Robinson (than whom there is probably no person more competent to frame the needed definition in words) in his exhaustive work on patents, defines invention "as the result of an inventive act," which does not assist me personally to a conclusion which I much desire to reach. The Supreme Court, as we have seen, declares that invention is indefinable, and surely it is not profitable to search further.

We all know, moreover, if we have reflected at all on the subject, that whether the "inventive faculty" has been exercised in any case, is a question that lies outside the perceptive faculties and beyond the investigation of the reasoning powers of mankind. Now the law manifestly did not intend to charge the Commissioner of Patents with the duty of granting an exclusive franchise whenever he should find a thing which is impossible to define, and which he could not recognize if presented to him. It charges him with the duty of investigating questions of *novelty* and *utility*, chiefly the former, and gives him the means whereby he can discharge that duty effectively, and to the public benefit. But what is "invention?" Who has ever seen "inventive faculty" at work, and by what signs can we distinguish its product from that of "ordinary mechanical skill?" There are no answers to these questions. There is no difference in kind between the one class of results and the other, and the dividing line will never be drawn. The Supreme Court has expressly and repeatedly declared that— "The patent is *prima facie* evidence both of *novelty* and *utility*." (Lehnbenter *v.* Holthaus, 105, U. S.) Gandy *v.* Main Belting Co., 143, U. S. Western Electric Co. *v.* La Rue, 139, U. S. These are some of the most recent decisions of that court, and as the proposition is purely one of law, it applies equally to all cases. We find here a recognition of the duty of the Commissioner under the law, to investigate and decide the *novelty* and *utility* of the improvements in arts, machines, manufactures and compositions of matter, for which patents may be sought; but I am not aware of any decision declaring or implying that the Commissioner is called upon to decide in any case whether "the inventive faculty" has been exercised or not, and it is impossible to suggest any reason why the Patent Office should assume so onerous a duty.

In making investigations and advising applicants of the results of such investigations, to the end that they may not through ignorance claim things that are really old, or already patented to others, and for want of such information be led to difficulties and loss, the Patent Office is performing a magnificent service to the country. For that service it is equipped with facilities and with a trained corps of experts, the like of which exists nowhere else in the world. It is in this respect that our patent system is incomparably superior to any other. To what end are these elaborate investigations made, and for what reason are they beneficial to the public? He who supposes that the main object and beneficial result is to suppress in defence of public interests the issue of patents that could not be sustained, is surely in grievous error. That such is not the case is proved by the workings of the English patent system for over a hundred years, and by the practice of every country of Europe where, with the exception fo Germany, patents are granted without any investigation whatever. Nothing but actual or wilful blindness can prevent recognition of the fact that, to arrest the grant of a doubtful claim, for fear the patentee might in some way use it unjustly or mischievously, is the least of all the purposes which the Patent Office is expected to fulfil. No, the object and the merit of the examining system is, that it *advises inventors of the state of the art*, and thus prevents them, not from imposing upon the *public*, but from deluding and injuring *themselves*. If, with the results of the examiner's researches before him, and with but a slender margin of novelty remaining, the applicant assumes the risk of a favorable judgment by the courts, and is willing to pay the required fee for a patent of doubtful value, I can conceive of no possible reason why the Commissioner of Patents should interpose objection. So far as I can see, after the best consideration I am able to give to the matter, the only question involved is a fiscal one; and while it would often, in such a case, be a friendly act to the inventor to prevent his paying twenty dollars into the treasury of the United States, that is surely his affair.

If then, there ought to be a general rule in such cases, as I think is manifest, why would not the best rule be, to give the inventor in every case where he is willing to pay for it, a patent to *the extent of the novelty of this improvement?* This is an important question, and if there be any reasons why such a rule as this should not be established, I should be glad to hear and consider them. Of all that could be urged in favor of it, I will here suggest but one thing more; namely, the intelligibility and ease of application of the rule. Of late years the efficiency of our Patent Office has been at a minimum, because of the enormous accumulations of work, and the unreasonable time that must elapse between the application and the grant of the patent. No one, who has ever had business before that office, need be told of the prodigious waste of time consumed between examiners and solicitors in the hopeless and unprofitable discussion of what is, or what is not an "inventive act." The disputants in such cases must be aware that they do not know, and never can know the truth of the matter; and surely it would seem that the time consumed in searching for and citing inappropriate decisions, which serve only to cloud a subject already hopelessly obscure, conld more profitably be employed in those searches for novelty for which the office was created, and for which, out of the pockets of inventors the Commissioner and examiners are paid.

Finally, not only are examiners (being but human) incapable of deciding whether a departure involves "invention" in the metaphysical sense, but manifestly it is particularly unfair to reject on this ground an application for a novel improvement, however seemingly trivial; because to decide that question at the stage of development which an invention has reached while yet under examination in the Patent Office involves prejudgment. When the court decides this question, it looks and gives heed to the *results* that have followed from the alleged invention and patent. This is but the application of the maxim of divine wisdom that a tree shall be known by its fruits. But to pass upon this question in the Patent Office, requires the examiner to judge by the fruits before the tree has sprouted, and to do that he must have, in addition to the gift of second-sight, that of prophecy. No one, I think, will dispute that this is too much to ask of a body of hard working and faithful public servants, particularly at the rate of compensation now fixed by law.

I submit then to my associates in this INSTITUTE, whose members are working in a field where, of all others, it is most difficult to predict the results of trifling departures from established practice, and to whom, therefore, the questions that I have so inadequately discussed are of the deepest interest, that the Patent Office can best justify the wisdom of its founders, and best promote the welfare of the whole people, by adhering to the rule of granting to every applicant a patent for all that is, or appears to be, novel in his particular improvement. If my conclusions be sound, and my reasons have anything like the force that I suppose, there is clearly room for a beneficial change in the policy of the Patent Office, but it does not lie in the direction which at this time it appears to be taking.

DISCUSSION.

The Secretary read the following communications from Messrs. Vansize and Thompson:

MR. W. B. VANSIZE:—The matter called to our attention by Mr. Mauro is of great importance. I understand that the present administration of the Patent Office is enforcing a policy which is undoubtedly novel. Our patent laws and the practice of the Patent Office have heretofore been regarded as liberal, just, generally satisfactory, and fully within the scope and spirit of constitutional authority. It seems that the novel policy recently inaugurated embodies a general rule of action to be followed by the various departments of the Patent Office, according to which an invention or improvement fully described and properly claimed, shall be rejected and a patent refused whenever the means sought to be patented is deemed to have been attained without the exercise of the inventive faculty, or when by comparison with the state of the particular art the examiner is of the opinion that no "invention" was involved in its production. This is equivalent to determining that if the particular examiner or department in question were called upon to supply a certain means to attain a certain end he would immediately, upon reading the prior patents or publications, supply the demand with the arrangement of apparatus described and claimed by the applicant for a patent. Of course this conclusion would differ with the different examiners. We cannot reasonably accuse each and every one of the thirty-six examiners of having this high degree of intelligence and technical skill. It would seem very natural and very equitable that in case of doubt in the mind of the examiner, such doubt would be resolved in favor of the applicant, and that the application would be allowed, but I understand from Mr. Mauro, and it is my observation from my own recent practice, that the reverse is the case.

It it very difficult for me to see why this should be so; it certainly is not in harmony with the practice of courts of equity when called upon to pass upon the validity of a patent in dispute. The courts are, and always have been, controlled by the doctrine that "A patent should be construed in a liberal spirit "to sustain the just claims of the inventor. . . . . Liberality, "rather than strictness, should prevail where the fate of the "patent is involved, and the question to be decided is whether "the inventor shall hold or lose the fruits of his genius and his "labors." (Rubber Company *vs.* Goodyear, 9 Wallace, 788) "Patents for inventions are to receive a liberal construction, "and under the fair application of the rule, *ut res magis valeat* "*quam pereat*, are, if practicable, to be so interpreted as to up- "hold, and not to destroy the right of the inventor." (Turrill *vs.* "Railway Co., 1 Wallace, 491.)

Those are the declarations of the Supreme Court of the United States relative to the construction of patents. Reasoning upon that doctrine controlling the treatment of patents, it would seem that the same doctrine should control the Patent Office in dealing with an invention upon which a patent is asked to be issued. The Patent Office is bound and controlled by the decisions of the courts upon any contested question. Why is it not bound and controlled in cases where the practice is so clearly parallel and coincident?

The novel policy of the Patent Office will often operate to "destroy the right of the inventor," and cuts off all chance for the application of any such liberal rule unless you choose to incur the expense of going to the court to secure the grant of your patent. This puts the Patent Office in the position of obstructing rather than encouraging advancement in the useful arts. I gather from Mr. Mauro's remarks that the Patent Office has been moved to initiate its novel policy by reason of the fact that several patents have recently been found by the courts to be void for want of invention. This causes us, naturally, to inquire:

Would it not be better policy and better practice for the Patent Office to follow the very liberal doctrine declared by the Supreme Court in pursuance of which an inventor might secure his patent rights, and this, even in cases where there existed reasonable doubt on the so-called question of "invention"? In this case the possible error of the Patent Office could be rectified in the courts. If you deny a patent, you cut off all chance which the inventor might have of securing suitable protection and remuneration for his possibly meritorious invention. You condemn his invention and you deny that you may in error. Our papal friend in Rome cannot exceed that doctrine. In my humble opinion, if change is desirable, it would be far better to abolish all examination as to merits, and grant a patent to any applicant upon payment of the proper fees. That is the English custom, and it certainly avoids the rank injustice into which this novel policy is forcing us.

Of course, I am cognizant of the fact that you can appeal from the primary examiner to the Board of Examiners, and failing in the Patent Office apply to the court. This is very expensive in time and money. Why not grant a patent in the first instance and then go to the court and test any doubtful questions? It is certainly more satisfactory and less liable to result in that loss and injustice likely to occur in many cases from the denial of adequate protection to meritorious inventions should the novel policy be insisted upon.

MR. EDWARD P. THOMPSON:—What the author terms a rumor as to rejection of applications for want of exercise of the inventive faculty and for giving the doubt of patentability to the public, is more than a rumor, as I can testify that comparatively lately I have had more applications rejected for alleged want of invention than formerly. The examiners do not deny novelty but declare lack of invention, in more cases than formerly. This is my experience. I agree with Mr. Mauro that this policy is not for the public benefit.

While I have had applications freely rejected upon the ground of want of exercise of inventive faculty, I can compliment the examiners upon their fairness in reversing their decision upon a proper showing and argument. In general, I argue that, independently of the decisions of the courts, independently of the commissioners' decisions and many other authorities and cases, the United States patent statute 4,886, shows very clearly what the examiner's duty is. Let us learn the exact wording. It states that: "Any person who has invented or discovered any "new and useful art, machine, manufacture or composition of "matter, or any new and useful improvement thereof may ob- "tain a patent therefor." The statute states other matters as to foreign patents, public use, etc., which have no relevancy here.

That which is new and useful is an invention. As soon as the Patent Office, United States courts, and other authorities will take this statute as their guide, they will find nothing whatever as to exercise of inventive faculty.

In no other department of law would the merits of a case be decided except upon facts, especially when facts exist. The two facts to be set forth are novelty and utility, which if existing prove invention. The degree of novelty can be determined and admitted by the examiner. The degree of utility can be known by affidavits and more accurately only after the invention has been used by the public. Let novelty and utility determine patentability.

MR. THEODOR J. W. OLAN:—I have lately had some experience with reference to the Patent Office, and the practice of dividing up patents, in the way examiners have tried to do it, has been especially pressed upon my attention. I lately had an application in for one patent—I supposed it was one—but the first communication I had from the examiner informed me that I had twelve inventions there instead of one, and he wanted me to divide it up into twelve applications. He said that the different parts of this invention had already entered into the art as subjects for different inventions, and it was unreasonable to expect that the Patent Office would grant a single patent on so many inventions. I could not find that this examiner's view was sustained by the law, so I wrote a letter in which I told him that I failed to find any section of the patent law, or any section of the rules of practice of the Patent Office, which would allow him to take such a view of the matter. I asked him: "What could not be said to have entered into the arts as subjects for different inventions." "If," said I, "one inventor whose inventive capacity should turn "around a head of a pin, should ask that a patent be granted to him "for a certain form thereof which he thought useful for one "purpose or another, should now another inventor, therefore, be "obliged to file his application for another patent relating to a "complete pin in two different applications?" I said that I thought there was no other way than this to find out which was an independent part in a patent case: namely to see if for any special part of an invention a special and independent patent could be granted without injury to the entire invention. That is to say, that the different patents which might have been issued for different parts, could, without injury to any of those parts or without injury to the whole invention, come into the hands of different contesting owners. I said that if they could not do that, then that was a proof that the parts in question were not separate inventions and that the inventor could not be called upon to file so many applications for what he thought was one single thing. I then received an answer from the examiner. He said that he maintained his views, but that he made it three instead of twelve. I said again that in order to remove this obstruction, I will cut away the claims he had asked for under protest. I had also said, in my previous letter, that I hoped in my own interest, and in the interest of other inventors, that should the examiner not approve my views there expressed, Congress might do it before long. Then shortly after, I saw in a Washington paper that the Commissioner had just thought of altering the practice, and I wondered if that did not have something to do with this practice of the examiners of cutting up and dissecting the inventions so much. It might not be so much their wish to refuse small patents as their desire to get as many fees from the inventor as possible for one single thing by dividing it up.

THE PRESIDENT :—I see Mr. Forney, the editor of the *American Engineer*, here. I know the INSTITUTE would be glad to hear him on this subject on which he is abundantly capable of speaking.

MR. M. N. FORNEY :—I do not know that I have anything that I could add to this discussion which would be of interest to the gentlemen here. I have gone through some of the tribulations which most inventors go through at the Patent Office. I have suffered from the division and sub-division of my patents, and I have also suffered, as others have who are in the habit of going to Washington for patents, in sometimes having my applications rejected. The fact of there being a new rule adopted in the Patent Office is, however, new to me. I was not aware that any new system had been introduced there, and it probably accounts for some things that I could not account for in my own experience recently. Anything, I think, which would take away from the liberality of our patent laws ought to be very much regretted, for the sake of the general advancement of the arts and sciences. Few of us realize, perhaps, how much this country owes to the patent system which was inaugurated by those wise old fellows who founded our government a great many years ago. The question has very often come up as to what the result would be in case the patent laws were abolished. I think some of us, perhaps, by citing our own experience can give an indication of about what would happen. I know in several instances in which I have had inventions, of probably very little merit, but at any rate, I made application for patents for them in the Patent Office, and it has happened, in a number of instances, that I have discovered that someone else had anticipated my inventions. The result of it was, I simply dropped the whole scheme, paid no more attention to it, and did nothing to develop it, although at the time I had the impression that the inventions were of value. Now, if that same principle is applied all over the country, to all inventors and to all inventions, the obvious result would be that invention would stop, and then instead of inventors devoting their time and thought and energy to the subject, they would simply devote their time and energy to some other purpose. They would go into trading or into speculating or something similar. The object of inventing to most of us is not entirely the love of invention, but because we hope to realize some profit from it. Therefore, it seems to me that any policy of the Patent

Office which looks to a less liberal construction of the patent laws or the practice would be very disastrous not only to invention generally, but it would be a step backward in the prosperity of the country.

MR. JOSEPH SACHS:—The rejection of an invention for want of patentability I really do not consider so very undesirable a feature of Patent Office practice. To my mind the examiner has in view, in doing this, to discover whether the inventor himself really believes that he has discovered any new process or machine By showing the inventor, or trying to show the inventor, that he has not, and citing certain instances where similar devices have been used in similar ways before, he brings forth from the inventor a series of ideas by which the inventor tries to show that he has invented something, and if those ideas which the inventor puts forth go to show that the invention is actually a valid one, then in most cases the examiner will take back his former decision and grant a patent for the invention.

I have found within the last two or three years that of very nearly fifteen applications that I have made, the greater part of them have been rejected for the want of patentability, the want of invention. I have never been quite as lucky as one of my predecessors in having found that there were twelve inventions in what I thought was one; but I frequently found that the examiner thought there was no invention at all. That in view of patent number so and so in combination with patent number something else, the mere addition of a screw or spring or something else did not constitute invention. But upon my showing that this spring or screw or something else was absolutely necessary to gain this new effect, the patent was generally allowed. I really think that the mere citing of instances to bring forth ideas from the inventor, showing that his idea is a new and valid one, is not detrimental at all to our inventive advancement. I think it gives the inventor strength and gives him opportunity to show up the points of advantage in his invention, and if his invention has no points of real advantage he should not be granted a patent thereon.

THE PRESIDENT:— The Chair may perhaps be pardoned if he adds his little to the history of the annoying experience in the Patent Office that may be summed up under that very disagreeable phrase, "lack of invention." Very early in the history of the art he attempted, in connection with Elihu Thomson, to take out a patent for what was practically the first transformer ever applied for in this country. We showed an induction coil with a long thin primary and a short thick secondary, and the erudite examiner rejected the application on the statement that it contained or showed no invention. I remember the phraseology fairly well, as it made quite an impression on me at the time, because I knew that we had a valuable new idea. The examiner informed us that it did not constitute an act of invention merely to change the relative proportions of the resistance in the primary and secondary wires. We know that it was, so to speak, a great invention as it gave to the world the transformer in place of the ordinary Ruhmkorff coil. The great trouble in this respect in the Patent Office comes from the fact that the examiners are inadequately compensated for their services. We should pay sufficiently high salaries to get unquestionably first class men. A broad-minded man that knew anything of the art never could have made a mistake like the one I have mentioned. I do not at all wish to be considered as sitting in judgment on the examiners in the Patent Office, especially in the very difficult department of electricity. They are excellent men. But I do think that a great improvement would come in urging the government to set aside a fair proportion of the profits of the office and apportion them more among the examiners in the way of increased salaries. I think a real reform could be worked in that direction.

[COMMUNICATED AFTER ADJOURNMENT BY MR. ALMON ROBINSON.]

Those of us whose specialty is the making of small inventions owe the author a considerable debt for setting forth so clearly and forcibly our claims to recognition.

It is generally admitted that inventors of the other sort, who patent their untried dreams and get broad claims on them, deserve encouragement; but it is thought that the more carefully considered things, which show fewer unheard of features, will come along by themselves. Anyone who has had occasion to closely watch the way in which new devices and methods come into use knows that this is a mistaken view.

I call to mind a case in which a manufacturing firm made a long and persistent effort to obtain a patent on a well tested improvement in their own line of work. Failing on account of alleged lack of invention, they decided to drop the thing entirely, not caring to fit up for making it without the protection of a patent.

Another case can probably be paralleled from the experience of many members of the INSTITUTE. A crude electrical idea was carefully spread out by a skilful patent lawyer. An independent inventor offering the same idea worked into practical shape, was told that there was no patentable difference. Neither of the devices are in use.

The necessary result of the threatened restriction would be to take away all inducement for trying to make something useful out of the impractical anticipation which is always standing in every one's way.

The real trouble, however, with the supposed plan of the Commissioner, is not that he is fighting an imaginary evil, but that the office will unavoidably work injustice in carrying out his ideas, and this will come from something that lies deeper than any personal shortcomings of the examiners; from the fact that an examiner has loaded upon him the distinct and incompatible duties of state's attorney and judge. He is first called upon to hunt up every possible objection to the granting of the patent, and must then sit in judgment upon his own work. It is not in human nature for him to judge fairly.

It is easier to point out the difficulty than the remedy, but I venture to suggest a modification of a plan that has been before brought forward. Let the examiner do his worst and set forth all the objections known to him, and give the applicant the privilege of taking out a conditional patent, on which all the examiner's references and objections are endorsed, and print these with the claims in the *Gazette*.

Endorse on the printed copies and publish in the *Gazette* anything further which comes to the knowledge of the examiner so that the full state of the art as he understands it shall be at all times accessible to the public. Then provide in the Patent Office a board of appeal before which the patentee must take all matters in dispute before attempting to collect damages of infringers.

I ask attention to one feature of this plan. It leaves the state of the art in full public view. Most workers in the electrical field can appreciate the advantages of this.

Lewiston, Me., February 27, 1894.

THE PRESIDENT:—Are there any further remarks on this paper? Shall we proceed to the next paper, or shall we now consider the election rules?

Nothing being said, I shall consider that it is the desire of the meeting to hear Mr. Leonard's paper.

Mr. H. Ward Leonard then read the following paper entitled "How Shall we Operate an Electric Railway 100 Miles from the Power Station."

A paper presented at the 84th Meeting of the American Institute of Electrical Engineers. New York, February 21st, 1894. President Houston in the Chair.

HOW SHALL WE OPERATE AN ELECTRIC RAILWAY EXTENDING ONE HUNDRED MILES FROM THE POWER STATION.

BY H. WARD LEONARD.

Let us suppose that we are called upon to act as engineers for a steam railway desiring to operate its line by electric locomotives. There exists a very economical source of power, possibly a water power, so situated that the length of railway to be operated in either direction is 100 miles.

Let us determine the leading points of the specification for such a road, based upon our experience to this date, and after making the specification, let us see whether we are to-day able to comply with the specification, and if not, what must be done before we can comply with it.

The following features seem desirable, if not essential, in such a railway:

1st. A single trolley contact shall be used for supplying current to the locomotive.

2nd. The E. M. F. upon the trolley shall not exceed 500 volts.

3rd. There shall be no apparatus in motion and requiring attention, between the power station and the locomotive.

4th. No commutator, rheostat or controlling device on the locomotive shall be subjected to a higher E. M. F. than 250 volts, and there shall be no sparking on any of the apparatus under any normal conditions.

5th. The entire control of the locomotive in either direction shall be effected by the movement of one lever.

6th. The load shall be started from dead rest by an amount of energy taken from the source of supply, which shall not exceed one quarter of the energy required to operate at full speed on the level.

7th. The retardation of the load in coming down grades, and in stopping, shall be effected by converting the motors into generators, which shall feed back current to the line, and thereby assist the power station in operating other locomotives.

8th. The motors must be reversible when operating at full speed, without damage to the motors or other apparatus.

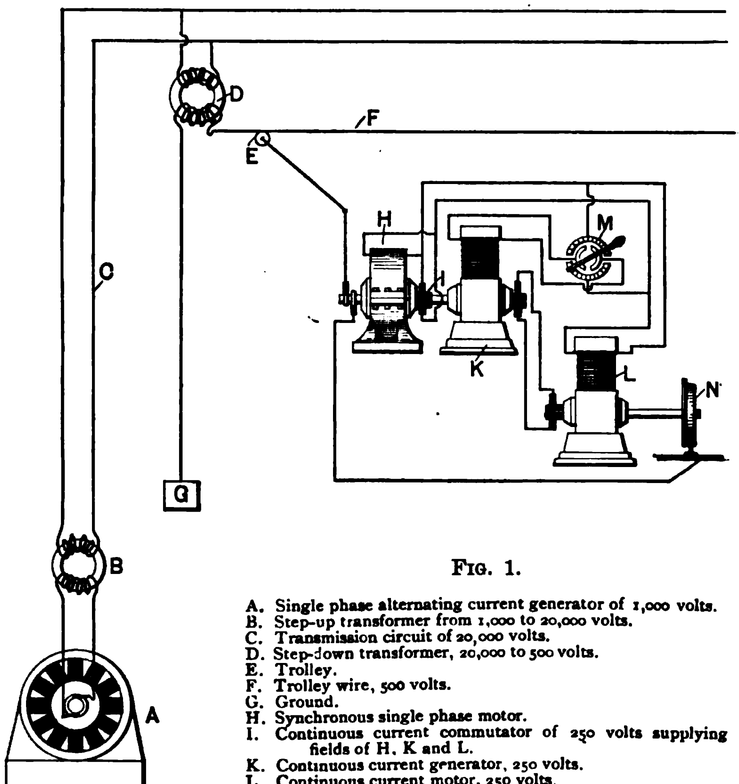

FIG. 1.

A. Single phase alternating current generator of 1,000 volts.
B. Step-up transformer from 1,000 to 20,000 volts.
C. Transmission circuit of 20,000 volts.
D. Step-down transformer, 20,000 to 500 volts.
E. Trolley.
F. Trolley wire, 500 volts.
G. Ground.
H. Synchronous single phase motor.
I. Continuous current commutator of 250 volts supplying fields of H. K and L.
K. Continuous current generator, 250 volts.
L. Continuous current motor, 250 volts.
M. Reversing rheostat in separately excited field of K.
N. Driving wheel of locomotive.

9th. The efficiency of the system from power on the generator shaft to the draw bar pull of the locomotive, shall be at least 50 per cent.

10th. The locomotive shall produce at least 500 H. P., when operating at a speed of 80 miles per hour.

It will be evident that we must use a high E. M. F. for operating over such great distances. The average distance over which the power is to be transmitted is 50 miles, and we find that in order to operate with a loss in conductors of 20 per cent., we must have an initial E. M. F. of 20,000 volts, in order to make the cost of copper about $20 per K. W. which is about the best figure for cost of copper under the conditions.

The alternating current must evidently be used for such an E. M. F. as this, and the single phase alternating, since we have but one trolley contact.

Let us start (see Fig. 1) with the standard 1,000-volt single phase alternators in our power station, and convert by step-up transformers from 1,000 to 20,000 volts required for the transmission circuit. Since we are limited to 500 volts upon the trolley, we must insert at suitable points, say every two miles, a converter, which will transform the energy at 20,000 volts in the transmission circuit, to energy at 500 volts in the trolley circuit, one pole of which latter circuit will be the rails.

We have our energy delivered at our point of use with reasonable cost and efficiency and by simple and well tried apparatus. But the energy is in the form of a single phase alternating current which is not very flexible.

We can operate a synchronous alternating current motor by this current, but it cannot be regulated in speed or reversed in direction, and cannot be started under load, and will be thrown out of step if a large load be suddenly applied. As all of these conditions are required of the locomotives, a motor operating by the alternating current evidently cannot be used directly.

But it is a simple matter to start the synchronous motor without load and when it reaches its synchronous speed it will perform work efficiently and satisfactorily, provided it be not subjected to violent fluctuations in the load applied to it.

Evidently, then, what we need is some form of gearing between the synchronous motor and the axle, which will give us the desired control and enable us to operate at any speed and in either direction.

It is quite possible that this can be accomplished mechanically, and many ingenious devices for this purpose have been invented, but none seem to be sufficiently simple, reliable and lasting for use on such a large scale.

The equivalent of such a mechanical gear can, however, be secured if we will make use of the synchronous motor merely to drive a continuous current generator on the same shaft at a constant speed, and use the continuous current so generated to supply the propelling motors connected with the axles of the locomotives.

Since the generator is used for the motors on one particular locomotive only, we can vary its E. M. F. at pleasure, and hence can produce a low E. M. F. for low speeds, and increase the E. M. F. to increase the speed and by this means avoid the loss of energy which is wasted in rheostats when motors are started under load, and when connected as usual upon a source of constant E. M. F.

In order to secure rapid changes in the E. M. F. of this continuous current generator at will, it will be best to have its field separately excited which will also enable us to reverse its field at pleasure. The propelling motors can be series, shunt, or separately excited. The best results will be obtained by separately exciting the field, and keeping it fully and constantly excited and reversing the motors by reversing the field of the generator which of course will reverse the current in the armature alone of the motor.

To secure this exciting current for the synchronous alternating motor, and also for the fields of the continuous current generator and motor, it will be best to drive by means of the alternating motor armature, and if desired in the same field, a continuous current winding connected to a commutator, from which will be led the current for exciting the fields of all three machines.

Let us wind the fields for 250 volts, and also use this voltage for the continuous current armatures. This pressure is perfectly safe and can be handled with impunity.

Suppose now the locomotive to be at rest. The synchronous motor is running and driving the generator armature at full speed in a field of no intensity, hence the propelling motors receive no current. We now make the first contact upon the rheostat in the generator field circuit and let the resistance in the rheostat be such as to produce say 25 volts at the generator brushes.

This 25 volts will supply a very large current to the motor armature at rest in its saturated field, and consequently will produce a sufficient torque to start the entire load and continue to move it at a slow speed.

We are using 25 volts and let us say 2,500 amperes in this circuit; this means 62,500 watts and disregarding transformation losses for simplicity, this means a current of 125 amperes from the trolley. When operating at the rate of 500 H. P. at full speed we shall need say 1,800 amperes and 250 volts in our propelling circuit, which is 450 K. W. and means roughly 900 amperes from the trolley. It is evident therefore, that we can start the load with but a small fraction of the energy required for operation at full speed, and that there will be no danger of throwing the alternator out of step by applying but about one sixth of its full load and applying that gradually as will be the case, as the load will follow the increase of the generator field strength, which although rapid, is gradual and not instantaneous.

If we are operating at full speed, and desire to bring the locomotive to rest, we gradually but rapidly reduce the strength of the generator field by manipulating the rheostat in its field circuit so as to reduce to zero the current exciting this field; the E. M. F. produced by the generator then rapidly falls below the counter E. M. F. of the motors, which are being driven in a constant field by the momentum of the moving load, and the motors consequently become generators, and supply current to the former generator which now becomes a motor, and driving the alternator, feeds current back through the trolley, thereby not only bringing the locomotive smoothly and rapidly to rest, but saving the energy usually wasted upon the brake shoes.

Under this arrangement, if we are using steam engines as the source of power, we never subject the engines to the violent fluctuations ordinarily met with in electric railways, and by reason of having a comparatively steady load, can secure very high economy in the consumption of steam, and since we have eliminated the excessive load in starting, we can very much reduce the capacity of the engines, generators and conductors over usual requirements.

The reversal of the motors is very simple and smooth by this method. The lever of the rheostat in the generator field circuit is moved, so as first to reduce the current to zero, and then increase it again to its maximum but in the opposite direction around the field. The reversal of the motor armature, following the gradual change in the strength of the generator field, is extremely smooth, and the armature is not subjected to any sudden strain. No sparking will be met with under any condition, upon either the generator or motor commutators, or upon the field rheostat.

The combination of apparatus, and method of use I have described, enable us to conform fully with the specification given above, and while it is possible that the future may make this arrangement appear clumsy and crude, it has the present advantage of making use of apparatus we are all familiar with, and manufactured by a score of different concerns, and such an arrangement as I have described will serve a useful purpose until we can get the perfect single phase alternating motor, or the motionless transformer for continuous currents, which we have needed and waited for so long, and which many people even yet expect will eventually be realized.

Personally, I have for some years believed firmly that the transmission of large amounts of energy over long distances must be done by the alternating current, and that the continuous current is the only suitable one for the efficient operation of motors which must be varied in speed, torque and direction.

Hence, I believe that the lines I have described above will be the lines of future practice; namely, the use initially of an alternating current which will be converted upon the locomotive to a continuous current, and used in this form in the propelling motors.

After reading the paper, the author described his apparatus, which was in operation upon the platform, as follows:

The two brushes on the armature of this motor are connected directly to the two brushes on the generator, there being nothing in the circuit of the armatures, and the field of the final motor being directly across the line, and consequently being continuously and fully excited in one direction only. Of these instruments—in order to give you an intelligent idea of what we have here, *this* is merely a starting rheostat for starting the motor generator under friction load. These two instruments read the voltage and current of the supply circuit, and I have selected an instrument for reading the current for supplying the whole system, with a rather large index, so as to clearly show the small current required to start the load. These other two instruments are connected with the armature circuit, the voltmeter and the ammeter. Both of them have their zero at the center and will read in either direction. As the current in this armature circuit is reversed, you can read the volts and amperes in both directions; the only instrument probably that you could observe the readings of, except perhaps later by closer examination, is the instrument reading amperes from the supply line at a constant potential of 110 volts. Now we have the motor generator in operation with its friction load, with its field excited, and with the field of the final motor fully excited also, but with no current around the field of the generator—the intermediate machine. I have here a brake which will enable us to give the final motor its full torque. But I will first remove the brake to show the performance of the armature under conditions of no load whatever. I have placed a chalk mark on the inside of this pulley, with the idea of having you see that the armature is in rotation. I have also made a chalk mark on the head of the armature at this end. It is now turning at perhaps one turn in four or five seconds, under merely friction load due to the residual magnetism of the field of the generator. There is no current in the field of the generator, but there is a little residual magnetism. I find from reading the instrument that the voltage is now about two volts, and the amperes something like half an ampere, and there is, of course, no work of any kind being done, but the very slow rotation of the motor armature in its bearings. Now under the conditions that we have in this final motor, it having a constant field, the torque will be proportional to the current, and as we are in a position where we can vary the voltage supplied from the generator, the speed will be proportioned altogether to the electromotive force supplied, and since the product of the volts and the amperes represents the electric energy that we are using in that circuit, and since the speed times the pull in pounds, represents the work which is being performed by the motor, the conditions that we have here are such that, disregarding the excitation currents, we have a perfect efficiency under any conditions of speed or torque in either direction. There is no waste of energy in rheostats, as is usual, and the very strong field of the motor fixes the lines of force so that there will be no sparking upon the motor commutator, and although a great many have anticipated that there should be sparking upon the generator commutator, there is no sparking upon that whatever, and I will briefly state what appears to me to be the reason for this. In the case of a motor of constant potential, we know that if we weaken the field with a large armature current it will spark very badly, and it is probably from this knowledge that it was anticipated that the generator under similar conditions would also spark. But the conditions are different in this respect. In the case of a motor we have the full electromotive force upon the commutator and therefore practically a constant "volts per bar" and then weaken the field under these conditions. In the case of the generator the volts are a function of the field, and as we weaken the field of the generator, the volts per bar are reduced simultaneously and proportionately, although there is a very marked distortion of the field generator, and a very strong lead of the neutral point under the conditions of a weak field and a very strong armature current, the neutral point being perhaps distorted thirty or thirty-five degrees from the line on which the brushes rest, the volts being broken by the brush are so slight on account of the low total E. M. F. being produced that no sparking is met with. Now as we cut out the resistance in the generator field circuit step by step, the amperes required by the motor will be practically constant as the torque is practically constant, but the volts on the armature circuit as we cut out the resistance will gradually rise, and the speed proportionately. Now the machine is running at full speed in one direction, and now we quickly reverse it to full speed in the other direction under full load, and as will be noticed without any sparking.

One point that is spoken of in the paper is the restoration of energy to the line of retardation of the moving load. I will now lift the brushes of the motor while it is running at full speed without load. The motor continues to run for a very long time, as the friction of its bearings is the only thing that is tending to stop it, and it is moving at a very high rate of speed, something like 1,800 revolutions per minute. But under the conditions of practice, by moving the generator field rheostat to the center, that is, weakening the field of the generator, the motor being driven by its momentum, produces an electromotive force which soon becomes higher than that of the generator, which is rapidly falling, and it therefore feeds current back. Consequently you will see that the motor comes to rest very quickly, and in so doing its entire effort is in the production of energy to drive the intermediate dynamo as a motor. While it is running at full speed in that direction, we can instantly reverse it to full speed in the other, as you see.

Now, the full brake load of the machine, that is, the full torque, is given it by this weight applied to this point. You will notice the index of the ampere meter here, and everything being at dead rest, this small current would represent the amount of energy that would be required to excite the field and overcome the friction load. Now, making the first contact here, we give the generator field a very small current. We are sending an amount of current through this motor armature which tends to make it go. The amount of current that it is receiving is six amperes, and it is not a sufficient torque to start under the brake load. The next contact gives us a little more than eight. It now starts and is turning very slowly, with an amount of current which is a little more than this motor is designed for. It has the full load and it can be made to move at a speed which is very slow, about 15 revolutions per minute, and, as you have noticed, the ampere meter here in the supply line has moved almost nothing, less than one ampere, from the position in which it was originally. As we now increase the field strength of the generator, we gradually get an increase of electromotive force from the generator and the energy from the line goes up just as we increase the load. Now we are running at full speed and doing full work. While running at full speed we now instantly reverse the field and go at full speed in the other direction.

Now, under condition of the ordinary rheostat control of apparatus, this full working current would be required for accelerating the motor under full load. Here I am recording in the armature circuit an amount of current which is something like 12 amperes, which is about 50 per cent. more than its full brake load, but the E. M. F. is only 20 volts. That same amount of current would be required to give the same torque if it were used in the rheostat method, and this ampere meter from the supply line in starting from dead rest under full load would read about 50 per cent. more than its full load and full speed reading. Instead of that, starting the load with this system makes only a deflection of less than an ampere from the line. Although I get in the armature circuit here something over eight amperes—by the way, in speaking of that I will point out one fact that I omitted—that the armature circuits of these machines are 250 volts here, so that in order to make any comparison of figures between the current from line, which is 110 volts, we must give due consideration to that. The reversal of the load takes place without any sparking whatever and is perfectly smooth. It is a feature of this system that the fields of both the generator and the motor have no connection whatever with their armatures—no electrical connection of any kind. Consequently in the handling of the apparatus there is no tendency to sparking at the brushes due to any rapid changes of the field magnetism or any breaking of the field magnetism. Another point is that at the time the reversal occurs, the electromotive force produced by the generator is zero and there is no tendency to spark. You will notice that the current from the supply line goes up perfectly smoothly in proportion to the work it is doing. It does not jump beyond where it should and then finally come back again. Of course, if we were handling anything like a train or any body of large momentum the movement would probably be tolerably slow as compared with the handling I am giving it now. I do not think of any other points that I wish to speak of in connection with the apparatus unless some questions are asked in regard to it.

The President:—Mr. Leonard's excellent and instructive paper is now open for discussion.

Discussion.

Mr. Charles G. Curtis:—I would like to ask Mr. Leonard why it would not be the better plan to place the armature field in series.

Mr. Leonard:—Its armature and its field?

Mr. Curtis:—Yes. In other words, in starting, a great deal of power is required to overcome the inertia, and the amperes required in the armature connected with the wheel would be exceedingly high. Don't you think so?

MR. LEONARD:—Yes.

MR. CURTIS:—Well, in railway motors ordinarily, it is found almost necessary, unless the field is intensely strong, to put the field in series with the armature, so that the field would have a very strong current flowing through it when the armature does.

MR. LEONARD:—Well, we have a field which is saturated, and I do not believe it would be increased materially by any increase of current. The field is constantly excited and fully saturated. It is directly across the line, and in this case it is the regular shunt winding of a shunt motor disconnected from the armature. It is connected directly across the line. Of course, if we were to attempt to work with a series wound motor for the last one, the reversal of the field of the generator would not enable us to reverse the motor, as it would reverse both the armature and field current.

MR. CURTIS:—That could be done by a separate switch.

Mr. LEONARD:—Yes. But I do not see that there is anything to be accomplished by that, and you certainly get a variation of the field of the motor which is one of the most undesirable features.

MR. CURTIS:—It seems to me, Mr. Leonard, if you apply a very heavy motor to the armature of your generator and weaken its field so as to get a very great reduction in the electromotive force, as much as 75 per cent. reduction—you would necessarily get a distortion of the field. I cannot see any difference between the case of a generator and the case of a motor as regards sparking.

MR. LEONARD:—Well, I do not know that I can do anything but try to make it more clear by repetition. In the case of this motor here, there are 250 volts across the brushes, and if we weaken the field of the motor it would certainly spark very badly. But in starting the load there are across the brushes of this generator but perhaps twenty volts, and its field is very weak. Now with a full current in the armature, that is, the full starting current, there is a very strong distortion of the field, and as I said before, it probably amounts to something like 35 degrees. We would find that the line of maximum difference of potential is some 35 degrees from the line where the brushes are. But at the point where the brushes are, the whole electromotive force around the commutator being but one-tenth of its full E. M. F., it is equivalent to multiplying the commutator bars by ten, at any rate, and it really has a much better result than that. There is absolutely no sparking from the generator brushes under any conditions where I have ever applied it as yet, and I have applied it under very severe conditions and in the movement of very large loads in electric cranes and elevators. Of course the distortion will be first in one direction, and then when the field is reversed it will be in the other direction. Therefore your brushes should be set for no load whatever.

Mr. Curtis :—Have you tried it on very large machines—large armatures?

Mr. Leonard :—Machines of fifty horse-power.

Mr. Curtis :—How much do you weaken the field when you are getting the first part of the starting torque?

Mr. Leonard :—There is a resistance in series with the field, in a motor of 250 volts and of such a horse power as fifty, of perhaps a thousand ohms in series with the field.

Mr. Curtis :—How weak will the field be compared with the full strength?

Mr. Leonard :—About one twentieth.

Mr. Curtis :—Do you find under those circumstances that it won't spark any?

Mr. Leonard :—Not a particle. I must smile a little at that, because it is the same question that everyone since I first had anything to do with the method has raised, and I do not know of a single person who has not expected that that generator would spark.

Mr. Curtis :—I never saw a generator yet where the field is weakened that would not spark where you do not shift the brushes, and I do not see any difference whether that is feeding current to a railway motor or something else.

Mr. Leonard :—It is not that. But if you will think a moment you will realize that it has been very rarely that a generator has been used in the past, where its field would likely be weakened to a very great extent, and where its field was separately excited. Separate excitation of the field may not be an essential factor for non-sparking, but it is influential in regard to having the armature entirely free from any disturbing field kicks, etc.

Mr. Curtis :—Commutator sparking is due to the presence of magnetism. In those machines there I should think that in all probability the field was so very strong, compared with the turns on the armature, that you would not get any field distortion unless your field was practically nothing.

Mr. Leonard :—It was on these identical machines that I spoke of a measurement of 35 degrees distortion. The motor armature has a full field, and its lines of forces are necessarily kept tolerably straight. But the generator has, at times, no field practically, and the lines are distorted tremendously.

Mr. Curtis :—How can you move the coils of the armature? How can you move it in the presence of lines of force without getting sparking?

Mr. Leonard :—Well, we are moving it through such a field that the lines are so very few that the electromotive force developed by the coil which is passing under the brush is so slight that the passing of those bars, perhaps, only produces a breaking of an electromotive force which is no greater than it would be at the neutral point with ten times the total electromotive force then in use.

Mr. Curtis:—Well, I cannot agree with you, Mr. Leonard.

Mr. Leonard:—It may be that I have not the right theory about it.

Mr. Curtis:—It may be immaterial. That is to say, in order to make this an efficient mode of operation it would not be necessary to reduce the electromotive force more than 80 per cent., perhaps. Then you would be working with considerable efficiency and the field might stand that very well with 20 per cent. of its normal strength. Although I never saw a generator yet that had many turns on its armature, with any such capacity as that, without burning up the brushes.

Mr. Leonard:—Well, you can in this case. I know that in practice this has been used in connection with almost every dynamo that is on the market.

Mr. Curtis:—What sort of machines have you used it on?

Mr. Leonard:—On the Crocker-Wheeler, the Eddy, the "C. & C," the Edison, the Thomson-Houston, the Waddell-Entz, the Eickemeyer, the Bilberg, and several others. But in each of these it has performed exactly as it has in every other case.

Mr. Curtis:—Did you ever try those machines as motors and see if they spark as motors with weak fields?

Mr. Leonard:—They will unquestionably spark as motors.

Mr. Curtis:—Did you ever try them?

Mr. Leonard:—Yes. I tried these very machines with weak fields.

Mr. Curtis:—But those machines have an exceedingly powerful field, and weak armature.

Mr. Leonard:—They have more turns on the armature than usual.

Mr. Curtis:—Compared with turns on the fields?

Mr. Leonard:—Turns on the armature. That is, the magnetizing effect of the armature current is rather more in these than in many other machines on the market.

Mr. Curtis:—This is all I wanted to bring out—that when you get to machines of any size, I think you will find that the magnetizing effects of the armature turns are so much in excess of what they are on the smaller machines that you will get sparking.

M. Leonard:—What size do you mean?

Mr. Curtis:—Take a multipolar machine of fifty, or take a railway motor.

Mr. Leonard:—Take the Waddell-Entz machine?

Mr. Curtis:—Have you tried it on a multipolar?

Mr. Leonard:—Yes.

Mr. Curtis:—How much have you weakened the field?

MR. LEONARD:—The same as here. From zero up to the full amount.

MR. CURTIS:—How much current did you put through the armature?

MR. LEONARD:—Enough to lift 50 tons on a traveling crane. It is a 40 H. P. motor.

MR. CURTIS:—How much above its normal current on the armature?

MR. LEONARD:—About double.

MR. CURTIS:—And it did not spark?

MR. LEONARD.—Not a particle.

MR. C. O. MAILLOUX:—I might mention a bit of experience which is *a propos* of this very subject and may tend to throw some light on it. I have found that a generator will be able to carry a much heavier current when worked at lower potential than it normally would if it were working at its normal electromotive force. The particular machine was a railroad generator having lower potential than usual, about 300 volts, and capable of carrying some 800 amperes. The armature insulation was found very low when tested, apparently because the armature was not sufficiently baked when finished and the shellac was not quite dry. There being no better way of drying or baking it, we determined to heat it electrically, by making it generate a very strong current. The method, by the way, proved quite successful, and the armature in due time became quite dry and the insulation increased until it became perfect. In order to avoid undue potentials that might cause a leakage through this low insulation, the machine was run at as low a potential as we could possibly run it, in order to get the necessary current. In other words, what we endeavored to do was to run the very heavy current through the armature at the lowest possible potential at which we could generate it, so as to have plenty of heating effect and very little potential that would cause any leakage through the armature or to the core. For this purpose we proceeded in two ways, first by reducing the speed of the engine which ran the dynamo to the lowest practicable speed. But as it was a Corliss engine, or an engine of the slow speed type, which does not admit of being reduced in speed very greatly without danger of "stalling" at the dead centre, we had to make the rest of the reduction by reducing the field of the dynamo. It was a compound machine, and we reduced the current through the field until we had only about one third the number of ampere turns and possibly less. We could not measure it exactly, because the amount of current was rather small and the ampere meters we had at our command would not give us very great precision. At any rate, the potential was reduced to something like 50 volts from 300. Under these conditions we found that we carried not only the full current that the machine would normally carry at its full potential, but we could carry a good deal more, 15 or 20 per cent. more, without any sparking at all, possibly less than with full load at normal E. M. F. I think that the explanation given by Mr. Leonard is fairly consistent with the facts. The amount of energy that is concerned in the reversal is small, because the voltage is very small, so that the energy involved is a very small quantity. Under those conditions the neutral point seems to be much wider. It is quite evident that with the full potential there would be a much greater difference of potential per segment of the commutator. At full E. M. F. the difference of potential between the first two segments on either side of neutral line, multiplied by the current carried, would have been a greater amount, and naturally would have caused a greater disturbance at the commutator. I simply mention this fact to verify the observation of Mr. Leonard that it is possible to run a machine with a much higher current than the same machine can be run at a higher potential.

MR. NELSON W. PERRY:—I would like to refer to the seventh condition mentioned by Mr. Leonard: "The retardation of the "load in coming down grades, and in stopping, shall be effected "by converting the motors into generators, which shall feed back "current to the line, and thereby assist the power station in operating other locomotives." Mr. Sprague tried some experiments, I think, on the Third Avenue Railroad some years ago, to determine what portion of the energy consumed was usefully employed, or employed in operating, and how it was employed. My recollection is, that he found that about 83 per cent. of all the energy consumed was utilized in overcoming gravity and the inertia of trains, leaving about 17 per cent. for traction. It is evident, if we can accomplish this and throw the energy absorbed onto the line, it will accomplish a very great economy, because on a hilly road there is as much down hill as there is up hill, and a car stops as often as it starts; so that theoretically we have got a road reduced to a level and the trains of constant speed, not varying. Now the question is, whether Mr. Leonard has accomplished this in his plan. Observe, that on each car, what may be called the prime mover is a synchronous motor. A synchronous motor does very little useful work unless it is in step with the generator. Suppose we are operating that synchronous motor as an alternating current generator. Its speed will vary with the speed of the car. If we are checking our car it goes from maximum down to zero. Now, the second car that is on the road, in order to do any useful work at all, would have to keep step with the generator. But we see that it is changing in the number of its alternations per second constantly, so that it seems to me that it would give no benefit at all, probably, to the second car, because that synchronous motor would have to keep in step with the other, because it is constantly going down to zero. Then the question comes up, *can* he throw any energy onto the line in that way? He has got to have an electromotive force which is equal to that of the line. Otherwise he can throw no current onto the line at all. Everybody knows that in coupling up generators in multiple arc, in our generating stations, we have got to take great precautions connecting across from the armature of one to the field of the other, so that one will not drive the other as a motor by a slight increase of electromotive force. There is only one condition in which he can throw energy onto the line by reversing his synchronous motor, and that will be when the armature is revolving at such a speed by producing 500 volts, if that is the voltage of his trolley wire.

Mr. Sprague has also said that by reversing the motor and converting it into a dynamo he could throw energy onto the line, but I do not see how he can, unless that motor generates an electromotive force of 500 volts. In series arrangements, if you reverse your motor and make a generator of it, it will contribute energy to your line up to the last turn of the armature. But on multiple arc I do not see how it is possible to throw it on except in a very special case.

Mr. Leonard:—I think that Mr. Perry has not understood the function of the synchronous motor. He speaks of it as varying in speed from zero to full speed. A synchronous motor is necessarily running at a synchronous speed constantly in one direction, and there will be a theoretical change in speed between its performance as a generator and a motor, but it will be very slight indeed. When the locomotive is being brought to rest it will be exactly equivalent to having two alternating current generators operating in multiple with each other, each producing current as a generator. But it will perform just as when a motor, and continue in synchronism and at the same speed as the generator. There is a theoretical slight falling off. There is not exactly a falling off in speed, even then. The armature moves a little in its relative position in the field. But the number of waves per second is the same as before and its speed will be identically that of the generator. As regards putting energy back into the line as against the voltage of the line, I agree with Mr. Perry, that it is impossible for any single motor on the multiple arc system to put energy back against, we will say, 500 volts, down to a condition of rest, because your electromotive force will necessarily fall below 500 volts before you come to rest. and at such a time as that you can put no energy back to the line. Therefore it is necessary to have something in the shape of a transformer, so that as your energy which was originally of 500 volts falls down from 500 volts to zero, it will by means of some intermediate device have that energy continuously transformed up to 500 volts and a little higher in order to send current back into the line, which is what we have in this arrangement. If you take this final motor and drive it so as to produce 20 volts, you can make the first machine there produce the full electromotive force that it receives from the line, and more. It would produce much more except for the retardation of its armature by the work that it is doing in feeding current back. We take 500 volts from the line to the first motor and through the transforming arrangement we produce 20 on the final motor. We can reverse that. We can produce 20 by the last one as a generator and have 500 feeding into the line from the first one operating as a generator.

MR. PERRY:—Mr. Leonard misunderstands what I said in regard to the synchronous motor. What I meant was that you are driving through a connecting link—through the momentum of the car acting on the car wheel. Now, as the car slackens up the armature of your synchronous motor, now converted into a generator, will slacken up.

MR. LEONARD:—No. You have not understood the arrangement, Mr. Perry. The final motor in the diagram, L, connected with the wheel is the only one whose armature has any relation to the speed of the car. It is connected with the wheel of the car and it will slow down and speed up as the locomotive does. The other two will run continuously at a constant rate of speed, regardless of whether the locomotive is at rest or in motion.

MR. PERRY:—The middle machine is excited then by the current generated by the street car motor, is it not?

MR. LEONARD:—No. It is excited as shown in the diagram there. There are various means of accomplishing that.

MR. PERRY:—As the armature of the car motor slackens down, the electromotive force of your generator, of your motor, will slacken down and its speed will be reduced, and it is connected with the armature of your synchronous motor, is it not?

MR. LEONARD:—Since they are both motors and generators at different times, which machine do you refer to?

MR. PERRY:—As the armature L slackens down, the armature of K will slacken down.

MR. LEONARD:—No, the armature of K continues at a constant rate of speed under all conditions.

MR. PERRY:—Not if it is run by N.

MR. LEONARD:—It is only run by N when N furnishes electromotive force sufficient to operate K at its full speed and tends to drive it faster.

MR. PERRY:—But under all conditions constant?

MR. LEONARD:—That is the only condition under which it will come to rest.

MR. PERRY:—Now as to the synchronism, the motor which is supposed to be helped along will have to keep step with that. You admit it will not feed back unless it generates an electromotive force of 500 volts?

MR. LEONARD:—Yes, but I did not assume that the speed of that machine K is going to change at all, practically speaking.

MR. PERRY:—It will not be when you are running that as a generator, because the electromotive force of L will vary with its speed.

Mr. Leonard:—There is no mechanical connection between L and K. When I have a low electromotive force produced by L as a generator, and supply to the armature of K as a motor, the field of K is very weak—so weak that the low electromotive force of that armature does run it at its full speed still.

Mr. Perry:—I see.

Mr. Curtis:—In order to have the electromotive force of the motor L—in order to have L a generator, when that car begins to slow down, it is necessary that the field capacity of L should be capable of being enormously increased, isn't it?

Mr. Leonard:—No, sir. The reverse is equally true, that you can make it a generator by weakening the field of K, which is what is done.

Mr. Curtis:—Suppose it is a generator, suppose you are going down hill and you want to have L generate current and feed it to the line. When you are running at full speed it will do that all right of course. But as you merely reduce your speed and net down to say about 25 per cent. of your original speed, in order to make that feed into the line, it is necessary to increase the speed.

Mr. Leonard:—No, sir. When you come to slow down you do it by weakening the field of K.

Mr. Curtis:—You do not have to feed against 500 volts. You feed against a correspondingly reduced voltage.

Mr. Leonard:—Yes, considering the flow of current from L to K. But as regards the flow of current from H to the line, that is continually 500 volts.

Mr. Curtis:—But you weaken your direct electromotive force from K just as fast as L does?

Mr. Leonard:—Yes. Perhaps after this discussion the operation of this apparatus can be better understood. When that motor was running at full speed, and the field rheostat horizontal, to bring it to rest I broke the field of K rapidly but gradually through a large number of steps. We will say that K was producing 250 volts and the counter electromotive force of L was perhaps 240. The moment I began to weaken the field of K, the momentum of L kept its counter electromotive force up temporarily and the counter electromotive force rapidly became larger than the impressed electromotive force of K. Consequently the current went back into K which was retarded in its efforts to go faster by the fact that it is driving H which becomes a generator to feed current against the line.

Mr. Curtis:—In one case K is feeding a surplus of electromotive force to L with a varying amperage and rising electromotive force and diminishing amperage, and when you are slowing exactly the reverse takes place, that is L feeds K.

Mr. Leonard:—That is right.

Mr. Curtis:—What net result of efficiency have you secured as compared with a rheostat control? Take a case, for instance, where you are operating a traveling crane under ordinary circumstances, constantly starting up and reversing, and so on.

MR. LEONARD:—Well, the efficiency of a series motor that is used on a traveling crane by the rheostat methods is practically proportionate to its speed. The field is saturated by the minimum amount of current that any load requires. There is but a slight change of field strength from the current required to handle the lightest load and the current required to handle the largest load. Consequently the field is continually saturated, and if it is saturated, the counter electromotive force, which is directly proportioned to speed, is the measure of efficiency. Consequently, theoretically my efficiency would be identical with theirs under the condition of full speed and would be ten times as great at one-tenth speed, twice as great as theirs at half speed, etc., and the tests made by William Sellers & Co., of Philadelphia, who are using the method, show that that is borne out exactly in practice. Every loss that was anticipated was exactly where it ought to be, and there was no loss and no result which was contrary to what was expected.

MR. CURTIS:—What I meant to ask is what is the net result on a thing like a traveling crane of using these three pieces of mechanism.

MR. LEONARD:—Well, the amount of energy saved, which would otherwise be wasted in the rheostat, may not be a very important factor. Of course it is all saved, whatever it is. There is no waste in this.

MR. CURTIS:—Have you made any test to show that?

MR. LEONARD:—Yes, but I cannot tell you what the efficiency is except by involving the speed. The efficiency in this system —barring the fixed losses of $C^2 R$ in the armature and the excitation of the field and the friction, its efficiency is constant regardless of speed.

MR. CURTIS:—You have no figures then of the net advantage of that mode of controlling?

MR. LEONARD:—No. I have not any at my tongue's end. The test made by William Sellers & Co. was this: They placed on the floor of their shop a jack-shaft running at about 360 revolutions a minute, upon which they placed a brake and also a large fly wheel of a punching machine which had about four feet diameter and about five inches square of metal in the rim. The jack-shaft was driven by a ten horse power 220-volt Sprague motor, whose current normally would be about 40 amperes maximum. This fly wheel was then driven at full speed in either direction and reversed at will. The biggest duty that it had, probably, was in accelerating and retarding that fly wheel. When it was at full speed in one direction, and while the brake was off, and therefore the effect of the momentum would be a maximum, the field controller of the generator was instantly reversed. Then the endeavor of the motor was to reverse, but the fly wheel insisted on its going forward temporarily as a generator. Finally the generating action was sufficient to cause the motor to be brought to rest, and then it started up instantly to accelerate the fly wheel to full speed in the other direction, and there was an entire absence of sparking. At that time of reversal of the motor armature, and at the time when the feed was first reversed, the armature current was 102 amperes while the rated full load current for the motor was 40 amperes.

The feature of the method, which in the case of traveling cranes makes it a point of advantage, is not the amount of coal that is saved or the effect of fluctuating load on the engine so much as it is nicety of control by this mechanism. No brake is used in handling a load. The retardation is effected electrically. That same thing is applied to electrical elevators. When I first introduced it both William Sellers & Co. and Otis & Co., who are using it, said that they were not at all inclined to use this method for braking, because while theoretically it might be all right they had not had confidence enough in it to retard fifty tons with it while running above the workmen in a shop, and in the other case to stop an elevator full of people and moving 250 feet a minute. But in normal practice they are using it without any mechanical brake at all, because the braking action in this device is far superior to what can be gotten by a mechanical brake. A mechanical brake should be applied with the maximum torque at the beginning. We are all familiar with the way a street car operator will put his brake on—very hard at the beginning and release it a bit as the car slows down. The braking torque should be a minimum as it comes towards rest. With the ordinary brake shoe it is a minimum at high speed. In this instance, the amount of current for the reversal of this fly wheel, the retarding torque is 102 amperes in a constant field in the first instance, and as it came to rest it was gradually reduced to almost nothing. So that that the effect of it with a moving load is extremely smooth, and far more smooth than can be accomplished by retardation by a friction brake.

Mr. Curtis:—How much fluctuation do you find that that makes in the primary current?

Mr. Leonard:—Very little. The primary current in any case I have seen is a maximum, with minimum load at full speed.

Mr. Curtis:—But when you reverse, dont you get an increase in the maximum?

Mr. Leonard:—No, we get a very large current in the secondary at that time but a correspondingly low voltage. I reversed it repeatedly there, and the minimum speed represents the minimum current from the supply line, under reversal or otherwise.

Mr. Curtis:—It can put an additional load on the generator, can it not—the primary generator of your system, the generator connected with your alternating motor?

MR. LEONARD:—It puts a very large current on that, but that large current is produced by very few volts.

MR. CURTIS:—I understand. But doesn't it rise above the normal?

MR. LEONARD:—Do you mean the full load?

MR. CURTIS:—When you reversed this fly wheel, for instance, what were you feeding with—an alternating current?

MR. LEONARD:—No, a continuous current.

MR. CURTIS:—And it did not affect the generator there at all? It did not affect the motor that you fed from your primary source of current?

MR. LEONARD:—I do not quite follow you there. But perhaps I can answer your question in this way. Supposing we were running at full speed under a brake load, for instance. We might have 40 amperes taken from the line If we reverse the motor that supply current would come down gradually to zero and then go up again to 40. When we are reversing and have no volts there are no amperes on that circuit.

MR. GEORGE HILL:—The paper starts with the statement: "Let us suppose that we are called upon to act as engineers for "a steam railway desiring to operate its line by electric locomo- "tives." This, it seems to me, removes the paper altogether from the realm of theory, and brings the method of operation down to the question of dollars and cents. We already have in our capital account the cost of machines that will carry a passenger from A to B with a certain expenditure of coal. The change then must be accomplished in such a way that the interest on the capital account shall be less than the cost of the fuel consumed by the steam locomotive, plus a certain amount for interest, repairs, depreciation and things of that sort. This is a question that the paper does not in any way touch on. It seems as though it were a matter of course that the capital account for such a complicated installation as is here given must give an interest account very largely in excess of any possible saving of fuel. So far as I can learn, the question of restoring energy to the line is one which works out very beautifully, theoretically; but as a practical result it is represented by zero, and in the present case is of no importance since the power costs nothing. The efficiency called for under heading 9, 50 per cent. seems to me to be very much in excess of anything that is practically possible. If we take the successive steps given on the diagram, Fig. 1, page 77, and assign an efficiency of 95 per cent. to generator A, and 95 per cent. to the transformer B, 80 per cent. to the line, which is Mr. Leonard's own figure, and then 95 per cent. to transformer D, 90 per cent. to the line F, 90 per cent. to the synchronous motor H, 90 per cent. to K, 80 per cent. to L, and then 80 per cent. for transmission, we get a resultant efficiency of 32.2 per cent., which is somewhat below the 50 per cent. limiting condition given by Mr. Leonard. If instead of that we take what is commercially probable, and run our efficiencies back 90, 80, and so on, we get 16 per cent., which is probably correct for the best possible commercial conditions. So that if we were acting as engineers for a line, the first thing we could do would be to figure up our capital account and see whether the entire cost of fuel when capitalized would afford a sufficient amount to install any apparatus, and then we would be in a position to take up the question of efficiency, when we would find that we would have to have an efficiency higher than any apparatus operating at the present day, to get as high as 40 per cent.; while if we took usual conditions as a guide we would probably run down somewhere around 10 which is only a little bit better than the absolute efficiency of the steam locomotive. Looking at the question from the very broad point of view of whether or not, in order to improve the character of the service, increase the speed, or for any other desirable change, we should advise a change, we would at once be confronted with a host of other difficulties. No one can be more desirous of seeing electric appliances brought into general use than myself, nor would I desire to take the position of captious criticism of anything proposed, but I do think that we should bear in mind the intensely practical nature of the age in making suggestions, and develop them along the lines in which we can reasonably expect success. Mr. Leonard's method of motor control under the conditions of operating printing machinery of various kinds, electric elevators under special conditions, cranes and other similar uses, is, no doubt, much the best that we know of at the present time. Whether it is the best for railway operation remains to be seen. The plan proposed, certainly contains many unknown quantities, and they should be solved before an attempt is made to apply the system. For example, what would happen to the synchronous motor if the trolley should accidently leave the line? How would we insulate the 20,000 volts? Why not put the synchronous motor and continuous current generator in power houses, say 15 or 20 miles apart, and send 500-volt continuous current through the line instead of the alternating current, thus putting our complicated machinery where it could be attended to, having on the car nothing but a motor. If our power costs nothing, the further question would come up of the desirability of using Mr. Leonard's device since it is strongest from the fact that it is economical in its consumption of power, and power, in the assumed case, is valueless.

MR. CHARLES HEWITT:—There is one point—I do not know whether Mr. Leonard has brought it out in this paper or not, but he has, in describing this method, intimated that by using it and avoiding certain fluctuations which are characteristic of street cars, that he can use smaller units. Now the size of the unit is limited rather by the grades on a road than it is by the starting effort, and the same would apply to a train that is true of a street car. Any machine that will overcome grades at satisfactory speeds on ordinary roads ought to be sufficient and will be sufficient to start the car. Therefore, as far as the units on the train itself are concerned, we cannot save anything in their capacity. With the old type of apparatus on street cars something might be saved perhaps in generators, but I do not see that it can in motors. On a steam railroad of 100 miles in length I do not believe we can save anything in generators. The two problems of the steam railroads and the street car roads are entirely different. The conditions are entirely different. In the first place I think we are all satisfied that we cannot compete with the steam locomotive, except for very high speed service. Until we are ready to put a service of 125 miles, say, or something above what we can do regularly with steam, I do not think any of us will attempt to compete, so far as original cost of installation or cost of operation is concerned. But the hope for electric traction on long distance roads is in high speeds which cannot be obtained with a reciprocating engine, and on such roads as that stops are very infrequent. I believe Mr. Crosby has shown in one of his articles that we cannot afford to stop a train running at that speed in distances less than say 100 miles. Well, the starts and stops on such a road as that would be a matter of insignificance; so that in considering such an apparatus as this, we must compare it only with other electric systems which we can use, such as the direct application, as we use it in the street car, or something developed from that—some simple arrangement. We would have to leave out of account the question of economy and compare this with the application of simple apparatus, and in that case it becomes a question as to whether the advantage gained in starting and stopping once in 100 miles would compensate for the extra cost of such an arrangement as this.

MR. LEONARD:—The gentleman who first spoke, figured out the efficiency. He called attention first to the opening paragraph, but he forgot to read the next line following: "There exists a very economical source of power, possibly a water power." There is no intention, although I do not grant that it is not possible—there is no intention in this paper to try to prove that the production of power by steam and its utilization in this way is going to be more economical than that of a steam locomotive. That is not the point. The point is that there may be instances where a very economical power, such as a water power, is running to waste and the cost of using it practically nothing after being once developed, and we are therefore putting that, and not coal, in competition with the present cost of steam, and furthermore the requirement of high speed service is probably, as Mr. Hewitt has pointed out, going to be the means of bringing electric locomotives forward if anything does. No one is going to change his equipment of steam locomotives for electric locomotives merely in a hope of economy in their operation under usual conditions. It must be to accomplish something that cannot be done by a steam locomotive, or to use some kind of power which is so much more economical than coal that the net result is better.

As regards the question of efficiency, I went through the same steps in arriving at the figure 50 per cent. specified, as Mr. Hill did. But my figures were based upon commercial apparatus such as makers of apparatus of first-class type would be willing to guarantee, and I did not assume for large transformers of say 500 horse-power an efficiency of 95 per cent., nor an efficiency for large generators of 90 per cent. The fact is, that you can secure transformers of a size such as this, which will have a full load efficiency of 97 or 98 per cent., and generators which will have an efficiency of 95 per cent, and there is no reason for supposing that the motor L will have an efficiency of 80 per cent. The efficiencies as quoted by him were unnecessarily low. Taking the figures such as would be guaranteed by makers, and remembering that the motor generator has much less loss by virtue of friction than two machines ordinarily belted, etc., the result which I obtained was an efficiency of 52 per cent. for the combined apparatus, even with 20 per cent. in the transmission line, but admitting, for the sake of argument, that it is 40 per cent., which may be so. still we were trying to operate a long railway 100 miles long. Now the question is, how are we to do it? And this paper is a suggestion in that line. Mr. Hewitt says we should use the simple method of to-day. By the "simple method of to-day," I suppose he means 500 volts.

Mr. Hewitt:—Not necessarily 500 volts.

Mr. Leonard:—Well *I* do not know how by any "simple method of to-day" we are going to operate a railway 100 miles long.

Mr. Hewitt:—I simply meant a motor built on the axle. In fact I have no definite plan in view, but I mean the development of some such plan as a simple device.

Mr. Hill:—I should just like to call attention to the fact that in my statement for comparison I did assume that the fuel would cost nothing. I said that an engineer must necessarily compare the interest of the increased capitalization of the plant with the saving in fuel, which I think effectually meets Mr. Leonard's point. Concerning the efficiency which he gets, that is purely a secondary matter. I find it, in practice, exceedingly difficult to get makers to guarantee machines for efficiencies ten per cent. less than I assigned here.

Mr. Kennelly:—I would like to ask why, in the opinion of the last speaker, the practice of engineering for railroads should depart so widely from what has been the recognized practice in ocean locomotion. Consider, for example, the journey from Liverpool to New York—a saving of twelve hours in transit would certainly not justify, on any basis of economy, the enormous increase in the volume of coal burned on the more rapid journey.

There could be no economical argument for any steamship construction to reduce by a few hours the transatlantic passage, when it is recognized that the amount of coal burned per hour, and the engine capacity required, increase approximately as the cube of the ship's velocity. Nevertheless we find that experience, which is the safest guide in these matters, is continually urging on steamship builders, higher rates of speed and further coal consumption. Surely, if the electric locomotive will enable us to attain a higher speed than existing methods produce, the considerations which have justified the builders of ocean steamships in seeking for higher speeds at the expense of increased coal consumption should be equally influential in favor of the adoption of the electric locomotive for land transportation.

MR. MAILLOUX:—There is one point that seems to have escaped attention in this discussion. Assuming that we are able to convert the power back from the car axle to the line and that we finally get it to the line, what will it do there? Those who have investigated the problem of regulating water-wheels for railway purposes would certainly not have much use for it. Nor do I think it would be of much more service where steam engines were used. If I am not mistaken, it has been found by actual experiment that the effect of restoring energy to the generating source in railway work is to interfere greatly with the regulation of the generator, if not to make it entirely erratic, because, as we know, the curve of supply of current on railway service is itself extremely erratic. There are periods when the consumption is very great, followed rapidly by periods when it is extremely small, or even zero What may be the effect of current being restored to line when there is no consumption on the circuit? We can readily see, since the energy must go somewhere, it will go back to the station, and will tend to make the engine "race," just as a corresponding increase of load would tend to slacken its speed. In a water-wheel we can do nothing but shut off the supply of water. Our devices at present are quite inadequate to prevent the water-wheel from racing, even when we take the load off quickly, to say nothing of what it must do when we put a negative load on; in other words, when we apply a force which tends to make it revolve without the application of any water power. We readily see that in the present state of the art, the question of restoring energy to a railway circuit, where the rate of consumption is at all unequal, is purely visionary and impracticable, because there is no means of utilizing that energy in such a manner as to render it serviceable without introducing very serious causes of disturbance. It will only be when we have means of balancing the circuit by some means of storing energy in the circuit, or some portion thereof, that we shall be able to properly and satisfactorily utilize the energy restored.

With regard to efficiency, it may not be out of place to mention the fact that what we have to deal with in railway practice is not the efficiency of full load by any means; that it is on the contrary the efficiency of a load which, when averaged for the whole day is a very small portion of the full load.

Mr. Hewitt:—Pardon the interruption. That applies on street cars and not on a long distance road. It would not apply even on a suburban road.

Mr. Mailloux:—Mr. Hewitt is correct on that point; but the remark still applies to the line transformer L which would not work at constant load, unless the number of trains be always equal to the number of such transformers, and unless the trains are always under the proper headway to keep each section of the line and each transformer constantly loaded.

Mr. Wm. Elmer, Jr.:—I would like to ask a question. The author of the paper says, when the field is reduced to zero, the armature of the motor, being driven by the momentum of the car will send the current through the armature of what was before the direct current generator, and that will drive the alternator. I do not see how it can act as a generator when there is no current in the field.

Another point I thought of was, that supposing an engineer should be in danger of collision: he would suddenly throw his switch from one side to another, the generator would be thrown out of synchronism and the whole thing would be left dead; and how can the generator be started when the engine is to be taken out of the round house in the morning?

Mr. Leonard:—In regard to the question of the performance of the intermediate machine as a motor, if you are running under condition of full speed and you gradually weaken the field of the generator to zero, that field does not instantaneously reach a condition of zero, but goes down gradually, and the more it tends to go down the more current tends to go into it as a motor and tends to drive it fast, and the greater the braking action becomes upon the armature on the locomotive axle. because it is producing a very large amount of current in that constant field; and, of course, that acts as a brake to stop it. I do not quite understand what the last speaker meant about throwing the alternator out of step. If the machine were running at full speed—the train—and the field of the generator were reversed so that the propelling motor of the train had a tendency to instantly reverse, the only result would be that the alternator, instead of running as a motor, would be driven as a generator to feed current to the line, but there would be no rapid change of load, nor would there be any excessive load on the alternator so as to tend to throw it out of step. The intermediate machine would tend to speed up, but it would not speed up because it would be continually feeding current into the line which would be acting as a brake for itself.

There are so many ways of bringing up a synchronous motor to speed without load that I did not indicate any particular method. One simple method would be, eliminating altogether the performance of the machine as an alternating current motor at slow speeds, to have a motor of perhaps two or three horse power connected to the alternating current machine, and a few cells of storage battery, which would run that little motor. This is eliminating all alternating currents and showing one means which it is self evident would work. A few cells of storage battery would run a small motor, which would run the alternating motor up to synchronous speed. After it is in step it will run that same small motor as a generator to keep the storage battery charged and to excite the field of the synchronous motor. Another method would be to use such a device as is used on the alternating current motor of the type of the Dahl Company. A third way would be to have the field wound for perhaps 50 volts and the armature wound for 500, and first connect it in series with the armature, in which case it would start up readily, and a switch could be arranged for connecting the commutator of a continuous current winding in the same field, to supply it when at full speed. There are half-a-dozen well known ways which are possible for starting an alternating current motor which is only to start under friction load, and which, when running at full speed, excites its own field.

Mr. C. J. Field:—I came here to-night seeking information. I think we have come to a time in electric railroad work where we have got to take these problems up, not only on long distance transmission but in city work. We are having enormous investments of capital in cables and other means, and we have got to take up some method of higher tension, or some method similar to Mr. Leonard's of alternating current to do our work. What we ought to do is to try to get more information on the subject and advance more. If Mr. Sprague, seven years ago, had objected to everything, as the majority have done to-night, I do not think we would be in the position with electric roads that we are in to-day. There are a number of points in Mr. Leonard's paper that are good, but there are quite a number of objections to be cleared away. One is the question of insulating a single trolley circuit of 500 volts with the alternating current. Those of us who have had experience trying to insulate a direct current with 500 volts would want to go to some extraordinary means to take care of a grounded circuit with 500 alternating volts. The proposed apparatus would go very well on a large electric locomotive; where would it be put on a double truck 36-foot car? That would virtually be having a small central station in the car. I do not want to be taken as trying to criticize, but I want information. How much room is the apparatus going to take up to operate an ordinary 34 or 36-foot car, and where are you going to put it? Furthermore, have we got sufficient advantages over every other means which are now being proposed, and with which some of the large companies are experimenting and claim they are about ready to put in—that multiphase motor? Of course, the objections to that the carrying too high tension and reducing down with some form of rotary or other transformer is a question of several trolleys—other means, the rotary transformer and the direct current, which Mr. Leonard has objected to, but which, on a road of 100 miles, if we put five or six of them along the line, distributing six or seven miles each way, seems to have many advantages if we are operating from a water power plant.

Those are all questions that I would like to hear discussed more, for the reason, among others, that it happens at the present time that I am worrying over a problem of this kind—a road 100 miles long which we have got to operate with electricity; to get up about 7,000 feet elevation at a good deal less *cost* than a steam road can be built for, because you can take higher and steeper grades than a steam road could take, and thereby largely reduce the cost. We can operate our cars with less expense than having a three or four dollar a day fireman, or engineer and fireman combined.

These are a few of the points I would like to present for your consideration.

THE PRESIDENT:—There is a gentleman here we would like to hear from—Mr. Woodbury, of Boston. I believe he is not a member, but I am sure the INSTITUTE would like to hear from him on this very interesting question.

MR. C. J. H. WOODBURY:—Although I am here as a guest, I willingly avail myself of this opportunity of expressing my appreciation of the paper of the evening on Mr. Leonard's method of connecting motors with generators; and in the consideration of this question I beg to depart somewhat from former speakers, who have confined themselves entirely to a consideration of principles "Because," as Jack Bunsby wisely concludes, "the bearings of this observation lays in the application on it."

I wish to call your attention to the results of the practical application of this method of coupling up electric motors in an instance where it served as a solution of the problem after other methods had failed, and that is in the application of electric motors to the operation of calico printing machines. Calico printing is done by means of a number of engraved cylinders, from four to eight inches in diameter and wider than the cloth, which press upon a large drum that carries the cloth under these rollers. This drum is covered by a belt of cloth about one-fourth of an inch in thickness, which is called the blanket, and the fact that these rollers are forced against the drum with great pressure renders this type of machine a very difficult one to start; the conditions being analogous to the starting of a heavily loaded wagon on a soft road.

The conditions of operation require that the printing machine must be able to run uniformly at any desired speed within its limits of operation, that it must be able to move the cloth a few inches, and that it must start and stop readily. The method of using a two-cylinder engine in the print room has been hitherto the best approximation to the desired results. The obstruction of desirable floor space, the heat and dirt from the machine, all being especially objectionable in this place, were regarded as necessary evils.

The reconstruction of a print works on the site of one that was burned gave an opportunity for an attempt at the application of an electric motor to driving a machine. After other methods failed, that devised by Mr. Leonard was successful. The motor is placed on the mezzanine floor of the print room, on what is known as the platform, and is controlled by a hand-wheel and switch at the front of the machine. It has been in operation over two years with the most satisfactory results. The cloth can be moved as little as one inch if necessary, and the speed can be increased to any desired result by uniform gradation without any shock.

The ability to stop and to start quickly, as well as to run the machine in such a uniform manner also allowed for its operation at a greater speed, and I have been informed that the production of this machine exceeded by one-third that of the other machines in the room. When it is considered that the value of a print works plant is at least fifty thousand dollars per machine, this increase in productive capacity is of great importance. It is said that when the question of introducing a motor into these print works was first considered, it met with opposition on the part of the help, and that afterwards when the practical results were reached, those employed at other machines wished that these also should be driven by motors In fact, as a result of the experiment, one of the firm has made the statement that if he were to build a new print works he should use electricity entirely for the distribution of the power, notwithstanding that the value of exhaust steam is greater in print works than in almost any industry.

MR. JOSEPH SACHS:—One of the points made in the paper is, that there should be no moving apparatus between the station and the car. Is not the objection to placing the apparatus, that is, the additional moving apparatus, upon the car, quite as great as that to placing the rotary transformer that we would use in a system of the kind, along the road? A rotary transformer does not take very much attention. and we are not limited to placing these transformers at a distance of exactly two miles apart. The location of the transformer along the line, transforming the alternating current, from a high tension alternating to a 500-volt direct current, which would be fed direct to the trolley wire, would be, perhaps, more efficient than the method illustrated, in which we have at least one more transformation to be met with than in the method spoken of. The location of a 500-horse power or more, synchronous alternating motor, and a constant current generator of equal power upon a car, with the necessary devices for starting a synchronous motor, would certainly take up a great deal of room and require considerable attention, and I believe that although the system proposed by Mr. Leonard of regulating is certainly a very practical and feasible and economical one, we are to-day obtaining very economical results with the series parallel arrangement, and we can certainly have some other form of keeping a constant field of the motor and varying the field and torque by the electromotive force and current passed through the armature. It is true that the location of transformers along the line would slightly increase the first cost of plant, but would not the additional simplicity of such an arrangement, and perhaps the somewhat higher efficiency of placing the apparatus in that position, make it advisable to leave off everything excepting the actual driving motors, from the locomotive? Certainly where we have a very long road of say one hundred miles and very few trains moving thereon, the devices proposed by Mr. Leonard would certainly be most advisable to be used. But where there are a comparatively large number of trains moving on a long road I should think the more acceptable plan would be to make the transformation from the alternating to the direct current outside of the moving locomotive. It must be remembered that a road of this kind would most probably be operated at a high speed allowing of very few stops in the distance specified. The regulating apparatus would, therefore, not be used very frequently. I think, furthermore, we are not absolutely limited to one contact but that two wires can very readily be used, and some form of motionless transformer with a multiphase current can be utilized without the various rotary transformers that would be necessary in a single phase alternating system.

THE PRESIDENT:—Mr. Leonard will close the discussion unless some other member wishes to speak.

MR. R. N. BAYLIS:—In view of Mr. Curtis's demand for specific figures on efficiency and in regard to what Mr. Hill and others said afterwards, it might be interesting to give a figure in the case of an actual test on that arrangement. About the time this method of regulation was brought out I had occasion to make a test of it and was very much surprised at first at the value of the efficiency obtained. The arrangement which I had, put the apparatus under very unfavorable circumstances. I had two ten horse power motors, one used as a motor and the other as a generator, and a five horse power hoist which had a double reduction gear. There was a prony brake used on the drum, so that it made the apparatus something like that here exhibited, and with all that gearing and with the full load on the drum, and the rheostat so arranged that the speed of the motor, and the speed of the five horse power motor—which was normally about 1,500, was about 190 revolutions, the efficiency obtained was about 70 per cent.;—it was 69 and a fraction per cent. This was the total efficiency from watts supplied first motor, to brake horse power output of hoist drum, and was certainly very high for such an arrangement as that, considering all the gearing. Regulating to the same amount with an ordinary rheostat on series would probably have given forty per cent. or less efficiency.

THE PRESIDENT:—If there are no others who wish to speak, I will call on Mr. Leonard to close the discussion.

MR. LEONARD:—One point that I made a memorandum in regard to, was Mr. Field's statement relating to the possible objection that might arise due to the difficulty of insulating a 500-volt alternating current. I had assumed that there would not be any particular difficulty in insulating that pressure. But it will be evident that there would be no difficulty—it will make no alteration in the arrangement, and it will be perfectly feasible to reduce the pressure to any voltage that will be desirable, and the limiting feature of that of course is, that if you have less volts you have more amperes to carry through your contact. Of course if you are going to deliver energy of 500 horse power, if you have low volts, you must have amperes correspondingly large. I supposed that 500 would probably be the best, because 500 volts, as I believed, was the maximum that it would be safe to have in a bare conductor overhead.

As regards the question of where we will dispose this apparatus, I have not made any practical tests of any kind with this apparatus upon railways. It is evident that until a considerable amount of money is available to be spent in tests of a comprehensive nature for railways, it is idle to take it up.

The last paper I read on this subject was relative to the application of it to street cars on existing lines, and the objection was raised to it which Mr. Field has raised, that there would be difficulty in disposing of the motor generator on any ordinary street car. In the case of a locomotive there is no such objection. There is plenty of room on a locomotive for the motor generator. You need the weight of the motor generator. If you do not have the motor generator, you will have to put an equivalent weight in the shape of cast iron on the locomotive to get the weight that is required for traction. I purposely in this paper dodged this question of where you will put the apparatus, by providing a place that is big enough to put it in.

As regards the question of whether it would be preferable to have the sub-station on the locomotive or the sub-station along the line, it seems to me there is every advantage in having the sub-station on your locomotive. In the only prominent experiment that has been made yet on electric locomotives, the whole central station is on the locomotive; that is, Heilmann's locomotive where he has his coal and water boiler, and engine, and dynamo and motor all on board. I notice that he is able to operate and run 75 miles an hour and pull 14 cars back of him, with every prospect of high speed work.

As regards the point of comparative merit of putting the transforming arrangements on the locomotive or along the line, it is evident, if they are along the line, they are going to require care. If they are in motion they require additional operators, which would be avoided if they are on the locomotive. With my system you have nothing between your source of power and your locomotive, about which your engineer has any question whether it is moving or whether it is not. He knows that when *his* apparatus is all right, that everything back to the central station is probably in good working condition. There are not a lot of rotating transformers and all that which must perform in order to be sure that he is able to go.

One of the most important points is this: The locomotive carries in itself sufficient energy to take care of its own train at any point on the road. But if you have a sub-station along the line the sub-station must be of such a size as will enable it to provide power sufficient for the operation of, perhaps, two or three times the usual maximum load because of trains that may perchance be upon that section at one time. Every one of those sub-stations must be big enough to take care of this unusual maximum load. Therefore, your sub-stations must have a very much larger amount of horse power installed, than the amount installed upon the locomotives when each locomotive is always taking care of its own load and no other load.

Another point is, where you have them all on the locomotive, you have the economy, not necessarily of stopping once in a hundred miles, as one speaker has said, but of taking advantage of down grades and effecting a saving of energy, which you would not have to any such extent in a sub-station.

A still further point is that the apparatus, it seems to me, is much simpler to handle when all on board of one vehicle, than when part of it is in one place and part of it in another.

As regards the restoration of energy to the line, I can only give the results of practice. It seems to me evident that there is every reason for having some kind of a device which will take advantage of the energy which is to-day wasted. The statistics which Mr. Sprague gave in the past, and which I have not heard challenged and presume are accurate, were that 59 per cent. of all of the energy in use upon the Third Avenue Elevated here was used in getting up speed—acceleration alone—and that 24 per cent. was used in overcoming the grade. Now, if 24 per cent. is the amount of energy which is required in overcoming the grade—the lifting effect, entirely independent of the acceleration and traction, certainly it will be worth considerable to save a large fraction of the 24 per cent., and any method which will restore that to the line will be beneficial, and on lines which have considerable grade, which is the kind of road that electric locomotives will be particularly adapted to, we will get a very much more marked saving due to this restoration of energy to the line than in such a road as the Elevated.

There are no injurious effects in practice upon the engines and prime movers by virtue of restoring energy to the line by this method. That has been very clearly demonstrated in the case of traveling crane service and in elevator service, and where a very large amount of energy is restored to the original circuit. Of course, if you take a hypothetical case of a motor going down a steep grade and only one motor in use, and a water-wheel, or an engine, or something of that kind, which is going to have no other work to do, it is possible to conceive that the energy restored to the generator driving it as a motor might cause some little trouble in the central station. But that is not a practical condition. The practical condition is, that you do have more than one, and that you will never reach a period where the energy restored is going to make your engine run beyond its full speed, and even if it does tend to, the engine soon begins to act as a brake by virtue of the partial vacuum—a sort of pneumatic pump—it tries to pump air and it acts as a brake first-rate.

[COMMUNICATED AFTER ADJOURNMENT BY MR. GEORGE P. LOW.]

One of the first impressions received from Mr. H. Ward Leonard's paper on "How Shall We Operate an Electric Railway 100 Miles from the Power Station," is the fact that in the adoption of such a scheme, or, broader still, by the general adoption of alternating current as the energy for operating electric railways circuits, a long step in advance would be taken in relieving the prevalent insurance idea that trolley circuits form "uncontrollable" hazards. A moment's reflection will show that 500 volts direct current is more hazardous to property interests than 500 volts alternating, in that the hazard of crosses between an alternating current trolley circuit, and the various aerial circuits is greatly reduced, if not almost eliminated by the self-induction of the various appliances in connection with such aerial circuits, and that the electrolytic destruction of water mains and underground metal work would be a thing virtually impossible of occurrence with alternating current equipment. The use of alternating currents for overhead trolley circuits is, therefore, to be commended from these points at least.

A belief that this feature is of material concern to electric railway people and the public in the larger cities, is my apology in presenting so marked a deviation from the original theme.

San Francisco, March 1, 1894.

THE PRESIDENT:—Gentlemen, there are one or two other items of business. We have an application from Chicago to elect a local honorary secretary so as to permit local meetings to take place. Of course I am aware of the fact that our rules require this to be acted on by the Council. Mr. Caldwell, the gentleman who was chairman of the committee making the application, made an endeavor to get the application before the Council at its meeting to-day. As I am very desirous that these meetings shall be held, and as I know that Council is desirous that they shall be held, in order that no time should be lost, I have taken the liberty of getting an informal vote from all the members of the Council here, and I am sure that you will authorize the little irregularity. I will therefore announce that the Council appoints Mr. Caldwell as local honorary secretary, he having obtained, in accordance with our rules, 17 votes of the 20 who have signed the report, as required by our rules.

There is a final piece of business before the meeting, and that is any action that you may wish to take as to the report of the committee on changes in Rule 5—the election rule. This report is signed by Mr. Herzog, Mr. Martin, and Mr. Upton. I am not quite sure as to whether we can act on it to-night under the rules. To amend the rules at any regular meeting a two-thirds vote of the members present is required, and written notice of the proposed amendment shall be given out at the previous meeting. If that has been done, I suppose these rules can be passed to-night. What is your wish in the matter.

Mr. Phelps:—There is some obscurity in respect to the rules, the organic laws of this body, and I think such a matter as adopting a change in the rules of election ought properly to be left to the annual meeting in May. I suppose that this meeting might express its views in regard to the report as a recommendation to the general meeting. The election occurs but once a year, and a great many of our members who live at distant points seldom reach us except at the annual meeting, whether it be held in New York or elsewhere. We can hardly, I think, with any propriety vote upon any change in the election rules in respect to their effect upon the election to occur two months from now, and it seems to me it would be wise to defer any action upon that report until the general meeting in May.

Mr. Leonard:—I move that this question be laid upon the table.

Mr. Upton:—I think some action should be taken on this report of the committee in view of the coming election. The present amendments here are merely to make clearer the intention of the former rules. This matter was brought up at the annual meeting and referred to a committee, and there was a general understanding that these amendments should be placed in the rules, and I think there should be a vote taken. There is no change from the spirit of the former rule.

The President:—What is your wish? There is a motion to adopt, and there is a motion to lay on the table. Neither motion is seconded.

Mr. Burnett:—I second the motion to adopt.

Mr. Phelps:—I am wholly of one mind with Mr. Upton in respect to the desirability of a revision. I have no doubt about that whatever. But in view of the somewhat anomalous situation in which we find ourselves, that is to say, with some doubt whether these monthly meetings at New York are the INSTITUTE, or whether the INSTITUTE is only the body of people convened at the annual meeting in May, it seems to me that we had better defer any change whatever in the election rules until May. Our whole organic law needs revision. It is in a somewhat inchoate condition. It would, I think, be wiser to let the forthcoming election be held under the old rules rather than to have any question raised hereafter whether this meeting had the right or not to deal with the election rules.

THE SECRETARY:—There is one provision of the revised rules which it appears to me can be carried out. Under the proposed amendment of the rule by the committee, it says: "Opposite the "name of each nominee in each list shall be printed a number "indicating the number of nominations received by him, and a "suitable explanation of these numerals shall be placed on the "sheet." There is nothing in Rule 5, nor in the existing rules, as a whole, so far as I know, to prevent this being done now. It would have been done last year, only, as far as I could find out, it was not thought judicious to put the numbers there. But if the committee in its wisdom, or the meeting, or any person in authority deems it proper to put those numbers there, there can be no question as to the right of inserting them. In fact they were put in type last year and taken out. As this requirement can be carried out under the present rule, it leaves nothing but the question in regard to the envelopes.

MR. PHELPS:—I think it would be hazardous to meddle with the rules in the slightest particular; just as hazardous as it would be to adopt an important committee report at this time. It seems to me quite clear that we are bound to go on till the next election under precisely the same rules as we had last year. If we adopt the suggestion of the Secretary we might as well adopt the whole report of the committee and make all the changes suggested by the committee. I believe the motion before the house is the adoption of the report of the committee.

THE PRESIDENT:—There is a motion to adopt the report, and a motion to lay on the table. The motion to lay on the table is not seconded. Does anyone second the motion to lay on the table?

MR. PHELPS:—I did not second it, as I understood the other motion had precedence.

THE PRESIDENT:—Then I will put the motion, if the meeting is ready for the question, to adopt the report of the committee on revision of the rules.

THE SECRETARY:—I would like to ask for instructions. Rule 5 goes on to say that the nomination circular sent out by the Secretary shall contain a copy of this rule; that is, a copy of the existing rule. Now, as I understand, that nomination circular when sent out containing a copy of that rule, is for the purpose of informing the members as to the rule of the INSTITUTE governing elections, and they are supposed to send in nominations in accordance with Rule 5 as sent out by the Secretary, which has already been done. Now if we adopt this amended rule, have we got the right to change a rule that has already partially gone into effect and has been promulgated under the lately existing state of affairs? That Rule 5 as it existed, has gone out to the whole membership, and under that the election machinery has proceeded. My opinion in regard to this is that having promulgated this rule as it stood we have no power to institute another rule.

MR. PHELPS:—Will the Secretary read Rule 5 again.

THE SECRETARY:—The paragraph that I referred to is as follows:—"During the first week in February of each year the "Secretary shall mail to each full and associate member of the "INSTITUTE a list of members; a list of the offices to be filled at "the ensuing annual election in May, giving the names of the "incumbents, and a copy of this rule, with the request that "nominations, propositions and suggestions as to desirable candi-"dates be made promptly and prior to March 1st." Now that had to be done according to the rule. I hold that the sending out of that rule to the membership is their law for the election. Copies were mailed the first week in February.

MR. BURNETT:—I withdraw my second.

MR. PHELPS:—Now that rule has been sent to all the members of the INSTITUTE by the Secretary, and that reinforces my point that no variation in the rule should be made between now and the election.

THE PRESIDENT:—The gentleman who has made the motion has withdrawn it.

MR. PHELPS: Then I second the motion to lay on the table.

The motion to lay on the table was carried.

[Adjourned.]

INCOMPLETED WORK OF THE INTERNATIONAL ELECTRICAL CONGRESS OF 1893.

The following circular letter has been forwarded to the chairmen of the sub-committees of the Institute.

Philadelphia, February 23rd, 1894.

A meeting of the Chairmen of the different sub-committees on Incompleted Congress Work, to whom was referred the consideration of suitable standards of light and illumination, was, in accordance with the notice already sent you, held at the rooms of the American Institute of Electrical Engineers, New York, December 28th, 1893.

At this meeting the proposals and enquiries of the various sub-committees were considered.

It was decided to defer the formulation of any selected course of experimental work until such time as the progress made in any branch might render collateral organized aid an advantage. In the meantime, however, it was thought that the various suggestions which had been offered by the chairmen of the different committees should be exchanged among the local committees, not only with a view of creating new interest in the work, but also for the purpose of indicating the direction along which the various sub-committees would be likely severally to pursue their investigations.

I therefore take pleasure in sending you an abstract of the written suggestions that have been made to me as general chairman of the committee.

(a.) Professor Brackett of Princeton University, (December 4th, 1893,) considers that a device which will enable a given source of light to be compared with a suitable standard, without the intervention of the eye as the means or comparison, need not be despaired of. That such standard, if found, should be referable to the fundamental standards of length, mass and time.

That for commercial purposes, a convention should be made as to the definite wave lengths that should be considered within the visual spectrum.

(b.) Professor Charles R. Cross of the Massachusetts Institute of Technology, Boston, (December 25th, 1893,) believes that a measured area of a flame should without doubt be a better standard than the complete flame.

Recommends that an endeavor be made to secure a constant and definite section of flame from oil or gas of definite composition, and that observation be made to ascertain how far this may be subject to variation of temperature or pressure.

Suggests that an examination should be made as to the errors which may attend the use of a chimney with the methven screen ; whether square chimneys could be used, or metal chimneys, or chimneys with an internal layer of lampblack.

Recommends that the influence of sectional dimensions in the carbon arc upon its illumination should be observed, and that the capabilities of various photometers to compare lights of varying colors should be examined.

Suggests that the device of rotating an incandescent lamp about its axis of symmetry in the photometer might be tried, to secure, if possible, a more uniform average horizontal intensity of emission.

(c.) Professor Reginald A. Fessenden of the Western University of Pennsylvania, (October 30th and December 9th, 1893,) suggests :

That there is no hope of obtaining a satisfactory standard light for purpose of visual comparison in photometry, for the reason that the optical effect of any standard light is not generally the same either for two observers at the same time, or for one observer at different times.

He consequently recommends that all laboratory measurements of light should be made in terms of radiant energy by examining, say with a bolometer, the distribution of energy through the spectrum of the illuminant, and mapping the same in reference to wave length.

That to interpret the optical and visual values of any such map of energy distribution, a series of experiments should be carried out, once for all of the optical effects pertaining to one watt of radiant energy at various wave lengths of the spectrum, referred to the optical effect of D-wave length as standard. In order to obtain a fair average, this could be carried out with a number of different observers. Having obtained the optical value of one watt of energy at a suitable number of points in the visual spectrum, the map of energy distribution for any light tested would be capable of direct conversion into a numerical valuation of optical effect.

That experiments should be made upon the effect of shape of the methven screen upon its light, and the influence of temperature and pressure of the air upon its constancy.

That the best form of bolometer for use in mapping energy distribution in spectra be studied.

That experiments on the optical effect of one watt in the different parts of the spectrum as outlined be made

That the exact determimation of the losses of reflection and absorption of the glass used to produce a spectrum should be studied.

That the determination of the absorption of different varieties of lampblack for long waves should be made.

That for practical standards of light the methven screen standard, and special incandescent lamps at a specified voltage should be employed, their optical valuation having been previously determined by the radiation method, and from these sub-standards, photometric measurements could be made in the ordinary way, or with absorption screens.

Also in another letter of February 8th, 1894, he states that he has succeeded in constructing a simple and extremely delicate form of thermo-pile by electrically welding together a number of wires so as to form a thermo-junction and subsequently rolling down the junction to a few ten thousandths of an inch, the junctions being in series, one set being presented to the standard of radiation and the other to the light to be measured.

(d.) Prof. Nichols of Cornell University, (December 4th, 1893,) hopes to test the following existing light standards: The Hefner, Carcel, Methven, Standard Candles, and the new arc standard for steadiness of light. Means have been found to indicate with the bolometer all the minor fluctuations in the brightness of such sources.

Also with a view to producing a practical standard of light, hopes to experiment upon the means of securing a surface of incandescent carbon maintained at a constant temperature by an electric current in an atmosphere of low but constant pressure of definite composition, the area of incandescent surface being such that direct photometric measurements can be made from it. The temperature of this carbon surface to be measured, if possible, in four ways, by change of length, by change of electrical resistance, by platinum-iridium couple, and by radiation.

Also in another letter, February 12th, 1884, announces that he has obtained satisfactory life curves of various light standards by means of the bolometer and galvanometer.

(e.) Professor Perrine of Leland Stanford Jr. University, (December 23rd. 1893,) suggests specifications as worthy of being prepared for adoption in all standard photometrical measurements. Such, for instance, might refer to the dimentions of the testing room, which could be 15 ft. x 5 ft. and 8 ft. high, lined with black flannel, and with screens of the same material hung from the ceiling every ten inches between the two lights used, which would be separately enclosed at the ends of a 100 in. scale.

Believes that a comparative investigation should be made of photometric screens. A proposed screen is composed of two pieces of clear glass, ground together, and viewed from the edge. The position of equal illumination on the sides being such as will cause the line of separation between the glass plates to disappear.

Also that two light standards should be adopted of different quality for use with tested illuminants of corresponding spectrum distribution. Thus the Hefner-Alteneck standard might serve for one, and the arc-standard for the other, the division being made with reference to the F-line of the solar spectrum.

(f.) Prof. Sheldon of the Polytechnic Institute, Brooklyn, (December 10th, 1893).

Considering that the whole subject of light and illumination is intimately connected with the question of color distribution and relative intensity in different portions of the spectrum, it might be advantageous to investigate the constancy of the Swinburne-Thompson standard in this respect. Also that it would be important to investigate the change in quality of light emitted by incandescent carbon as dependent upon its temperature.

(g.) Mr. Edison, (February 28th, 1894,) suggests: "Passing slowly by clockwork a definite sized platinum-iridium wire through a hydrogen flame, surrounded by a chimney, and using a section only, of the incandescent wire as standard. The moving of the wire would eliminate variation in size and deterioration of the surface."

I should be pleased as General Chairman of the Committee to receive any other suggestions or recommendations from you, or any information as may in your judgment be of interest to the other chairmen.

I append a list of all the chairmen and members of the sub-committees as far as yet appointed.

Very respectfully yours,

EDWIN J. HOUSTON,
Chairman.

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[COMMUNICATED BY MR. THEODOR J. W. OLAN.]

[In discussion of Mr. Mauro's paper see p. 56, *ante.*]

This very interesting paper read by Mr. Mauro at the last meeting of this INSTITUTE has reference to a subject of too much importance to the members of this institution, and advanced too many startling points to allow the matter to be dropped, after having had no other practical result than a merely platonic discussion. I deem it very likely that abler hands and pens than mine will take advantage of the invitation for a continuation in writing of the discussion of the subject in question. Thinking it proper, however, in this respect to take no chances, I have accepted the invitation to the end, that the INSTITUTE may arrive at a practical result therefrom, of benefit to the INSTITUTE itself, as a promoter of right and justice in legislation and probably of benefit to most of its members, as representing a large portion of the inventive intellect of the nation.

The first startling point we meet in the paper is contained in the following sentence on page 56: "It is a natural desire of every citizen to see the affairs of the Patent Office so administered as to produce the greatest possible benefit to the public" That sentence seems startling, in-so-far as it appears to acknowledge or justify the desire of the public to violate the rights granted to the inventor by the patent law. I have already had proof that the patent officials have seen fit to interpret their official duties in an analogous manner. On one occasion, when differing with an examiner in patent matters, I was advised by him that I had the right to appeal from his decision. I answered that I would rather give up, as I did not like to come into controversy with the examiner in my first patent case. "Now," he said, "it is very wrong for you to have such an opinion, as, according to that principle, an inventor takes all the chances not to get what he is entitled to." He laughed at his own words, and his assistants laughed also, apparently all conscious of the absurdity of the words expressed, and of the sophistic audacity of such an interpretation of the law—this by such an official himself, who had been put into his position in order to exercise justice and impartiality to the best of his judgment. I took the examiner's words at that time merely as an improper joke without consequence. To-day, however, when such an interpretation of the patent law prevails to such an extent that its discussion is undertaken by this institution, I think the time has come for an earnest and decided protest against the policy outlined, and for a clear and conclusive demonstration of the real principle, aim and object of the law. In its principle, the patent law is an acknowledgment from the side of the state, of the inventor's title to his invention, and in consideration of a certain fee it grants him for a certain period the exclusive right to use for his own exclusive benefit, if he deems proper, the fruit of his genius.

With the exception of the fee payable to the state by the inventor, the patent law provides for no other profit for the public. The benefit from the patent itself during the patent period lies entirely with the inventor; and the public benefit that may be derived from the invention after the patent has expired can not have anything to do with the patent itself, nor consequently with the patent law or the administration thereof.

An illustration may serve to make the evidence thereof conclusive. If, for instance, an inventor had invented a flying machine, and a patent therefor had been granted to him, said patent would grant him exclusive right to make and use the invention for 17 years. If the inventor now felt it his ambition to fly alone for that period, the patent law allows him that privilege without interference from the public. He may use it for exercise alone, or he may chase swallows, mocking birds, or wild turkeys; it is none of the public's business. If the people desire to see the affairs of the Patent Office so administered as to benefit the public in other respects than as a mere source of revenue from patent fees, instead of the benefit of the inventor, in acknowledgment of whose right and for the benefit of whom the patent law was created, such desire, as abolishing the principle of said law, must be checked in such a way that it will not re-appear; and it must be checked now, since from a timid and sophistic argument from the side of the patent officials it seems to have entered into the public's mind in earnest and to such an extent as to claim acknowledgment from the inventors themselves. Even the patent officials,—if they are so misconstruing the law, and whilst exercising their official duties are aiming to benefit the public at the expense of the inventor, for the benefit of whom the law exists,—must b· checked, and checked in such a way as to be able no longer to defy the law and violate or obviate its purpose

It might be said that the inventor claiming such great advantages is really asking too much. I think not. Another illustration may serve to enlighten us on that subject. The state says to the inventor: "In consideration of a payment of $35.00, I sell you a sufficient piece of ground upon which to build a house. You must build your house yourself, and you may afterwards use it as you deem proper for your own benefit and for that of your wife and children. I agree not to take it away from you until after a period of 17 years. I will instruct a commissioner to see that you touch only your own ground and to guard you during the erection and thereafter for 17 years, from public assaults. The inventor does not ask why the state fails to rob the property holder of his house after 17 years, or the farmer of his farm, or the banker of his cash after the same period. Although he knows that he has as much right to remain the owner of the fruits of his labor as anybody else, he makes no objection; he pays his money, he builds his house—and after 17 years it is taken away from him. Has the state thus granted the inventor too much? The socialists may say it has, but the adherents to individual ownership will say it has not, and they will appreciate the inventor's endeavors to defend the little right and justice that has once been accorded to him. Now, since it is evident from the very principle of the patent law as I have tried to point out, that said law exists for the benefit of the inventor and not for that of the public, the law must be altered so as not to allow that principle to be subject to any assaults, either from the public or still less from the patent officials who have charge of the administration of said law. It must be altered so that it will become impossible for those officials without serious consequences for themselves to place obstruction in the inventor's way under his endeavors to secure his right. The law must be made so clear and concise as to allow neither "liberality" nor "illiberality" from the officials, which will at once do away with all arbitrary treatment of the inventor. The law must be altered so as to become logically consequent to its different paragraphs or sections, so as not to give room for any interpretation leading to confusion. The law must finally be put in accord with general principles of justice and right and in general be made so clear, consequent and concise in its form that the inventor will know that he has to deal with the law itself and not with the different individualities of various officials.

The principal changes to be made, and how they are to be made we may largely determine from various points brought out in Mr. Mauro's meritorious paper. The first thing which has to be made clear is this. What may be subject for a patent? The law answers in this respect distinctly, that subject matters for patents are inventions or discoveries of a certain specified nature, but the law does not give any definition of said expressions.

It is evident that if the highest authority in patent matters, the Supreme Court, does not know, what, for instance, an invention is, since that body thinks the expression invention undefinable, the law itself must give a clear definition of the expression. If it is a fact that the Supreme Court has denied a patent because the inventive faculty had not been exercised, they have given proof that they can not define an invention, since they have used "*definiendum in definitum*," which is a fundamental logical mistake. If the expression "invention" is clear, the expression "inventive" is also clear, but if the meaning of the former is obscure the latter will be the same. The conclusion must be, that the Supreme Court denied a patent, because something, they did not know what it was, had not been exercised. It is evident from this that the patent law as it is, does not offer the inventor sufficient guarantee for a treatment in the spirit of the law, not even in its last instance of appeal. I do not think, and I feel certain that this INSTITUTE will agree with me that it is not impossible to give a definition of an invention, as the Supreme Court seems to hold.

An invention is simply: "A solved problem, having reference or relation to matter."

An invention is always caused or has its origin from a question put to the inventor's mind, how may this or that be done or made, and the invention is his materialized answer to that question.

An invention in the sense of the patent law is any construction, composition, combination or proceeding adapted to answer for, or to accomplish a predetermined, useful and legal purpose.

A patentable invention is:

a. Any construction, composition, combination or proceeding adapted to answer for, or to accomplish a predetermined useful and legal purpose, said purpose not previously publicly known or perceived.

b. Any new construction, composition, combination, or proceeding adapted to answer for, or to accomplish a predetermined useful and legal purpose, said purpose previously publicly known or perceived.

c. An improved construction. composition, combination or proceeding adapted to answer for or to accomplish a predetermined useful and legal purpose.

Improvement, is any construction, etc., adapted to answer for a given purpose in a cheaper, simpler, more effective or more perfect manner.

A discovery is any disclosed and previously unknown fact with reference to the existing.

A patentable discovery is evidently any disclosed fact with reference to the existing, which can be usefully applied for a patentable invention.

If we now agree, that the definitions I have given of patentable inventions and patentable discoveries are sufficiently clear, not to allow any doubt of what is subject matter for a patent, according to the patent law; the necessary amendments of said law in order to make it just, consequent and all through consistent with its demonstrated principle and purpose, can be easily concluded with a review of the different sections of the law itself.

In section 4887 of the present patent law it is said: "No person shall be debarred from receiving a patent for his invention or discovery." We have there the fundamental command of said law, and after the clear and unmistakable meaning of that command, all the other sections and paragraphs of the law ought to be tested as to suitableness and wording in order to make the law just, harmonious, consequent and consistent with the principle of the law plainly expressed in the command.

The first, the most important and evidently undeniable conclusion we may draw from the cited command, is that the law must provide sufficient guarantees for the inventor to attain his right, at least with the last deciding authority in patent matters. The Supreme Court of the District of Columbia is this authority.

How has this authority proven fit for the task conferred upon it? According to Mr. Mauro, it does not know what an invention is, and it has declared it impossible ever to know, since its members have decided that the meaning of the expression invention is undefinable. And still it has seen fit to give decisions in patent cases; judging, without knowing what it was judging about. Does this indicate sufficient guarantees for the inventor to secure his rights at last? Certainly not. Without intending a slight to the Supreme Court, I think it impossible for a body of lawyers (if lawyers simply in training and education) to exercise proper judgment in this matter; it lies in the very nature of things themselves. The thought of a mere lawyer directing a number of inventors reminds me of a hen put in charge of young ducks. Their instincts are too different for mutual satisfaction. The requirement for ability regarding all the various questions that may arise, necessitates too fine distinctions for allowing us to hope fair decision from unquestionable integrity alone. Common sense does not constitute the only qualification for a suitable judge in patent matters. It is the trained skill and fine instinct derived therefrom which is equally necessary in the various cases. For illustration. Supposing a man has invented a composition of nitrate of potash, sulphur and carbon, which he calls powder; and that he had received a patent therefore, claiming broadly the composition of said three bodies. His composition was of a non-explosive nature; when he lit it, it burned, it whizzed and it smelled badly. Supposing now another man had invented a composition of the same substances in such proportions as to constitute our ordinary gunpowder. He wants a patent for his invention, but he cannot receive it because he comes in interference with the first inventor refered to. A Supreme Court judge will probably in this case decide that, as there is no need of exercising the "inventive faculty" for putting a little more or less of each of three known substances into a composition, the patent cannot be granted; a man skilled in the art would decide that, as the proportion between the substances in question in this case was just as essential as the substances themselves, a patent to the latter inventor must also be granted. I am sure this INSTITUTE is of the same opinion. Whilst the first inventor ought to retain his patent right, so as to enable him to rest peacefully in the evening by the use of his compound to drive mosquitoes out from his room in summer-time, the second inventor ought also to have his patent granted in order to allow him to work our mines, open our tunnels and help the kings, the emperors and the presidents to make war against each other and amuse themselves. Many similar illustrations could be given for the delicacy of the task conferred upon the last deciding authority in patent matters, and circumstances point direct to the following conclusion: The last deciding authority in patent matters must be a jury of men, skilled in the appertaining art in each case, selected from among men outside the patent office by the contesting parties themselves. This would give the inventor, if not absolute certainty, a fair chance to get his right in the last instance.

It is, however, not enough that the inventor should be able to secure his right in the last instance. The law says he shall not be debarred from obtaining it, which apparently means that no obstruction or delay should be put in his way, since he has made his application for a patent. What provisions are made in the following sections of the law for giving effect to the command and what is the inventor's practical experience with reference thereto?

Section 4888 directs that the inventor, to receive a patent, shall make an application in writing describing his invention in such full, clear, concise and exact terms as to enable any person skilled in the appertaining art thoroughly to understand the nature and use of the invention.

This requirement complied with, the law provides that the Commissioner shall cause the examination of the alleged new invention or discovery. On whom is this task now conferred? On the principal examiner and his assistants. The principal examiner—who is he, according to law? Nothing is provided for his qualification by law. We may, however, take for granted that he must be able to read and write, that is all. Thus the specification legally composed for a man skilled in the art, is to be judged by a man who may have common school training and who may not. The inventor, although having complied with the law as to specific completeness and clearness of his specification, is subjected to innumerable objections due to inferior ability It is not at once clear how two times two make four; it has to be shown how four plus three gives seven, etc., may be taken as standards for the objections very often raised. To satisfy the examiner the inventor will have to try and make those obscure points clear; he will have to compose as many finished lectures suitable for a primary school, as letters with reference to objections made. When he has at last succeeded in finishing one objection, another one is raised; and when this is done with—yet another. He will have to spend money for legal assistance often many times more than the entire patent fee. He will have to sacrifice his time, armor his patience, dominate his temper, and be up early in the morning.

If he at last succeeds in suiting the examiner and gets his patent allowed he may regard himself fortunate. In many, perhaps most cases, he has to appeal to the examiner-in-chief; and now at last he has come into the hands of an authority legally qualified to understand his description, perhaps. The examiner-in-chief must, according to law, be of scientific ability, and there are great chances then that he will understand a specification written for a man skilled in a certain art. The legal number of examiners-in-chief are three, and there are therefore threefold chances for the inventor that one of them may have some ability in the respective art to which his invention belongs. If the inventor now gets his right from the examiner-in-chief, he is again fortunate; but if he does not get it, he can again appeal. To whom? To the Commissioner. The Commissioner, who is he? The law says nothing of his qualifications, but from circumstances we may conclude that he is either a republican, or a democrat, or a mugwump, that is all. The inventor, unable to judge from that as to the skill and training in the arts possessed by said gentleman, has to shut his eyes as to the possibilities of his chances and blindly cast his twenty dollar pieces in the air. Perhaps he will hit a sparrow, perhaps not. If, however, the Commissioner acknowledges his rights and grants his patent he is fortunate. At all events his case is finished as far as the Patent Office directly is concerned. Now, supposing the inventor was accorded his right at last by the Commissioner, is he to be indemnified for all delay caused by obstructive and uncalled for objections and for his loss of time and money in his endeavors to secure his right? Is the principal examiner to reimburse him the appeal money—paid on account of his wrong decision, or the examiner-in chief for the increased expenditures caused by his? Certainly not; his case is closed, and the examiners are left undisturbed to continue the same course with reference to other inventors.

The more obstruction there is raised in the inventor's way, the more wrong decisions to be appealed from, the more money the Patent Office will make; and when the year is at an end, the office will in this way have paid in a surplus to the Treasury of one million or two, no matter if most of said surplus is ill earned pelf. Is this state of things a desirable one? Is the praxis here referred to consistent with the clear fundamental command of the law, that no man shall be debarred from receiving a patent for his invention or discovery. The sections of the law not preventing such obstruction but rather favoring it, are they consequent with cited fundamental command; are they based upon the invariable principles of justice and right, which ought to be the desirable aim for all legislatures. Decidedly not. I do not mean to say, that, as long as the law provides for an examination of applications for patents, there should be no objections raised; but what is undeniable is this, that the inventor should not be obliged to fight with the patent officials for "what he is entitled to". It should be clear not only from the fundamental command of the law, but also from all the subsequent sections thereof, that the patent officials have been placed in office not in order to try to defeat the inventor under his endeavors to come to his right, but to benevolently assist him under his endeavors. The law should therefore be altered so as to clearly point out this as the always directing rule for the various duties of said patent officials, and provision should be made to enforce said rule, if they afterwards failed to see, or failed to fulfill their duties. The law should be altered, so as to do away with all undue obstruction not only due to inability, but also that heretofore experienced from over zealousness of misunderstood duties, or from a desire to benefit the Patent Office financially, in excess of what is required by law. It needs no demonstration to prove, that the present provisions in appeal cases may be construed as a direct invitation to the patent officials to raise in the inventor's way as many adverse decisions as possible in order to cause as many appeal fees as possible to be paid. Nobody argues, that adverse decisions should not be made if they are just and qualified, and that on the other hand some inventors may appeal from an adverse but correct decision; but that the inventor shall pay the appeal fee whether he is right or wrong; there is where the injustice comes in.

The provisions in reference to appeals should therefore be altered not only on account of the misuses to which they clearly may lead, but also in order to accord with principles of justice and right.

I think we are able here to make the following conclusions:

First: The patent law should make provisions for necessary qualifications of all the patent officials so as to provide guarantees for their necessary ability to fulfill the duties conferred upon them, so far as possible without error or mistakes.

Second: The examiners of patents should be well paid so as to enable them to direct their entire attention to their duties, as they should be of adequate number for carrying on the business of their office without overwork or strain, and the work required from them should be so limited, as to enable them to follow the progress in the art, and to make themselves more and more fit for their duties.

Third: The patent officials should be made independent of political influences whereby able men who have either already entered in service or who may in the future be appointed could be retained in office.

Fourth: For evidently misconstruing the provisions of the law for making arbitrary decisions in defiance of said law the officials should be removed from office.

Fifth: An official record should be kept, with reference to decisions of each separate examiner, subsequently reversed upon appeal; and no examiner should be allowed to retain office, after having had a limited number of his decisions reversed. The inventor should be reimbursed by the losing examiner for all expenditures for appeal fees, in cases ultimately decided in his favor.

Sixth: No authority in the Patent Office should have any adjudging power in appeal cases, if he is not by scientific merit and skill in the arts, fully qualified for such duty in the strictest sense and spirit of the law.

That the last and highest authority in patent appeal cases should be a jury composed of men of scientific ability and skill in appertaining matters as we have already concluded.

When the patent law has been so altered as to contain provisions embodying the changes here above suggested, and deduced as necessary conclusions from the discussions of the matter, then I think the law will become harmonious and consonant with reference to its fundamental command, principle and aim.

I think if this INSTITUTE took the initiative in promoting the necessary changes in the patent law referred to, it would not only highly favor its own interest and authority, but also benefit a great number of its members and earn the gratitude of the inventors of the nation. The latter have long enough spent their time and money in futile efforts to get the benefit of rights, granted to them and acknowledged by law, but not accorded to them by the administrators of said law.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS,

New York, March 21st, 1894.

The eighty-fifth meeting of the INSTITUTE was held this date at 12 West 31st Street, and was called to order by President Houston, at 8 P. M.

The Secretary read the minutes of the last meeting, which, on motion, were approved.

The Secretary read a list of associate members elected, and transferred to full membership at the Council meeting March 21st as follows:

Name.	Address	Endorsed by.
AGNEW, CORNELIUS R.	Electrical Engineer, Kinsman Block System Co., 23 West 39th St., New York City.	W. B. Vansize. C. F. Brackett G. A. Hamilton.
BILLBERG, C. O. C.	Electrical Engineer, Thomas H. Dallett & Co., 3200 Arch St., Philadelphia, Pa.	Carl Hering. E. G. Willyoung. Fdwin J. Houston.
BLISS, WILLIAM L., *B. S., M. M. E.*	Electrical Engineer, Riker Electric Motor Co., 24 Irving Place, Brooklyn, N. Y.	Andrew L. Riker. James Hamblet. T. C. Martin.
CARICHOFF, E. R.	Electrical Engineer, Sprague Electric Elevator Co., 126 West 34th St., New York City.	Henry W. Frye. W. D. Weaver. E. C. Davidson.
COHO, HERBERT B.	Engineer, Waddell-Entz Co., 203 Broadway, New York City.	T. C. Martin. F. J. Sprague. C. T. Hutchinson.
GALLETLY, J. FRED.	Electrician, Swift & Co., Chicago, Ill.	Clark C. Haskins. George Cutter. Fred. DeLand.
JACKSON, HENRY	Telegraph Supt. and Engineer, The Lancashire & Yorkshire Railway Co., Horwich, Bolton-le-Moors, Lancashire, England.	Ralph W. Pope. T. C. Martin. Joseph Wetzler.
KELLER, EDWIN R.	4823 Springfield Ave., Philadelphia, Pa.	E. G. Willyoung. Carl Hering. Edwin J. Houston.
KIRKLAND, JOHN W.	Electrical Engineer, General Electric Co., Schenectady, N. Y.	Jas. B. Cahoon. C. P. Steinmetz. H. G. Reist.

PHILLIPS, LEO A.	Westinghouse Electric and Mfg. Co., 98 Green St., Newark, N. J.	Philip A. Lange. Nikola Tesla. L. A. Osborne.
ROUQUETTE, WILLIAM F. B.	Proprietor, Rouquette & Co., 12 Wooster St., New York City.	Charles Hewitt. James Burke. Geo. R. Metcalfe.
ROWLAND, HENRY A.	Professor of Physics, Johns Hopkins University, Baltimore, Md.	Frank J. Sprague. Louis Duncan. C. T. Hutchinson.
SMITH, J. BRODIE	Supt. and Electrician, Manchester Electric Light Co., 142 Merrimack St., Manchester, N. H.	Geo. F. Curtiss. Sidney B. Paine. Caryl D. Haskins.
VOIT, DR. ERNST	Professor of Electricity, Technical University, Schwanthalerstrasse, Munchen, Germany.	Benj. F. Thomas. C P. Steinmetz. F. W. Tischendoerfer.

Total 14.

TRANSFERRED FROM ASSOCIATE TO FULL MEMBERSHIP.

Approved by Board of Examiners, December 7th, 1893.

ALMON, GEO. H.	Supt. Construction New England District, General Electric Co., Boston, Mass.
EGGER, ERNST	Electrical Engineer, Vienna, Austria.
BRENNER, W. H.	Electrical Engineer, Montreal Street Railway Co., Montreal, P. Q.
HASKINS, CLARK C.	City Electric Light Inspector, Chicago, Ill.
FITZMAURICE, JAMES S.	Chief Engineer Electric Light Branch, Sydney, N. S. W.

Total 5.

THE PRESIDENT:—Mr. Kennelly notified the INSTITUTE at the last meeting that he would move to take from the table the report of the Committee on Units and Standards. Mr. Kennelly has the floor.

MR. HAMILTON:—I would like to move that the report be taken up for reconsideration now.

[The motion was carried. The report is as follows:]

REPORT OF COMMITTEE ON UNITS AND STANDARDS.

NEW YORK, Nov. 15th, 1893.

To the President and Council, of the
American Institute of Electrical Engineers.

GENTLEMEN:—Your committee on "Units and Standards" begs to recommend to the Institute the provisional adoption of:—

The term "gilbert" for the C. G. S. unit of magnetomotive force, the same being produced by 0.7958 ampere-turn approximately.

The term "weber" for the C. G. S. magnetic unit of flux, sometimes described as the C. G. S. line of flux.

The term "oersted" for the C. G. S. unit of reluctance.

The term "gauss" for the C. G. S unit of flux density, or one weber per normal square centimetre.

The committee, it will be remembered, in its previous report, dated June 20th, 1891, advocated that the above terms should be accorded to magnitudes in conformity with the "practical" electromagnetic system, and therefore following in natural order and extension from the volt, ohm, ampere, and other units in universal use.

As, however, so important a series of new unit magnitudes could only meet with general recognition and favor under the authorization of an International Electrical Congress, which authorization has been withheld at the recent Congress at Chicago and since objections have been raised to those magnitudes, your committee considers that the urgent need for names specifying the principal quantities dealt with in magnetic circuits can best be met with general favor, by adopting for those names the fundamental unit magnitudes of the international C. G. S. system after the precedents already established in the cases of the C. G. S. units of force and work, entitled respectively the "dyne" and "erg."

Yours very respectfully,

COMMITTEE ON UNITS AND STANDARDS

F. B. CROCKER,
W. E. GEYER,
G. A. HAMILTON,
A. E. KENNELLY, *Chairman.*

THE PRESIDENT:—Does Mr. Kennelly wish to open the discussion?

MR. KENNELLY:—I think, sir, that on the occasion of the meeting before last, the vote by which that report of the committee was laid on the table, was a vote given by some of the members under a misconception. I think you will remember, sir, that there were two motions before the Chair, and that some of the members informally stated that they had voted upon the second motion believing it to be the first, and in case the intention of the meeting were taken again it might be found that a majority of the meeting were in favor of the adoption of the report.

MR. TOWNSEND WOLCOTT:—Mr. Kennelly's remarks on this subject are printed in the March issue which we have to-night.

In regard to the adoption of the names of electricians for these C. G. S. values, Mr. Steinmetz objected to that on the ground that it was not according to the other C. G. S units, and I do not think that anybody outside of this country would approve of that point. I objected myself on the same ground, but that has nothing to do with what I now say. I do not think that foreign electricians would approve of it. If you could get names like erg and dyne, I should be strongly in favor of them myself, but I do not think it is a fact that if we fail to adopt names for those quantities, there will be any great difficulty experienced. Mr. Kennelly's argument here is that 300 grammes is very much more definite than 300 units of weight in the C. G. S. system. Of course, that is a very cumbersome expression if it were necessary to use it. But we have in common practice expressions somewhat of that nature. We have several kinds of units which are called by the name of degree Degree does not mean much more than the word unit. There are two or three different kinds of degree. In the first place, the quadrant is divided ordinarily into 90 degrees, and by the French it is divided into 100 degrees. I do not think it would make it any more plain what these words were, or would give any more of a concrete idea of the things themselves, if you called them Le Verriers after the astronomer who did the work. In the ordinary thermometer scale we say the temperature is so much. For instance, we say, it is very warm to-day; it is 90. We mean to say 90 degrees. Everybody knows what you mean if you just say 90. If there was any doubt about it you would have to say 90 degrees on the Fahrenheit scale, because we use Fahrenheit in this country. When we come to magnetic units, if there is a name attached to a unit, unless it be a very short one and very convenient to use, it would not be used at all by a great many people. I, myself, always speak in this way of the density, if I am talking to a man who knows what I mean by the density of magnetic induction. You just say it is a density of 16,000. You do not say it is a density of 16,000 C. G. S. lines per square centimetre. That is entirely unnecessary. I wish it understood that I do not disapprove of naming those units at all, if you can get some good names. But I do not think that science is going to suffer so very much if we do not name them immediately.

Another thing is, the provisional adoption of a name is worse than no adoption at all, I think. We already adopted one name provisionally The foreign electricians adopted the name "weber" for unit of current, and then changed it to "ampere." Some books have "weber" in them still. If you adopt a name and then change it afterwards, it makes it worse than if you never adopted it. We have heretofore made mistakes by being in too much of a hurry. If they had not been in too much of a hurry to get a new value for the ohm, there would be only two values for the ohm in use. At present there are three—the British Association, the legal ohm, and the international ohm. So it would be with names, if you adopt any names provisionally.

Some one says—I do not know whether officially or not—that if any one would find good Greek names that would do, the committee would adopt them, but we have not got the Greek names. It seems to me that we ought to have some Greek scholars. Mr. Kennelly says we do not possess the facility for creating Greek names in this country that would meet general apprehension and support. Well, although that may be so, I do not know why we should not be able to make Greek names in this country as well as in England.

MR. KENNELLY:—I think that the point that Mr. Wolcott mentions is the very one that I would have selected on the other side if you would give me a moment's hearing. When you say 16,000 you do not at the present time know what that means. It may mean 16,000 lines per square inch or it may mean 16,000 per square centimetre, and for that reason there is an argument in defining what it means by saying 16,000 gausses.

PROF. FRANCIS B. CROCKER:—If you are thoroughly familiar with the subject and expect a person to make a certain remark, why then, of course, you can understand it. But assume that you are teaching some one who knows absolutely nothing about it. I have had some experience in that direction, and I find that until I can attach a concrete name to a unit it is rarely ever comprehended and it certainly takes a great deal longer to make it understood. That must be evident to any one who has made any attempt in that line. Mr. Wolcott probably referred to conversation between two electrical engineers both of whom are perfectly familiar with the subject, each knowing about what the other is going to say. But in speaking to some one who is learning, I think you will find that the absence of a concrete name will do more to give an indefinite idea than any other one thing, and the presence of a concrete name would do a great deal towards giving a definite idea. In fact I deny that a general name like "C. G. S. unit of magnetic flux," or even "unit of flux" will ever be clearly understood by any but technically educated persons. Now we all know that common workmen can use the terms "volts," and "amperes" and "ohms" just as intelligently, and for their purposes just as accurately and just as satisfactorily as the most learned electrician. I do not think that would be possible, in fact I am quite sure it would be utterly impossible if we called them "absolute units of electromotive force," or even "units of electromotive force," because such men are not accustomed to abstract ideas. They can measure electric pressure in volts and they understand exactly what it means—at least well enough for their purposes and for the purposes of the persons who employ them, but if they had to resort to abstract terms they could not apply them intelligently This is true also of persons beginning the study of electricity even though they are educated, and in both of these cases I say the giving of definite names would be of enormous assistance. Now in the case of men who are thoroughly familiar with electrical and magnetic science and who know exactly what they are talking about, I admit that the necessity is not so great; but even then, unless the context or the circumstances indicate exactly what is meant, there is apt to be the ambiguity to which Mr. Kennelly referred, that we unfortunately use both centimetres and inches in this country and in England, and we use them about equally often, and I should be at a loss to know when any one said "10,000 lines" whether he meant per square inch or per square centimetre.

I think that electrical science owes a great deal of its exactness and its progress, and its extremely perfect condition at the present day, to the fact that we have definite units with definite names, and no other science approximates electricity in that respect—even mechanics which is much older and is supposed to be more exact than electrical science. But mechanical science at the present moment is much less definite, and there is much more uncertainty even in the use of such words as "power" and "work" and "force" than there is in the use of electrical units. I attribute, as I say, the definiteness of electrical science to the very satisfactory condition of electrical measurements and terminology. I attribute it largely to the fact that we have definite names for definite things and we do not call them units of something or "degrees," or other indefinite terms that mean nothing.

MR. WOLCOTT:—I would say that, perhaps, I did not make myself clear enough in what I said before. The examples I took were things that are used by uneducated people as well as educated people. Take the mariner's compass. Certainly ordinary seamen are not better educated than the students who begin to study electricity, and they know what a point on the compass is. Knowing it by the name point, it means just as much for instance, as if they called it a Marco Polo, because Marco Polo had something to do with introducing the compass into Europe. On the same line of reasoning, I say one line of force means just as much as though you called it by some man's name. As to what Professor Crocker said in addition to that, that people do not get mixed up when they have definite names to deal with, we certainly have a definite name in "watt." Watt is supposed to mean something very definite, and yet a great many people, including college professors, talk about "watts per minute" and "watts per second." That is a mistake that is made very often. I think it occurs just about as often as if you did not have the name "watt." They mix up "watts" and "joules."

MR. C. S. BRADLEY:—This subject seems to have been submitted to a well chosen committee. I think there is none of us who could spend the time they have spent on it, and if the thing is to be adopted in any shape, manner, or form, it would be well to take it as the committee have submitted it, with one exception. I think the whole turning point is on the question whether this body wants to assume the responsibility of deciding this question alone. I would suggest that it be submitted to at least two of the foreign bodies of electrical engineers—the English and the French. It will make some delay, but it would seem to be a good policy, and I would, therefore, make the motion that we submit this question to the foreign bodies and await an answer.

DR. WM. E. GEYER:—I understand it is not proposed that we adopt this unit once for all, and dictate to the world to accept it. It is only a provisional adoption, with a view, if possible, to inducing other bodies to follow us. I think we did something similar in the case of the henry, and succeeded very well. We did not attempt to lay down the law to the rest of the world. We simply suggested it, and by doing a similar thing in this case we may also succeed.

MR. BRADLEY:—I think it is far more dignified to submit the question prior to any adoption, although it may take a little more time.

THE PRESIDENT:—Will you pardon your President for speaking a moment on the subject? I quite agree with what Prof. Crocker has said respecting the advisability of having definite names for definite ideas, especially when you wish to convey those ideas to others. As a teacher, I find it almost an absolute necessity if I wish to convey an idea, to clothe that idea in the briefest language possible. I know the dangers that arise from contrasting things that do not exactly resemble one another, but any of you who have endeavored to give to a class of young men ideas of magnetic flux, desiring to show the relation existing between the magneto-motive force and the reluctance, and to draw comparisons between the ideas of electric flux or current and to show the similar relation between the electromotive force and the resistance, will, I feel sure, find great advantage in having names adopted for the units of magnetic flux, magnetomotive force and reluctance. Of course it does not express any more, when you say there is a similarity between the amperes, between the volts divided by the ohm, and the webers and the gilberts divided by the resistance, but still it does help very greatly to give the ideas.

I do not at all agree with the member who thinks it is necessary or even advisable to submit our action to any foreign bodies. It is only an action which, if we are ready to take, we simply announce to the world that pending the adoption of better names, we propose to use the terms "gauss," "weber," "gilbert," and "oersted." It does not seem to me that the argument of Mr. Wolcott is really deserving of very great consideration—namely that heretofore we have adopted for the names of units, the names of electricians who have passed from their labors, only in the case of the practical unit. If they have served well in the place of the practical units I see no reason why they should not also serve in the place of the C. G. S. units. It seems to me, therefore, that the INSTITUTE would be quite warranted in taking this step, which is recommended, after due consideration, by certainly a very able committee appointed by you.

MR. BRADLEY:—I think it is a courtesy to the foreign bodies, and I think all those things will draw us all into sympathy with them. We are all living in a world, and the more nearly we are working together the more we are in sympathy, and it seems to me a good opportunity to offer courtesy.

DR. CARY T. HUTCHINSON:—It would seem that the advisability of doing what is proposed, might depend to some extent upon whether you can get other people to follow your example. Were not all these names suggested to the Chicago Congress, and did not it decline to act on them? It seems rather absurd to attempt to adopt a lot of names which others will not use.

THE PRESIDENT:—I do not think these names were actually acted on.

DR. HUTCHINSON:—I think they declined to take any action on them.

Prof. Crocker :—That same argument was used against the henry, and in that case the result was extremely satisfactory; so satisfactory that a Frenchman moved the adoption of the name, and an Englishman seconded it. It was not even necessary for the Americans to urge it. Now, in this case we can simply do exactly as we did in that previous case. We suggest something to the electrical profession of the world, which we think desirable. If they see fit to use it, as they did in the case of the henry, then the next international congress will, probably, adopt it officially, Furthermore, all those who took part in the Chicago Congress were unanimously of the opinion that the real work of introducing a new unit or term has got to be done *before* a congress meets. The unit has got to be adopted, unofficially, by electricians throughout the world before it can be adopted officially by the congress. A congress has no time to discuss the merits of introducing a unit. It is simply a question of whether they will accept it or not, and they barely have time to decide even that question. So it was, I think, the opinion of every one at the Chicago Congress, that the real material on which a congress can work must be provided for it, and that the suggestions must all be threshed out before the congress meets, and a long time before, if possible.

Dr. Hutchinson :—My point is simply that these names have been before the Congress. Prof. Crocker is right in saying that they should be adopted by the entire electrical community of the world, and since the representatives of other countries in common with those of this country have declined to take action on this matter it seems rather like trying to force things down their throats.

Mr. A. E Kennelly:—I think it is only fair to say that these names came before the Chamber of Delegates at the Chicago Congress, not for these magnitudes but in connection with a system of practical units, so called, so that the fact that they were not endorsed by Congress need not be construed to their disadvantage in this presentation. It is out of our province, of course, as a practical body of this kind to create fundamental scientific magnitudes. But here are units already in existence. Here are things that we use in our every day work needing names. It surely is within our province to give those provisional names.

Dr. M. I. Pupin :—Mr. President, as Mr. Kennelly remarked, the units have already been fixed. They are born; all they need is to be christened. Now, the question is, who is to be the godfather. That seems to be the whole difficulty. The Germans would probably not care to stand godfather to anything that does not bear a German name. The English probably feel about the same way, the French the same. We know what difficulties they had at the first Paris Congress. The difficulties arose, not so much in connection with the magnitude of the units, as in connection with the names to be applied to these magnitudes. There seems to exist considerable international jealousy, naturally so, on the other side. There is no international jealousy on this side, because we are all one. It is probably owing to this circumstance that there is so much less of mental inertia, on our side, when it comes to naming units. We do not hesitate to give some name to a unit if we think that such and such a name deserves the honor. Now, I do not think that there is much doubt about the names "gauss," "weber," "gilbert," "oersted," etc., as being appropriate to the units to which our committee on units assigned the distinguished honor. The question is this—who is to propose these names for the units? I think that we here, in the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS, occupying as we do a neutral ground, are really called upon to make that proposition, and our friends on the other side, upon careful consideration, will find that we, standing on neutral ground, could see with a much clearer view, with much less prejudice, that the names just fit the things to which we have applied them. I think that the suggestion of Mr. Bradley, if carried out, will cause considerable delay. It is not necessary for us to wait for the other engineering societies to express their sentiments in the matter. Let us propose the names and let them act in any way that they may please.

DR. GEYER:—Of those names proposed, two, "gauss" and "weber" are the names of men so pre-eminently great, that if proposed by what might be called a neutral, as we in America, are. I think the rest of the world will really be ready to lay aside its jealousies, and even if that were not quite the case, these men were so great in their line that it seems to me a matter of common justice that they should be somehow commemorated. We are all very grateful to the British Association for getting out a physical unit called the ohm, and we all appreciate the work they did in giving us the name of "volt" and "farad." But the British Association merely adopted the work of Gauss and Weber, simply modifying the magnitude of some of the units. But the system was really that of Gauss and Weber, and I have very little doubt that if, in those times, magnetic units had been of as much importance as they are now, Gauss would have immediately received some recognition, had the measurements of the current played any great role in those days. Then it was essentially resistance and capacity and electromotive force that were important in the electrical measurements. Currents were not used of any appreciable magnitude, except, perhaps, in electroplating, and that is more or less an occult art. I mean to say, that if current had been used in those days in any considerable magnitude, and Weber had not been at that time living, I should venture to say that he would at once have received recognition. I do not mean at all to disparage the labors of Ampère, but I should say that he was, at least, the equal of Ampère. Now, the name for the unit of current is adopted. You cannot change that. But we might do the next best thing, it seems to me, and give one of these great men, at least, a recognition in something that is analogous. The objection has been made that the name "weber" is unavailable because it has been used in another connection. But that other connection is so different, that no one knowing the simplest element of electricity and magnetism would ever run the least chance of confusion.

Mr. F. S. Holmes:—At the meeting before the last, when this question was brought up to be acted upon, there were two questions in my mind that led me to hesitate; the first, as to the magnitude of the units, and the second, as to the permanence of the names which we might apply to them. We certainly do not wish to apply the names of several scientists to certain units for an indefinite period; for, it may be, one, ten, or fifteen years, and then have the name entirely changed, or the magnitude of the unit altered, and, therefore, a confusion arise at that time. I think we all of us are in hearty sympathy with this committee in their effort to objectify, to bound, to term these units, so that they have a substance that we can describe and impart to others in short order. But I am very glad to say that the discussion, as it has gone on to-night, has led me to believe that possibly we in America may help to roll on this cause by applying these names tentatively, and may overcome some foreign jealousies and difficulties that would lead scientists abroad to object to having nearer neighbors name the units.

Dr. Hutchinson:—The point is just that—applying names tentatively when you do not know that they will be adopted, and the possibility of others applying other names tentatively, which would lead to confusion.

Mr. Kennelly:—Since somebody has to make a start in obtaining names either in this country, or in England or in Germany or France, and since various inchoate attempts have been made, as evidenced in the technical journals from time to time, to create names, I beg to move that the names be adopted provisionally by the Institute at this time.

[Seconded].

The President:—It is moved and seconded, as you have heard, that these names mentioned in the report of the Committee on Units and Standards be adopted provisionally by the Institute.

[The motion was carried.]

The President:—I take pleasure in introducing to you Prof. William A. Anthony, who will read a paper on the Effect of Heavy Gases in the Chamber of an Incandescent Lamp.

Prof. Anthony read the following paper.

A paper presented at the Eighty-fifth Meeting of the American Institute of Electrical Engineers, New York, President Houston in the Chair; and Chicago, Manager Hibbard in the Chair. March 21st, 1894.

ON THE EFFECT OF HEAVY GASES IN THE CHAMBER OF AN INCANDESCENT LAMP.

BY PROF. WILLIAM A. ANTHONY.

Since the issue of Edison's incandescent lamp patent in 1880, it has been the generally accepted theory that the principal cause of the decay of the filament of an incandescent lamp other than oxidation, was the mechanical action of the small amount of gas remaining in the chamber, wearing away the filament by the attrition due to the air currents, or as Edison called it "air washing." Electrical forces are also supposed to have some effect, carrying the carbon from the filament to the walls of the chamber. The perfection of the electric lamp upon this view was to be sought in the direction of a higher vacuum, and more firm and solid carbons with a smoother surface. Improvements in carbons have done much to improve the lamp. As carbons are now made—built up by deposit from a hydro-carbon vapor—every interstice is filled, and the surface becomes almost as smooth as, and has almost the appearance of, polished steel. Such a carbon withstands well the "air-washing" effect of the residual gases, and the life of the lamp at a given temperature is greatly increased. But as it is greater economy, up to a certain point, to increase the temperature and save energy, than to keep the temperature down and save lamps, temperatures have been increased as carbons have been improved, and efficiencies have been raised until, in new lamps at least, a candle power is obtained from less than three watts, instead of four or more. But with these higher efficiencies comes a new cause of deterioration not so apparent in the low temperature lamps, the blackening of the bulb and consequent loss of transparency and decline in candle power. When I see incandescent lamps hung for use, I involuntarily find myself looking for blackened lamps, and it is no uncommon thing to see those through which objects look dim. Sometimes I see lamps so black that they would do for smoked glass in viewing an eclipse of the sun. I have taken down such lamps and measured their candle power and efficiency. Very often they run as low as ten candles and six or seven watts per candle at their normal voltage, and I have found lamps below six candles and ten watts per candle. The customer was still using these and had not "kicked" so far as I know, though he was sure he was not getting the light he did at the beginning. Generally he does not know what the matter is, thinks, perhaps, the company is parsimonious with the "electricity," but hasn't said much, "because he doesn't want to be all the time making a row." Of course, the illuminating company cannot be expected to go around and hunt up such lamps and change them; so, as long as the customer does not complain, but pays for his six or ten candles at the price of sixteen, the old blackened lamps remain on the circuit until they burn out. Since such lamps increase in resistance they take less and less current, and the older and more feeble they become, the more tenacious they are of life. It is no uncommon thing to hear of such lamps that have lived through six, eight, or ten thousand hours of active service, when, probably, not one of them was able to do its full duty, and should have been placed on the retired list, at three hundred hours.

Now the question arises, is there no way to preserve the illuminating power of the lamp throughout the life of the filament! Surely a long-lived filament is of little value if it fails to give a reasonable light after two or three hundred hours.

Loss of candle power might be avoided by lowering the temperature and with it the initial efficiency of the lamp, but this is an improvement in the wrong direction, although I am sure the customers, whether they pay for their electric energy by the meter or by the year per lamp, would be the gainers if they were furnished with lamps of lower initial efficiency, unless their lamps are changed oftener than is usually done. The present high efficiency lamp would do very well if there were any way to insure its retirement from the service when its legitimate usefulness was passed. The *ideal* lamp would be one that, while giving a high efficiency and a reasonable life, should die by the rupture of its filament while still doing its best work.

About a year ago Mr. John Waring began experimenting upon the effects of heavy gases in the lamp chamber, using principally the vapor of bromine as obtained from the liqnid bromine of commerce. It is not necessary to describe in detail the various experiments. The final result reached was that, when bromine vapor was present in the chamber in proper quantity, the lamp, as compared with a vacuum lamp, could be run at a higher efficiency without blackening or increase of resistance, therefore without loss of candle power and with an increase in its *useful* life. These facts were abundantly demonstrated by numerous experiments. Below are the results of some experiments that I myself recently performed. I had made a number of 50-volt 16-candle lamps. Half of these were exhausted in the usual way as vacuum lamps. The others were made into bromine lamps after the method employed in the Waring factory. The lamps were all "volted," then four of each were run for three and a half hours at 65 volts, and for one and a half hours at 70 volts pressures—15 and 20 volts in excess of the normal. Two of the bromine lamps broke at the end of the run. The results are given in the following table:

TABLE I.

	Lamps, New.			After 5 Hours at 65 and 70 Volts.			
	Volts.	C. P.	Current.	C. P.	Current.		
1	49	16	26	9.4	24	Vacuum Lamps.	Bulbs all Blackened.
2	49.6	16	26	10.3	24.6		
3	49	16	26	10.6	24.8		
4	49.2	16	26	9.8	24.8		
5	49.8	16	27	Broken	....	Bromine Lamps	Bulbs not Blackened.
6	48.8	16	26.5	15.5	26.2		
7	49	16	27	15.1	27.2		
8	50	16	26.5	Broken	...		

Comparing the two kinds of lamps, it is seen that all the vacuum lamps had fallen more than one-third in candle power while the bromine lamps fell only five per cent. The current in the vacuum lamps had also diminished, showing an increase in resistance of the filaments of over five per cent.

Two others of the same lot of 50-volt lamps, one vacuum and one bromine, were run for five minutes at 90 volts, at which pressure the light was of a dazzling whiteness.

The results are given in Table II.

TABLE II.

	Lamps, New.			After Five Minutes at 90 Volts.		
	Volts.	C. P.	Current.	C. P.	Current.	
Vacuum..	48.2	16	26	6.9	25	Greatly Blackened.
Bromine..	51	16	27	12.9	26.1	Not Blackened.

In this experiment also, the vacuum lamp shows a very great loss in candle power as compared to the bromine.

It may be said that these experiments were performed under abnormal conditions. They were, to save time. But if the bromine will, when the filament is carried to such an abnormal incandescence, preserve to such a marked degree the illuminating power of the lamp, so much the more should it do so when the incandescence is normal.

But to remove any doubt as to the effect under normal conditions I have obtained from Mr. Waring the results of a life test in progress at the Waring factory, and with his permission I embody them in this paper. The results as furnished me give the individual readings for eight vacuum and eight bromine lamps all identical in material and construction up to the point of exhausting the bulbs, when eight of the lamps were taken at random to be exhausted as vacuum lamps, and the remaining eight filled with bromine vapor in accordance with the regular process pursued in the manufacture of the "Novak" lamp.

As these lamps all had similar filaments, the bromine lamps would, if run at the same candle power, have been less efficient at the start than the vacuum lamps. The bromine lamps were, therefore, "volted" for 28 C. P. and the vacuum lamps for 25 C. P., and each lamp marked with its appropriate voltage. The bromine lamps varied from 52.4 to 55.8 volts, and the vacuum lamps from 50.5 to 52 volts. During the run the pressure was maintained for the bromine lamps at 54.15; for the vacuum lamps at 51.3. At approximately 200, 400, and 600 hours from the beginning, the candle power of each lamp was measured at its marked voltage. From the individual readings furnished me, I have computed the mean values which are given in Table III. on the next page.

TABLE III.

LIFE TEST OF 8 VACUUM AND 8 BROMINE LAMPS.

	Mean of 8 Lamps at 0 Hours.			Mean of 8 Lamps at 210 Hours.			Mean of 7 Lamps Remaining at 400 Hours.			Mean of 6 Lamps[1] Remaining at 625 Hours.		
	Volts.	Amp.	C. P.	Volts.	Amp.	C. P.	Volts.	Amp.	C. P.	Volts	Amp.	C. P.
Vacuum....	51.35	1.6888	25	51.35	1.739	22.31	51.26	1.704	20.17	51.38	1.698	18.18
	8 Lamps			7 Lamps Remaining			5 Lamps Remaining			3 Lamps Remaining		
Bromine....	54.16	1.768	28	54.36	1.816	28.26	54.92	1.792	29 24	55.37	1.857	29

[1] One of the 6 vacuum lamps has a very bright spot on the filament, and can only last a few hours longer.

The efficiencies in watts per candle are as follows:

TABLE IV.

	0 Hours.	210 Hours.	400 Hours.	625 Hours.
Vacuum ..	3 469	3.99	4.33	4.8
Bromine	3.421	3.49	3 41	3.54

What is the teaching of these results? Of the bromine lamps one had failed in less than 200 hours, two more in less than 400 hours, and two more, or five in all, before the end of the test at 625 hours, but the candle powers of the lamps still doing service were even higher than at the beginning. At 400 hours there had been no loss of efficiency. At 600 hours the efficiency had only fallen about 3 per cent., and even this was partly due to the fact that these three remaining lamps were, at the start, below the average efficiency. The vacuum lamps on the contrary had failed in candle-power nearly 20 per cent. at 400 hours, and 27 per cent. at 625 hours. In efficiency they had dropped from 3.47 at the start, to 4.33 at 400, and 4.8 at 625 hours.

Although only one lamp had failed, those vacuum lamps *were all practically dead at 400 hours*, and in comparing their useful life with that of the bromine lamps, they should be so considered. But in whatever way the comparison is made, the bromine lamps in this test will appear as the better lamps, and yet up to the point of exhausting the chambers the bromine and vacuum lamps were precisely alike.

These results do not accord with the generally accepted belief as to the effect of a gas in the lamp chamber. Again and again

I have been asked in reference to the claims for the "Novak" lamp:—"*Is* it true that they maintain their candle power?" and, second, with considerable skepticism thrown into it:—"Well, how do you account for it?" This means, that I must submit overwhelming proof of the alleged facts, or I must account for them. I do not object to this. It would be my own position in a similar case.

I remember once, some years ago, an inventor of a new dynamo asked me to come and see it and give him an opinion of it. When I reached his shop he, unfortunately, could not show it in operation, but he assured me that his dynamo was a most wonderful machine, several times as efficient as any other known. He told me that, running it by means of a little engine that could not by any possibility give more than two horse power, and which also ran his shop, he was able to run thirty-two Edison 16 C. P. lamps of the old eight-to-the-horse-power type, to full incandescence. It seemed to me it must be a wonderful dynamo to develop four horse power from two. I asked him how he accounted for it. He could not explain it very clearly, at least I could not get the force of his explanation, and, though I saw the engine, and the dynamo, and the lamps, I expect I came away somewhat skeptical as to the performance of that machine.

Now I hope to be able to convince you before you go away to-night that the facts claimed for these bromine lamps are fully in accord with other known facts, even if they do not seem to accord with previous theories as to the action of gases in the lamp chamber.

I may say that the *fact* that there is less blackening in a lamp that contains some residual gas is no new discovery. It was noted early in the history of the incandescent lamp. Edison noted it and made it the subject of a patent in 1883. Bernard S. Proctor, in 1883, states that the manufacturers of Swan lamps had found "that these carbon deposits which occur when a lamp is overheated occur even more in lamps with the highest *vacua* than in those less perfectly exhausted."[1]

Other observations pointing in the same direction have been made from time to time, although the true bearing of the observations do not seem to have been recognized. Professor Thomas, in a paper read before this INSTITUTE, detailing the results of his life tests of various makes of incandescent lamps,[2]

1. *The Electrician*, London, vol. ix., p. 603.
2. TRANSACTIONS, vol. ix, p. 271.

states that he found the lamps exhausted by metallic pumps to blacken less than those exhausted by mercury pumps.

Professor Thomas suggests no explanation, but various references to his paper seem to take it for granted that the difference is due to the presence of mercurial vapor in the lamps exhausted by mercury pumps. Mr. E. E. Cary, in a note "On the Blackening of Incandescent Lamps," takes this view.[1]

I do not know of any foundation for the belief that mercury vapor causes the blackening, or helps to cause it. So far as I know, no one has ever demonstrated that mercury vapor exists to any extent in the chamber of an incandescent lamp. It *cannot* exist at a pressure greater than about 1–15,000th of an atmosphere unless the temperature of the mercury is much above that of the pump room.

Since the degree of vacuum possible by means of a mercury pump, is limited by the tension of the mercury vapor, and since, as long as any gas is being removed, the flow is from the lamps through long narrow passages toward the pump, I do not see how it is possible for vapor of mercury in any quantity to find its way back from the pump to the lamp. I believe, notwithstanding Mr. Cary's statements in the article cited as to the degree of vacuum obtained by mechanical pumps, that the greater freedom from blackening in lamps exhausted by them, is due to the poorer vacuum, and that this is another case of blackening prevented by the presence of an inert gaseous atmosphere within the lamp chamber.

Now let us consider the question: *Why* should the presence of a gas lessen the blackening of the lamp? To answer this question we must first consider the cause of the blackening.

The coating consists wholly of carbon, no metal deposit ever being found except when the filament breaks near its junction with the leading-in wires, so forming an arc which vaporizes the metal, the vapor formed depositing upon the glass as a metallic coating. But as this coating is only formed at the moment of rupture of the filament, it has nothing to do with the true "age-coating" with which we are dealing.

The carbon coating is uniformly distributed within the bulb. [2] The carbon forming the coating must come from the filament.

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1. *Elec. Eng.*, vol. xiv., p. 118.

2. See Age-coating in Incandescent Lamps. Prof. E. L. Nichols. *Am. Jour. of Science*, xliv., 277.

Is it taken from it by chemical action? Is it worn off by attrition by the residual gas? Is it thrown off by electrical action? Is it thrown off as a vapor in consequence of the high temperature?

Explanations of the phenomena have been based upon all these different agencies.

It has been supposed that the oxygen remaining in the chamber, combined with the carbon forming carbon monoxid or dioxid which was dissociated by contact with the cold glass, leaving the carbon as a deposit, while the oxygen returned to the filament to combine with more carbon, and so on until the filament was destroyed. This, of course, cannot be, since the oxygen compounds of carbon are only dissociated at a very high temperature, far above that at which they form, and, therefore, far above the temperature of the filament.

The suggestion that the carbon is worn off the filament by air-washing and settles upon the walls of the chamber, never seemed to me to account for the continuity and uniformity of the coating, and in the light of the facts demonstrated by the experiments I have described to-night, the theory of blackening by air-washing is untenable, since there is *less* blackening while there must be more air-washing in the bromine lamps.

The carbon must then be thrown off from the filament by the electrical action, or by the high temperature, or by both combined. *In either case it is gaseous carbon*, which will return to the solid form wherever it comes in contact with matter at a sufficiently low temperature to condense it. This it will surely do when it reaches the walls of the chamber, if not before. I repeat that, whether the high temperature or the difference of electrical potential is the cause of the separation of the carbon from the filament; the carbon that leaves the filament is gaseous carbon. I expect this statement will be questioned, and I, therefore, state as fully as possible the considerations that lead to this conclusion. It is objected that carbon cannot be vaporized except at enormously high temperatures. Also, that in the lamp the particles of carbon are projected in straight lines which shows the action to be a "Crookes tube effect." What is a Crookes tube effect? It is a radiation of the molecules of a highly attenuated gas from one of the electrodes of a vacuum tube, the exciting cause being a great difference of electrical potential. Possibly, as claimed by Schuster and others, these gaseous molecules are separated by the electrical forces into still smaller elements, but whether they are or not, will make no difference with the bearing of these phenomena upon the question under consideration.

In an ordinary vacuum tube, the passage of the electric discharge produces a light extending from electrode to electrode wherever these may be placed. As the vacuum is improved, a dark space will appear around the negative electrode. Apparently the molecules are repelled so strongly that they are able to drive back the body of the gas to a certain distance, but the inevitable collisions finally take up their rectilinear motion and convert it into the vibratory motion of luminosity. As the vacuum is still further improved, the dark space becomes wider and wider, until it reaches the walls of the tube; the molecules, finding no insurmountable obstruction in their path, continue their rectilinear motion until stopped by the walls, where the impact produces a phosphorescent light. An obstruction within the tube intercepts the molecules in whose path it falls, and casts a shadow upon the walls beyond, demonstrating the rectilinear motion. The rectilinear motion can also be demonstrated in many other ways. But what does the rectilinear motion indicate? Nothing, except that the molecules have been projected through a space from which all obstructions have been carefully removed. I said *all* obstructions, but that is not quite true. There are still millions upon millions of molecules in the most perfect vacuum in our power to make. The flying molecules projected from the negative electrode must make their path through these, and they can only do so when projected with considerable force. For the success of these experiments a great potential difference is necessary,—a potential difference measured by tens of thousands of volts,—much greater than is needed for producing the phenomena of the ordinary vacuum tubes.

But, for the rectilinear motion and all the phenomena dependent upon it, we need only a sufficient propelling force, and a space sufficiently free from obstructions. The propelling force need not be electrical, and the rectilinear motion *per se* is no sufficient evidence that electrical force was the exciting cause.

In the old Edison lamps the filament was copper-plated to the platinum wires. When a break in the filament occurred near the junction, the arc vaporized the copper and covered the bulb with a coating of metallic copper, except that a line of clean glass was often left on the side opposite the break, the line being the shadow of the unbroken leg of the filament- This is described by Dr. J. A. Fleming[1], who says that he has never seen this shadow except in the *copper* deposit, that it is never seen in the deposit of carbon. To quote his words: " Hence there must " be some essential difference between the vaporization of the " carbon and that of the copper. The carbon deposit resembles " more the condensation of a vapor and is uniformly distributed, " but the copper deposit exhibits the character of a molecular radia- " tion or shower taking place from a certain point." He adds: " The whole phenomenon calls at once to mind the beautiful re- " searches of Mr. Crookes with vacuum tubes. Here, however, " we are dealing not with an induction coil discharge, but with a " comparatively low potential." Let us analyze the phenomenon. The filament breaks at the copper junction, and an electric arc is formed, in which copper is vaporized with comparative ease. A large volume of copper vapor is formed almost instantaneously in a space almost devoid of other matter. The sudden expansion of that vapor is sufficient cause for the projection of the molecules in straight lines to the walls of the chamber. It is not necessary to assume any refined electrical forces to account for the rectilinear path. The plain old-fashioned, unpretenious vapor " tension " that bursts our steam boilers is all-sufficient to account for this rectilinear projection across the lamp bulb, when there is nothing in the way.

I might quote many references to show that the carbon deposit never shows a shadow; and Mr. Proctor in the note already cited, states that a *platinum* deposit formed under precisely the same conditions as the copper deposit described above, never shows the shadow. This, no doubt, is due to the much lower tension of the platinum vapor produced at the temperature of the electric arc. The so-called Edison effect, which consists in a derived current through an outside circuit between one of the terminals of the lamp and an idle pole placed between the two legs of the filament, has been held to demonstrate a rectilinear movement of the gaseous matter within the bulb. W. H. Preece in a paper read before the Society of Telegraph Engineers and Electricians,[2] describes experiments which he claims prove the rectilinear character of the motion, but either

1. *The Electrician*, (London,) xi., 65.
2. *The Electrician*, (London), xiv., 436.

there is some error in the publication of his experiments, or they do not bear out his conclusions. The paper records the details of a number of experiments. In Exp. 6, the idle pole was placed in a long tube attached to the lamp bulb opposite the filament near the bend, and extending in a direct line from the filament. At a very high incandescence a very feeble current was detected.

In Exp. 7, the idle pole was in a tube sealed to the lamp bulb at the top, and then bent at a right angle so that the gaseous molecules which carry the current would have to turn a right angle to reach the idle pole. *The current developed was nearly the same as in Exp.* 6, showing that the molecules *may* turn a right angle. This effect has been relied on more than any other, perhaps, as demonstrating that the carrying of the carbon from the filament to the walls is a "Crookes tube effect," but it only shows that an electric current flows through the gas within the chamber. It does not show that the vehicle is carbon in vapor or in any other form. Indeed, the carrying of a current through a gas seems to be an electrolytic action, the molecules of the gas being separated into positive and negative ions, which convey the current exactly as in a liquid electrolyte. I cannot see that the fact that a current is carried from the positive to the negative heel of the filament, *explains* the deposit of carbon. Even if it is carbon vapor that carries the current, the fact that no current is apparent except at high incandescence would indicate that at a lower incandescence there was too little vapor. But whether it is carbon vapor that carries the current or not, and whether the motion is in right lines or not, I do not see that this experiment has any bearing upon the formation of the carbon deposit on the walls of the chamber. As I have said before, I attach no significance whatever to a motion in right lines. There is nothing peculiar or mysterious about it. Newton's first law says: "Every body continues in its state of rest or motion *in a straight line*, except in-so-far as it may be compelled by force to change that state." All you want is a force to put your molecules in motion, and then keep every thing else out of the way and they will move in straight lines. All that the rectilinear motion demonstrates is the absence of interfering forces; it does not in any way indicate the character of the force that imparted the motion. We must look farther in order to determine that. Probably in the Crookes tubes the force that projects the molecules is largely electrical repulsion, but even here it is open to question. We know that under suitable conditions, the material of the electrode is volatilized and the vapor deposited on the walls of the chamber or other surface within it. Prof. Wright, some years ago,[1] investigated the formation of these coatings and produced in this way beautiful metallic mirrors. Prof. Crookes, in a paper read before the Royal Society, June 11, 1891,[1] upon "Electrical Evaporation," describes experiments to determine the increase in the amount of vapor formed under the influence of the electric discharge, as compared with that produced by heat alone at the same temperature. Water shows a decided increase in evaporation when connected to the *negative* pole of the coil, and metals vaporize at temperatures at which they would not under ordinary conditions vaporize at all.

Prof. Wright, in a private letter to me, states that to obtain his metallic mirror deposits, it was necessary to use a small wire one-fourth of a millimetre or so in diameter for the electrode, in order to concentrate the electric action. This was frequently heated-red hot. The best vacuum for the purpose was $\frac{1}{300}$ to $\frac{1}{600}$ of an atmosphere. With a higher vacuum, the metallic vapor spread out and deposited all around on the vessel instead of being confined to the object to be covered. With the proper vacuum, and a coil capable of giving a spark of four or five centimetres in length, the discharge near the electrode is dense vapor, which gives usually the characteristic metallic spectrum, and in this the surface to be coated must be placed. If placed too far from the electrode the deposit is sooty. In this experiment the electric energy vaporizes the metal partly by raising the temperature of the electrode, and partly by the direct action of electric forces. The vapor formed is hot enough, that is, the molecules separated have motion enough to give the light required for the characteristic spectrum. The tension of the vapor is sufficient to force back the gas, and almost exclude it from the space near the electrode, further away the vapor mixes with the cooler gas, the vapor molecules give up to the gaseous molecules some of their motion, and then coalesce into a metallic dust that gives a sooty deposit on a surface in that region.

The case is exactly that of the vapor issuing from a tea-kettle under atmospheric pressure. The vapor forces back the air and

1. *Am. Jour. of Sci.*, xiii, 49, and xiv, 169.
2. *The Electrician* (London), xxvii., 197.

at a short distance from the spout is pure aqueous vapor. A little further away it mixes with the cool air and condenses into a cloud of *liquid* particles.

But in all these cases where vaporization is aided by electrical forces, the results are obtained by the use of great differences of potential—thousands of volts; while in the incandescent lamp the difference is never much over one hundred volts.

And, moreover, all incandescent lamps, whether made for 110, 50, 30, or 7½ volts, blacken when run at an abnormal voltage. The blackening depends upon the *temperature* of the filament, and not upon the absolute potential difference in the lamp. Surely, if electrical forces played any part in the vaporization of the carbon, the effect should be greater in a 110 than in a 7½ or even a 50-volt lamp.

Let us consider for a moment the other objection to the vaporization theory, that carbon cannot be vaporized except at an enormously high temperature. What proof is there that it cannot be? The fact that carbon cannot be melted or even softened at any temperature we can produce, has been adduced as proof that it cannot be vaporized. But there are many solids that can be vaporized, although they cannot be melted.

Ammonium chloride is a notable example. *Ice* cannot be melted in a vacuum, although it will rapidly vaporize. Ice will also vaporize rapidly under full atmospheric pressure at a temperature below the melting point, if the vapor formed is kept out of the way by a current of air.

Depretz, some fifty years ago,[1] experimented upon carbon at high temperatures, first in a vacuum and then under pressure in an inert gas. In a vacuum the carbon was rapidly vaporized and deposited on the walls of the vessel, and broke before a temperature could be reached at which it showed signs of softening. Under a pressure of three atmospheres, however, in an atmosphere of nitrogen, a much higher temperature was reached, and when the carbon rod broke it sometimes bent by its own weight in the form of a letter S. He was also able to *weld* together sticks of carbon by means of the electric current, probably one of the earliest instances of electric welding.

It is a well known fact that in the electric arc, carbon exists in the form of vapor. The vapor often condenses on the cooler negative carbon, producing a sort of mushroom growth. That

1. *Comptes Rendus*, xxviii. and xxix.

it is not in the form of a finely divided solid in the arc is evident from the faint luminosity, and from the fact that the arc gives the carbon spectrum. Carbon vapor is, as shown by the spectrum, also found in the non-luminous base of a coal gas flame, but the molecules soon begin to coalesce into carbon dust which, raised to incandescence, gives the luminosity to the flame. We cannot very much increase the luminosity of a gas flame by raising its temperature by forced draft or hot blast, as the increased temperature prevents the condensation into solid dust and may destroy the luminosity altogether. Dr. Nichols[1] has pointed out that the temperature of a gas flame is not far from that of the filament of a stable incandescent lamp, and called attention to the bearing of this fact upon any attempts to improve the incandescent lamp by raising the temperature of the filament.

I think the facts and considerations I have brought to your notice, show not only that there is no reason for denying the vaporization of the carbon filament, but that there is every reason for believing that such vaporization takes place, and whether that vaporization is aided to any material degree by the electrical forces or not, it is subject to the same laws as the vaporization of other substances.

What is vaporization? Modern theories of the constitution of matter assume that the molecules of all bodies are in a state of motion. In consequence of this motion, some of the molecules at the free surface of some liquids and solids fly beyond the sphere of molecular attraction that binds them in the liquid or solid form, and when once beyond that attraction they continue on in straight lines until turned from their course by impact with other molecules or with the boundary of the space in which the body is.

These free molecules constitute the vapor of the substance. Place a lump of ice under a bell-jar perfectly void of other matter. Molecules will leave its free surface and shoot across the space in *straight lines* to the walls, where they will rebound and on their return may collide with other molecules, or may go back and join their fellows in the solid lump. But the number of molecules that escape, will exceed those that return until, if the temperature be zero centigrade, the pressure of the vapor reaches 4.6 millimetres of mercury, at which pressure the return-

1. Electric Club Pamphlets, *No.* 24.

ing molecules will exactly make up for those that escape, and the ice will no longer lose. Suppose the bell-jar had contained air. It would make no difference to the fact of vaporization. If the ice is at such a temperature that the molecules escape at all, they will still escape when the air is present, but many of them will at once collide with air molecules and be turned back, and the result will be that there will soon be so many vapor molecules in the layer next the ice, that the returning molecules will nearly equal the escaping molecules, and evaporation will take place very slowly; but all the while some molecules of the ice vapor will find their way among the air molecules to greater and greater distances, and the perfect balance between the outgoing and returning molecules will not take place until, at the same temperature, the number of the free molecules is the same as before. Suppose in the first instance, when we had the vacuum to start with, the bell-jar was kept at a temperature of say — 5°, while the ice remained at 0°, some vapor molecules would coalesce to form frost on the inner surface of the bell-jar, and since the pressure of aqueous vapor at — 5° is only 3.1, while at 0° it is 4.6 mm. of mercury, there will be a constant flow of vapor toward the walls until the lump of ice is all gone. If under the same conditions, as to temperature, the bell-jar contains air, precisely the same thing will take place, except much more slowly. If the air in the bell-jar be at — 5° the vapor molecules in their collisions with the air molecules will lose some of their motion, some of them will coalesce to form frost crystals in the air, and we might have a cloud or even a fall of snow. If our lump of ice were made the negative electrode of an induction coil the agitation of its molecules would no doubt be increased, more of its surface molecules might be driven off, and with greater velocity, they might go in straight lines to much greater distances before being deflected, but it would in no way alter the general result. The presence of air would retard both the deposit of frost on the walls, and the waste of the lump of ice.

The bearing of all this upon the action going on in the incandescent lamp is obvious. At the high temperature of the carbon filament, carbon molecules do sometimes get beyond the reach of molecular attraction. These molecules will, when once they leave the filament, if there is nothing in their path, go straight to the walls of the chamber. Because of the lower temperature of the walls, the molecules will lose something of their motion by the impact, and, after successive collisions, will have lost so much that they will coalesce and form a coating of solid carbon on the walls.

The condition here is precisely the same as that of the ice in vacuo. The free molecules of carbon constitute a true carbon vapor. Nothing that we know of carbon warrants us in deny ing this, and all we do know favors the conclusion. The maximum tension of the vapor is extremely low, equivalent to saying the velocity of the molecules is small, hence the presence of a gas of low pressure is sufficient to check the motion and cause the molecules to quickly accumulate at the maximum density near the carbon filament, when the molecules will return to it as rapidly as they escape.

But in the lamp a new condition is met with, one that would be hardly appreciable in the bell-jar in our ice experiment. In consequence of the great difference of temperature between the filament and the walls, convection currents will be set up in any gas contained in the chamber. The layer of saturated vapor near the filament will be carried away by these currents of gas, leaving room for the formation of more vapor and further waste of the filament. But there is a great difference in gases as to the formation of convection currents and it is, therefore, not a matter of indifference what gas is used in the lamp chamber. It should be a gas in which convection currents are a minimum.

Gases of great molecular weight, like bromine and iodine, possess the desired qualities, and when used in proper quantity in the lamp chamber not only prevent the blackening but retard in some degree the waste of the filament. If we could in any way, by mechanical means or otherwise, still further check the circulation of the gas within the lamp chamber, we should still further check the waste of the filament and prolong the life of the lamp.

I call your attention again to the results of the experiments with bromine lamps. Under all conditions of running at normal or abnormal temperatures they blacken less than high vacuum lamps, they also increase less in resistance, and, therefore, for both reasons their candle power is better maintained. I have shown that this is just what we should expect from all known laws of the formation and condensation of vapors. I have, perhaps, spent more time than was needful upon the question of the electrical agency in the formation and carrying of the vapor, but there seemed to be such an air of mystery surrounding the "Crookes tube effect," or "electric carrying," or passage in "straight lines," that I thought it worth while to try and clear that up, so far, at any rate, as it relates to incandescent lamps. It all comes to this, that electrical vaporization—vaporization mainly produced by the direct action of electrical forces—is only observed in connection with great differences of potential, sufficient to give sparks several centimetres long in air. The rectilinear motion is only a consequence of the freedom from interference. In the incandescent lamp the potential differences are small, the temperature is higher than that of a gas flame where carbon vapor does exist. Why, then, should it not exist in the lamp, as a result of the high temperature alone? Recognizing its formation there, and considering all the various conditions that exist in the lamp chamber, it is seen at once that the light and mobile gases might, although they prevent the blackening of the walls of the chamber, increase the waste and shorten the life of the filament, and we are led at once to the selection of a heavy and viscous gas. With such a gas in the lamp, and a properly proportioned filament, the initial efficiency may be carried as high as in the vacuum lamps and the efficiency and illuminating power will be well preserved to the end. Since the resistance of the filament does not increase, it has not the advantage of working at a lower and lower temperature as is so often the case with the vacuum lamps, the strains due to sudden changes of temperature in extinguishing and relighting the lamps are greater because of the higher temperature, and it is to be expected that the rupture of the filament will occur earlier in its life. But experience goes to show that the bromine lamp will outlast the *efficient* life of the vacuum lamp and give a more satisfactory, because a more uniform, service.

DISCUSSION.

THE PRESIDENT:—I will call on Prof. Robb, of Hartford, to open the discussion.

PROF. WM. LISPENARD ROBB:—I think I voice the sentiment of the INSTITUTE in saying that we are indebted to Professor Anthony for a very interesting paper on a most important subject. I have been more and more impressed of late with the great need of a lamp of constant efficiency, rather than of a lamp of high initial, but constantly decreasing, efficiency. All the work of manufacturers of late seems to have been devoted to getting lamps of as high initial efficiency as possible, and consequently we have lamps whose candle power decreases very appreciably from week to week, and month to month. Now, in a large room, that does not cause any very great difficulty, because after the service has been going on for some time there will be a continual renewal of lamps, and we shall soon get into a condition where we will have an average amount of light in the room. But in the application of electricity to house lighting, where we have from one to four lamps in a room, it is a cause of great annoyance to have lamps, which first give out sixteen candle power, and at the end of six weeks or two months give twelve candle power. Professor Anthony seems to have pointed out a way to overcome this difficulty. I confess the first lamp I ever saw of the Waring company's manufacture rather prejudiced me against the lamps. I remember having a few submitted to me by the Hartford Electric Light Company for test, and finding that they required about 4.5 watts per candle power. That was at the time when the Waring Electric Company was using filaments which were manufactured for use in high vacuum lamps. Before shutting down, they were manufacturing lamps which had comparatively high efficiencies. I have never seen before, the result of any tests of Waring lamps made under normal conditions, but they evidently coincide with what was expected from the tests which various electricians have made of the lamp under forced conditions. There is, however, a certain value to be attached to the tests under forced conditions. As is well known, the deterioration in candle power of an incandescent lamp is very different on a commercial circuit from what it is in an ordinary test on a battery. The deterioration is caused to a great extent by the abnormally high voltages which are furnished by nearly all commercial companies at certain times, and the forced tests certainly show the advantage of bromine lamps in withstanding this hard usage.

I fully agree with Prof. Anthony in his opinion that the blackening of the bulb is chiefly due to the evaporation of the carbon. We have three theories put forward to explain the wasting away of the carbon and the consequent blackening of the lamp. We have the air-washing theory what we might call the Crookes effect theory, and the theory of simple ordinary evaporation. Now it seems to me, if it is an air-washing phenomenon, due to convection currents, this air-washing must be nearly proportional to the temperature of the filament. If it is an electrical phenomenon, it must be approximately proportional to the voltage at which the lamp runs. Now what do we find? The most complete tests that I am acquainted with in regard to the efficiency of electric lamps, are an exhaustive set which was made at Zurich by Professor H. F. Weber, to whom we are indebted for those extremely valuable tests of the efficiency of the Frankfort-Lauffen transmission plant. I selected at random a test of what he denominates the newest Edison lamp. The lamp was marked 16 candle power at 100 volts. He states that at 100 volts and a fraction, it gave 16 candle power with an efficiency of 3.04 watts per candle power and that the filament was at a temperature of 1573° C. At 108 volts and a fraction, he gives the candle power as 25.8, the efficiency as 2.25 watts per candle power, and the temperature as 1610° C. We have there a difference in voltage of eight volts, and a difference in temperature of 37 degrees. Now it seems to me that a difference in temperature of 37 degrees in that part of the scale, that is from 1573° to 1610°, would not produce any great difference in air-washing—anything like enough to account for the difference in the rate of blackening of the bulb of the lamp when run at those two different efficiencies, at 3 watts per candle power and at 2¼ watts per candle power. Nor could a difference of 8 volts, from 100 to 108 volts, cause sufficient difference in any electrical phenomenon to cause the great difference in the rate of blackening. But it seems to me that the rate of evaporation at those two different temperatures could be very different. If we consider the evaporation of ice, or what bears more fully on this subject than any other research I know of, one by Professors Ramsay and Young on rate of evaporation of camphor under different pressures and at different temperatures—we shall find that a very slight difference in the pressure, or a very slight difference in the temperature, when you are at a certain point on the scale, can cause a very great difference in the rate of evaporation. It seems to me, therefore, that of the three explanations, that is, air-washing, the Crookes effect and evaporation, the theory of the wearing away of the carbon by evaporation pure and simple is the one which is most in accordance with the experimental facts.

THE PRESIDENT:—Gentlemen, before calling on the other members for remarks, I would say that I have two communications here, one from Professor Elihu Thomson and the other from Mr. Edward P. Thompson. Is it your wish to hear these before the rest of the discussion or subsequently?

DR. PUPIN and others:—Before.

THE PRESIDENT:—In the absence of the Secretary I will read them. This is from Professor Elihu Thompson.

PROF. ELIHU THOMSON:—The facts and reasoning put forward in Professor Anthony's paper are the more interesting to me, as his conclusion as to the cause of blackening of bulbs or "age coating," agrees with my own view of the matter, a view which I have had for several years, and which I brought out last year in a paper on the Life of Incandescent Lamps in the *Lehigh Quarterly* (Lehigh University). It has been known for a long time past that the very highest vacuum was not so beneficial to the life of lamps as a more moderate exhaustion; and Mr Edison patented the use of a residual chlorine atmosphere in about 1883. A residual bromine or iodine vapor would appear to be possibly better than chlorine, but in the same line of work, and owing to the close chemical relations of the three elements, any effect obtainable with one might be expected to be obtained in a greater or less degree from the substitution of another, or even the substitution of two others together in the bulb. A mixed bromine and iodine vapor may give even better results, or a mixed chlorine and bromine or iodine. After all, the merit of such admixtures, or even of the use of a diluent like bromine, may require some further experience to settle. It is a well known fact that incandescent lamps starting with a rather low vacuum often improve their vacuum automatically during the run. My opinion as to the cause of this improvement has long been, that the first beginning of the production of soot or separation of solid carbon at normal potentials, causes, by the separated carbon particles, absorption of the residual gas so as to improve the vacuum. Fine carbon has, as is known, a powerful gas absorbing tendency, and although the amount formed may be so slight as to be totally imperceptible in the bulb, it may still be present in sufficient quantity to have the effect noted.

The bearing of these facts on the sustained brilliancy of a lamp started with a lower vacuum is evident. If the *gas present be gradually taken up, the efficiency of light production will rise and compensate for the change in the filament as the time goes by.*

I agree with Professor Anthony, and my views were brought out in the paper above referred to, that the age coating is more evidently due to simple vaporization of carbon, than to the many causes which have been assumed for it. I quite agree with him that the carbon leaving the filament is vaporous, but I think it must condense at once just outside the filament, and not pass out into the bulb as vapor in ordinary cases. I have a 25-*volt incandescent lamp* in my possession which was accidently connected for a moment to alternating mains of large carrying capacity of 1,000 *volts potential difference.* The lamp actually survived the shock, and is in good condition with one exception. While the bulb glass is perfectly clean and clear, there exists a flocculent mass of soot which is coherent and falls about within the lamp. It is a comparatively large mass, and at first surrounded the filament as a delicate mantle, evidently formed by a dense evolution of carbon vapor condensing immediately around the carbon in the vacuous space. Here the age coating (?) was too dense to reach the sides of the bulb. The lamp, however, is a significant example of the normal action of vaporization highly intensified. The filament retains its steely lustre.

Contrary to the statement of the paper that no shadow on the bulb is ever formed, except from the metal vapor, I am confident that I have seen it in series lamps when the carbon broke, and in cases of as sudden evolution of carbon vapor as of the metal vapor from the supporting wires. In a good vacuum, the carbon vapor condensing would give particles which would move in rectilinear paths as the metallic vapors. The shadow effect is dependent on a *local* evolution of vapor, with a part of the wire or filament between the point of evolution (as the joint) and the opposite glass surface. It would not be expected where the evolution of carbon vapor is nearly uniform from all parts of the carbon filament as in the normal wear of the carbon. I am also confident that I have seen platinum shadows, where only platinum and no copper was present with the carbon.

As bearing on the statement of the paper that the carrying of a current through a gas seems to be an electrolytic action, and confirming the same, I may say that I have a lamp which has copper terminals in the bulb, and which shows attached to the copper wires, fine hair-like or leaf-like projections, evidently produced by current passing the vacuous space between the wires and carrying copper, the development resembling lead and other metallic trees formed by electrolysis of solutions. Lamps have been known to short-circuit from this cause.

I, at one time, experimented on arcs formed between metal electrodes in a highly vacuous chamber, and found that it was easy to coat objects with metallic shining mirrors—silver giving a very bright coating. With a large current the effect is instantaneous.

The paper states that " The fact that carbon cannot be melted, or even softened, at any temperature we can produce, has been adduced as proof that it cannot be vaporized." This is not strictly a correct statement. Carbon can readily be softened and bent at temperatures approaching that of the positive crater of an arc lamp, as I have proved conclusively. I have some bent sticks in my possession made from straight sticks of five-sixteenths carbon. The carbon in the crater of a large arc is always soft and putty-like. As a matter of opinion, I think that carbon will readily melt when subjected to the highest incandescence surrounded by inert gas at very high pressure. I am not aware that the experiment has yet been tried under extreme conditions.

I think no one now doubts that the arc stream, or flame between the carbons of an arc lamp, is due to carbon vapor and not to carbon particles evolved from the crater surface.

I am glad that Professor Anthony has put forward in such a clear and comprehensive manner, the arguments showing that slow evaporation of solid carbon is the true cause of lamp bulb blackening. How often it is that complicated theories are put forward to account for simple facts, when at last it may be found that such theories were never needed. Little further proof is needed to establish the fact that simple evaporation at the high temperature amply accounts for wear of carbon in incandescent lamps, and for crater emanation in arcs.

MR. EDWARD P. THOMPSON:—The author has alluded to the question as to whether any injurious action takes place between mercury and highly incandescent carbon. I made repeated and crucial experiments upon this subject, but have not heretofore published an account of them. They may, perhaps, be appropriately considered as a part of this discussion. Professor Anthony, as I remember, states that it does not seem probable that mercury vapor exists in the lamp, which during exhaustion has a direct connection with liquid mercury. One object of my experiment was to be absolutely sure that the filament was in an atmosphere of mercury, and then to turn on the current and raise the temperature to whiteness, probably 3,000°; I, therefore, put mercury into a lamp and boiled the mercury for half an hour, letting the excess of vapor pass out of the lamp. The boiling was effected by burying the lamp bulb in sand which was heated greatly above the boiling point of mercury. At first the air was crowded out, then a mixture of air and mercury, and finally, nothing but pure mercury vapor and boiling mercury remained in the bulb with the filament. The vapor could be seen issuing at a distance from the outlet, and further still, condensing upon a cold surface. The filament was, therefore, surrounded by an atmosphere of mercury. In such a state the current was passed through the filament, which did not burn up in the mercurial atmosphere, as it would have done in oxygen or many other substances which are combinable with mercury. Perhaps no better condition can ever be obtained for practically proving that mercury will not combine freely with carbon directly at a high temperature. The atmosphere was rich in mercury, being actually all mercury. The filament was at a white incandescence. The experiment was continued for half an hour, and was repeated with different lamps twenty-five times. These experiments, as thus far described, proved certainly that carbon and mercury do not freely combine; but how about a slow combustion? While the mercury was escaping, the lamps were sealed and cooled, and the mercury allowed to run into a little tube extending from the lamp, so that this tube could be sealed off, and the remaining mercury liquid thus removed from the bulb of the lamp. These lamps lasted almost uniformly only about fifty hours, because the filaments were *slowly* consumed by the mercury vapor. The condensation of the mercury had left so perfect a vacuum as to the air, that a trace of the mercury vaporized and formed an attenuated atmosphere of mercury. This result agrees with those of others made in other directions.

For example, it has been noticed by those who exhaust lamps, that it is not well to continue the exhaustion too long for the lamps do not last so long, and the reason is thought to be that when the vacuum is too complete, a point is reached where the mercury of the pump vaporizes and surrounds and destroys the filament. Mercury appears, therefore, as one of the heavy gases, which should not exist in an incandescent electric lamp.

MR. JOHN W. HOWELL:—In my discussion of Prof. Anthony's paper, I will consider first the blackening of lamps, and then the effect of heavy gases in lamps. The theory which Prof. Anthony has set forth in regard to the blackening of lamps by evaporation, has been accepted as the proper explanation of one kind of blackening by lamp manufacturers. But all blackening is not caused by evaporation as I will show later. Carbon filaments can be softened very easily. If a little weight be fastened at the end of the loop of a filament and it be run at a pressure a little above its normal, it can be bent in any shape. The filament will be just as rigid in its new position as it was before. I have seen an incandescent filament in an upright position so softened by an excessive current that it would not sustain its own weight; it wilted, so that the parallel legs crossed one another. When the current was turned off the carbon was perfectly rigid in its new position. The molecules of carbon set free by evaporation, fly from the filament as Prof. Anthony says, in straight lines They are projected from every part of the filament and do, contrary to Prof. Anthony's opinion cast shadows on the globes. The reason why they do not cast shadows in most lamps is that the filament is not all in one plane, they are twisted somewhat, so there is no part of the glass that is shielded by one leg from molecules projected from the other. But if the two legs of the filament are in the same plane it will make a shadow always. I have lamps here which show carbon shadows very distinctly. I have them both large and small; but I think the large ones are more easily seen than the small ones. That lamp [showing] has a complete shadow entirely around it; a complete line from which half the blackening is shielded. There is blackening in incandescent lamps which does not proceed from the filament in rectilinear lines and which is not caused by evaporation. I will show a lamp which has very plain evidence of blackening in it, in such a position that it could not possibly have proceeded in straight lines from the filament, because there is a piece of glass inside the lamp, the underside of which is blackened, where it is completely shielded from the carbon.

Here [showing] is a lamp which has a line which looks like the shadow of the filament, but which passes around the lamp in a plane perpendicular to the plane of the filament. I would like to have some one account for it; I am sure that I cannot.

Here [showing] is one that has several bands running around the lamp in this way. I would be very glad to have Prof. Anthony explain the blackening of this lamp.

This [showing] is an incandescent platinum lamp—it shows a well defined shadow caused by evaporation.

Blackening is not the chief cause of the fall of candle power in lamps; some lamps lose their candle power and show very little blackening, while other lamps get quite black and lose little candle power: 115 volt lamps will lose their candle power and blacken very little indeed. This loss is due to changes in the structure of the filament, or in its surface, which reduce the candle power considerably with very little blackening. The filaments of low volt lamps are much more stable than the filaments of high volt lamps; low volt lamps often show considerable blackening with very little loss of candle power.

The effect of gases in lamps is well known, and is clearly stated in patents of Edison and Scribner. Gases have not been used commercially in lamps, because the results of experiments

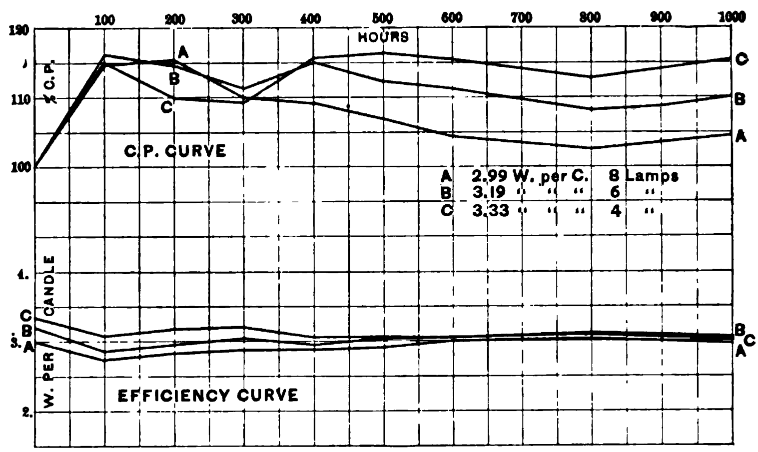

FIG. 1. Vacuum Lamps (32 C. P. 52 volts.) Tested at Lynn.

have shown, to the satisfaction of the people who made them, that the total effect of a gas in a lamp was disadvantageous. In the exhaustion of lamps, gases are very often left in the bulb, and the amount of gas in the bulb has a very great effect on the candle power curve of the lamp. A lamp which is perfectly exhausted should give a candle power curve which is a straight line. The angle between this line and the datum line indicates the quality of the filament. If a lamp is not perfectly exhausted this will not be a straight line. The effect of the residual gases in the lamp is to change the character of the candle power curve of the lamp. The vacuum will improve as the lamp is burned. The cooling effect of the gas will diminish and the candle power will rise, and then go off in a straight line. The amount of this rise of candle power will depend on the amount of gas in the globe. A rise of 30 per cent. in candle power can be produced in this way. If too much residual gas be left in a lamp, the opposite effect will be produced, the candle power will fall at first, then rise a little, and finally start off in a straight line. These effects are produced by the residual gases which are left in the lamp in its normal exhaustion, not by other gases, such as bromine. The lamps from which Professor Anthony makes his tables are 54-volt lamps of 25 and 28 candle power. Those are the very best type of lamps which can be made. A 54-volt lamp is as good a lamp as can be made, much better than a similar high volt lamp. A 32 candle power or a 28 candle power lamp is better than a lower or higher candle power lamp. In manufacturing lamps there are reasons why you would expect better results from a 50-volt 32 candle power lamp than almost any other kind of lamps. In the bromine lamps and vacuum lamps which Professor Anthony compares there are two differences. One has

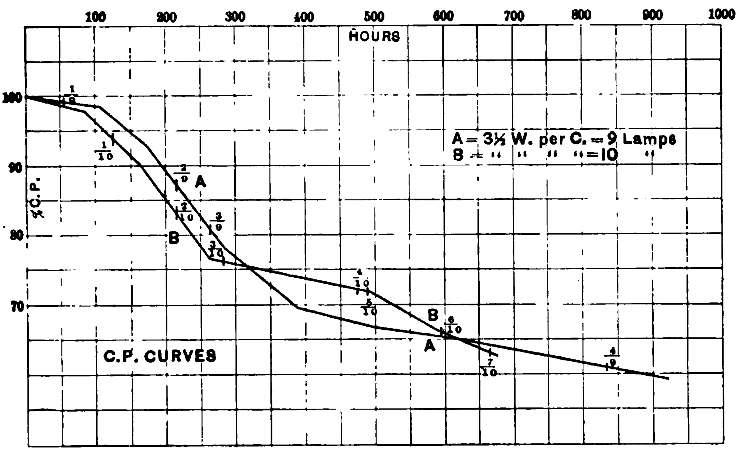

FIG. 2. Novak Lamps (16 C. P. 115 volts.) Tested at 3½ W. per candle.

an atmosphere of bromine, and the other is a vacuum lamp. There is also another difference, which is not stated in the paper, and which I infer from the knowledge of the Waring process, which was brought out in the late litigation between the Waring company and the Edison company. This difference is due to the fact that the bromine lamps are exhausted without current. The occluded gases which are driven out in ordinary conditions of exhaustion are allowed to remain in those lamps. These bromine lamps are exhausted cold. The bromine is passed through them a couple of times and then they are sealed off. The residual gases which are left in these lamps produce the variable effects I have referred to, so that in the comparison between the bromine lamps and the vacuum lamps as he made it, there are two differences and not one. We have two experiments carried on at the same time, and all the effects credited to one of them.

These bromine lamps which Professor Anthony tested which ran for 600 hours and maintained their candle power, would have been excellent lamps if they had not broken so quickly. Their breakage is very normal. But the vacuum lamps he compares with them are poor lamps. The comparison is not made between the best vacuum lamp and the best bromine lamp

I have a curve [Fig. 1] showing the results of a test made on good vacuum lamps of 52 volts and 32 candle power. The history of these lamps is as follows: When the consolidation occurred between the Thomson-Houston and the Edison companies there were two lamp factories in operation, one at Harrison and one at Lynn. In order to compare the methods in use at the two factories, lamps were made in each place and sent to the other place for test. The Lynn people sent us lamps and we tested them in comparison with our lamps, and we sent them lamps which they tested in comparison with their lamps. We

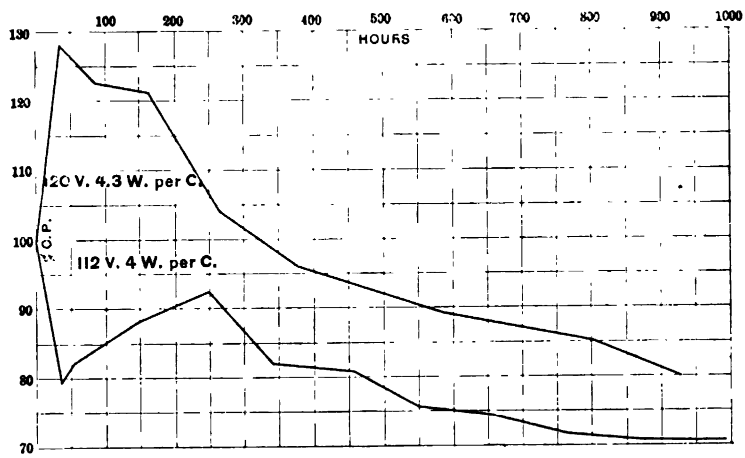

FIG. 3. Novak Lamps.

made several kinds, and I will show you the curve of the 50-volt 32 candle power lamps. The tests were made at Lynn by people who were somewhat opposed to us and who would not favor the lamps. These are good vacuum lamps of 50 volts and 32 candle power and are fit to compare bromine lamps with. They tested 18 lamps which were run for 1,000 hours and no lamp broke. The candle power is plotted in percentages of the initial candle power. The candle power at the end of 100 hours had risen 15 per cent., and at the end of 1,000 hours it was still above 100 per cent. No lamp there had fallen as low as it originally started, and at the end of 1,000 hours no lamp had broken. This is a very remarkable result and could not have been obtained from high volt lamps. These lamps are good lamps of 50 volts 32 candle power and will bear comparison with the lamps which

Prof. Anthony shows as good bromine lamps. But the vacuum lamps he tested were not good lamps.

The efficiency of these lamps is shown on these lower lines. They are practically straight lines. They started at 3 watts per candle and at the end of the test they were still 3 watts per candle. These are ideal lamps, according to Prof. Anthony, in all but one respect—they did not break. But why does the author make short life one of the characteristics of his ideal lamp?

These tests which Prof. Anthony quotes would not satisfy me if I were having tests made in a factory. He took lamps which varied three or four per cent. in voltage and tested them at an average voltage. In making lamp tests every lamp should be set up at its own proper voltage. There should be no averaging of voltages; some of those lamps were burned at a higher pressure than their normal rating and some at a lower pressure. You will find that those burned at the higher voltage than their rating broke, and those burned at a lower voltage lasted, and of the lamps which at the end were 29 candle power there was one rated originally 55.8 volts which was burned at 54.15 volts. That lamp was not burned at 29 candles at all; it was burned at 23. When they tested for candle power they raised this lamp to 29 candles. I would not be satisfied with that kind of testing. I admire one thing about this test. The pressure was held constant to 1-100 of a volt which is extremely accurate work, and not in keeping with the rest of the work done.

I have been speaking of 115-volt lamps and 55-volt lamps. I have made tests of a great many Novak lamps. During the late unpleasantness between the Waring Electric Company and the General Electric Company we bought a great many lamps directly from the factory of the Waring Electric Company. I had them tested in the factory under my supervision, but I did not test any 50-volt lamps. We generally test 115-volt lamps or thereabouts; it is the most usual type of lamp sold. It is also a hard lamp to make. I have brought with me results of some tests on Novak 115-volt lamps and you will find they are very different from curves on 50-volt lamps.

There [Fig. 2] are curves of 19 Novak lamps of 16 candle power 115 volts. They were tested at 3½ watts per candle. The candle power I represent here in percentages. They start at a candle power which is designated by 100 per cent; they do not hold constant at all. They drop right down. They also break very rapidly. The breakage is marked on the curves. At 500 hours they were half gone. It took those lamps just 250 hours to arrive at the candle power at which Prof. Anthony to-night describes lamps as practically dead. Referring to his vacuum lamps, he states that at the end of 400 hours they are practically dead. At that time they had reached 80 per cent. of their original candle power. These lamps reached that point at 250 hours and they are bromine lamps. I do not consider them dead, however. I do not agree with him at all that a lamp which has lost 20 per cent is dead. I think he is entirely wrong in saying that a 25 candle power lamp which has fallen to 20 candle power is practically dead.

In tests which I have made on bromine lamps I found quite a variation in the early part of the curves. I mean the curves which I sketched on the blackboard as the curves due to residual gases. It is natural to expect a variation in that curve because the residual gas is left in it and in combination with other gases it causes rather erratic movements. Figure 3 shows the performances of two individual lamps. These lamps belong to a lot which I set up for test which when averaged up made an average uniform curve. The two lamps started together. Their candle power is represented by 100 per cent. At the end of 35 hours one lamp had gained·28 per cent. in candle power and the other lamp had lost 20½ per cent. At the end of 35 hours two lamps which started together differed 48 per cent. in candle power. The lamp which rose 38 per cent. then fell off, and the lamp which fell 20 per cent. rose again, according to the theory which I have expounded.

Bromine gas in a lamp undoubtedly is an advantage in preventing blackening. In the lamps that I tested there was very little blackening. But I do not agree at all with Prof. Anthony that it also prevents the wasting away, or change in resistance of the carbon. At least it does not on 115-volt lamps. These 115-volt Novak lamps increase in resistance very materially—they increase in resistance more rapidly than good vacuum lamps. I do not know of any other advantage which a bromine gas or any other gas has in a lamp, except preventing blackening. It does not prevent the fall in candle power. It does, however, have some disadvantages. A gas in a lamp conducts heat, and you have got to run the lamp at a higher candle power in order to get its original efficiency. That necessarily shortens the life of the lamp, as is abundantly shown by the experience of the lamp up to date. It does not prevent the disintegration of the filament. The resistance increases very materially, and the candle power drops fully as rapidly as it does in a good vacuum lamp. It is my opinion, and I believe that all the facts which are known and which are before the public to-day in regard to the bromine lamp confirm that opinion. Indeed, I state it as a fact that the weight of evidence is in favor of the conclusion that the disadvantages of the bromine vapor are greater than its advantages.

I am sorry that the author came here with such meager data. I think he should have come here better prepared. He should have had curves which he made himself, not on a few lamps, but on a good many lamps of a good many varieties, and especially he should have presented tests on 115-volt lamps, because those lamps are far more generally used than others. It is the test of the lamp-maker's skill to make a good 115-volt 16 candle power lamp. If any man comes to me and says "I can make an improvement in lamps," I would say, "Make me lamps of 115 volts and 16 candle power." If any process, bromine or other, can produce 115-volt 16 candle power lamps which will show a good candle power curve, I will be the first man in the room to acknowledge the great advantage in the process.

MR. HAMMER:—I wish to make just a remark upon one point which Mr. Howell and Professor Elihu Thompson have already dwelt on. Sometime ago I made an examination of over 600 lamps, which had been made at various periods during the last 13 or 14 years. I have here a list of 12 of the different makers' lamps, representing 50 in all. Every one of these shows the "phantom shadow." I think it is unquestionably a fact that every lamp in which there is this blackening will show a phantom shadow, provided the carbons are in alignment, notwithstanding the statement in the paper. I have here a number of photographs of some of these "phantom shadows." Some of these photographs have been made of lamps in which the "phantom shadow" is scarcely visible to the naked eye, but photography has brought it out distinctly.

There is another matter I am requested to mention that has no connection with this subject, but it is a very important one. To-night there is a meeting of western members being held in the city of Chicago, the first which has been organized under the plan adopted in November. The meeting has probably just been called to order. The paper which has been presented here to-night is being read and discussed there. Through the courtesy of the American Telephone and Telegraph Company, the Metropolitan Telephone and Telegraph Company, and the Chicago Telephone Company, the long distance telephone line between New York and Chicago, a thousand miles in length, has been placed at the disposal of the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS, thus bringing the two meetings, although a thousand miles apart, into close communion.

The President is requested to step to the telephone and shake hands, metaphorically speaking, with the Chicago members. It is only fair in this connection to remember also that the Metropolitan Telephone and Telegraph Company, has for some time past, placed at the disposal of the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS a telephone exchange connection without charge. This is spoken of as I think it is a courtesy that the members should be aware of. Many of them do not know that they can avail themselves of it in calling up the Secretary.

DR. HUTCHINSON:—I move that the President be requested to extend the congratulations of this meeting to the Chicago meeting. [The motion was carried.]

THE PRESIDENT:—Is it your wish that I should call a Vice-President or some one to the chair so that the discussion should go on while I extend the congratulations of New York to Chicago?

Mr. Hammer:—I think that is an excellent suggestion.

The President:—Will Mr. Kennelly take the chair for a moment?

[Vice-President Kennelly took the chair.]

Mr. S. E. Doane:—At the time of the consolidation of the Thomson and Edison interests, Mr. Howell and I were very much gratified to find that we so well agreed in our deductions from tests made independently. My part of the discussion to-night will be to confirm, with all the weight that my confirmation can give, what Mr. Howell has said. We made these 52-volt 32 candle power curves. I brought them to Harrison from Lynn a few months ago. All makes of 52-volt 32 candle power lamps do not behave like these. The lamps were excellent lamps. Previous to the appearance of Novak lamps, the Thomson-Houston company tested independently for their own information, various gases in the bulbs of incandescent lamps. Chlorine, I think, was the heaviest gas we used. These lamps behaved very much like ordinary lamps exhausted to various degrees. The candle powers rose, as Mr. Howell has shown, and then fell again. In other words, the effect was exactly as if in making initial measurements you had placed a resistance in series with the lamp, and had measured the voltage around both lamp and resistance, taking other measurements, candle power, etc., as if the resistance was not there, and then had made your future measurements, and had run your lamps with the resistance removed.

The candle power of the lamp ought not to rise extremely and it ought not to fall extremely. The ideal lamp, as Professor Anthony has told us to-night, is a lamp in which the candle power is maintained practically constant. If the candle power be allowed to rise, it should not get beyond the point at which the carbon is stable. In the case of the 52-volt 32 candle power lamps, in this test it did not. With this in mind there may be no objection to allowing a vacuum slightly inferior to that which it is possible to obtain.

Concerning the blackening of incandescent lamps, it has long been a theory of the Thomson-Houston factory, and I think of most manufacturers, that this is due entirely to evaporation. Not knowing Prof. Thomson was going to discuss this paper I brought down an abstract from the article he refers to, in which he says:—"The writer has long been convinced that in a well "exhausted lamp it is due almost entirely to evaporation by high "temperature. Just as ice near its melting point and even far be- "low that, evaporates in vacua and even in dry air, so carbon, "practically fixed at low temperatures, acquires in vacua a certain "volatility at an increasing rate with the temperature"

Concerning the blackening of the lamp, as being the distillation of carbon, it is of course known that with filaments partially carbonized, the lamp bulb will blacken because of gas thrown off by completing the carbonization. But it is a fact, appreciated I think by lamp manufacturers, that the deposit made by hydro carbon has practically constant physical characteristics. Its specific resistance is practically a constant, and this being true, demonstrates that there is a chance for improvement on the distillation theory. The theory concerning the volatility of carbon seems about the only theory at present that satisfactorily accounts for the blackening of incandescent lamps.

This morning in looking over a lot of Novak lamps that had not been opened from the original package from the time they left the Novak factory, we selected and lighted a number of lamps and I give you as an indicative sign of those tested, the result on one only. It was a 110-volt 16 C. P. lamp and was run on a forced test, at 140 volts which was about equivalent in per cent. increase in voltage to the 65-volt tests shown by Prof. Anthony this evening. At the start this lamp took .757 amperes. After running five minutes it increased to .799. The candle power increased in this time from 59 to 71. This was a forced test. The cause of this was not the change in the resistance of the filament. It was not due to a depositing action, for the resistance increased a little. The change was simply due to the improvement in the vacuum which we noticed from time to time by tests on an induction coil.

Had we continued this test, we would have found that after a while this lamp fell in candles and it would, had it lasted long enough, have again reached its first candle power. Had we made only the first and last measurements, we could have said the candle power had been maintained constant throughout the run, while really it had fallen at least 17 per cent. It was the same in effect as if we had treated a 71 C. P. lamp as I instanced, that is, as if we had made the initial test with a resistance included in the wires. Of all lamps in which the candles rise at first, the rise occurs so soon that for comparison with others they may be considered to have started at the candle power and time when the candle power attains its maximum. It is highly important, therefore that we should have frequent measurements during the early life of a lamp that we may be able to interpret properly the observed facts of its later existence.

The figures shown us by Prof. Anthony are open to serious criticism for this reason, as well as for other reasons mentioned. We do not know, according to the above, the candle power these lamps really were. The results shown, are average results of quite different lamps and the maxima candles probably varied even more than the initials, which still further qualifies the figures presented.

MR. THEO. J. W. OLAN :—I should like to call your attention to some facts, one of which is that dioxid of carbon in connection with carbon at white heat will take up one equivalent of carbon and be transformed into monoxid of carbon. Another fact is that if we lead oxide of carbon through a tube that is heated to a yellowish white, there will appear as a result in a portion of the tube a carbon deposit and dioxid carbon mixed with carbon dust will pass out of the tube. I see that this can account very reasonably for this phenomenon of the blackening of the globe, just as well as all the other theories that have been advanced to-night. If we suppose that we have at the beginning pure oxygen together with carbon in the globe, dioxid of carbon would first be formed. This dioxid of carbon would, however, in its turn, combine with more carbon, and the result would be monoxid of carbon alone. But at a limited distance from the filament we have that heat which may be said to correspond to the yellowish white hot tube. At that temperature this monoxid of carbon would be transformed into carbon and dioxid of carbon. That carbon deposits on the globe and the dioxid of carbon attacks the filament again. I do not think we can accept such a theory as that the carbon filament should consist of hydrocarbons which was said in the revelation we heard to-night, simply because that seems to stand in opposition to all previous experience. I have made many tests myself, and I have read the results of other experiments also, and from those I conclude that it is not at all difficult to completely decompose by heat a heavy hydrocarbon. If we exclude the air we will have as a last result from such decomposition a substance that can be called pure carbon. One cannot at least discover any lessening in weight by heating it more and more. That tends to prove that it cannot very well be a series of hydrocarbon that remains in the filament when the lamp is ready. But in accordance with what I have before mentioned we can easily account for several of the other phenomena that have been spoken of here to-night. We heard, for instance, that where a bulb is filled with bromine gas, there will be no blackening of the glass. Why? Because bromine is one of those few elements that will not directly combine with carbon at any heat or as far as known under any circumstances. Should it not therefore be possible to conclude just from this, that the chemical action has something more to do with the blackening of the globe than has been suggested to-night? I think so. I will not say that the other suggestions cannot have any bearing on the matter. Still I think that the chemical reaction is to be considered in the first place. Now, it is a fact, that in the lamps we have generally oxygen, also nitrogen; we have finally a little hydrogen originating from moisture. I would not say it is a fact known by everybody, but at least it is stated in the handbook of chemistry written by authorities, that nitrogen at a very high temperature will combine with carbon just as well as oxygen and we have therefore in that fact another possible cause for the blackening of the globes as a result of chemical reaction. Even hydrogen, under influence of the electric current, can with carbon directly form a hydrocarbon. I think that it is too early to make a conclusion with reference to any one of these many suggestions we have had, until we have paid thorough attention to them all, and I cannot find that in the theories advanced to-night by various gentlemen there was anything at all conclusive.

MR. E. A. COLBY:—Mr. Howell in his remarks a few moments ago, said that he thought there was no question but that bromine gas reduced the blackening or deposition of carbon upon the bulb, but that he knew of no other use that could be made of this gas. I found some eleven years ago when making lamps of 125 candle power—which were lamps of relatively high potential—125 volts, that there were serious troubles, in the construction of such lamps due to short-circuiting, and the only way in which we could overcome that difficulty was either to not exhaust the lamp to the same extent that we ordinarily did a low candle power lamp, or else having exhausted the air, to leave within the lamp bulb some inert gas. This was in Mr. Weston's laboratory in Newark, New Jersey. We tried at that time a great many different gases, amongst others, bromine and several of the chlorine compounds. We finally concluded, as a result of a long series of experiments, that the most suitable vapor to use in this connection was that of chloroform. Chloroform being somewhat rich in carbon, would, of course, upon entering the bulb (the filament being at the temperature of incandescence) be decomposed, and carbon would be deposited on the filament, whilst the nascent chlorine would combine with any mercurial vapor which might be present in the bulb. As a matter of fact we obtained within the bulb quite a heavy deposit, white in color, and resembling in all its physical characteristics, mercuric chloride. This deposit was so heavy that the filament itself was at times concealed from view very much as it is in an opalescent globe. To remove this deposit we applied a Bunsen burner in the ordinary way, and immediately the deposit was vaporized and the bulb was left clear. But the important point in this connection was this, that the difficulties which we had experienced previous to this treatment, viz., short-circuiting, had entirely disappeared, and whatever blackening we had been troubled with was diminished. It was a matter of some experiment to determine what proportion of vapor it was expedient to leave within the bulb, and one simple method which I adopted at that time suggested certain lines of research which I have been unable to follow, and which I would like to mention here in the hope that some of you who are still in the incandescent lamp field can take it up. I had felt certain that there were two causes, as we all know, for the diminution in the candle power of the lamp. The first was the vaporization of the carbon, and its subsequent deposition upon the bulb reducing the transparency of the same, and the second was the change in the condition of the carbon filament itself. The vaporization, of course, is a function of the temperature to which the filament is subjected, and is to a very much less extent dependent upon the difference of potential between the terminals. However, working at higher potentials this difficulty increases. The other defect was due, as I said a moment ago, to the change in the structure of the carbon itself. Apparently a deposit of carbon was formed upon one leg of the filament, which carbon was separated from the opposite leg. I was interested in ascertaining whether I could prevent this deposit, and so maintain the normal appearance of the filament by the interposition of a shield between the two legs of the carbon. I supported a glass plate between the two legs of the carbon filament, and suspended the lamp vertically, the loop hanging down, and noted the deposition

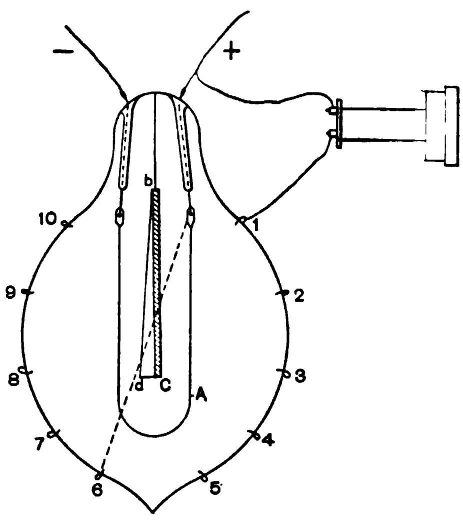

FIG. 4.—Mr. Colby's Experiment.

of carbon upon this shield between the two legs of the filament. This, of course, would be expected. But a curious feature of it was this, that the deposit was principally on one side, and that the depth of deposit over this glass shield increased from the top of the shield down towards the bottom, that is, the loop of the lamp being in this form (A Fig. 4) and the shield being down here (*b c*), the depth of the deposit was heaviest at the bottom instead of heaviest at the top where the greatest difference of potential existed. Fig. 4 represents the general outline of the lamp. It was known at that time as the Mogul lamp. A number of them were placed in the Equitable Building, 120 Broadway, in 1883 and 1884, and attracted considerable attention at that time on account of the large candle power of the lamps. They were made up to 600 candle power. The difficulty we had in the first construction of these lamps, was due to a discharge directly across from stub to stub. I then put here a glass plate (*b c*), which was supported on a neutral wire at that point, to increase the distance between the terminals and prevent any direct path between the two. The deposit that was formed upon that plate I will illustrate by a line like that (*b d* Fig. 4,) showing increase in thickness at the lower edge of the glass plate. Both legs of this filament under these conditions with the interposed shield, remained perfectly clear and as brilliant as they were in the original lamp, although with increase of temperature the deposit of carbon on the bulb was materially increased. I then sealed into another bulb, omitting the glass shield, a series of platinum wires going all around it, in the plane of the enclosed carbon, just little hooks, the wires passing through the bulb. I connected a telephone to the terminal which we will say was the positive pole and to loop No. 1, and with the lamp working at its normal candle power, after I had obtained a certain degree of exhaustion, I succeeded in getting a musical note in the telephone. By then transferring the conductor to loop No. 2, I got a note of a slightly higher pitch, and upon transferring the connection to each succeeding loop the pitch of the note correspondingly increased until I reached a point which was nearly diagonally opposite the stub + or in the line + 6, which would be the diagonal of the area enclosed by the loop, when I got the maximum note from the telephone.

As I passed along up here (7–10) the note diminished in pitch until I got up here (10) when it was inaudible entirely. By connecting the telephone to the negative terminal and the sealed-in wires, I got no sound whatever. It was always a mystery to me and it is not yet solved, for the reason that I have been out of the incandescent lamp business for several years, and I would like to know whether any one else can explain the matter. As I understood Prof. Anthony in his paper to-night, he stated that the action which produced the blackening was not purely electrical, but was in all probability simply the volatilization of the carbon alone at high temperatures. If a telephone was inserted between any two of these loops, I got no musical note whatever, indicating that there was no electrical action between those two points—no difference of potential sufficient to give a telephonic current. A peculiar feature about this experiment was this: that the lamp was on a direct current circuit, and the pulsation which would give that musical note must have been in all probability due to inequalities in the current of the dynamo. I used this dummy lamp as a means of indicating when I had secured the proper degree of rarefaction in the other lamps from the same pump. That is, this lamp was attached to the same fork by which other lamps were being exhausted, and the exhaustion was continued until I got a slight note in the telephone when I discontinued the exhaustion. These lamps, as they went out, had a residual atmosphere of chlorine gas, and possibly some little undecomposed chloroform which was subsequently decomposed in use. I made another preliminary experiment with a different construction which was known as the U tube lamp. I believe it was subsequently patented by Mr. Stanley. In it I had a U loop of the same shape as shown in Fig. 4. The object of this construction was to protect each leg of the carbon filament from any direct electric action across from the other leg. This lamp was raised to a very high temperature without the filament changing its appearance, although the blackening of the bulb was very pronounced. I am very glad that Mr. Howell has brought some samples of lamps here to-night showing the shadow cast by the carbon filament. I think any one who has had very much practical experience in the manufacture of lamps will be surprised to learn that carbon does not cast a shadow at all. It certainly was a very common thing in my experience to see lamps exhibiting this charactistic. I have succeeded in obtaining it with various lamps, and with platinum, iron and copper filaments, and I know no reason why, if the conditions are made proper, it may not be produced with all lamps. Of course if the carbon is raised to a comparatively low temperature, and not subjected to sudden changes, the volatilization will be very uniform and the shadow of the filaments will hardly be perceptible. Whereas if there are irregular or weak points in the filament or it is subjected to sudden changes in potential, then the shadows become quite marked.

Dr. L. K. Böhm:—Volatilization is due to the mechanical action of the current. The theory advanced by the gentleman who spoke before me, was, to my knowledge, put forward first by Professor Crookes, of London, about 1880, in *Nature*. He explained that volatilization is due to that trace of oxygen still contained in that trace of air which cannot be removed by exhaustion. This trace of oxygen is said to combine with a particle of carbon, forming carbon dioxid. The carbon dioxid is said to be decomposed on the inside glass walls, leaving the carbon on the inside surface of the glass; then the oxygen is said to return to act on a new particle of carbon, etc. This is certainly impossible, because there is no chemical power on the glass walls to decompose a chemical combination, and further, in the presence of such a great excess of carbon it is impossible to form carbon dioxid; only the carbon monoxid can be formed.

I accept the theory that chemical action takes place within the lamp chamber, and that some carbon monoxid is formed, but this gas will remain in the globe just as the rest of the residual gases remain there.

I further wish to say something about fusing carbon. That carbon can be fused to a certain extent, everyone of you has seen, and I am surprised that this has not been mentioned here to-night. I refer to the treating of carbon filaments. In treating the filament, the hydro-carbon in the treating apparatus is decomposed by the electric current, and we have then what the chemists term carbon *in statu nascendi* that is, the moment in which it is set at liberty, in which it is born; and in this moment it is more readily capable of entering into combination with any substance it combines with, or with other carbon atoms, forming with them carbon molecules. So we have there, practically speaking, atomistic carbon, while otherwise, what we generally see before us is molecular carbon. Of how many atoms the carbon molecule consists is not known. Some speculative scientists suppose, and I suppose with them, that the number of carbon atoms in the molecule produce the different forms of carbon with reference to their physical condition, that is amorphous carbon, graphite and diamond. If, while treating a filament this atomistic carbon set at liberty from the decomposing hydro-carbon, combines with other atoms to form a molecule, it settles on the surface of the filament in that half füsed condition referred to above. The steel-like appearance of a well treated filament is nothing more than a coating of *fused* carbon on a porous filament.

Regarding pure carbon, I might just state that this deposit of carbon on the porous filament, that is, the steel-like looking carbon is really pure carbon, the purest that can be produced.

With reference to the bromine lamp a great many experiments have been described to-night, but I want to ventilate for a moment the theory of that lamp. It is not disputed by anyone that the bromine lamps do not blacken, but how is it that these lamps remain practically clear while carbon lamps do blacken? In one of Professor Anthony's tables, it is stated that a bromine lamp lost in one instance 3.1 candles. That means, as Mr. Howell explained, that the resistance of the filament increases in a bromine atmosphere as well as in a vacuum lamp. How do we account for that now? Where does that carbon go to which is missing? I do not believe that a mere molecular change in the structure of the filament causes the higher resistance. I believe that volatilization of the filament takes place in the bromine atmosphere. Where does the carbon go to since it is not found on the glass walls? It must be somewhere. In my opinion it combines chemically with the bromine, and such a combination does exist; it is tetra-bromine of carbon. This substance has not been produced out of the elements, bromine and carbon directly, but it can be produced with the ordinary facilities of chemical laboratories. Tetra-bromine of carbon is not black, and it may be in the lamp globes in infinitely small quantities, but enough may be in there to prevent the blackening. Combinations take place in the lamp chamber: for instance, the formation of carbon monoxid has not been disputed by the other gentlemen here, and has been held up by every one who talked about it to-night. So if one chemical combination takes place another one can take place. Although this substance (tetrabromine of carbon) has not been produced from the two elements directly, it has been produced in chemical laboratories and may be produced from the elements directly by the intense heat of the electric current. This will, in my opinion, reasonably explain the freedom from blackening of the bromine lamp.

I should like to say something now about carbon particles and carbon vapors or gaseous carbon in lamps; volatilization of the filament takes place, that is a pretty well settled question, but in what form is the carbon present? Is it in the form of vapor, that is, as gas, or in a finely divided condition, that is, as particles of carbon? If we consider that it is very hard to get carbon into the form of gaseous vapor, then it is easily understood that, even if we accept that carbon gases exist near the incandescent filament, it must certainly condense like mercury vapor or any other volatilized liquid when it goes a little away from the heated filament, and then it will certainly fall as particles of carbon on the glass walls I want to be understood distinctly about this. If there is really carbon vapor, that is, carbon gas in the lamp, it can be there only in the closest neighborhood of the white glowing filament, and must certainly condense like everything else when further removed away into cooler portions of the lamp chamber, and must settle on the glass walls as finely divided particles of carbon, as carbon dust, and not as gaseous vapor.

Mr. Olan:—I am surprised to hear that dioxid of carbon could not in first hand be formed from oxygen and carbon, where there is an excess of carbon. That it does form is a fact on which the metallurgy of the iron is based. Dioxid of carbon is formed just before the blast. Later on, as this gas arises in the furnace, it is transformed by the carbon into monoxid of carbon and receives as such the reducing qualities which makes the iron from the ore. I do not think I have heard that fact denied before.

Dr. Otto A. Moses:—I have been very much edified listening to the general discussion, both from a chemical and practical standpoint. But in a matter of such importance as that, which seems to be generally recognized as fundamental, we ought to be very careful to consider all available theories of action, in order to be able to guide ourselves and others where vast sums of money have to be expended in the pursuit of some development of them. Many different views have been expressed, apparently discordant in character; and it seems to me that the time has about arrived, after fifteen years of experiments in the direction of incandescent lighting, for them all to be correlated so that the practicioner can take new points of departure in the future manufacture of the carbon incandescent lamp. It is a very important thing to suspend judgment during the progress of any invention or discovery until sufficient data have been collected, to enable us afterwards to formulate theories. The time, I think, has now arrived (and it may be of some interest for it now to be promulgated) for the advancement of one more theory which has not been referred to this evening, nor published, to my knowledge, although it was cast into shape some 14 years ago. The use of millions of incandescent lamps demonstrates the truth of the theory then propounded. The precedent is given to us by Faraday, when he said that the thing of all things that he admired most in a man was his power to suspend judgment, and he added, it was due to this power that Ampère had himself lost the opportunity of predicting phenomena which have since rendered Faraday's name immortal. When Ampère first noticed the retardation of the movement of the magnetic needle above a revolving disk of copper he could give no substantial reason for it, and, therefore, he suspended his judgment of the facts. Experiments in the hands of Faraday afterwards developed the full value of this observation.

When the first hundred incandescent lamps were made and shown at Menlo Park, the world had become very much excited over the subject we are now discussing. The lamps lived but from 50 to 100 hours each. Very few exceeded 100 hours. The globes became densely coated with a black deposit, and altogether the expectations of those who had devoted their time to the subject, were very much clouded from that ominous fact. Professor Crookes was inclined to believe that no carbon filament could last long in a residual oxygen vacuum, and in his argument revived an idea that had developed in the hands of St. Claire Deville about the dissociation of carbonic acid in high temperatures, and the subsequent deposition of carbon at lower temperatures; and many scientists were then inclined to believe that this act of dissociation would finally and inevitably lead to the disintegration and rapid destruction of the filament. Just at that time the problem of determining what was the chemical composition of that coating was placed in my hands by Mr. Edison and was investigated for several months. Those hundred or more lamps were submitted to all kinds of tests and analysis to discover what that coating was, and I am glad to say that it was possible to determine beyond all doubt that it was not, as was surmised by Professor Crookes, a deposition of pure carbon, but a hydrocarbon and a paraffin. In the beginning it was thought to be the body of the filament, itself disintegrated, projected electrically upon the inner surface. But there were some difficulties in the way of that hypothesis, and further analysis demonstrated that while approximating in composition to the filament, the substance was not identical with it. Some of the experiments have the very greatest bearing upon the subject discussed in Professor Anthony's valuable paper, because the gases chlorine, bromine and iodine were used for the purposes of attacking that inner coating in order to find out by substitution what was the composition of the pellicle. It is interesting to recognize the fact that chlorine and these other gases do not attack the body of the filament; and by the change of color and the clarification of the pellicle from a dark substance into one quite translucent, and occasionally almost transparent, it was demonstrated that this pellicle had been attacked by the chlorine, and that consequently it was not a pure carbon. Further experiments demonstrated also that in the pellicle there was hydrogen, carbon and some nitrogen. The pellicle was submitted to the action of chemically pure, dry chlorine gas. The formation of substitution compounds seemed to be almost instantaneous. The pellicle was attacked and became of a horny nature. The vacuum globes had their ends broken off after insertion into vessels containing chlorine gas. They were partially buried in snow for several days, in order to see what effect would be produced by the condensation of any liquids that might have been formed by the action of the chlorine on the pellicle. Where the heat was taken away at that particular point in contact with the snow, a concentration of action was observed to have taken place. These experiments, and quite a number of others with which I will not fatigue you, led to this conclusion in my mind, that the carbon filament is nothing but one of an infinite series of hydrocarbon compounds commencing with the most volatile liquids, or, perhaps, even with the gases, and extending up to the diamond. The hydrocarbon is invariably present. At about that time Professors Young, Barker, Rowland and Brackett were at the laboratory, and some of the carbon filaments were subjected to the most extraordinary temperatures for spectroscopic analysis of their light. One, in particular, fused at its point of rupture. While it was being gradually heated, the spectrum was observed by Professor Young. The hydrogen line was invariably present in all, except at the point of rupture of this filament, when it was thought to have disappeared several seconds before breaking. But up to that point there was no doubt in the minds of any one of the observers that hydrogen was present. That fact I shortly afterwards mentioned to M. Dumas, the permanent secretary of the French Academy, and the greatest authority at that time on carbon in the world, to whom was due the determination of the atomic weight of carbon. At that time I ventured to make the statement, that I did not believe there existed such a substance as pure carbon. He said to me, "I am very much inclined to believe it, perhaps, with the exception of the diamond." In the manufacture of carbons for incandescent lamps, the increase of temperature commencing with the heating of the organic matter in a vacuum gradually, causes distillation of the volatile matters present, until you get to the so-called carbon skeleton. Submitting that finally to greater and greater heats, you find invariably a shrinking of the carbon skeleton.

That shrinking has no absolute limit. Towards the end of the carbonization, however, when the temperatures are enormously increased, it is scarcely perceptible. The shrinking, between the temperature of the air and that final temperature at which carbons are now produced, is about one-third in bulk. So that there we have evidence of a continuous distillation of volatile portions of the hydrocarbon filament, until the limit is reached by the disintegration or, perhaps, volatilization of the carbon. If one considers this phenomenon we have at once a solution of all these discordant phenomena reported during our present discussion. The known paraffins will distill off, together with the long series of them, more and more infusible in their nature, and yet unknown and unstudied until finally a residual carbon (?) is left, which, as Professor Anthony has said, is in appearance, like the finest steel and is exceedingly hard.

An analogy strikes me here which may be of service in considering that question. We have all observed in the druggists' windows the volatilization of camphor at ordinary pressures, and we have also been inclined to believe that the deposition of the condensed vapor, formed towards the light, is connected in some way with the action of light, since it would seem that at the place where the light entered the vessel would be a greater temperature than elsewhere in the enclosing chamber. But there is a time in the night when radiation takes place, if the vessel be opposite to an open window, where the heat radiates from the vessel into space, and there will be a deposition on the inner surface of the vessel in the direction of the window, which, during the daytime, was really the point of greatest heat. In some such way we may suppose that the paraffins and all that series of hydrocarbons that are more fusible than the residual hydrocarbon left in the filament while it incandesces, would precipitate upon the colder surface of the chamber. That is made evident, too, by a study of the phantom shadows cast. This deposition of carbon is but a process of simple distillation continuously taking place. Why should it not take place at once on the first ignition, is a natural inquiry. Because the particles have been rendered semi-plastic, and have been fused throughout the mass of the hydrocarbon, and a rupture must take place in the gas occluding cells before the enclosed hydrocarbon, of lower melting point than the filament, can escape. Even if the temperature is kept constant in the filament, the wearing away still continues. There is a constant evaporation taking place and a deposition upon the colder surface of the globe.

It may be asked why these observations have not before been given to the world. They were formulated and submitted to Mr. Edison at a time when they became practically valuable, for only when the cause of this blackening was known did the future of the incandescent lamp become assured. It simply became necessary to increase the temperatures at which the filaments were carbonized to the very highest point possible, in order to prolong the life of the lamp. That method was adopted, and it was not generally known outside of that laboratory. But it was the turning point in the commercial production of the incandescent lamp. When, on discovering this fact, it was referred to Mr. Edison as of sufficient interest to be published to the world, he made one of his characteristic remarks. He said; "We are on a forced march and we haven't time to bury our enemies, or to put up tombstones over them."

THE CHAIRMAN:—If there is no further discussion of the subject, we will ask Prof. Anthony to reply to what has been said.

PROF. ANTHONY:—Mr. President, at this late hour I will touch upon a few points only of those brought out in this discussion. With reference to Professor Elihu Thomson's remarks upon the softening of carbon, I did not wish to be understood as endorsing the statement that it could not be softened. The supposed fact had been adduced as an argument against its vaporization, and I pointed out that even if it were true that melting, or softening never occurred, it was not a valid argument. A little further on I referred to the fact that Depretz had succeeded in softening, and even welding, rods of carbon more than fifty years ago.

With reference to the remarks of Mr. Howell, when I went to the Mather company, six years ago, I had considerable to do with incandescent lamp manufacture, and had observed that lamps sometimes improved in candle power for the first 50 hours or so, but this was generally, and my impression is, always, accompanied with decrease in resistance and consequent increase of current. It was not the usual result, and I always ascribed it to differences in the filaments such as might occur from different temperatures in the carbonizing furnace, or even to the different positions of the filaments in the boxes in which they were packed for carbonization. Table III. shows an increase in current and, therefore, decrease in resistance in both vacuum and bromine lamps at 210 hours, and yet the vacuum lamps had fallen in candle power.

Mr. Howell states that these lamps are poor vacuum lamps, and exhibits the results of tests that show a remarkable life and remarkable uniformity in candle power and efficiency. I can only say that I have never met with such lamps in commercial use, and remember well that when I was obliged to use Edison lamps in my own house because of the injunction restraining the Mather company from using others, I found the failure in candle power very serious. If the lamps of Table III. are *poor* lamps, they were *all alike* up to the point of exhausting, and the tests show a remarkable difference in behavior. As to the use of 55-volt lamps for these tests, as I understand 55-volt lamps formed a considerable portion of the product of the Waring factory and of the Perkins factory before it. I understood that they found there was less to fear from competition in lamps of this voltage than in those of 100 volts or above. In other words, 50-volt lamps on the market were less satisfactory than those of the high voltages. I hardly see how this agrees with the statement that *any one* can make 50-volt lamps.

As to the variation in voltage in the individual lamps of Table III. they were taken from lamps all made at the same time from the same "batch" of carbons, in order to remove any question as to the cause of the differences which it was expected would develop in the final test. They could not be volted until they were finished, and they then had to be taken as they came. Whatever difference there was in individual lamps, it was greater for the bromine than for the vacuum lamps. The bromine lamps were started also at a slightly higher efficiency, so that every advantage was given to the vacuum lamps. I fail to see how these differences in conditions affect the comparison between the two.

I wish to add that my object in presenting this paper was to point out the theory as a matter of scientific as well as practical importance. I am glad to find among those best qualified to judge, that the vaporization of the carbon is generally accepted as the cause of the blackening, but I had not found such general acquiescence before, and in the argument for the Edison company in the suit to which Mr. Howell has referred, it was pronounced absurd. As pointed out by Professor Robb, the rapid increase in the blackening due to small increase in the voltage at which a lamp is run, shows the vaporization to be due to heat rather than to electrical action. It is what we should expect from the rapid increase in the vapor tension with rise of temperature that occurs in other cases. I cannot agree with Dr. Böhm and Professor Elihu Thomson, that the vapor would condense as soon as it left the region of the filament This might be true if a gas were present to which it could give up its heat, but if the vacuum were perfect except the carbon vapor, to what would it give up its heat except to the walls of the vessel? With reference to the claim of another speaker, that gases in the chamber may serve as the carriers, I would call attention to the fact, that the presence of *any* gas in the chamber, in proper quantity, retards the blackening. There are other points I should like to touch upon, but considering the lateness of the hour will leave the matter here.

Mr. Howell:—In regard to Professor Anthony's experience with Edison lamps I have no doubt he is entirely right. You cannot expect results like those I have shown, from anything but the type of lamp upon which this test was made. If Professor Anthony has burned in his house 115-volt lamps they will not give any such results, and if I were to come here and show you these curves, and say they are characteristic curves of Edison lamps I would be deceiving you. I have shown you that curve because it is made by lamps of the same type as those tested by

Professor Anthony, and quoted in his paper. If he had produced another type I would not have exhibited those curves at all. I would have shown the same thing that he showed.

[President Houston here resumed the chair.]

THE PRESIDENT:—Before going any further the INSTITUTE may like to know that I have had the pleasure of sending the following message to Chicago:

"As President of the AMERICAN INSTITUTE OF ELECTRICAL "ENGINEERS I desire to send hearty congratulations on the suc-"cess of our first dual meeting. It gives me great pleasure to "address at once two meetings, over 1,000 miles apart, and to "know that that has been rendered possible by the advances in "that branch of engineering which it is the privilege of our "society to represent.

"The INSTITUTE has agreed, provisionally, to adopt the name "'gauss,' 'weber,' 'oersted,' and 'gilbert' for the magnetic units "of flux density, magnetic flux, magnetomotive force and re-"luctance."

I sent them word that Professor Anthony's paper had been read and listened to with great attention, and was now going into debate. The Chicago meeting sends word:

"The Chicago meeting sends congratulations to the New York "meeting, and trusts that the intercommunication thus, for the "first time, inaugurated by means of the telephone will not be "the last. It believes that considerable advance will be made "in electrical science by the idea of holding simultaneous meet-"ings in different parts of the country. It also sends word that "Professor Anthony's paper has been read and is now being "discussed."

If there is no other gentleman who desires to discuss Professor Anthony's paper, I would like to make one or two brief remarks. I would like to discuss this paper at great length, but time does not permit.

At the time the Sawyer-Man lamp came out, it was my pleasure to make some investigation and experiments in lamps containing very low vacua. The Sawyer-Man type of lamp was a lamp with a fibrous carbon filament in which instead of the ordinary vacuum produced by a pump, an attempt was made to produce a vacuum, and I believe a fairly good vacuum was made by rarefying a gas, heating it to high temperature and sending a current of nitrogen through it, and some very fair results were obtained.

While I do not wish to criticise Prof. Anthony's paper, since I know that the term that he uses is a term that is commonly employed, still I think the term is a bad one, namely "efficiencies in watts per candle." This would make the efficiency equal the watts divided by the candles, or the activity divided by the luminous energy. Now if we consider the electric lamp as a device for converting electrical into luminous energy, as it of course is, then this expression would necessarily be faulty, since it would show a higher efficiency, the poorer the lamp is as a translating device. Of course the phrase should be "candles per watt." It should be "efficiency equals candles divided by watts:" or the luminous energy divided by the activity. This, however, would not be strictly accurate in itself, since the luminous energy would require for actual practice to be multiplied by a physical constant; that is multiplied by a constant, varying with the distribution of the wave lengths in the different parts of the spectrnm. I would propose for use in this connection, if indeed it has not been used, the term "candles per kilowatt," thus making the efficiency equal to the luminous energy divided by the total energy.

PROF. ANTHONY;—I perfectly understand that criticism, and agree with the Chairman entirely. I was simply using the term as it is generally used. [Adjourned.]

[Owing to lack of time the following remarks by Mr. Moore were submitted after adjournment.]

MR. D. MCFARLAN MOORE:—Mr. President, the statement that the most important problem in connection with electric lighting to-day, is the successful production of a more efficient lamp, I do not think will be questioned. Our present knowledge seems to indicate that we have about reached the limit of efficiency in dynamo construction, and that by far the most inefficient portion of an electric lighting installation is that where the current is transformed into light, viz., the lamp. The subject under discussion to-night is primarily the blackening of lamp bulbs and the consequent decrease in efficiency. The blackening material is the disintegration of the filament, that is, the blackening is caused by the volatility of the carbon. Therefore, the apparent remedy is to get a filament that will not disintegrate and volatilize, and at the same time have high efficiency and long life, or better still get rid of the filament entirely. This *would* be an "ideal" lamp, although the ideal filament lamp described by Professor Anthony is by no means an impossibility. It is not difficult to conceive of a filament so constructed that it will rupture the moment a certain degree of disintegration (it may be very small) has been reached. A filament with a very hard and smooth surface, yet soft interior, might accomplish this purpose. That is, the filament to be so designed that it shall have but little life after its glaze has been punctured or destroyed due to the combined action of heat and electrical action, but not by "air washing."

Heat means molecular action, which, if sufficiently violent, partially overcomes cohesion, and the molecular action is then in accordance with Newton's first law. Since this process is continuous and in one direction, that is, from the filament, there is little liability of particles, after having once escaped from the cohesive forces, of returning to their original positions in which they constituted the filament.

The paper attributes the so-called "phosphorescent" light (which, by the way, seems to be a very misleading and poorly adapted name for this phenomenon) to the molecular impacts upon the enclosing chamber, but the study of the results obtained with different degrees of vacuum would seem to indicate that the most light is produced, when the enclosed gas is attenuated to a degree most suited to be thrown into a state of high vibration by the electric impulses of the current. It is also stated in the paper that a great potential difference is necessary to produce this effect, but it does not follow that a high tension current is necessary.

I respectfully refer you to my paper before this body on September 20th last,[1] wherein I suggest the construction of a lamp without a filament, the light being produced by molecular disturbance due to rapid, successive, conductive discharges of a low potential current, which are, of course, oscillatory in character; and, on account of self-induction, produce the high tension necessary to molecular vibrations of sufficient frequency to cause luminosity in the surrounding space, filled with the proper quantity of vapor or gas, and a vapor or gas best adapted to the purpose, viz., that of producing light.

The lamp of to-day cannot withstand the demand of these progressive times much longer, and a new method of electric illumination must shortly succeed it. The lamp of to-day may evolve into either a straight continuous light-giving tube of any length, or evacuated space in every conceivable form, depending upon the principle that luminous molecular vibrations generated in one portion of an evacuated space (best concealed) will travel throughout the confines of such space. It is upon these general lines that the electric lighting of the future will probably be carried out.

[COMMUNICATED AFTER ADJOURNMENT, BY PROF. REGINALD A. FESSENDEN.]

Professor Anthony's paper deals with a subject of great importance from a practical standpoint. As I understand him, he considers that the blackening of incandescent lamps is due mainly, if not entirely, to a thermal vaporization of the carbon, as distinguished from the vaporization produced by electrical means. This conclusion can hardly be accepted by those who have done much experimenting on the subject.

To mention one single experiment (due originally to Mr. Edison, I believe,) which will be a familiar one to most lamp manufacturers. If we take a small bundle of glass fibres, clean them carefully, then spread them out like a broom, and seal them in an incandescent lamp, so that the plane of the broom is perpendicular to the plane in which the filament is, and lies between

1 TRANSACTIONS, vol. x., p. 437.

the legs of the filament, we shall find, on running the lamp at its normal voltage for a few hundred hours, most decided evidences of Crookes' effect. For it will then be noticed that, while the side of the broom next the + terminal is perfectly clean. so far as the eye can tell, the side next the — terminal is covered with a thick, black deposit of carbon.

This experiment would appear to be conclusive, for if the carbon deposit were due to vaporization by heat, both sides would be blackened, whereas one is perfectly clean, and the other is coated.

Other experiments might be mentioned, but this will suffice.

It is also stated that the carbon deposit never appears as a line deposit. This is not quite correct, for though it is rare, I have seen it on more than one occasion, and so most probably have others. I conceive that the reason why it is so rare, is that the two legs of a carbon filament very rarely lie throughout their whole length in the one plane, and unless this be the case, a line deposit is, of course, an impossibility.

I would also say that the carbon deposit is not evenly deposited, though it may be so in certain cases. I have seen quite a number of lamps in which the deposit was in segments, like those of a football, but with the spaces corresponding to the seams much wider in proportion, also lamps in which there were bare spots symmetrically arranged in the centres of dark deposits. Some circumstances led me to think that these latter were caused by the presence of conducting impurities in the glass, and I endeavored to reproduce them in predetermiued forms, but failed, the theory being probably wrong.

In the writer's opinion, the phenomenon is a true Crookes effect, and the decrease of blackening is due simply to the fact that it takes a greater potential to start a negative discharge into one gas from an electrode than into another gas. For instance, it takes twice the potential to create a negative discharge from an iron electrode into nitrogen, that it does to create a discharge from the same electrode into air. Some facts in thermo-chemistry seem to throw a light on the subject, but at present there is not sufficient datum to prove the connection. It may, however, be mentioned, that if it is correct, phosphorous should act even better than chlorine or nitrogen, provided its vapor tension were high enough.

[COMMUNICATED AFTER ADJOURNMENT, BY CHARLES J. REED.]

Professor Anthony quotes Fleming and Proctor as authority for the statement that the filament never casts a shadow in a carbon deposit. I have frequently seen the shadow in a carbon deposit in large lamps having a long but rigid filament. A very remarkable and unmistakable case I remember in 1887, was an Edison 100-candle 100-volt lamp at Idaho Springs, Colorado. The lamp gave a very bright light, being evidently "volted" too high. After burning about a month the filament ruptured about an inch from the base. There was no trace of copper in the deposit, though the filament was attached by copper plating. The green color by transmitted, and the red color by reflected light, were both entirely absent, but the globe was intensely blackened by a carbon deposit everywhere except in the plane of the filament, which showed a very strong and sharply defined shadow. The shadow was very deep on the side of the globe farthest from the rupture, while it was nearly obliterated on the side nearest the rupture where the blackening was most intense. This showed :

(1.) That the shadow had been partly formed before the rupture.

(2.) That it had been mostly formed while the arc lasted at the time of rupture.

(3.) That it was formed by particles of carbon moving in straight lines, and not by particles of copper.

In regard to the evaporation of metals in vacuo mentioned by Professor Anthony, I have found that silver evaporates without the use of an induction spark at a temperature far below its melting point. If the carbon filament of a lamp be attached to platinum wires by a globule of pure silver, a bright mirror of silver (blue by transmitted light) will be rapidly deposited on the glass at the nearest point, even when large beads of glass are melted onto the wire close to the joint to keep it cool.

Meeting of Western Members at Chicago.

A meeting of the western members of the Institute was held simultaneously with the New York meeting, in the lecture room of Professor Stine, at the Armour Institute. At this meeting the paper of Professor Anthony was read, upon the invitation of the author, by Prof. Dugald C. Jackson, of Madison, Wisconsin. Through the kindness of Mr. A. S. Hibbard, the General Manager of the Chicago Telephone Company, the meeting rooms were placed in telephonic connection with the Institute rooms in New York City, and before the meeting of the Chicago members was called to order, a half hour or more was very pleasantly spent in conversation between members present at the Chicago and New York meetings. The possibility of bringing distant audiences in touch with the author of a paper, was satisfactorily established, and before adjournment, President Houston, at New York, spoke over the wire to attentive listeners in both cities, extending congratulations upon the success of the dual meeting, and informing the Chicago members that the report of the Committee on Units and Standards had been approved.

The Chicago meeting was called to order with about 45 members and guests present, by the Local Honorary Secretary, who read the INSTITUTE rule under which the meeting was held. It was stated that 20 members, the requisite number, had signed a petition, and that in accordance with the action of the Council, the Secretary had issued the call for the holding of this meeting in Chicago. Upon motion of Mr. B. J. Arnold, Mr. A. S. Hibbard was named as Chairman. Mr. Hibbard at once took the chair, and the Secretary read the following communication:

CHICAGO TELEPHONE CO.

Chicago, Ills., March 21st, 1894.

EDWARD CALDWELL, ESQ.,
Local Honorary Secretary, American Institute of Elec. Engrs.,
1432 Monadnock Block, Chicago.

Dear Sir: The use of the Long Distance telephone lines between Chicago and New York has been extended to the INSTITUTE for this evening by the Vice-President and General Manager, Mr. Edward J. Hall, of New York. Telephones have been placed in an adjacent room, and may be used by the members present in Chicago in communicating with members who are present at the New York meeting.

Yours truly,
A. S. HIBBARD,
General Manager.

The Chairman then called upon Professor Jackson, who read the paper as announced.

DISCUSSION AT CHICAGO.

The discussion was opened by Mr. Francis E. Jackson, of Harrison, N. J., who exhibited the diagrams, Figs. 1, 2 and 3, and who also presented similar arguments, and called attention to the points alluded to by Mr. John W. Howell at the New York meeting. [See p. 155 *ante*.] He was followed by Professor Stine, as follows:

PROF. WILBUR M. STINE:—The paper we have just listened to deals primarily with the "age coating" within the bulb of an incandescent lamp; it argues that this is due to simple volatilization of the carbon filament, and that the presence of certain heavy gases in the chamber more or less prevents the formation of the coating. While some points have been overlooked and others but scantily noticed, the paper as a whole is broad and suggestive in its treatment of the physical causes involved in the blackening of lamp bulbs. Much has been written on this subject, but writers have usually been too one sided in the data and explanations which they have presented. It is only when the work of many experimenters and authors is compared, that harmony is established and the true explanation becomes apparent.

The supposition, I had almost said belief, that mercurial vapor, supposed to be present in the lamp, was, somehow, the agency by which the black deposit formed, is so thoroughly explained away that it ought not again to be advanced. It has always seemed strange that so many writers caught at this straw for an explanation. I do not recall having seen any good reason advanced as to why it *should* form the coating and it is rather singular that the suggestion ever gained headway. However, E. E. Cary, for example, in an article in the *Electrical Engineer*, seems satisfied that the blackening is due to remanent mercurial vapor. As usual, he attempts no explanation and even his experiments are open to criticism and further demonstration. He further states, what is generally accepted, that the rate of blackening in mercurially exhausted lamps (equally true for all types of vacuum lamps) varies with the density, elasticity, and lack of uniformity in filaments.

A careful summary of experiments bearing on the production of the black coating, together with certain well known facts in the life history of the incandescent lamp, entirely supports Professor Anthony's statements. I will now attempt to summarize such experiments and discuss their bearing in order. In an article by Professor Nichols we find:

(*a*) That the rate of deposit is greatest in the early life of the lamp.

(*b*) The distribution of the coating within the bulb is uniform.

(*c*) No marked difference in the blackening exists between treated and untreated filaments.

(*d*) Lamps increase steadily in resistance as they grow older by use. Again from an article by L. S. Powell in the London *Electrical Review*.

(*e*) That carbons baked or flashed at a low temperature blacken the bulb more than those finished at a high temperature.

(*f*) That the film is a good conductor of electricity.

If we discard hydrocarbon theories as not sufficiently proven, the initial rapidity of deposition seems due to the less dense portions of the carbon volatilizing early in use, the rate then is also decreased by the lower temperature due to decreasing current.

That the coating is uniform, supports the gaseous carbon view, since either by diffusion or convection currents, the carbon would be evenly distributed over the entire interior of the bulb.

The fact that no marked difference exists in the blackening, whether the filament be treated or not, scarcely warrants much confidence being placed in the view that carbon is deposited by the formation and dissociation of some hydrocarbons.

It is a well known fact that the resistance of the filament steadily increases with use. The deposited carbon must, in the nature of the case, come from the filament and nowhere else, thus constantly attenuating it. But why may not the successive heating and cooling of the filament cause it to become somewhat crystalline and graphitic, and add to its resistance and liability to break-down?

Various conductivity and other tests, seem to prove conclusively that the deposit is pure carbon, but a peculiar metallic looking film which is sometimes found spread over the filament has been by some considered to be due to an alloy of carbon and lead, the lead coming from the glass of the bulb. It has not yet been proven that such an alloy is possible under the conditions present, and the layer may be only graphitic carbon mistaken for a metal. And again, if carbon baked or flashed at a lower temperature shows more marked deposits, is it not due to less density of structure, giving rise to greater ease of volatilization? The evidence in favor of the blackening being due to simply volatilization seems quite conclusive. It is sustained by what physics teaches us of the behavior of carbon at high temperatures. When heated in a vacuum it does not soften, because like NH_4Cl it passes at once into the state of vapor, but, if a gas under considerable pressure be present to prevent the flying off of its molecules, carbon softens and becomes waxy.

Horizontal filaments do bend progressively, often touching and breaking the glass, and if a filament so softens and becomes like sealing wax, a liquid of great viscosity, there are good grounds for believing that it also slowly vaporizes. Professor Anthony proves very conclusively that the presence of remanent air or a heavy gas prevents the blackening; but he attributes its action wholly to convection currents. If this were the case, the distribution of the deposited layer would not be uniform, for it is the nature of such currents to rise. The deposition would then be greater on those portions of the bulb first impinged upon, since the gas leaving the filament would be more richly charged with carbon vapor.

The uniformity of the deposit is so general, that we cannot assign the principal cause to these convection currents. Undoubtedly convection currents do exist, but they are a minor cause of the deposit, and it seems entirely reasonable to suppose that diffusion, pure and simple, of carbon vapor, through the enclosed gas, is the leading factor. This diffusion, in lamps having a high vacuum, may become, to a great extent, a Crookes action. In such lamps the production of a "shadow" shows that the freed carbon molecules are not often deflected from a straight course. But such shadows are more pronounced in the neck than in the bulb of the lamp. In the neck the Crookes effect does occur, but in the bulb it is modified into diffusion.

The distinction between diffusion and the Crookes effect is simply the presence in the one case of deflecting and impeding molecules.

In the type of lamps in which certain gases are purposely present, it would seem that diffusion chiefly, and convection currents to a less extent, were the means by which the carbon reaches the bulb. This view weakens the convection-current explanation of the permanence of the filament of the bromine lamps.

An important action of these heavy vapors has been entirely overlooked. The molecular weight of the carbon atoms is but 12 against that of bromine, which is 80. When a carbon molecule, charged to incandescence with heat energy, comes in contact with the colder and heavier bromine molecules it is so greatly cooled by the interchange of energies that it can no longer exist in the state of a vapor, but is redeposited as a solid on the filament. The heated bromine molecules rapidly dissipate their increased energy and return cooled for a fresh impact. This view is supported by the fact that carbon volatilizes in a vacuum, but, when under gas pressure, simply softens. This type of lamp is capable of much improvement, for but little, comparatively, has been done in this direction. The tendency would be to search for the heaviest possible transparent gas, not decomposed by high temperature, and to use it under relatively high pressure. It would be queer indeed to see incandescent lamps pass from the vacuum type to the other extreme of high pressure of a contained inert gas. Further, the contained gas should be without effect on the filament. That the filament in the bromine lamps is short lived seems to indicate a chemical combination with the carbon at a high temperatures.

Further discussion was carried on by B. J. Arnold, Ludwig Gutmann, Mr. Kammer, and others.

Professor Jackson then answered a number of questions, and closed the discussion on behalf of the author, with the following remarks:

PROF. DUGALD C. JACKSON:—We have here a remarkable paper with which to inaugurate the meetings of this section of the INSTITUTE, and I am most happy to have been designated by Professor Anthony as his representative, and to have received your invitation to read the paper before you. The conclusions of this paper clear up in a straight-forward scientific manner several of the mysteries of the incandescent lamp. Some of the conclusions may not be fully borne out, but the discussion shows them to be correct in the main.

There are a few points in the discussion of Mr. F. E. Jackson (the other Mr. Jackson) which deserve much fuller consideration than we can give them at this late hour. Mr. Jackson has shown us some life and efficiency curves of Edison lamps which evidence the wonderful perfection of the incandescent lamp filaments now manufactured by the General Electric Co. I think we owe Mr. Jackson our most cordial thanks for traveling all the way from Harrison, N. J., to Chicago to show us the curves. The evidence presented, however, does not prove that these filaments give as good results in vacuum lamps as might be obtained from them in bromine gas lamps. The curves which Mr. Jackson presents as representing Novak lamps no doubt correctly represent the lamps tested, but my observation shows that there is likely to be considerable irregularity in the lamps produced in the smaller factories. This is a result which may be expected, on account of the refinement required in the manufacture of filaments. My observation shows that the gas lamps (Novak lamps), in the main, retain their brilliancy throughout their life more satisfactorily than do vacuum lamps on the same circuits. On the other hand a few of the Novak lamps perform more poorly than their vacuum brethren.

There is indeed a weakness in Table III, as pointed out, but eliminating that weakness does not, I think, vitiate Professor Anthony's conclusions. Neither does the fact that a carbon shadow is sometimes found. The shadow is sometimes very well marked in the carbon coating. This simply shows that the carbon molecules which leave one leg of the filament and are deposited on the opposite side of the bulb, do not come into collision with other molecules sufficiently often to be swerved materially from straight paths. The part of the bulb which is in the plane of the filament is therefore partially shaded from the "molecular shower" by the filament itself. Upon the explanation offered in the paper, the shadow in the carbon deposit can only occur in lamps with a very high vacuum, which agrees with the statement made by Mr. F. E. Jackson. The argument that the carbon deposit is made by molecular deposition is strengthened by the fact that the deposit cannot be analyzed under a very powerful microscope. That is, the deposit seems to be perfectly smooth or homogeneous when viewed under the microscope.

At the close of the discussion of the paper, a vote of thanks was tendered to Mr. Hibbard, as the representative of the Chicago Telephone Company, and to Mr. Hall, as the representative of the American Telephone and Telegraph Company, for the use of the telephone lines, and also to Professor D. C. Jackson and to Mr. F. E. Jackson for their interest in coming such long distances to attend the meeting. Professor Stine and the Armour Institute, which had placed such admirable facilities at the disposition of the local meeting, also came in for a very hearty vote of thanks for their hospitality.

It was stated by Professor Stine that the electrical and scientific journals containing the articles to which reference had been made by Professor Anthony in his paper, were upon the desk for consultation by any who wished to refer to them.

Professor Stine, B. J. Arnold and Edward Caldwell were appointed a committee to devise means to pay local expenses and to arrange from time to time for the holding of meetings.

The meeting then adjourned.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

NEW YORK, April 18, 1894.

The eighty-sixth meeting of the INSTITUTE was held this date at 12 West 31st street, and was called to order at 8 P. M. by President Houston.

The Secretary read the minutes of the last meeting which were approved.

The Secretary read the following list of associate members elected and transferred at the Council meeting in the afternoon:

Name.	Address.	Endorsed by.
BEST, A. T.	Electrical Engineer, Hotel Ponce de Leon, St. Augustine, Fla.	W. G. Whitmore. Chas. D. Shain. Aug. Noll.
CARUS-WILSON, CHARLES A.,	Professor of Electrical Engineering, McGill University, Montreal, P. Q.	T. C. Martin. Joseph Wetzler. L. Stieringer.
COLVIN, FRANK R.,	Treasurer and Business Manager, *The Electrical Engineer*, 203 Broadway, New York City.	James Hamblet. Geo. M. Phelps. A. E. Kennelly.
GEORGE, JOHN C.,	President, Raleigh Electric Street Railway Co., Marine Bank Bldg., Baltimore, Md	Louis Duncan. Samuel Reber. S. W. Huff.
GERRY, JAMES H.,	Superintendent, The Self-Winding Clock Co., 163 Grand Ave.	Edward Durant. James Hamblet. Geo. A. Hamilton.
GLADSTONE, JAMES WM.,	Manager, Edison Mfg. Co., West Orange, N. J.	T A. Edison. A. E. Kennelly. Edwin J. Houston.
HOBART, HENRY M.	Engineer, General Electric Co., Schenectady, N. Y,	Ralph W. Pope. F. W. Tischendoerfer. C. P. Steinmetz.
HOOD, RALPH O.,	Electrical Engineer with General Electric Co., 180 Summer St., Boston, Mass.	A. E. Kennelly. Elihu Thomson. Caryl D. Haskins.
HUBBARD, WILLIAM C.,	Electrician, Royal Arc Electric Co., 143 Liberty St., New York City.	Louis B. Marks. E. T. Birdsall. Chas. D. Shain.
INGOLD, EUGENE,	Consulting Engineer and Expert, Pittsburgh, Pa.	T. C. Martin. A. L. Rohrer. J. R. Lovejoy.

KEEFER, EDWIN S.,	Supt. of Electric Light Construction, Western Electric Co., 22 Thames St., New York City.	G. A. Hamilton. Ralph W. Pope. J. Stanford Brown.
MACLOSKIE, CHAS. H.,	Engineer, with B. J. Arnold, 436 The Rookery, Chicago, Ill.	B. J. Arnold Fred. DeLand. Lemuel S. Boggs.
NEILER, SAMUEL G.,	Ass't. Electrical Engineer, The World's Columbian Exposition, 4318 Berkley Ave., Chicago, Ill.	R. H. Pierce. Lemuel S. Boggs. Fred. DeLand.
PROCTOR, THOS. L.,	General Manager, Riker Electric Motor Co, Newtown, L. I., N. Y.	Philip Mauro. Joseph Wetzler. Andrew L. Riker.
SEARLES, A. L.,	Engineering Dept., The Royal Arc Electric Co., 73 Watt St., New York City.	Chas. D. Shain. E. T. Birdsall. Louis B. Marks.
TOERRING, C. J., JR.,	Electrician, Royal Arc Electric Co., 143 Liberty St., New York City.	Louis B. Marks. Franklin L. Pope. Edw. L. Nichols.
WILEY, WALTER S.,	Supt. South Omaha Electric Light Co., South Omaha, Neb.	W. F. White. Harris J. Ryan. D. C. Jackson.

Total 17.

TRANSFERRED FROM ASSOCIATE TO FULL MEMBERSHIP.

Approved by Board of Examiners, December 7th, 1893.

GREENE, S. DANA	Assistant General Manager, General Electric Co., Schenectady, N. Y.
EICKEMEYER, RUDOLF	President Eickemeyer and Osterheld Manufacturing Co., Yonkers, N. Y.

Approved by Board of Examiners, March 20th, 1894.

MORROW, JOHN THOMAS	Supt. Electrolytic Plant, Boston and Montana Consolidated Copper and Silver Mining Co., Great Falls, Mont.
JOHNSTON, A. LANGSTAFF	Consulting Engineer, Hestonville, Mantua and Fairmount Passenger R. R. Co., 4300 Lancaster Ave., Philadelphia, Pa.
CRANDALL, JOSEPH EDWIN	Electrician, C. & P. Telephone Co., 619 Fourteenth St., N. W. Washington, D. C.

Total 5.

The following applications for associate membership have been received and will be acted upon at the meeting of Council, June 20th, 1894.

A. L. Croxton, San Francisco; L. G. Lilley, Wyoming, O.; Maurice Oudin, Schenectady; Frank H. Knox, Baltimore; Paul A. N. Winand, Philadelphia; Frank W. Brady, Wellsburg, W. Va.; Geo. H. Harris, Birmingham, Ala.; Frederick L. Hutchinson, Elizabeth; Edwin H. Bennett, Jr., Bayonne; Herbert Lloyd, Philadelphia; John E. Criggal, Springfield, Mass.: Albert Scheible, Chicago; John B. Blood, Schenectady; Arthur E. Childs, Philadelphia; George Stephens, Peterborough, Ont.; Jas. P. Malia, Chicago; H. C. Eddy, Chicago; Philip G. Gossler, Brooklyn; Wm. K. Archbold, Boston; Jos. C. Mayrhofer, New York; C. C, Chesney, Pittsfield, Mass.; F. C. Caldwell, Columbus; Joel W. Stearns, Jr., Denver; George S. Bliss, Pittsburg. Total 24.

Any objection to the election of these candidates should be filed with the Secretary before that date.

THE PRESIDENT:—Have you any other communication to make, Mr. Secretary?

THE SECRETARY:—I have the sad announcement to make, Mr. President, of the death of one of our esteemed members, Dr. Franz Schulze-Berge, who is well known to the profession, and who died on the 21st of March, in Brooklyn, N. Y. A suitable obituary notice has been handed in, and will be printed in the TRANSACTIONS.

THE PRESIDENT:—There is a communication here from the Committee on Units and Standards, which I will ask the Secretary to read.

THE SECRETARY:—This recommendation of the Committee on Units and Standards is based on a letter from T. C. Mendenhall, Superintendent United States Coast and Geodetic Survey, Washington, D. C., in which he requests the support of the INSTITUTE.

NEW YORK, April 18th, 1894.

To the President and Council.
AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

Gentlemen:—Your committee on Units and Standards begs to recommend that a resolution be forwarded to Congress from the INSTITUTE urging the passage of the bill legalizing the electrical units adopted by the Chicago Congress.

F. B. CROCKER, GEO. A. HAMILTON,
W. D. WEAVER, A. E. KENNELLY,
WILLIAM E. GEYER.

THE PRESIDENT:—Gentlemen, you have heard the communication from the Committee on Units and Standards? What is your wish? What disposition of the case will you make? Will you take action on it now?

MR. TOWNSEND WOLCOTT:—I would like to ask about the question of jurisdiction—if that is a proper thing for our society to do. I think it is a very desirable thing to do, and if it is also proper I should be in favor of it.

THE PRESIDENT:—The Chair does not quite understand.

MR. WOLCOTT:—Are we authorized to do such a thing? Is the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS authorized to do such a thing?

THE PRESIDENT:—I suppose it is competent for the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS to do what they may see fit in the premises. Of course, all they could do would be to make a recommendation.

SECRETARY POPE:—I think this communication from the chairman of the committee will throw a little light on it.

THE PRESIDENT:—The Secretary will please read the letter directed by the committee to the President.

The Secretary read the following letter:

NEW YORK, April 18th, 1894.

Professor Edwin J. Houston,
President.

Sir:—Our committee has signed the appended rough draft of a recommendation to Council, which can be considered at the next Council meeting. In the meantime, however, in case of urgency arising, we suggest that the sense of the meeting be taken informally. This might also serve as a notice that formal action will be taken at the next (annual) meeting of the INSTITUTE.

Yours respectfully,

A. E. KENNELLY.

THE PRESIDENT:—If Mr. Hamilton or Mr. Crocker, or any other member of the committee would like to say anything about this matter, the Chair would be pleased to extend the courtesy of the floor to them.

PROF. FRANCIS B. CROCKER:—The desirability of doing something of this sort is unquestionable, I think. But as to its formality, possibly, some doubt might arise. Therefore, to avoid the latter question the committee suggests that the sense of the meeting be taken—to which, of course, there can be no objection—and that formal action could be delayed until the next meeting of the INSTITUTE, which will be the annual meeting, and which would certainly be proper time to take formal action. This statement will also serve as a notice that such formal action will then be taken. In the meantime the bill might come up in Congress, and it would be well if the officers of the INSTITUTE could use the fact that the sense of the meeting had been taken at a regular meeting.

THE PRESIDENT:—I see Mr. Hamilton and Professor Geyer; do they wish to add anything to what Professor Crocker has said?

MR. GEORGE A. HAMILTON:—I think what Professor Crocker has said covers the ground very thoroughly.

THE PRESIDENT:—Professor Geyer.

DR. WILLIAM E. GEYER:—Of course, anything that we may do will not bind anybody, and as Professor Crocker has said, the idea is to find out whether the INSTITUTE would approve of such action.

THE PRESIDENT:—Does Mr. Weaver wish to add anything? Does Mr. Kennelly?

MR. A. E. KENNELLY:—I have not anything to add to the remarks made by Professor Crocker.

THE PRESIDENT:—Does any other member of the INSTITUTE wish to speak on this matter? If not, what action will the INSTITUTE take on the recommendation?

PROF. CROCKER:—I move that it is the sense of this meeting that the passage of the bill legalizing the electrical units adopted at the Chicago Electrical Congress of 1893, be recommended.

MR. GEORGE M. PHELPS:—Mr. Chairman, if you will permit me, I will suggest a slight addition, to make the action possibly stronger; and that is, that this meeting recommend to the general and annual meeting of the INSTITUTE to be held in May, a more formal endorsement and recommendation of the measure. I offer this because one or two speakers suggested that this meeting give its sense on this subject, and that the general meeting in May might take a more formal action.

Prof. Crocker:—I accept that amendment.

The President:—Gentlemen, it is moved and seconded, as you have heard. Are you ready for the question?

[The motion was put and carried.]

The President:—I take great pleasure now in introducing to you Mr. I. H. Farnham, of Boston, who will read a paper on the Destructive Effect of Electrical Currents on Subterranean Metal Pipes.

Mr. Farnham:—I have had the honor and pleasure of being a member of this society for some time, though have never attended but one of your meetings, so I feel almost like a stranger; I hardly know what your customary methods are. Perhaps, as the paper is printed, a synopsis is all that you usually have presented here. It has occurred to me that as we need the room darkened in order to see the lantern diagrams, which renders it impossible for you to read the paper as it is presented; perhaps, it will be as well for me to keep pretty close to the printed paper, and this I will attempt to do.

The subject before us has frequently been discussed and sometimes it has been very poorly presented. I will give you one example. There was a meeting of engineers in a city—no matter what city—a few months ago, and as this subject of electrolysis was to be talked about I was invited to be present. A committee was to report upon the matter. The committee had been in existence for a year, and the three members of it each made a separate report. The first one had found some gas pipes in months gone by, which he believed, had been destroyed by electrolysis, but of late he was not so sure about it. He thought that there might be some doubt as to whether electrolysis was really doing any damage in that city. The next member of the committee related the fact, that in his city they had connected an incandescent lamp from the gas pipe to the water pipe in a cellar, and the current which flowed there lighted the lamp. The last one, whose remarks were really the most interesting, came forward with a specimen pipe in his hand and said, "I have something to show you, electrolysis materialized." He knew it was a case of electrolysis, because he saw it take place. He said that a trolley wire broke, and it swish-swashed sometime through the air, and finally struck a gas post, and in striking the gas post it produced "this hole in the pipe," which he showed. I don't know whether that is a sample of all these meetings and papers, but that is one at which I happened to be present. If, now, I am able to give you a little outline of the work we have accomplished in this field it may form a sufficient basis for your discussion.

Mr. Farnham then read the following paper:

A paper presented at the 86th Meeting of the American Institute of Electrical Engineers, New York, April 18th, President Houston in the Chair, and at Chicago, April 25th, 1894, Lieut. Samuel Rodman, Jr. in the Chair.

DESTRUCTIVE EFFECT OF ELECTRICAL CURRENTS ON SUBTERRANEAN METAL PIPES.

BY ISAIAH H. FARNHAM.

For the past year or more, there have been read before water, gas and electrical engineering societies all over this country, papers on the subject of electrolytic corrosion of water pipes, gas pipes and lead cables. In fact, a meeting of such societies is incomplete to-day without some discussion on this subject. It was, therefore, with hesitation and misgivings, that I considered the written invitation from the officers of the INSTITUTE, to prepare a paper on the "Electrolytic effect of currents on subterranean gas and water pipes." A prominent officer of the INSTITUTE urged that as I was undoubtedly the first to discover and satisfactorily prove that this action was destroying cables, I ought to give the society an account of my investigations and the results. On this suggestion, the promise was made to lay before you such facts as opportunity would allow. If sufficient data may be presented to form a nucleus for the evening's discussion, it will, I am sure, be of some practical value.

Early in the summer of 1891, some lead-covered telephone cable removed from wooden ducts in Boston, showed very marked yet local spots of corrosion. The cause of the corrosion was generally attributed to acetic acid contained in the wooden conduit, which had, years before, caused corrosion on a few cables in certain sections of the city. In the case just mentioned, the corrosion was so severe, and located in spots only, that it led me to attribute the cause to electrolytic action from the railway currents, and a letter was written to my company to that effect.

A few months later, the lead covering of a cable, (No. 208) resting upon the ground in manhole chamber No. 76, located at the corner of Berkeley and Newbury streets, was found eaten entirely through at the point of contact with the earth. I then felt certain the cable had been destroyed by the action of the current. With Mr. W. I. Towne, my assistant, I proceeded to prove the theory.

We took measurements between the cable and the earth, the cable having been repaired and raised from the ground, and found 1.5 to 2 volts difference of potential, the cable being positive to the

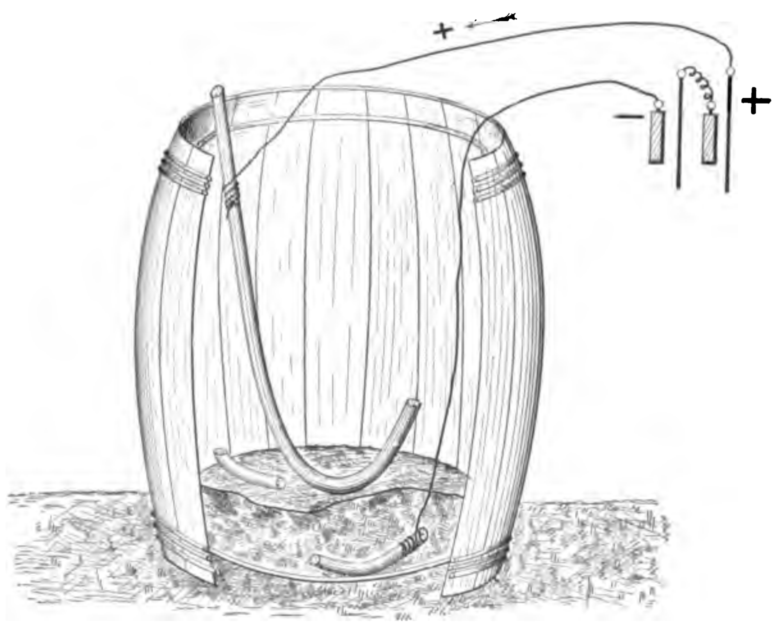

Fig. 1.

earth. A barrel of earth was procured from an excavation in the street, a metal plate placed beneath the earth in the barrel, and two short pieces of lead cable placed side by side on top of the earth. The plate in the bottom of the barrel was then connected to the negative side of a storage battery giving 4 volts potential, and one piece of the cable lying on the earth, was connected with the positive pole of the storage battery. The second piece of cable in the barrel was left without electrical connections. The earth was then saturated with water, and the circuit was closed, allowing the current to pass from battery to cable, to earth, to plate and to battery, for seven consecutive days. The pieces of cable were then examined, and the piece which had been connected with the battery found badly pitted, closely resembling the cable which had been destroyed, while the second piece of cable showed no corrosion whatever, proving conclusively that a current such as was found in the manhole, was sufficient to cause the damage that had been found, and that the corrosion was not, (in the case of the experiment at least), due to any acid or salts in the earth.

Fig. 1 shows the barrel experiment, and Fig. 2 is a photograph of the cable No. 208, which has been described as found resting on the earth in the manhole chamber and corroded through; also the pieces experimented upon in the barrel. That

FIG. 2.

shown in the center of the photograph is cable No. 208.

In addition to the experiment just mentioned, we placed in the bottom of manhole chamber No. 76, two short pieces of cable, one of which we connected by a wire to cable No. 208, which had been damaged by electrolysis. (It should be understood that the damaged cable had been repaired, and removed from the bottom of the chamber.) Fig. 3 shows the arrangement of this experiment. At the end of six weeks, the pieces of cable were removed and examined. The one which had been connected with cable No. 208, was deeply pitted,[1] while the other piece was free from corrosion, as shown in Fig. 4, which is from a photograph of them.

1. The plumbers of Omaha, Neb. apply the name of "small-pox pipe" to that pitted by electrolysis.

These experiments, with several others of minor importance, satisfied all who were interested, that electrolytic action was destroying cables, and probably gas and water pipes.

It next became necessary to prove to the electrician of the railway company, that the current causing electrolysis, was from the railway system, and not from a leak in the Edison or some other electric lighting system.

Measurements were made between the cables in all manholes, and the earth near the cables, for voltage and direction of current.

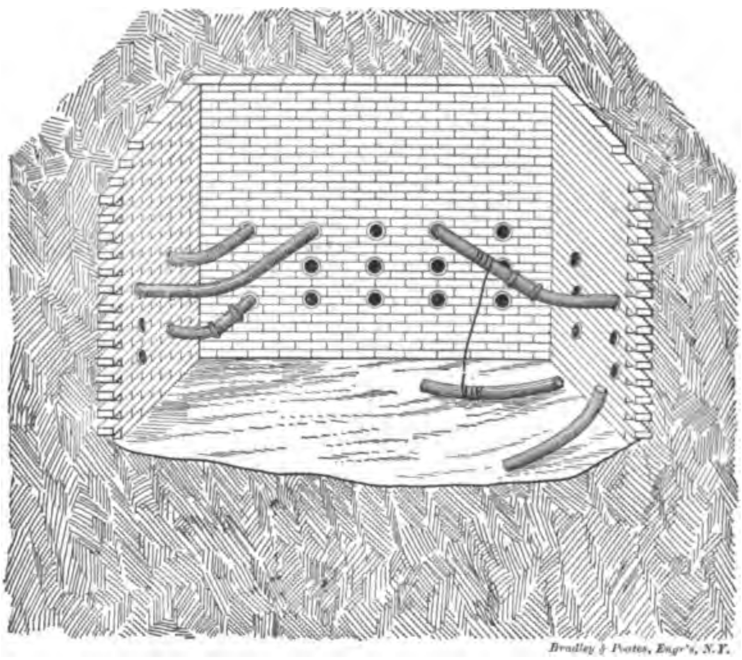

Fig. 3.

It was found that within a radius of about 2000 feet from the Albany street power-house, cables were negative to the earth, ranging from zero to 2 volts, and that outside of this neutral line, they were positive to the earth from zero to 12 volts. This condition prevailed until a point was reached near the East Cambridge power-house, when they again passed a neutral line and became more and more negative as that power-house was approached. The same conditions were found as the Allston railway power-house was approached. On obtaining sufficient data, maps were drawn, showing voltage between cables and earth throughout all sections of the city. This is shown in map, Fig. 5.

In addition to the figures placed beside the several routes of cable conduits, showing the direction of current and its pressure, we have colored red, such portion of the map where at that time we found the cables positive to the earth. We may call the red portion of the map, the danger territory. These potential measurements, though taken for other purposes, incidentally furnished all the proof needed to convince one that the railway power was the source of the troublesome currents.

At the time the map was made, and previously, the railway was

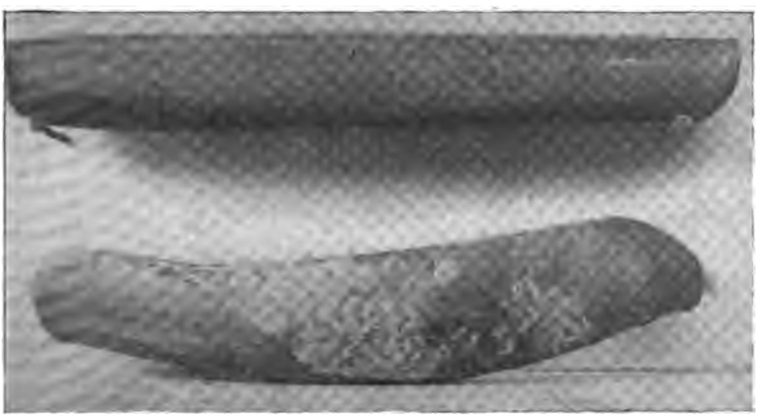

FIG. 4.

operating with the negative pole of the dynamo to the trolley, the positive side being to the rails.

Fig. 6 is intended to illustrate this condition. It shows the passage of current from the dynamo to the rails, and the passage of a portion of the current from the rails to the cables within the neutral or zero line, and from cables to rails outside of this zero line. The danger of electrolysis is only where the current is leaving the cable or pipe through the moist earth, hence the dangerous district was at this time outside of the zero, or neutral line, as shown both on the map (Fig. 5) and in this Fig. 6.

Having outlined our early experience in running down this new trouble, we will next mention some of the proposed and applied remedies. Several conferences were held for the purpose

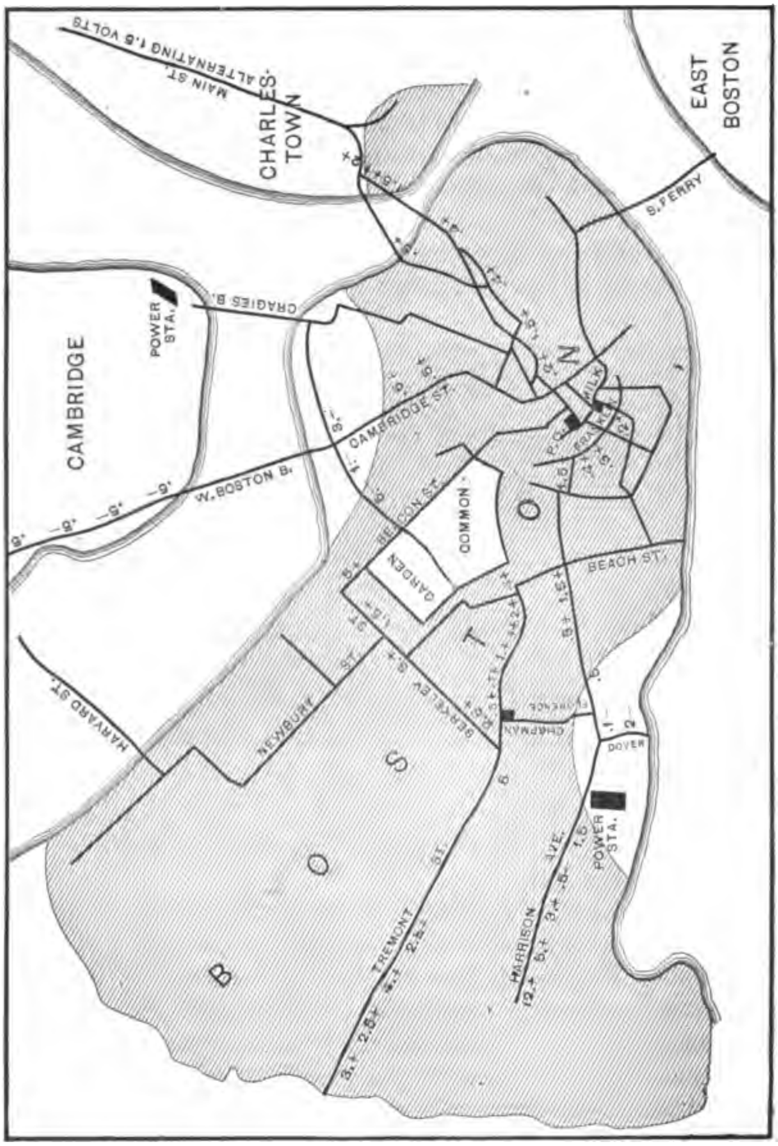

FIG. 5.—Showing where corrosion was going on when first brought to notice. The shaded portions of the map are referred to in the paper as colored red.

of suggesting and discussing means for preventing the destruction of the cables, at which the officers and experts of both the railway and telephone companies were present, and it should be said, that the railway company in Boston has shown a disposition to adopt any promising plan for overcoming the evil, save, perhaps, the abandonment of the rails and earth as a part of the circuit.

First: It was proposed to remove all cables from the wet bottom and sides of the so-called manholes. It was found very difficult to place and retain cables free from the wet sides, and even could this have been accomplished, the action at the mouth of the ducts and within them, would still have continued. They were, however, all removed from the bottom of the manholes.

Second: It was suggested that the cables might be connected to ground plates in the manholes, and so transfer the electrolytic

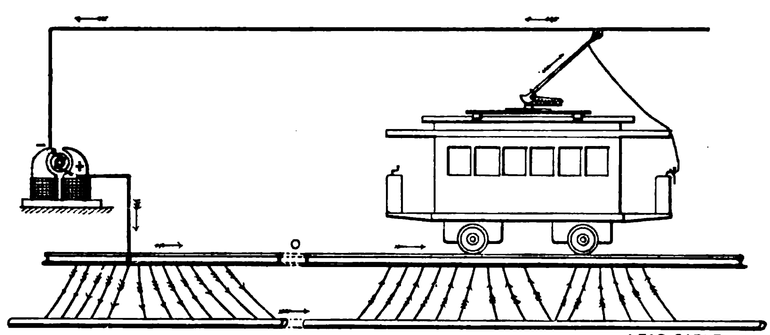

FIG. 6.

action to these plates, and thus save the cables. This experiment was tried on an extended scale, but though many ground plates having a surface of several square feet each, were connected with the cables over a large portion of the city, it was found that voltmeter readings taken between the cables and a point on the earth a short distance removed from the ground plate in any manhole, gave nearly the same pressure as before the ground plates were connected.

In some cases, the voltage between the cables and the earth was reduced 25%; in many others, no noticeable reduction was made. The ground plates were constructed from pieces of old lead cable, 6 to 10 feet in length and embedded in the wet earth at the bottom of the manholes. It was evident from this test, that ordinary ground plates would not prove of material advantage for protecting the cables.

Third: Prof. Elihu Thomson suggested, among other possible remedies, the placing of motor generators at different points along the railway line, wherever the cables and pipes are found

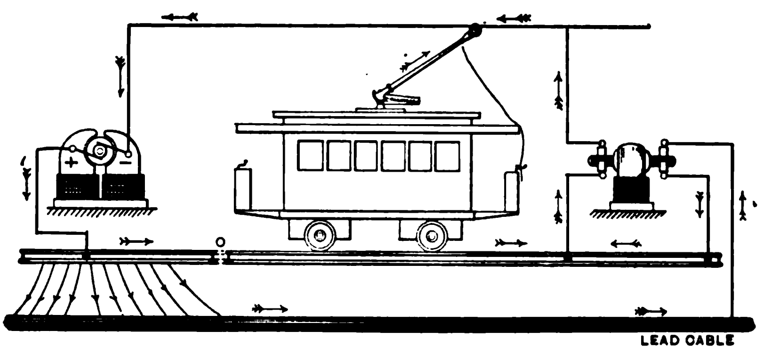

FIG. 7.

to be in danger, the motor generators to be operated by the railway power current; the secondary current developed by these generators to be utilized to lower the potential in the cables and pipes to zero, with respect to the surrounding earth or rails. The suggestion included means for automatically starting and stopping the generators, as cables might become positive or negative to the rails. The motor generators would, so to speak, pump the current out of the cables, and force it into the rails whenever the potential of the former should rise above zero. Fig. 7 illustrates this suggestion. This plan has not yet been put into operation so far as I am aware.

Fourth: Insulating the cables and pipes from the earth was proposed. As some of the worst cases of corrosion of cables by electrolysis occurred where they were painted with asphalt,

FIG. 8.

taped, painted again, and finally covered again with a heavy braiding also saturated with asphalt, it was apparent that to insulate cables sufficiently to protect them, would be difficult and expensive, if indeed practically possible. Figures 8 and 9 show specimens of corrosion of cables which had been treated with asphalt, tape and braiding. To protect water and gas pipes by a sufficient insulating jacket was seen at once to be impracticable.

Fifth: Breaking the metallic continuity of the cable sheath and pipes was proposed. From the fact that severe action is frequently found in comparatively isolated spots, where cables and pipes cross each other, or pass near or across the rails, it follows that any system of breaking the metallic continuity, would have to be studied with reference to the entire complicated system of pipes, cables and rails ramifying through the streets of a city. There would also be a difference of potential between the several sections of cable or pipe, severed metallically, tending to cause electrolysis at one end of each section, as illustrated in

FIG. 9.

Fig. 10. In case of water pipes, treated in this manner, the action might be expected on the interior as well as on the exterior.

There appears to be some evidence of such an action as this in gas and water pipes where the electrical continuity is partially broken by leaded joints. Fig 11 shows an iron service pipe from the Cambridge gas system. It will be noticed that the action is most severe at points immediately on either side of the coupling. The reason the corrosion appears on both sides of the coupling in this case is not clear; it may be due to reversal of current on the railway system. We have observed other specimens similar to this, which may tend to show that for currents of low pressure, the resistance of joints materially affects the results. I will again refer to this question in connection with potential differences in water mains.

Sixth: My assistant, Mr. Towne, suggested that the railway current might be so frequently alternated, as possibly to prevent serious action on the pipes and cables. The theory was, that before the oxygen gas, liberated by the current, should have time

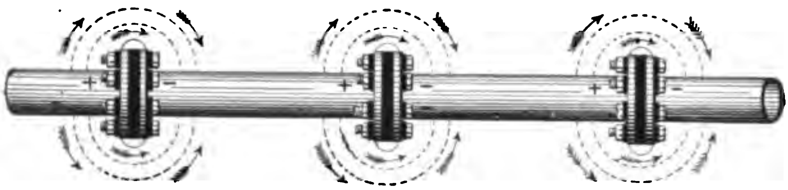

FIG. 10.

to attack the metal, the reversal of the current would disperse it. A careful experiment was conducted, extending over a period of ten days, employing a pressure of current of from three to seven volts, and alternating its direction at regular periods of one minute, by specially devised apparatus. No material change had taken place in either plate during this period of time. We then considered the practicability of reversing the railway current frequently. It seemed possible to reverse it once each 24 hours, at a given time in the night when the load is comparatively light. To do this in a large system involving several power

FIG. 11.

stations would require either a loss of current for a few minutes in order to guard against one station reversing before some other had opened or reversed its current, or would require some elec-

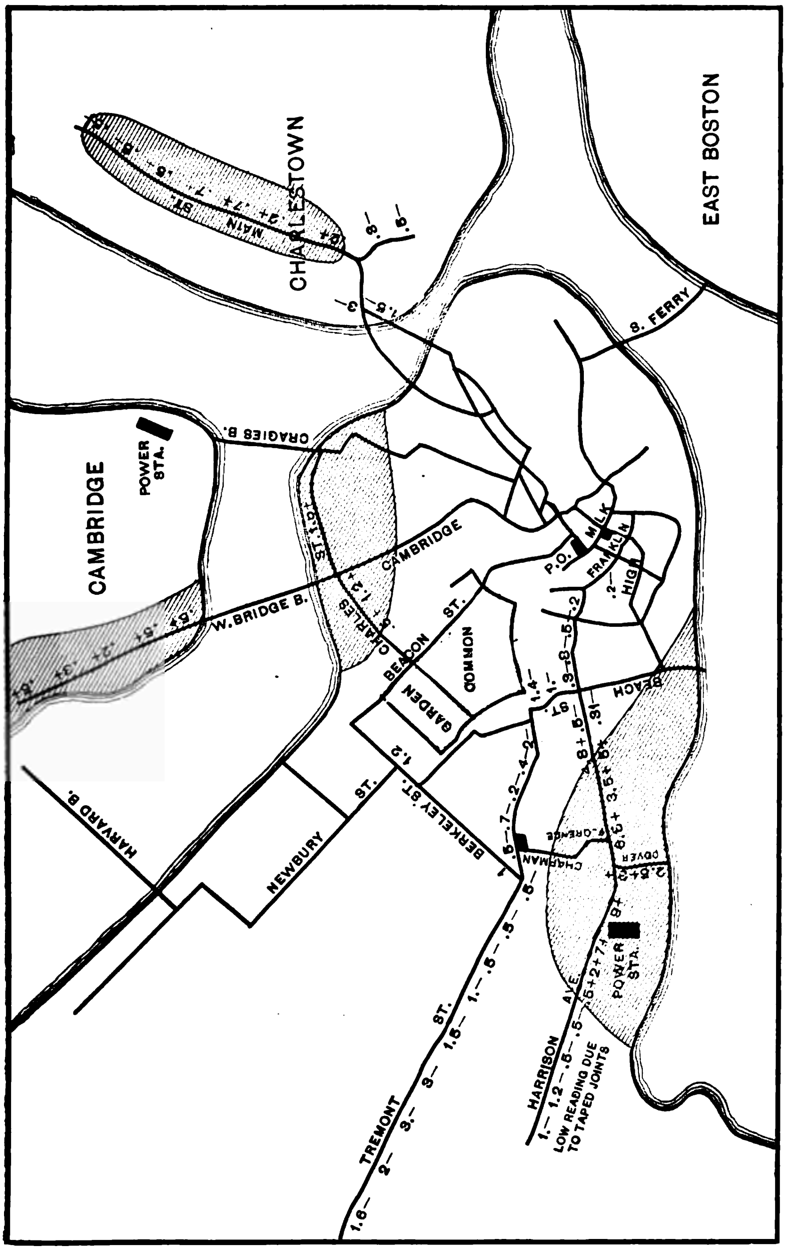

FIG. 12.—Condition after current was reversed by West End Co. The shaded portions of the map are referred to in the paper as colored red.

trical system connecting the several stations together and operating the reversing apparatus simultaneously. We concluded it would be very difficult, if indeed at all practicable, to reverse such heavy currents during regular traffic. We then renewed the reversing experiment, giving 24 hour periods between each alternation, but found at the end of two weeks, to our sorrow, that the plates subjected to the action of the current were seriously electrolyzed. It seemed useless to pursue this line of work further at that time. When alternating current motors become practicable for use on street cars, advantage may be taken of the fact that such currents appear not to cause electrolysis to the extent of injuring pipes and cables exposed to them.

Seventh: At about this stage in the study of the problem, Mr. Fred S. Pearson, then engineer of the West End Street

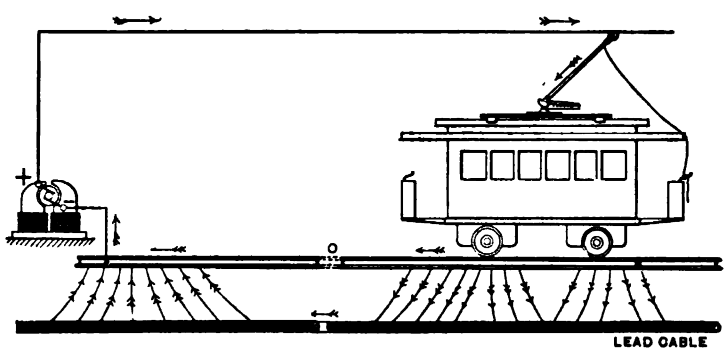

Fig. 13.

Railway Co., made two suggestions which, though separate in themselves, and presented at different dates, yet carried out in conjunction, have proved exceedingly helpful in overcoming the difficulty, at least so far as relates to telephone cables. It occurred to Mr. Pearson first, that if the railway current should be reversed so that the positive pole would be connected with the trolley, the danger of electrolysis would be removed from the greater and more scattered portion of the city, and be brought near the power stations where it possibly could be more easily dealt with. This reversal was made and the expected potential changes between cables and earth followed. Fig. 12 is a map of Boston, showing the condition after the reversal of current. The red or dangerous portions in this map, correspond to the white or safe districts in the map shown in the first of this paper (Fig. 5), the only variation being, that by the reversal, the neutral or zero line was thrown out a little further from the Albany street power-house than it was located before. It was also noted that the cables near the power-house which had been from one to two volts negative to earth before the change of current, were now one to nine volts positive to earth ; that is, they were raised higher above zero than they had been below zero prior to the reversal. Fig. 13 is a typical representation of the current flowing through trolley, car, rails and cables at this time. It will be readily understood that with the conditions as illustrated in this figure, the electrolytic action would be confined to the territory comparatively near the power stations where the current is leaving the cables to reach the negative or rail side of the dynamo.

Mr. Pearson next suggested the plan of running out large cop-

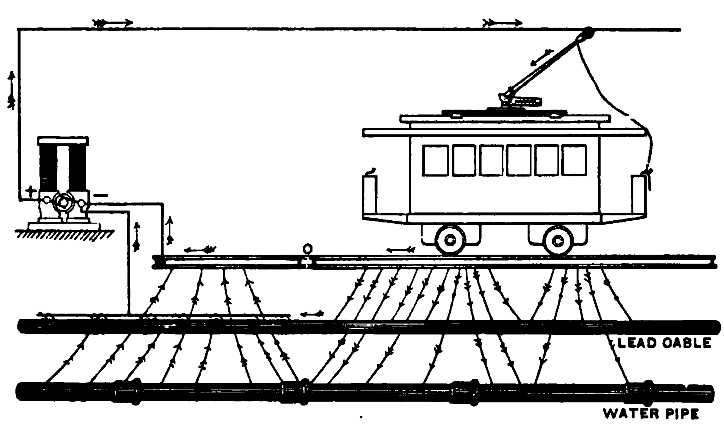

FIG. 14.

per conductors from the negative side of the dynamo and extending them through the dangerous district, connecting them at frequent intervals to the cables. Fig. 14 diagrammatically illustrates this plan. On the principle involved in Prof. Thomson's motor generators, this low resistance conductor connected directly to the dynamo, was to pump the current from the cables and so prevent its passage into or through the moist earth. Some of us were skeptical as to the completeness of this proposed remedy. It seemed possible that even with such a good return conductor. some of the current might still pass into, and through the earth. Voltage measurements, however, at once dispelled the doubts, for we found that the cables measuring 9 volts positive to earth, gave a reading of 22 positive to the return conductor ; that is, the return wire as relating to the cables, was at all points, more negative than the earth (if we may be allowed the expression). The return conductors were made up of a large number of No. 18 copper wires formed into cables about one inch in diameter, known as conductors of 500,000 circular mils. These conductors were extended in each direction from Albany street power-house entirely through this dangerous district, its longest section being about 4,300 feet. The cables in every manhole within the district, were connected by several No. 12 copper wires to the return conductor and soldered. On first connecting the cables to the return conductor, which took place Dec. 24, 1892, the current was sufficient to melt several strands of No. 12 wire. A measurement for current flowing in the main return conductor which was used for relieving the cables only, gave over 500 amperes.

It may be interesting here, to note comparative voltages in the district near the power-house, as given in the accompanying table.

Manhole No.	First Measurement to earth.	After Reversal of Railway Current.	Between cable and Return Conductor.	Between Cables and Earth after Return Conductor was connected to Cables.
263	1.5 +	0.3 —	22 +	4.5 —
264	1.5 +	2. +	22 +	4.5 —
265	0.5 +	0.5 +	22 +	
266	0.5 —	2.8 +	22 +	0.8 —
267		4.8 +	22 +	
268	1. —	5. +	22 +	2. —
310	1.5 —	3.5 +	22 +	1.5 —
269		4. +	22 +	
270		6.5 +	22 +	
271	2. —	6.5 +	22 +	1.5 —
272		6.5 +	22 +	0.5 —
273		9. +	22 +	1. —
274	1.5 —	9. +	22 +	2.8 —
276	0.5 +	7. +	22 +	2. —
277		2. +	22 +	
278	2. +	0.5 +	22 +	5. —
279		1. —	22 +	7 5 —
280	3. +	2. —	22 +	7.5 —
305	1. +	0.3 —	22 +	3 8 —
306	0.5 +	0.5 +	22 +	3.8 —
307	0.5 +	2.5 +	22 +	
308	0.5 —	4. +	22 +	
309	1. —	4. +	22 +	3. —
311	1. —	2.5 +	22 +	
12	2. —	2.5 +	22 +	
313		3.0 +	22 +	0.5 —
314	1. —	2.5 +	22 +	2.5 —

Fig. 15 is from a photograph taken in one of the manholes showing the connection of the cables to the return conductor; the limited size of the manhole prevented my obtaining a view of all the cables.

The map, Fig. 16, illustrates the condition after the installation of the return conductor at the Albany street station. The red patch which existed in that locality is now removed, and the cables are all negative to earth. The remaining red patches or dangerous sections were corrected by taking similar means of reaching the East Cambridge power-house. In treating this latter case, many measurements were made to determine whether or not the railway return wires put up to take the current in a measure from the tracks, would answer for a return for the cables

FIG. 15.

instead of using a special return conductor as had been employed at the Albany street district. It was found that they would not serve the purpose, since the potential of these track return wires varied constantly and was frequently above that of the earth.

The cables on the Boston side of the draw of West Boston bridge, proved to be positive to both the rail and the water, while on the other side of the narrow draw, the opposite condition existed, showing at once, that it was unsafe to assume any neu-

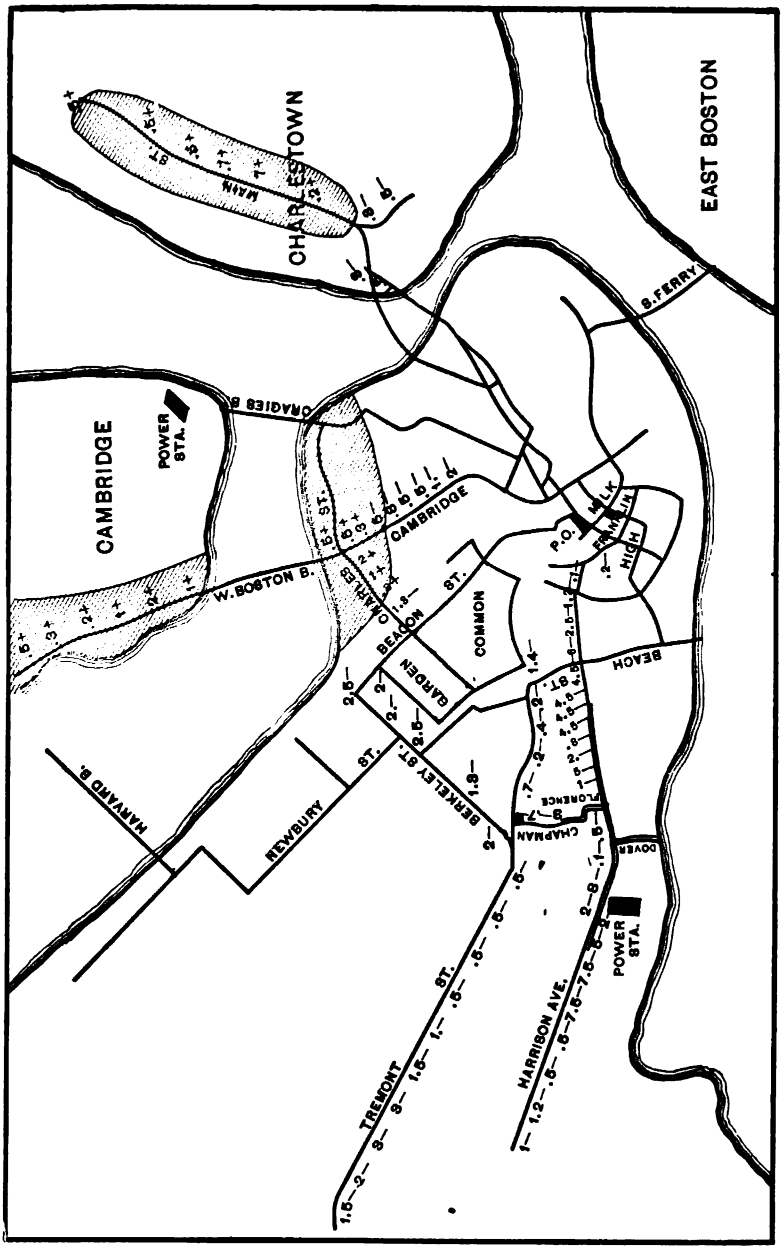

Fig. 16. Condition Jan. 4. after connecting West End ground wire to cables near Power Station. The shaded portions of the map are referred to in the paper as colored red.

tral lines or potential difference, without making measurements to determine the absolute facts.

So far, this paper has dealt particularly with the subject of protection of lead covered cables. It might be inferred that water and gas pipes can be treated in precisely the same manner with the same results, or as water pipes have a much greater sectional area of metal, it might be presumed that simply a connection of such pipes to the dynamo at the power station would be sufficient to bring their potential down to zero throughout the dangerous district. The facts so far coming to our notice, would materially modify such inferences, and therefore should find a place here.

That iron pipes are as truly subjected to the corrosion as lead, need not be stated to the members of this society, but for the benefit of city officials and others who may read the paper, it should be plainly stated that they are quite as readily destroyed

FIG. 17.

by electrolysis. Fig. 17 is from a photograph of an iron gas pipe taken from Brooklyn, N. Y.

The City Engineer of Milwaukee, Mr. G. H. Benzenberg, has kindly sent me a photograph of a six-inch iron water main, badly corroded. It is the best specimen of cast-iron pipe I have been able to obtain, although not the most serious case of corrosion. Fig. 18 is from the Milwaukee specimen.

Mr. Benzenberg writes that the trouble in that city was chiefly noticed upon the six-inch water main extending 100 feet on each side of a point opposite the railway power station. Services entering this main were also destroyed, and all were renewed three times during the past two years. He states further, and I quote his own words, "at other points where power-houses were established thereafter, the mains were immediately connected by extra heavy copper wires with the generator; we have had no trouble with them so far".

Mr. O. H. Tripp, engineer at Rockland, Me., recently furnished me with a specimen of wrought-iron pipe destroyed in five months; the fact is of special interest as it comes from a city having but a small railway system.

In Boston, there have been water, as well as gas service pipes corroded through by electrolysis. I have not learned of any mains having burst from this cause. Measurements of water pipes in the city indicate they are still in danger, notwithstanding several thoroughly made connections with the pipes at the power station; the same is true in Cambridge, Mass. This leads me to call attention to an interesting series of inquiries.

FIG. 18.

The engineer of the Water Board at Rochester, N. Y., suggested to me, a short time ago, while looking into the question of electrolytic action upon the pipes in that city, that possibly there might be sufficient resistance in the joints of the water mains to cause an action upon the lead ring which forms the connection between sections of pipe. He stated that not unfrequently there is found a film of moisture between the pipe and this lead ring, and as the pipes are coated with a preparation of tar or asphalt on both the inner and outer surfaces before they are laid, there might be a poor electrical connection. Without having made any inquiries or tests upon this point, it seems to me probable that the careful calking which is given these lead rings, would form in some portion of each joint a good electrical connection; that is, one of very low resistance. Recent measurements however made in Boston, and others made in Albany, during the latter part of March, this year, convince me that there is a very appreciable resistance in such joints.

Fig. 19 will illustrate the conditions at Albany. We found the negative side of the dynamo to be connected with the rails, and with ground plates in old wells; no connection had been made with water or other pipes. Directly in front of the power station the voltmeter indicated a pressure of 20 volts

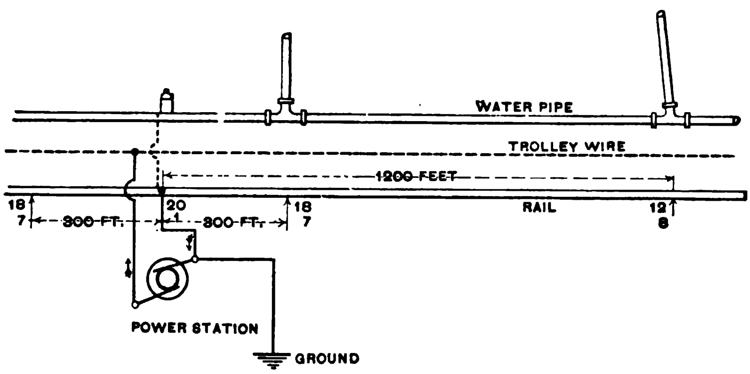

FIG. 19.

between water pipe (an 8 inch cast-iron pipe) and the rail, the pipe being positive. A reading taken about 300 feet in either direction, up or down the street, indicated about 18 volts. At a point 1200 feet north, the reading was lowered to 12 volts. We then connected the rail side of the dynamo to the street hydrant and took new readings, finding 1 volt at the station, 7 volts at 300 feet distant, the same south, and 8 volts at a point 1,200 feet north.

These measurements, with similar indications in Boston, show plainly that there is a very appreciable resistance in the water-main joints. At the same time the measurements give fair evidence that the difference of voltage between any two sections of water pipe is very small. The interested parties at Albany have kindly consented to allow any facts or figures obtained there in reference to this subject to be placed in this paper.

Fig. 20 is from a photograph of a piece of lead service pipe at Albany.

A few measurements made through the danger district will be of interest. The station is situated near the southern extremity of the city. The danger district extends north about one mile, and over this portion of the district the following figures were obtained. They were taken at nearly uniform distances of about 500 feet, beginning at the station.

MEASUREMENTS IN THE DANGER DISTRICT.

At Station,	Cable to Earth,	Positive,	12	volts.
" "	" " Rail	"	25	"
" "	Water " "	"	20	"
500 Ft. North,	Cable " Earth	"	10	"
" "	" " Track	"	22	"
" "	Water " "	"	12	"
1,000 "	Cable " Earth	"	6	"
" "	" " Track	"	18	"
" "	Water " "	"	12	"
1,500 "	Cable " Earth	"	6	"
" "	" " Rail	"	18	"
" "	Water " "	"	18	"
2,000 "	Cable " Earth	"	8	"
" "	" " Rail	"	16	"
2,500 "	" " Earth	"	6	"
" "	" " Rail	"	13	"
" "	Water " "	"	8	"
3,000 "	Cable " Earth	"	4	"
" "	" " Rail	"	11	"
3,500 "	" " Earth	"	3	"
" "	" " Rail	"	12	"
" "	Water " "	"	7	"
4,000 "	Cable " Earth	"	3	"
" "	" " Rail	"	8	"
4,500 "	" " Earth	"	1	"
" "	" " Rail	"	1	"
" "	Water " "	Negative	1	"
5,000 "	Cable " Earth	Positive	½	"
" "	" " Rail	"	½	"

It is proposed at Albany to extend large wires (0000) through the dangerous district, one wire for each system of pipes, connecting the pipes to them at frequent intervals.

It is probable that the remedy which has been applied to telephone cables in some cities, has been the more positive from the very failure, so far, to thoroughly protect the other systems of pipes against electrolytic action. Fig. 14 (already shown) may assist to a clear understanding of this. The cables are here connected by a large wire to the dynamo, while water pipes are not so treated. Therefore, the current which enters the water pipes at points outside the danger district passes to the neighborhood of the power station, and, in leaving them there, raises the potential of the earth about the cables. In other words, the current flows from the water pipes to the ground and thence to the cables in order to reach the dynamo.

Connecting any one system of pipes to the dynamo, will, in a measure, protect other systems of pipes, but connecting all systems reduces the certainty or margin of certainty of protection to any one system. This will be apparent from a little study of Figure 14 just referred to.

When all cables and pipes in the danger district, are connected by sufficiently large conductors to drain them, a careful adjust-

FIG. 20.

ment in resistances in these several conductors may be found necessary in order to insure a balance between the several systems of pipes. It may lead to the necessity of reducing the carrying capacity of the conductor returning to the dynamo from the rails themselves.

The question has already arisen, and it doubtless will be repeated here,—"How small a difference of potential between pipe and earth will cause electrolytic action?" In reply to this, it may be stated that some of the worst cases of corrosion in Boston, have occurred where the difference was but one and one-half volts. Mr. A. T. Welles, of Chicago, in describing to me an examination of some of the first cases in Cincinnati, states that the "difference of potential between the cables and the rail, was never more than one-half, and usually less than one-quarter volt." Such a difference between cable and rail would mean a much less difference between cable and earth, where electrolysis takes place. Mr. John C. Lee, of Boston, has experimentally caused the corrosion on lead and iron by a difference of potential of $\frac{1}{100}$ volt.[1]

These facts certainly indicate that but a very small pressure is necessary to produce the action and should dispel the numerous statements that well bonded rails or a large amount of rail return wires will alone overcome the trouble. In some cities, where electrolysis is in progress to-day, the return copper nearly equals that of the trolley and feed wire system. We cannot force the current to take one path exclusively when others are open to it.

The facts given above, with others similar, though not enumerated, lead me to these conclusions:

1st. All single trolley railways employing the rails as a portion of the circuit, cause electrolytic action and consequent corrosion of pipes in their immediate vicinity, unless special provision is made to prevent it.

2nd. A fraction of a volt difference of potential between pipes and the damp earth surrounding them, is sufficient to induce the action.

3rd. Bonding of rails, or providing a metallic return conductor equal in sectional area and conductivity to the outgoing wires, is insufficient to wholly prevent damage to pipes.

4th. Insulating pipes sufficiently to prevent the trouble is impracticable.

5th. Breaking the metallic continuity of pipes at sufficiently frequent intervals, is impracticable.

6th. It is advisable to connect the positive pole of the dynamo to the trolley lines.

7th. A large conductor extending from the grounded side of the dynamo, entirely through the danger territory and connected at every few hundred feet to such pipes as are in danger, will usually ensure their protection.

8th. It is better to use a separate conductor for each set of pipes to be protected.

9th. Connection only at the power station, to water or gas pipes, will not ensure their safety.

10th. Connection between the pipes and rail, or rail return wires, outside of the danger district, should be carefully avoided.

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1. Experiments completed since the writing of the paper produced marked corrosion by currents of less than $\frac{1}{4}$ volt, this was illustrated by a lantern slide, in which the pieces of lead wire themselves were shown magnified upon the screen.

11th. Frequent voltage measurements between pipes and earth should be obtained, and such changes in return conductors made, as the measurements indicate.

In closing this somewhat rambling paper, I can do no better than use words which will remind you of Patrick Henry; 'eternal vigilance" will be the "price" of pipes and cables where conditions favorable to electrolysis exist.

Boston, March 30, 1894.

At the conclusion of the paper, the author presented a number of views upon the screen, in addition to those which form part of, and are printed in the body of the paper.

Among the most interesting were:

1. A map of Boston, showing the district in which the water pipes (by recent measurements) are in danger of electrolytic action.
2. Many samples of electrolyzed pipes, both iron and lead.
3. Specimens of lead pipe, showing the effect of corrosion produced by a current having a pressure of less than one-half volt.
4. Two views, showing method of making the voltage measurements between pipes and the earth.
5. A view, showing the process of electrolysis going on, by which a tinfoil pipe was eaten through, allowing a colored liquid to mix with water, and making a very interesting picture on the screen.

DISCUSSION.

THE PRESIDENT:—I will call on Professor Plympton to open the discussion on this interesting paper.

PROF. GEO. W. PLYMPTON:—I came here as a learner to-night. I had many questions to ask, and a good many have been answered by Mr. Farnham's paper. I have been accustomed ever since I had any experience with this troublesome question to consult Mr. Farnham's opinions, which I have obtained frequently at second hand through the scientific papers. We have a large mileage of trolley in Brooklyn as everybody knows. The papers assure you of that every few days. Our first experience with electrolysis dates from about a little more than a year ago. We had heard of that in Boston, of course, before we had any experience in Brooklyn. In framing the franchise for the trolley roads there was fortunately a clause inserted, not through any provision of the aldermen or commissioners, but which placed responsibility upon the trolley companies for any mishaps to any underground pipes, wires, cables or the like. That was placed there, not because electrolysis was thought of at the time, I think, but because it was supposed that in the process of digging up streets they might interfere with gas and water pipes, so that the responsibility is clearly acknowledged and they accept the situation, and they are taking all the means that they can with due regard to economy, which they always observe, of course, to prevent any serious deterioration. So in their experimenting, as there are four companies and four sets of engineers, their experiments have been to some extent along different lines. I am sorry to believe that we have not got the full measure of the damage that has been done. So much as has been exhibited, and you saw some of the specimens on the screen, was such as has made itself known through the entire failure of pipes and cables. It is reasonable to suppose that ten times as many examples could be found which would be nearly as bad as those exhibited, because there has been no digging down in the ground in search of such cases of corrosion. Water pipes, gas pipes and lead coverings of cable have all suffered. Telephone companies were the first to find it because of slight perforations of the insulation. Leakage of water pipes have made themselves known in one or two cases where they were near enough to the surface. In one of the cases exhibited, one pipe had been destroyed in three or four months. It was an iron pipe very near the rail. It was replaced by a larger pipe, and that was perforated in thirty days. So that the rapidity of the action warned us there, that it was time to look about quite actively. We have held numerous conferences with the engineers and they have promised to take all proper means for taking care of their own current. That is the thing, of course, for the trolley people to do. That they are aware of; they have been sufficiently warned of it. One thing I feel a little concerned about, and I take time only to mention it and that is this seventh conclusion: "A large conductor extending from the grounded side of the dynamo, entirely through the danger territory and connected at every few hundred feet to such pipes as are in danger will usually ensure their protection." True, so long as that connection can be kept up. Will it not be necessary to make such a connection in such place that we can examine it every once in a while? Will it do to make a connection and cover it up? What shall it be—copper wire? Can we solder a copper wire to an iron pipe? Can we make a connection that will remain good and without increasing its resistance? Local action will set up a corrosion, and as to soldering, in the ordinary sense of the term, it seems to me impracticable. So that we have some fears that whatever precautions they take of that kind, may prove insufficient after a little while or else that they will need constant attention, and that we shall need a large corps of inspectors to prevent serious injury.

The numerous ways of bonding the wires are the things that the trolley people exhibited there as the means of preventing injury, and two or three have been exhibited in the last week or two. One, that of making the rails continuous has been, I believe, tried in Boston. It is about to be tried in Thirty-ninth street, Brooklyn. The apparatus arrived the other day for welding the rails. The engineer of the company assured me that they would make their rails continuous within the limits of the city. Another bond, which bears the name of Vail, I think, has been suggested, where three large rivets are made tapering, and driven into the rail and riveted down, each rivet, half an inch in diameter, and then with a half-inch conductor extending beyond the joints, and then another set of three rivets. That has been exhibited as one of the best bonds that is known.

Of course, as Mr. Farnham says, the current will still be divided between the rail and the moist earth in proportion to their relative conductivities. You cannot convey all of that current back by the best conductor that you can put in the ground. So the method of relieving the pipes of that positive charge, where the corrosion has been set up, is the best way to ensure the protection. But in regard to its permanency, as I said before, I am inclined to feel some fear.

The question with us, of course, is in the experimental stage. We shall accumulate more within a year, but in that time I fear we shall find a good deal more damage done, for the reason that it is only where the corrosion has been so bad that the pipe or the cable has failed, that our attention has been called to it. No doubt corrosion is going on at this hour over a very large extent of territory. We shall accumulate more information, I presume, within the next season. But for the best we have been able to do, with the information we had, we felt all the time indebted to the very able and systematic way in which the investigations have been carried on, and the methods taken to ensure success in Boston.

Mr. Townsend Wolcott:—One very interesting point is the small voltage which will corrode a cable in the earth. We are generally familiar in electrolyzing solutions with voltages of one volt or over—almost any solution—over one volt. But I take it that the conditions in the damp earth are such that the corrosion is just about ready to go on anyway, and a very little help will make it go. In decomposing water, for instance, it takes about 1.48 volts, if I remember; but in that case you have to supply the energy to break up the chemical combination. But the way it probably is with a cable in the earth, the chemical action is just ready to take place, and it only requires very little to start it.

With regard to the question about the voltmeter, it may be well enough to say that, if the voltmeter has high enough resistance you do not have to be at all particular about contacts. With a high resistance voltmeter you can take a storage battery out of the solution, and when it is apparently dry you get exactly the same voltage as with the acid. A low resistance voltmeter, of course, would not show that. But if it is a high resistance voltmeter it will give the full voltage.

Mr. A. E. Kennelly:—Mr. Chairman and Gentlemen, I think that I only echo the general sentiment in saying how much I have enjoyed listening to the paper of the evening, which is the first, I think, that has given us clear and precise information, thoroughly and interestingly placed before us, upon this subject which is of great practical importance. But in some of the generalizations which Mr. Farnham makes, I have the honor to differ from him, and I beg to submit certain criticisms upon them. I wish to take the stand that it is not, as stated in the sixth generalization—that it is not necessarily good policy to connect the positive pole of the dynamo to the trolley lines, and for this reason, that supposing you are grounding one terminal of your dynamo which is supplying a total current distributed to your trolley wires, say of a thousand amperes, that thousand amperes goes into the ground on your district, and it has to come out of the ground at the point of your ground connection to the dynamo. Now, if you bury in the ground a large mass of iron pipe, or lead tube, or metallic conductor of any kind, that metallic conductor will, perhaps, absorb a large fraction of the thousand amperes. Let us say it absorbs 750 amperes. The metallic system will have 750 amperes entering it and 750 amperes issuing from it. Now, when you have the negative pole to line, the current will go from your ground plate, will enter all this mass of metal in the vicinity, and there it will do no damage, because where it enters the iron or lead, hydrogen is given off, as we have seen represented on the screen. But it will issue from the iron or lead over a larger area in the remote districts—a very large "danger" area, an area as shown on the chart this evening of several square miles. That large area will be in danger of corrosion, and there will be corrosive electrolysis going on all over that area to the extent of 750 amperes collectively. But when you reverse the current, as is suggested as advisable in the sixth conclusion of this paper, you reverse that condition of affairs. All the distant district is free from danger and all the oxidizing and corroding effect is close to you; but the 750 amperes are still there and are now actively corroding a much smaller surface. It is the surface in the immediate vicinity. Instead of being spread over, as shown on the map in the previous case, several square miles, the same total corrosive electrolysis is spread over, perhaps, half a square mile. The danger area is reduced, but the danger is greater, because the activity is consequently augmented in that district. You have, say, twenty times the amount of corrosion going on over a given surface of pipe, and the result is you will eat through those pipes twenty times as rapidly, and if the danger is in bursting a pipe you will probably burst it twenty times as soon under those circumstances. But if, as Mr. Farnham says, we prevent that, as was done so skilfully in this case, by throwing out a ground feeder, which prevents the current from emerging out of the pipes into the surrounding soil near the power-house, why then, coupling together the sixth and the seventh conclusions, all is well; you have stopped the corrosive action. But unless you do couple together the sixth and seventh suggestions, you are likely to cause more danger by having the positive pole to line, than if you have the negative pole to line. The fact Mr. Farnham mentions, that he did have trouble with his lead-covered cables while the negative pole was to line, but did not have trouble when the positive pole was to line, is an argument in his favor. But he would probably have had electrolysis on the cable in one district or another whichever happened to be the danger district, if the lead had been suitably placed for electrolysis, and a lead cable of this kind is singularly liable to be spoiled by electrolysis. The resistance of an ordinary lead telephone cable sheath, as we know, is much greater than that of a large iron water pipe. But being continuous, and having very few or no unsoldered joints, it has far less resistance than a large iron pipe with a large number of poor electrical joints. The result is that where there is an opportunity for the lead sheathing to be corroded at any point, there will be active corrosion, and at that point those destructive effects so fully brought out in this paper will be produced.

Furthermore, I would like to point out that the difference of potential as measured by a voltmeter between a cable sheathing and the ground in its vicinity, is not necessarily a criterion of the degree of corrosive activity taking place at that point. If the direction of the P. D. is such that the cable is positive, there will be a corrosive current there, or a tendency to produce a corrosive current. But if the P. D. is three volts or four volts, the corrosive current is not necessarily twice as strong as if the P. D. were one and a half or two volts respectively. For suppose you had a perfectly insulated cable and sheath, but at some distant point, say, half a mile off, the sheathing of lead was exposed to the ground, and that there destructive action was being produced; there might be at that point, half a mile away, a difference of potential between sheath and the ground of three volts, but at the point where you stand the P. D. might happen to be five volts. Now, the five volts could not be so active in producing corrosion as the distant three volts, in fact it could not be active at all, owing to the perfect insulation of the entire cable in the vicinity. The point I want to make is, that though the observed P. D. is an evidence of action, it is not an evidence of quantitative or corresponding intensity of action.

Again, while all admit that iron is corroded, and iron pipes are corroded electrolytically, and the evidence has been amply brought forward to-night, I do not think that Mr. Farnham means that as much corrosion takes place with iron as with lead, for the reason that we all know a given weight of lead is much more readily consumed by electrolysis than a given weight of iron. Roughly, an ampere in a year will dissolve seventy-five pounds of lead by electrolysis. It will dissolve, roughly, only about twenty pounds of iron, or nearly four times less. So you have a more reduced activity of corrosion on an iron plate than on a lead plate, and that is reduced again by the fact, in actual practice, that in water mains, the electrical conduction from length to length of pipe is very imperfect, as evidenced, for example, by the tests and measurements shown on the screen in the last diagram, Fig. 19, where we had an indication of two volts fall of potential per hundred feet of pipe within the first three hundred feet from the station.

The destructive effect of electrolysis, while it is serious, is often exaggerated by not taking into account the actual amount of decomposition that can take place, electrolytically, under the most favorable circumstances. If you have a mile of eight-inch water main, which is half an inch thick, that is, its exterior diameter is nine inches and its interior diameter is eight inches; and a thousand amperes are kept steadily flowing day and night, with uniform density, out of that surface into the surrounding soil, it will take about six years for that current to reduce, by electrolysis, the thickness of the iron to one-half. Of course, it would be unfair to make a positive statement of that kind, because we assumed uniform corrosion, whereas corrosion does not take place uniformly.

The resistance of the ground is really far higher than we ordinarily attribute to it. We are so accustomed to use the ground universally in telegraphy, we are so accustomed to the idea of a ground return circuit with very little resistance in it, that we come to grasp the idea, unconsciously, that the ground has very low resistivity, whereas it has very high resistivity. We may take the position, in fact, that the ground itself has an enormous resisivity, and what we really measure in the resistance of the ground, is the resistance of the water that happens to be suspended in the ground. The resistivity of ground under ordinary circumstances is something like 50 or 60 ohms. The result is, that if you had two iron water pipes, each nine inches in diameter, deeply buried in the soil, and 30 feet apart, at a constant difference of potential of ten volts, you would not expect less than 2.5 ohms resistance between the pipes per linear foot of either, nor more than four amperes of current between them per linear foot. In the case of ten volts between one such buried pipe and two surface track rails, supported on wooden sleepers, the resistance between track and pipes would probably be much more, and the current strength per linear foot of pipe, perhaps less than one ampere.

One of the most interesting observations that ever fell to my lot, in connection with the high resistivity of the ground, is, perhaps, worth mentioning. It was occasioned by trouble in an Edison tube, and the fault, a ground, was situated about three-quarters of a mile from the station. We were trying to locate the ground by means of a compass needle moved over the surface of the soil and over the buried conductor, while strong direct currents of about two seconds duration were applied with ground return circuit, every three or four seconds in the power-house. We took the magnetic needle along the surface of the trench and tried to find a point where the needle ceased to respond to the intermittent currents. We came to a spot where the line of Edison tubes departed from the road and entered a field. Round the field there was an iron wire fence with iron uprights, which fence ran for several hundred feet parallel to the road. We knew that the ground existed in the iron Edison tube somewhere in this vicinity. I happened, quite by accident, to stand near the fence, looking in at the field and wondering where the trouble might be, and as I rested my hand on the fence, I fancied I felt a shock. I thought this a mere delusion and paid no attention to it. But presently some one else came up, and without any suggestion on my part, rested his hand also on the fence and declared that he felt a shock, and finding my own imagined sensation corroborated independently, we investigated it and found the reason to be this: within 20 feet of the fence was the "ground" we were seeking in the Edison iron tube. The intermittent current escaping from the conductor into the iron tube 30 inches below the surface of the soil, was able to raise the potential of the surface of the road in the vicinity where we stood, to such a degree that we were able to deliver through our hands enough current to the iron fence wire to make a shock perceptible, and, on taking measurements later on with a galvanometer we found the resisivity of the soil, which was damp, but not wet, to be something like 60 ohms.

There is a very curious, although not very essential, point brought out in this interesting paper, and that is the extension of the neutral line at the time when the reversal was made in the direction of current to the trolley lines. It was pointed out in the paper that the zero line of pressure moved further from the station when the reversal was made. I would like to ask Mr. Farnham if he has any information which would throw light on this interesting peculiarity. In the absence of any information, I might suggest that possibly the counter-electromotive force of the developed hydrogen over the surface at which the current entered the system of pipes, might be sufficient to disturb the zero line.

MR. FARNHAM:—I will say in reply to the question, that we have made no special investigation, nor have we found any particular reason to account for the neutral line being thrown out from the station by the reversal of the current. But measurements obtained several weeks after the change was made, as well as immediately at the time, gave the same result. It would seem to me that this rather disproves the hydrogen theory: that is, the zero line has continued farther out than when the current was in the opposite direction.

I would like also to say in reply to the able remarks just given to us, that the conclusions summarized in this paper were written with the thought that they should be considered together in all cases. There would be no advantage in doing some of this work suggested unless we did other parts of it also. Conclusion No. 6 was intended to go with No. 7. But if that had not been the intention, I can hardly agree with the speaker that it would be more serious to have the corrosion take place in a limited territory, even though it were more rapid, than in a large territory. Would it be more convenient to dig up all the paved streets of a large city once in ten years, or to dig up a radius of a thousand or of two feet once in a year? It is an open question which hydraulic and gas engineers must pass upon. But it would seem to me better to confine the trouble in a small territory, even though you had to take other measures, put in larger pipes if you please, in that territory, rather than have the destruction slowly but surely going on all over your city? Is it not preferable to have a very sore finger than to be moderately sick all over? The suggestion of putting the positive side of the dynamo to the trolley and thereby bringing the danger territory near the station, was primarily for the purpose of rendering it more easy to treat the trouble with the return wire system which I have described.

I would like also to remark in connection with the first speaker who opened the discussion, that I recognize the importance which he named, and the difficulty of having a good connection with all the pipes. As to how the connection should be made on water pipes has been considerably discussed. I am hardly able at present to advise. Whatever we do in this line we must watch constantly. We must take our voltage measurements frequently, as noted in conclusion 11. We can tell certainly by this whether there still is danger or not. As to the number of years that may elapse before pipes will be eaten through, is a question we hardly need to discuss. If the action is slow it ought to be prevented. We may easily determine whether there is danger or not, and whatever we do to remove the danger by these means, we must watch the electrical conditions constantly or we shall find ourselves again in trouble.

In illustrating this fact let me say that since the system just described has been applied in Boston, the West End company has run out in one direction several large return wires in addition to those previously in use in that locality, and thus practically moved the power station, that is, it changed the zero line from its former location to a point very distinct, making it necessary to rearrange and extend the cable return wire system.

The Secretary read the following communication from Mr. Thomas D. Lockwood:

MR. LOCKWOOD:—I much regret my inability to be present when Mr. Farnham's excellent paper, at which I have had an opportunity to glance, is read.

I have admired greatly the philosophical spirit in which Mr. Farnham's researches have been made, and am greatly pleased that he has found time to prepare and deliver to the INSTITUTE an account of them, and desire to place on record my personal opinion that the paper is one of the most interesting, and at the same time, one of the most lucid and valuable contributions to the TRANSACTIONS of the INSTITUTE that have been received.

It is evident that with the plus pole of the generator to line, the tendency of the trolley current will be to pass at all outlying points to the pipes and cables which lie in its path, except at points which are closely adjacent to the generator.

But when the minus pole of the generator is connected with the trolley and feed wires, the current, or some portion of it, having once strayed upon the pipes and cable tubes lying generally in its way, tends to leave them at all outlying points.

In the second case, then, the destructive effects of electrolytic decomposition tend to become widely distributed, while in the former case, they tend to concentration.

A superficial examination might lead to a superficial conclusion that, because of such distribution and division, the arrangement in which the minus pole is placed to line, is under all conditions the most desirable and advantageous, because by reason of such division the strength of current leaving the pipes at any one point, would be relatively small, and possibly so small as to be neglible and innocuous.

On the other hand, under the arrangement which provides that the plus pole shall be to line (if pipes, or lead covered cables enter at all into the question), a current of very considerable volume may be concentrated in a very small area of space, and is liable to bring about in an extremely brief time, intensely destructive corrosive effects at points within such circumscribed space.

I do not say that because the first named conclusion is superficial, it is under all conditions incorrect, and conditions can readily be conceived, such as those concerned in the operation of a railway in a small place, or on a lightly traveled trunk line, where it might even present preponderating advantages.

It seems, however certain, that for city work, and under all conditions where traffic is heavy, and cars numerous, and current consequently of great volume, it is (so far, at least, as this phenomenon of destructive decomposition is concerned) infinitely more advantageous to connect the plus pole of the generator to the line, and thus concentrate the electrolytic effects to a limited area near the source of current, where it can readily be located and where preventive as well as remedial measures can be applied conveniently.

I am heartily in accord with the propositions of the paper, and do not doubt that the familiarization of our minds with this important subject will be productive of beneficent result.

A MEMBER:—I would like to ask Mr. Farnham what he estimates would be the relative cost of the overhead copper, and the cost of the underground copper to properly protect a system such as the Boston system, relative to the overhead structure, including the bond wires. Would it be larger, or about the same?

MR. FARNHAM:—I should think it would be very much smaller. As I have shown you, the danger district extends only a comparatively short distance from the station, and a moderately sized wire will overcome any trouble in that district. I should say the amount of copper to prevent the trouble would be very small as compared with the overhead system.

A MEMBER:—I believe that in Brooklyn you use overhead structures, do you not, for the return? Has there been any sign of corrosion near to it?

PROF. PLYMTON:—It is different for different roads. They are trying all sorts of things. That is one. I think there has been no corrosion. But the time has been too short to announce it a success.

MR. FARNHAM:—I want to add one more word. Allusion is made again to the possible advantage of putting the positive side of the trolley to the ground in small places. I will simply call your attention, as some of the members may wish to look up the matter, to the English communication. It is in the London *Electrical Engineer* of April 6, 1894. The writer of the article states his assumption of fact very positively, and he also uses as an argument that this method will cure the trouble, that in their "large" system where this has been applied, something like 900 amperes of current is used to run the cars. Now, this is not a "large" system as we all know. Nine hundred amperes is a small amount to use on a city railway system, and the fact that the engineers have not found any pipes eaten through since they made this provision is no proof at all that the action is not progressing. I do not believe it is advisable to wait until pipes have been eaten through, before we know whether the thing is taking place or not.

I would like to say one other word and that is, that while a moderate sized wire running through the danger district will remove the current from pipes it will not do it, of course, if you extend that wire indefinitely.

If you extend the return wire throughout the entire railway district, and connect it at frequent points to the water or other pipes, as I understand has recently been done in Milwaukee, you may prevent a difference of potential between the rails and the pipes, but you will not wholly prevent a difference of potential

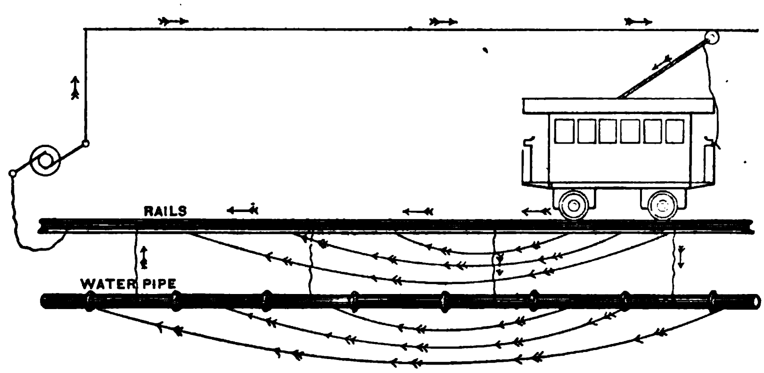

FIG. 21.

between the pipes and the earth. You will simply transfer the action from the pipes near the power station to those in more distant territory; for in this case, the current will naturally leave the pipes at the same point it now leaves the rails, though in a less quantity (see Fig. 21).

Imagine, now, an opposite extreme: the return wire from the rails is made small, or removed altogether, while the return wire from the pipes extends over the danger district. You will see the current all returns by the pipes and pipe return wire; the current passing at all points from the rails through the earth to the pipes, raises the potential of the earth above that of the pipes and so assures protection to the latter.

This is an extreme measure and may never be necessary, but should it be found so, as the railway companies cause the trouble, it is to be presumed they will be willing by such means, to transfer the electrolytic action from pipes to the rails.

PROF. HOUSTON:—I think one of the most interesting points that has been brought forward in this paper to-night, and one which has surprised me very much is the exceedingly small potential difference which the paper states has been necessary to effect electrolysis, namely, that a fraction of a volt difference of potential between the pipes and the damp earth surrounding them is sufficient to induce the action. It may be true that electrolytic corrosion did occur on portions of the sheath that showed a potential difference between the sheath and the ground in the vicinity "of never more than half, and usually less than a quarter of a volt." But it seems to me that an error was clearly made here, of not taking into account the potential difference between the cable and the dynamo at work upon the lead sheath through the total resistance of the ground. I am the more ready to believe that there is some error, or misunderstanding possibly on my part here, for I know that to effect the disintegration of lead electrolytically in a storage cell, requires a potential difference sufficient to electrolyze the water, namely, over two volts. If it be true that this small potential difference can electrolyze lead, then I would like to know it, for I believe that practical application could be made of this in the storage battery. I think it is reasonable to suppose that where a fraction of a volt is discovered between the sheathing of the cable and the ground close by, that many times this fraction would exist between the sheathing and the ground at the dynamo terminals. Of course, if actual measurement, as Mr. Farnham has said, of one one-hundredth of a volt can produce electrolytic corrosion of lead, why there is nothing to be said against actual measurement. I should, however, look very carefully at the source used, and the method by which the experimenter assured himself that he did not actually limit the potential difference to the small fraction stated.

MR. FARNHAM:—I might say in reply to this, some of the text-books inform us that we cannot produce that action under less than 1.4 volt or something like that. But Mr. Lee, the chemist of the American Bell company, to whom I referred in the paper, made some very careful tests, and I have reason to believe that he is a man of sufficient knowledge to perform them accurately, and he tells me that he has produced the action easily by .01 of a volt and less. Now the reference which I made to cables in the test, in which it was said that the voltage was usually less than one-half volt, sometimes less than one-quarter, I know nothing about, except the report that was given to me. I know in our own case we make the measurements where cables have been corroded, between the cable sheath and the ground, exactly where they lay on the ground, by a Weston voltmeter. I know that in the case of the experiment which I showed you through the lantern, where lead was corroded in three weeks' time, we took the voltage between the two terminals at the test tube in which the lead wires were placed before they were connected, that is, without their being dipped in the water. We took measurements there every day and found it less than one-half volt. You can easily perceive the action that has gone on there. The lead wires were immersed in hydrant water without any acid, salt or any other condition to induce or help the action—not distilled water but such water as we draw from our Boston waterworks. So you see it is true that while it does require a volt or more to electrolyze or decompose water in the ordinary meaning, we do get the action on lead and iron pipes with very much less pressure.

DR. LEONARD WALDO:—I think this is a very interesting paper, and contains a great deal of valuable information. The coal miners have had much trouble in keeping their valves from disappearing and their pipes from being eaten up, and inasmuch as we have been interested in the metallurgy of that sort of thing lately, we have looked into the question. In several instances we have had valves, parts of pumps and similar articles sent to us from the depths of the mine, and several gallons of the water which was supposed to do the mischief sent along with them. We boiled the water holding specimens of the metal of which the valves were made, and exposed the specimens for weeks at a time, with no chemical action at all on the metal similar to that used in the valves or in the pipes. When you insert an iron pipe and connect the two specimens through a voltmeter, you will get a very rapid disintegration of one pipe or the other, and you get an indication of anywhere from four to six-tenths of a volt. It is my observation that very efficient deterioration of these underground materials of different potential takes place with differences under half a volt. There is another question which has come up in our minds and I have not heard it touched upon to-night, but, I think, it is a fundamental question in talking about deterioration of metals under electrolytic or acid or water action. The lead which our friends use for sheathing cable is probably over 95 per cent. lead, but it certainly is anything but pure lead, and the secondary couples that are formed with antimony or the other impurities that the lead contains when brought in close contact with the acid waters, even having a very small percentage of acid in them, produce a corrosion of the surface, which I do not see any way of taking account of. The difference between a copper pipe containing a tenth of a per cent. of certain impurities and containing one per cent. of the same impurity is very great. But it is no greater than that which contains none at all, and that which contains one-tenth of one per cent. In all these questions, therefore, of rapid deterioration of pipe and that sort of thing, I think, the most important consideration will have to be given to the actual corrosion which occurs because of the molecular, chemical and electrical action which take place between the metal and its own impurities. As to the small voltages existing, and having the effect of corrosion, I think any one who has actually made measures on those pipes in position, with and without the presence of electrical action is quite prepared to testify that the smaller voltage and the corrosion of the pipes are present at the same time; whether the corrosion is wholly due to the action of the current on the principal metal alone or not is quite another question.

Dr. Chas. E. Emery:—I did not think the subject of the corrosion of steam and water pipes underground appropriate to this discussion, until the last speaker mentioned the influence of various conditions on the rate and nature of corrosion. In that connection I will say that it is found in practice that a steam pipe covered with a non-conductor, and maintained at a temperature of 250° to 300° F. will not corrode underground, whereas a hot water return pipe similarly covered and kept at about 212° corrodes very rapidly. The corrosion is external and appears to be ordinary rust. A study of the phenomena indicates that there is a critical temperature where the gases in the soil act more intensely than at other temperatures. Apparently ordinary carbonic acid gas has the most important influence. It appears, therefore, that the temperature should be considered in connection with the other influences on corrosion which have been mentioned.

[Adjourned.]

[COMMUNICATED AFTER ADJOURNMENT BY PROF. ELIHU THOMSON.]

Very early in the history of street railways in Boston, I had conferences with Mr. F. S. Pearson, of the West End road and others, concerning the means to be employed for preventing electrolytic action on pipes and other metal structures underground. Reversing the current at intervals was suggested and tried, but it soon became evident that this would only shift the areas of action and no restorative effect or plating of corroded metal could be expected. It soon became evident that if the current could always be made to enter the metal pipes, etc., from the ground, and leave them by metallic connections to track or to returns, the protective action of the current with the pipes, etc., as cathodes, might be beneficial, and as Mr. Farnham has ably shown in his very interesting paper, this is the principle which has given the best results in practice. Indeed may we not be justified in expecting that in many cases the natural corrosion may be even arrested by the nascent hydrogen set free at the cathode, or by the negative polarization of the pipes with respect to earth?

Could every line of pipe be relied upon as a complete conductor without bad joints, the problem of protection would be much simplified, but the current in crossing a bad joint in a line of pipe will undoubtedly be partly shunted through the earth around the resisting joint, and cause corrosion to a greater or less extent. Again, if in a line of pipe, every joint was of poor conductivity there would be very little chance of corrosion by electrolysis, as the pipe line would not offer any good path for current.

Earnest endeavors will be made in the present year to prove the practicability of continuous rails electrically welded in place. In cities this would give a network containing a vast amount of metal in the rails, acting as a conductor for the return current. Such a track system, or even a track system well bonded, would, if worked with a zone system, in which successive zones of trolley conductor are made positive and negative respectively, remove all possibility of trouble, but might increase the risk in the accidental crossing of wires on the trolley conductors. Another system of working which would remove most of the difficulty in our cities, would be the employment of potential reducing motor generators at various points over the area, the high potential side being fed direct from the power station by metallic circuit, and with either alternating or continuous currents, as found most expedient; while the low potential side, 500 volts or less, would go to trolley wire and track respectively. In this case the return current would never have to travel very far, and the drop on the whole system would be much lessened. Copper would be saved and the congested districts could be readily supplied with a lower potential than the high speed outlying districts. If alternating currents were used from the power station to the motor generators, the generators in the station could be made of very large capacity, say 5,000 H. P. in a large city, and the motor generators could range from 200 to 500 according to the density of traffic, etc. I mention these facts to show that avoidance of all electrolytic corrosion is within the capabilities of electrical engineering, while still retaining the continuous current motors on the cars, and the single overhead trolley wire. It is even possible that considerable economy of transmission might be secured over the direct supply systems as now used.

Swampscott, Mass., April 17, 1894.

[COMMUNICATED AFTER ADJOURNMENT BY HERMANN LEMP, JR.]

The paper read by Mr. Farnham has a great value as it gives in a clear and concise manner facts found under every day working conditions. Not only can we by its study analyze the various causes producing the deleterious effects of electrolysis, but we can follow step by step the means employed and the effects produced for combating these evils.

In the conclusions reached by the author a few vacancies are left which it seems are all well worth considering.

Two principal causes are apparently considered as unavoidable, which to me seem to be the contrary. Mr. Vail, in his paper before the National Electric Light Association, has taken the same ground. They are:

1st. There should be no ground connection established from dynamos at power station, in fact all precautions taken to prevent even accidental ones.

All connection between dynamos and track should be made by means of well insulated feeders, such as are used in any properly constructed lighting system.

2nd. The track itself ought to be as nearly as possible one continuous rail.

The leakage currents which are to-day causing all the mischief, are mostly due to insufficient carrying capacity of the track, not of the rails themselves but of the joints between the rails. When copper bonds are used, the unequal expansion of the two metals soon produces a loosening of the joint, increased resistance and finally complete deterioration. To this is added the electrolytical effect between the copper and iron.

Iron bonds, on the contrary, have to be made so large to be of any use, that the vibration of the passing cars loosens them with the same result as with copper. In the discussion following Mr. Vail's paper, mention was made several times of the electrically welded track.

Having been intimately connected with the experiments carried out in Cambridge during last summer and winter, I think a few remarks in reference thereto will be interesting at the present time.

The tracks experimented upon were all, before any welding was done, condemned by the West End Co. The process was, therefore, expected to save them. The original method consisted in welding two U shaped yokes on either side of the joint through the web of rail. Experiments have proven that from 90 to 110,000 lbs. was the highest tensile strength that could be obtained on such joints by this device.

Of the welds made on Main street, Cambridge, about 10 per cent. have broken through the winter, leaving quite a number of uninterrupted sections of 600 feet and even one of 1,000 feet. It was soon discovered that the method of welding first used was not quite satisfactory, as it produced a local strain in the rail by heating only the web part of the rail to a welding heat. Experiments were made at once with a view of welding the rails *right at the joint* and extending over the whole surface of the rail, including rail and foot. By this process a tensile strength of 279,000 lbs. could be obtained where one of 100,000 only was possible before.

Allowing for maximum variation in temperature experienced in our climate, it was found that 150,000 lbs. would be sufficient to withstand any strain brought upon the rails through contraction and expansion alone.

The season being already far advanced when these experiments were concluded, only a part of the track in Cambridge could be provided with the new joints. Every one of these welds stood through the winter without a break, with the exception of one which broke the day it was made and showed poor workmanship. A heavy snow storm setting in at that time prevented its being repaired.

From all the experiments made up to date we can draw the following conclusions:

1st. A continuous rail can be used for electric street rail-car practice with absolute safety if an expansion joint is inserted every 600 feet.

2nd. A continuous rail can *probably* be used without any expansion joint by using the latest method of welding.

3rd. The electric conductivity of any joint is as great as the rail itself.

4th. All joints being of the same material and continuous even when elastic, will not change by vibration, heating or electrolysis.

5th. By frequently cross connecting the rails—making up a track of electrically welded joints, any trouble from interruption in the track can be obviated.

6th. The resistance of the feeders from track to power-house is immaterial aside from *economy*, provided the ground in power-house is taken off.

In conclusion I will say that Mr. A. J. Moxham, President of the Johnson Co., who has, and is carrying out all these experiments, has such faith in the ultimate success of the electric welding of rails that through his instructions three machines will shortly be in operation to weld one double track from Marcy Avenue, Brooklyn, to Manhattan Beach, a total of 35 miles of track, also seven miles in St. Louis, and other orders for this work are pending.

THE CHICAGO MEETING, APRIL 25TH, 1894.

The corresponding meeting of the INSTITUTE at Chicago was held on April 25th, one week later than the New York meeting, owing to the fact that Mr. Farnham's paper was accompanied by a large number of lantern slides, which it was not convenient to reproduce for simultaneous use in Chicago. The meeting was held as usual at the rooms of the Armour Institute, where Professor Stine kindly gave the use of the necessary lantern, and provided other needed apparatus. The meeting was well attended, the total number present, including members and guests, being more than 60. The meeting was called to order by the Local Honorary Secretary and, upon motion of Mr. B. J. Arnold, Lieut. Samuel Rodman, Jr., was appointed Chairman.

Mr. Farnham's paper was read by Mr. A. V. Abbott, the Chief Engineer of the Chicago Telephone Company. A number of slides, not included in the illustrations of the paper itself, were placed upon the screen, showing sections of corroded pipe from various cities. After the reading of the paper, the discussion was opened by Mr. B. J. Arnold.

MR B. J. ARNOLD:—We all remember the trouble which was caused about five or six years ago by the action of the electric railway current upon the telephone wires, through induction, which rendered speech almost inaudible over many of the telephone lines, and created a very general agitation throughout the country. The telephone companies, after much ineffectual argument with electric railway officials, city councils and others, finally met the issue squarely, and applied the heroic and effective remedy of a complete metallic circuit, which very largely eliminated the trouble. It now seems that after the rival interests have satisfactorily settled that subject, that the telephone companies have encountered a new difficulty which threatens to be much more serious than the former.

In my judgment this question should be met in a fair spirit by the railway companies and some means proposed and adopted that will not compel the telephone companies to stand practically the entire expense of the necessary changes, as they did in the former case. It should be borne in mind also by the railway companies that the telephone people are not the only ones who will rise up in their wrath against this new trouble, as it is equally detrimental to the water mains and the lead pipes of the water companies, the gas pipes and other metallic underground constructions.

The subject of electrolytic action of the railway current on underground pipes and conductors is now being agitated in almost all cities where railways have been running long enough for the destructive effect of the current to make itself known, and it will not be long before we shall hear from it in our own city, if the many ordinances now being considered by the Council are passed without some stipulations regarding the construction and carrying capacity of the rail return circuit of the roads which the companies propose to build under these franchises. In my judgment there is no reason why an electric road cannot be built so as to almost effectually prevent the destructive electrolytic action that has been pointed out to us to night.

The original method of constructing the return circuit of the electric railways was to join the ends of the rails with No. 4 bond wire, depending upon this and the fish plates for whatever metallic circuit was necessary, and the earth to make up for the lack of metal.

The idea of the supplementary wire was not contemplated on the first roads, but as the small bond wires soon became eaten off or broken, they were found inadequate, and a No. 0 copper bond wire was added to supplement the rails.

Examination in some cases has shown that the supplementary wire has disappeared entirely after a few years' use, which resulted in the abandonment of it entirely, and the adoption of the larger bond wires between the ends of the rails. It is now customary to use a No. 0 wire to bond the rails with, and depend upon the rails alone for the return circuit. I am of the opinion that if the rails are supplemented by a system of feeders, so that the resistance of it is not greater than that of the out-going circuit, and the rails joined with a bond equal in conductivity to the rail itself, which is most effectually accomplished by welding the rails together electrically, that the entire difficulty caused by electrolysis will disappear. The Pennsylvania Railroad Company has had a section of the track with the rails welded together, in operation for some months, and on the Baden and St. Louis R. R. of St. Louis, rails are now being welded together by electricity, for use as the return circuit of an electric road, and we shall look for the results of this experiment with interest.

The suggestion made by the author of the paper to attach feeders to the water pipes near the power station is a good one, and should be adopted in all cases.

The suggestion of Professor Thomson of the application of motor transformers for the purpose of relieving unequal potentials seem to be impracticable to construct, as well as difficult to look after, as it would necessitate numerous electric motors running in various parts of the city, the location of which would have to be changed from time to time to correspond to the fluctuating danger line, and would result in constant expense for maintenance as well as a large additional equipment. The author of the paper states that this has never been tried, and I think it is safe to assume that it will not prove practical if it is attempted.

Mr. A. C. Balch, of Portland, Oregon, in a communication to one of the railway papers recently, described the method of using the three wire system on the electric railway in that city.

The trolley is divided into sections of about one thousand feet in length, the positive wire of one dynamo being connected to one section and the negative of the other dynamo to the adjoining section, while the neutral wire is connected to the rails. It will be noticed that with this arrangement there is a difference of 1,000 volts between the adjoining sections of the trolley wire, but as they are thoroughly insulated and the motor cars pass from one section to the other quickly, there seems to be no difficulty caused by the sudden reversal of the current through the motors when passing from one section to another. So long as the cars are properly distributed on the line, I see no difficulty in the operation of an electric railway with the three wire system, but if the cars were to become "bunched," as sometimes happens in large cities, the feeder wire supplying that particular section would be overworked, although I do not regard this difficulty as serious. With this arrangement the electrolytic action on pipes is almost, if not entirely, avoided, as there is practically no current flowing from the trolley to the earth when the road is working under normal conditions. I believe there is also a three wire electric road in operation in Bangor, Maine, and so far as I am able to learn it is working with entire satisfaction.

There are many who advocate the double overhead trolley system, which was used on many of the pioneer electric roads, all of which have been abandoned, so far as I know, except one or two roads in Cincinnati. The chief difficulty with such a system is the complication of the overhead construction, marring the appearance of the streets, and difficulty in operation during snow and sleet storms.

If the alternating motor ever becomes perfected so as to make it applicable for electric railroad work, our grief from electrolysis will cease, but unfortunately this millennium seems far away.

I have a communication from Mr. Alex Dow, City Electrician of Detroit, Michigan, who is thoroughly qualified to speak on this subject. I wish to submit it as a written discussion from him, as it contains valuable data concerning the condition of potential differences existing in Detroit, and the result of the consideration of the subject before us to-night, reached by the municipal engineers of Detroit.

Detroit, April 23, 1894.

B. J. Arnold, Esq.,
Chicago, Illinois.

I enclose a print of a table of measurements of potential differences between the railroad tracks and other underground constructions in this town.

TABLE OF THE POTENTIAL DIFFERENCES EXISTING BETWEEN WATER AND OTHER PIPES AND THE STREET RAILWAY RAILS, DETROIT, MICH., MARCH 30th, 1894.

Location.	Sign.	Observed P. D. Max.	Min.	Mean.	Remarks.
Woodbridge and Antoine	Water pipe positive	0.4	0	0.1	Observations taken on a branch track to car barns about 70 feet from Station.
Jefferson and Antoine	Water pipe positive	1.7	0.2	1.0	Observations about 350 feet from Station and within 50 feet of connecnection to a return or track feeder. Cars passing frequently.
" " "	Gas pipe positive	4.0	1.2	2.0	
Jefferson and Randolph	Water pipe positive	1.3	0.5	0.6	Distance from Station about 1,405 feet. Cars passing frequently.
Jefferson and Baldwin	Rails positive	7.3	0.3	1.2	Distance from Station about 12,900 feet of which 8,500 is 70 lb. girder rail remainder strap rail.
Jefferson and Holcombe	Rails positive	9.8	0.3	2.0	Distance from Station about 17,500 feet of which 8,850 feet is of 70 lb. girder double track.
Jefferson and Cadillac	Rails positive	29.6	0.5	4.6	End of water pipe system. Distance from Station about 4 miles, track continued 3 miles further, single track 60 lb. "T" rail.
Elmwood and Mack	Rails positive	90.9	0.1	7.0	Straight line from end of track to Station 11,000 feet; no return or track feeder; single track. Strap rails, 42 in. water main parallel to tracks. P. D. small until cars come on particular rail to which wire was attached.
Woodward and Canfield	Rail positive	3.9	1.5	2.5	Distance from Station about 10,200 feet, 70 lbs. girder rails.
Champlain and Seyburn	" "	8.7	0.5	2.5	Distance from Station 6 miles.
Champlain and Mt. Elliot	" "	7.2	0.8	1.8	Distance from Station 5 miles.

The tests were made by a committee of the Municipal Engineers' Association with the view of learning the actual state of affairs, and particularly in view of street railway legislation now before the City Council. We desired to know the results obtained with the present street railway constructions, and, if it appeared necessary, to ask for protective clauses in the street railway ordinances.

The Association has not yet acted upon the committee's report, but the opinion of the members seems to be in favor of requiring the street railway company to put in a complete return circuit of such efficiency that there will be no appreciable tendency to leakage to the water pipes, underground cables, etc., for which we municipal engineers are responsible.

We admit the theoretical possibility of connecting our constructions with the street car return and with the negative pole of the dynamo so as to maintain a uniform earth potential all over the city; but we conclude, after consideration of the different depths at which our various pipes, etc. are laid in the soil, of the different ages and characters of the said pipes, etc., and of the varied nature of the soil itself, that carrying out the theory into practice will be very difficult; so that we are minded to ask for restrictions as to the potential difference permissible between the rails and neighboring underground constructions such as we want to protect. We don't entirely believe that this difference of potential is a measure of the risk of injury by electrolysis, but we are positive that when the potential difference is inappreciable the return circuit of the railroad company is sufficient for the work put upon it, and our pipes are not being called on to assist it to any dangerous extent.

ALEX DOW, City Electrician.

I am sure that we have all been very much benefited by the reading of Mr. Farnham's paper and the excellent manner in which Mr. Abbott has presented it to us this evening, and I hope that it will result in a thorough discussion by the members present.

Mr. Welles of the Western Electric Company, whose name was mentioned by Mr. Farnham in his paper, was present and was asked to contribute to the discussion. Mr. Welles in reply said:

MR. A. T. WELLES:—Not being a member of the INSTITUTE I came here with no idea of being called upon to speak, but as Mr. Farnham has quoted me as saying that "in examining the first cases in Cincinnati" I found never more than one-half volt and generally less than one-quarter volt current on the underground cables there, I wish to say that Mr. Farnham has unintentionally misquoted me, as was also the care in the interview in a Cincinnati paper which brought about my correspondence with him. The statement would lead you to suppose that some electrolytical trouble had developed in cables laid by my company in Cincinnati, but this is not the case as far as I know.

In December last a cable which we had laid in Louisville less than a year ago for the Ohio Valley Telephone Co. began to give out. We sent a cable splicer there and he located the trouble in two sections of several hundred feet each, near one of the power-houses of the electric street railway. About Christmas, Captain Gifford telegraphed my company requesting that Mr. Patterson or myself should come at once to investigate the cause of the trouble, and I was sent. I found that the two sections had been partially pulled out of the conduit and in examining the pieces cut off, found that holes were partially or wholly eaten through the lead covering at regular intervals. These holes occurred every 18 inches and corresponded exactly with the joints of the vitrified clay conduit.

With Mr. Maxwell, the electrician of the telephone company, I made a number of potential tests in the manholes near the place of the trouble and found as high as two and one-half volts from the cable to the rail of the trolley road. In these manholes there were also gas and water pipes which had about the same voltage as the cable. I also made 40 or 50 tests across the joints of the rails with a voltmeter reading down to one-tenth and I think we could have noticed one one-hundreth of a volt, but there was not the slightest movement of the needle.

These tests were reported to Captain Gifford, who then called in the electrician of the street railway. This gentleman explained that besides having the rails perfectly bonded at their joints, they had two return wires of No. 0 or larger copper run underneath the rails and bonded to them at frequent intervals.

In Louisville the underground telephone cables run from all points of the compass into the company's office. These cables, some 30 in number, are practically bunched at or near the exchange, so that any currents coming in on their sheaths can, without trouble, reach those of such cables as will most readily return them to the power-houses. All of these cables run parallel with trolley lines or with gas or water mains which parallel the trolley, so that each carries to the common point more or less current. Taken singly, these currents are slight and would probably cause no trouble for a long time at least, but one-half or two-thirds of their total was sufficient to eat through the pipe of the cable in trouble in seven or eight months. At present there are but two cables which run from the exchange to points near power-houses. One of these runs to a point within a block or so of a power-house about two miles from the exchange in one direction, and the other to within three blocks of a power-house in the opposite direction. The first one giving the best return and also running for a long distance parallel with a trolley line and water mains, where it picked up additional current is the one which gave out. Preventions which we used will, I suppose, save the other. From Louisville I went to Cincinnati where we had laid a large quantity of cable for the City and Suburban Telegraph Association. Here, with Mr. Robinson, their superintendent of construction, I made a large number of voltmeter tests between the cables and the trolley-road rails in different parts of the city, and found at no point more than one-half volt current in either direction. These tests led to the interview previously mentioned. Some weeks later Mr. Farnham wrote me that he had read an "interesting" interview with me in a Cincinnati paper and asked me for data on the subject of electrolysis. As the interview as published *is* rather "interesting" in some respects, I will read it if you will allow me.

[Mr. Welles read the article and proceeded as follows:]

I simply wish to make one other statement. Since Mr. Farnham collected the data for his paper, we have run across another very peculiar case of electrolysis in Cleveland. A cable which my company laid there in January last for the Postal Telegraph

Co. was reported as having given out entirely about two weeks ago. The cable ran from their office down about 1,500 feet and then through a tunnel about 80 feet deep under the river to a pole on the opposite side near a manhole at the mouth of the tunnel. Through this tunnel a large water main also runs. We sent a splicer there at once and he located the trouble in this manhole where he found the cable pipe entirely eaten through.

Mr. L. L. Summers, of the Postal Company, made voltmeter and ammeter tests at this place and found 18 or 19 volts on the cable, and also on the water main in the tunnel and 45 amperes current. The cable pipe was eaten through in ten weeks. What is going to become of that water main, and what is going to become of that tunnel?

PROF. W. M. STINE:—I would like to ask Mr. Welles what conditions he found where they were using the double trolley in Cincinnati.

MR. WELLES:—My tests were made on New Year's Day, which was particularly dry and clear. This probably accounted for the very low potentials found on the cables. On account of the intricate system of water and gas mains running close to the conduits, and often through the manholes, it is impossible to say whether the leakages came from the single trolley or the double trolley system. We found current on cables which, in no part of their circuit, paralleled either system. I was told, however, that in rainy weather much trouble is experienced by the telephone company on account of the splashing of mud against the car motors of the double trolley system, which often brings down 200 or more drops at a time in the switchboard. But this, I should think, is simply a matter of construction of the motors.

Professor D. C. Jackson of the University of Wisconsin had sent in the following contribution to the discussion which was read by the secretary.

[COMMUNICATED BY PAUL BIEFELD AND FRED. D. SILBER.]

Having made an extended series of experiments relating to the subject of Mr. Farnham's paper, we desire to present some of the results of our investigation as carried on in the laboratories of the University of Wisconsin, and along the line of the electric railway of Madison, Wisconsin, under the direction of Professor Dugald C. Jackson.

This railway line has been in operation for about a year and a half, having nine cars in service, but no troubles due to corrosion have shown themselves thus far. Measurements of potential between water pipes and rails, near power station, show a difference of 4½ to 5½ volts, water pipe being positive to rail, and 2½ to 3½ volts between water pipe and the earth near the rail, water pipe being positive to earth. The potential reversed at a distance of from 1,600 to 1,800 feet from the station, the range being zero to four volts outside of this neutral zone, the water pipes being negative to rails and earth. Samples of soil were taken from points along the line, and subjected to electrolysis between iron plates, when the anode showed a loss ranging from .7 to 1.15 grams per ampere hour, the cathode remaining unchanged.

Experiments are at present in progress to determine the actual current flowing between pipe and rail. This is done by means of a modified form of a copper voltameter devised for this purpose. A water-tight box containing a solution of copper sulphate is buried in the ground. This box has for two of its opposite sides, facing the pipe and rail respectively, double copper plates, of which the inside ones are weighed. The gain of cathode will then give in the usual way an integral measure of the current. The shunting effect of the voltameter of low resistance, will introduce only a small error, as the distance between pipe and rail is comparatively great. Having thus found the average current flowing, and knowing the loss of anode per ampere hour, a fair estimate can be made of the loss of metal from the pipe per month.

A series of experiments to determine the minimum potential at which corrosion may go on between iron plates, showed that a mere *directive force* was needed; the potential having been reduced to .001 of a volt, electrolytic action still went on. In each case the action was manifested by the formation of a ferrous salt at the anode, and the hydroxide of the alkaline metal at the cathode. This phenomenon shows at once the cause of the corrosion. The main object of our investigation has indeed been to determine exactly what goes on in an electrolytic cell under parallel conditions to those found in practice.

Whenever iron electrodes were used with soil or sand containing small quantities of soluble salts in solution, a layer of ferrous hydroxide was invariably formed at some point between the two plates. The earth used was found to contain small quantities of chlorides and sulphates of the alkaline metals, and showed no trace of soluble iron salts or alkaline action, before electrolysis. After the current was put on, ferrous salt began to be formed at the anode spreading toward the cathode; the hydroxide of the alkaline metal was formed at the cathode spreading toward the anode; where the two met, a layer of ferrous hydroxide was precipitated, recognized at once by its dirty green color.

In all these cells a peculiar heating effect was noticed, reaching a maximum in the ferrous hydroxide layer, and diminishing in general toward the electrodes. Measurements of potential taken within the cell at different points between the plates showed the greatest difference of potential at the zone of maximum temperature, and a diminishing difference in the same way as the thermometer had shown a fall in temperature. It must be stated here that the readings of potential were taken when no appreciable heating effect was noticed, only a very small and constant current flowing at that time. These observations show that the unequal heating was caused by unequal resistances in different parts of the cell due to the formation of the hydroxide layer, together with the unequal concentration of the iron salt and the alkaline hydroxide, which diffuse toward the point where the precipitate of iron hydroxide is formed.

A similar variation of temperature, only much less marked, showed itself in a cell containing pure sand (practically free from soluble salts) and distilled water. The platinum electrodes were six cm. apart, 500 volts were used and a current of one ampere sent through. Oxygen and hydrogen were given off at the positive and negative poles respectively, as secondary products of the acid which was formed at the former, and of the hydroxide which was formed at the latter pole. There was no electrolysis of water, as seems to be still held by some. Although the electrolyte was thought to be free from soluble salts, the temperature effect was still present, and was caused by the diffusion and consequent unequal concentration of the acid and hydroxide toward a point between the two plates where the zone of maximum temperature showed itself. This is to be expected since the conductivity of acids and alkaline hydroxides varies within certain limits directly as their concentrations.

The theory has been put forth that the corrosion is due to the electrolysis of water; the nascent oxygen attacking the iron at the positive pole. In the first place, there is no electrolysis of water; oxygen and hydrogen being in each case set free at their proper poles by virtue of secondary reactions, as explained above. The fact, however, that nascent oxygen even then does not attack the anode when the iron electrodes are used, is clearly pointed out by an experiment, wherein six such cells were run in series at 100 volts terminal pressure, the current varying from .2 to .04 amperes during the experiment. The electrolytes consisted of pure glass sand saturated with a one-third of one per cent. solution of chemically pure nitrate of ammonia, chloride of ammonia, nitrate of potash, chloride of potash, nitrate of soda, and chloride of soda respectively. All nitrate cells showed acid reactions at the anóde, and gave off oxygen at the same pole, and hydrogen as usual at the cathode. At .06 amperes, the acid reaction and oxygen evolution ceased, in the cell containing nitrate of ammonia; at .045 the same occurred in the cell containing nitrate of potash, the cell containing nitrate of soda still showing a faint acid reaction, and giving off of oxygen at .04 amperes, at which point the current was taken off. The chloride cells showed no such phenomena at the anode. The following table gives the corresponding losses of the different anodes per ampere hour, the cathodes remaining unchanged:

NH_4NO_3	0.921	gms. loss.
NH_4Cl	1.314	" "
KNO_3	0.887	" "
KCl	1.346	" "
$NaNO_3$	0.729	" "
$NaCl$	1.299	" "

It will be noticed that the loss in the chloride cells is considerably greater than that in the nitrate cells; the acid and oxygen formation in the latter having evidently not assisted in the corrosion of the anode. Furthermore, had the nascent oxygen attacked the iron, ferrous oxide, (FeO) would have been the result, which, in presence of water forms ferrous hydroxide ($FeO + H_2O = Fe(OH)_2$). This ferrous hydroxide would have showed itself next to the anode; no such layer, however, appeared in that region. Moreover in most cases where pipes have been pitted, the "secondary products seemed to have been carried away."

The real facts and explanation are given in the following conclusions: In an electrolytic cell with iron electrodes, having any salt or salts of the alkaline metals or earths in solution in the electrolyte, the salts are electrolyzed; their acid radicals (Cl, NO_3, SO_4, etc.), attacking the anode, forming an iron salt. The metal set free at the cathode forms with water the hydroxide and liberates hydrogen. The ferrous salt diffuses toward the cathode, the alkaline hydroxide toward the anode, and at the point of meeting the ferrous hydroxide is precipitated and the original salt reformed. As the amount of electrolysis varies directly with the strength of current, a comparatively high current will liberate an excess of the acid radical; the critical point depending on the affinity of the radical for the iron, the surface exposed being the same. This excess forms in the presence of water an acid, and liberates oxygen.

Neither the acid nor the oxygen have any effect on the iron, the anode being already engaged in the formation of the iron salt with the acid radical; hence, the oxygen escapes, and the acid diffuses through the electrolyte. Many important physical and chemical questions arise at this point; these, however, cannot be considered here.

The last experiment, together with the fact that the ferrous hydroxide layer invariably forms at some distance away from the anode, points very strongly to the conclusion that the corrosion is *solely due to the attack of the acid radical of the salt originally in the electrolyte.* As chlorides, sulphates and nitrates of the alkaline metals and earths are almost invariably found in street solids, and as a mere directive force in the way of an electric pressure is required to set up an electrolysis of these salts, it can be plainly seen that the iron pipes may be corroded wherever the potential at the latter is positive to the rail. The amount of corrosion in a given time only depends upon the amount of the current and the nature of the salts in the soil.

Our investigations lead us to indorse Mr. Farnham's method of preventing the corrosion, and the following points in his summary we desire to particularly emphasize:

1. A return circuit of as low resistance as possible.
2. A positive pole of the generator connected with the trolley.
3. Careful location of the danger district and thorough connections between pipe and rail inside of this district; the rails to be connected to the negative pole of the dynamo.
4. Frequent measurements to be made to see that the danger district does not change, and no connections to be made outside of the district.

Madison, Wis., April 23, 1894.

Mr. C. G. Armstrong:—I cannot help but feel that we are guilty as charged in this indictment, that we cannot have a great good without some small damage, and to offset the manifold blessings and benefits of the commercial use of powerful currents of electricity, we undoubtedly do some damage to other and adjacent interests.

Some years ago I was requested to inspect an electric railroad in Indiana, where I had some experience with the erratic action of electricity under ground, and where I made some measurements of differences in potential between adjacent conductors. Unlike Mr. Farnham I did not use a voltmeter, but used a mule: that is to say, the owners had noticed a variation in the operation and speed of their motors at a certain point. I watched a mule team driven over this place and found that at a certain point one of the mules became very much agitated, to the extent of planting his hind feet on the dashboard of the wagon many times in rapid succession. I had no means of calibrating the mule, but would judge that the potential difference must have been considerable at this point, and upon inspection I found that the rail bonds were broken, and a very disagreeable shock could be felt by touching the ends of the rails at this point.

The construction of this plant had been very unscientific and extreme differences of potential could be found in different parts of the town, but being only a country village with few pipes and fewer scientists, the matter of destruction to anything except the railroad property itself by reason of this faulty work was not thought of.

Wherever we have a flow of current through the earth, owing to its irregular conductivity, we are bound to have differences in potential, and where we have differences in potential, electrolysis and chemical decomposition will occur. Even if this difference is extremely slight we will have some destructive action; at the same time I doubt whether we can have any serious destruction without we have one and one-half volts, or sufficient to decompose the moisture, liberating the oxygen which in its nascent state, to my mind, is the most destructive agent produced by electrolysis.

I cannot feel that electrolysis is guilty of all things charged to it. I do not believe that every defective water pipe and gas main was destroyed by electricity. I have seen miles of water and gas pipe that was in much worse condition than that shown on the screen to-night, where the nearest electric wire was twenty miles away. I believe the gas companies are responsible for two-thirds of the trouble found in pipes to-day. Where we dig up the streets we find the earth permeated with ammonia and other destructive products of the gas retort, which within themselves are sufficient to attack and decompose any metal they come in contact with; in fact, one of the pipes shown on the screen was a gas pipe, and there was a serious decomposition of the metal on each side of the union. The author of the paper attributes this to the resistance of the union; why might it not have been the leakage of the gas? I have found gas pipes that could not possibly be acted upon by electricity that were decomposed in exactly the same manner, from the gradual leakage of the gas.

Undoubtedly we are guilty to a certain extent for the destruction of underground pipes, and the cause I believe can be removed. The use of the three wire system as mentioned by Mr. Arnold is undoubtedly one solution, but offers complications as he mentioned. The use of the return wire as suggested in the paper is another remedy, but one which I am afraid will greatly interfere with the "returns" at the meeting of the stockholders.

As mentioned in the paper, every additional pipe protected, reduces your protection to all others and to obtain absolute protection, it would be necessary to literally fill the ground with copper wire, and have the different resistances nicely and mathematically balanced, and even then with a large number of cars at the extreme end of the line, the resistance might be thrown out of balance and electrolytic action result therefrom.

I think this problem should be viewed just as we do the wiring of a building, viz., avoid grounded circuits, insulate both poles and thereby dispense with electrolysis, danger from fire, and at once solve the difficult problem of lightning arresters for grounded circuits.

There seems to be no practical difficulty in the use of the two trolleys. They are running in many places to-day with a fair degree of success and I believe if it had not been for the violent opposition of one of the large companies, as suggested here to-night, it would have been used much more extensively than the single trolley. I really do not see after all why it is not the best way out of the present difficulty. It certainly would not cost as much to run a double trolley system as it would to run a complicated system of underground returns connecting to all water, gas and other pipes and what not that we meet in the street. The system advocated by Mr. Stetson, of an underground double trolley, or a similar system with his catch box elevated on posts between the tracks, would afford another method of accomplishing the same end.

If electrical engineers would lay aside the creeds laid down by their various commercial interests and attack the problem from a purely scientific standpoint, following out the practice of electric light engineers, viz., insulate both poles from the ground, the question of electrolysis would be solved just as the metallic circuit has solved many more serious problems in telephony.

Brief remarks were made by Professor Stine, Mr. Beach and Mr. A. S. Hibbard.

Mr. Abbott closed the discussion on behalf of the author of the paper, as follows:

Mr. A. V. Abbott:—A previous speaker has alluded to the use of the double trolley system, as offering a solution of the electrolytic question.

The double trolley system certainly does afford a perfect solution, but only at the expense of a greater investment of capital in the original line construction, as the amount of copper required for a double trolley system over that needed with the single wire would be increased about fourfold, and as much complexity would be involved in the erection of the wiring. The objections to the double trolley system, other than those of increased capitalization, are entirely mechanical ones and in ordinary lines can be overcome. The double trolley, about three years ago still survived, and it is probably chiefly due to the Thomson-Houston company that it does not still exist.

In all railways which are reasonably straight, and do not encounter a great number of intersecting lines, the introduction of the double trolley is not a serious obstacle, and by affording to the street railway an independent return, places the railway circuit entirely under control of the railway managers, presenting to the company in this respect considerable advantage. It is probable, nay, almost certain, that should street railway companies be obliged to protect all present existing underground structures by means of special return feeds, as indicated by Mr. Farnham, the expense of these feeds, and their introduction would be considerably less than the original cost required to equip the line with a double trolley system.

Personally, I have always been in favor of all electrical companies operating entirely upon metallic circuits which should be peculiarly and appropriately their own. I think the advantages to be derived from this principle of operation will, sooner or later, be appreciated, and that street railway companies, electric lighting companies, telephone corporations, in fact, all electrical industries will, in the not far distant future, be each equipped with its own individual and independent complete circuit.

The presence of overhead wires in the crowded city streets, is constantly urged as an objection to the trolley system, whether it be single or double. Inventors have been constantly called upon to devise methods whereby the streets could be relieved from this objection.

A conduit electric road is at the present time perfectly feasible, and its successful construction and operation is merely a question of the amount of capital that the promoters are willing to invest. Ordinarily speaking, an electric road can be built and equipped in running order at an expense of from thirty to forty thousand dollars per mile including all items, excepting that of real estate, franchises and buildings. The cable road is typical of the conduit system, and is always expected to cost from one hundred twenty-five, to one hundred fifty thousand dollars a mile. The widespread and rapid introduction of electric roads has chiefly resulted from the fact that they require so much smaller capitalization in the outset, and that they, therefore, may be introduced in districts that are not thickly settled, in which the traffic could never be made to pay the interest and depreciation on the more expensive cable plant. Cable roads could never, for a moment, be considered, in many of the districts where electrical roads are now successfully and remuneratively operating, having superseded animal traction.

If a street railway company is willing to invest in an electric road the same amount of capital as is called for by the ordinary cable road, a successful conduit system can be at once introduced. The success of the conduit electrical road simply depending upon its being built well enough to do the work required of it.

In a consideration of the return system for an electric road, the railway company should not forget, that by providing an adequate return circuit which will protect other underground structures, they are not only securing immunity for themselves from damage suits, but at the same time are putting more money into their pockets in a saving of coal pile, than the interest and the depreciation upon the investment involved in the return circuit will amount to.

In building some 300 miles of electric road I have universally found that the grounded return circuit absorbed more energy from the station than any other part of the line, the car motor only excepted. In one instance in mind, the return circuit of the road was so poor, owing to defective rail bonding and the dry condition of the soil that, in many instances, the rail bonds had actually burned their way through the ties of the road and allowed the rails to separate.

In another instance, motor repairs were reduced several hundred dollars a month by the addition of an appropriate amount of feed wire.

In a third case, the amount of power required for operation was reduced to 80 H. P., by the provision of an appropriate return.

I feel quite confident that, if the engineers of the majority of electric railways in this country would carefully study their circuits, making accurate measurements thereon, they would be immediately convinced that the expenditure required for an appropriate return circuit which could in a majority of instances be arranged to protect existing underground structures, would result in an actual saving to them in fuel, and would be an investment upon which they could immediately enter.

After the discussion was closed it was moved by Edward Caldwell, and upon motion it was carried, "that it is the opinion of the western members of the American Institute of Electrical Engineers that the expenses incurred in connection with the local meetings held in Chicago should be paid out of the general funds of the Institute, and that a committee of three, of which Mr. Hibbard shall be chairman, be appointed to bring this matter to the attention of the Institute at the Annual Meeting in Philadelphia next month for the purpose of securing, if possible, such legislation as will make the general funds available for this purpose."

The meeting then adjourned, leaving the other members of the committee to be appointed later.

[REPLY TO THE CHICAGO DISCUSSION, COMMUNICATED BY MR. FARNHAM.]

I have read with a great deal of interest the able discussion in Chicago which followed the reading of my recent paper. The work of Professor Jackson and his assistants is of special service, as it ought to entirely dispose of the doubt heretofore existing in many minds, that currents produced by small differences in potential are sufficient, and actually do cause corrosion of metal buried in the earth. I hope readers of this paper will not pass too lightly over this fact. If we appreciate the danger as it exists, we shall be more likely to apply a sufficient remedy, and we shall look for the danger in important places which otherwise may escape detection until too late to controvert serious loss. The Chicago discussion calls attention to serious effects of electrolysis upon metal lined underground tunnels. How may it be with anchorages to suspension bridges, and to the iron feet of elevated railway structures, especially those employed to carry in a measure the heavy currents of the railway?[1]

Referring to Mr. A. T. Welles' remarks. I did accidentally confuse Cincinnati with Cleveland.

It was unfortunate, as Cincinnati having a good example of the double trolley system might be expected to escape the trouble from electrolysis. This expectation would undoubtedly be realized but for the fact that there is also in the city a small electric railway system employing the single trolley.

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1. The most powerful and important agents in nature are slow in their operation.

I have but little criticism to offer upon the discussion. I agree entirely with all that has been said, both in New York and Chicago, as to the importance of large railway returns and thorough bonding of rails. Mind you, I do not say "ample" railway returns, for returns ample to prevent escape of current to the earth and adjacent pipes cannot practically be provided.

I am in hearty accord also with the able remarks of Mr. Abbott and others with reference to the employment of the "double trolley system," or in fact any system which maintains the entire circuit well insulated from the earth. My paper implies this, and to this end I have written several articles in the early days of the railway ventures.

I do not agree that it is as well to connect pipes to the rails within the danger zone or area as to employ a special return wire. for relieving them. A careful study of the situation which may be met in large cities will make it clear that not only must the pipes have good conductors back to the dynamo, but that the potential of the earth about them must in some cases be raised above the normal condition; this can be accomplished in no way easier than by so proportioning the return system of the rails and the pipes as to cause a small passage of current from rails to pipes through the earth, within the danger territory.

Nor do I agree with Mr. Armstrong, that the ground may have to be filled with copper to accomplish the remedy if applied as I recommend. A careful perusal of the paper itself, and the New York discussion will make it unnecessary to explain further why a comparatively small amount of copper properly placed in the danger area will transfer the damaging action from the pipes to the railway system itself.

I must note again in closing, my great satisfaction with the handling of the paper and the discussion at Chicago, and I wish to express my personal thanks to all who have aided me in the preparation and presentation of this paper, and to others who have added so much of interest and value by the discussions.

Boston, May 9, 1894.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

ANNUAL MEETING.

PHILADELPHIA, PA., MAY 15, 1894.

The Annual Meeting of the INSTITUTE was called to order at 10.30 A.M. on Tuesday, May 15th, at the house of the Engineers' Club, 1122 Girard Street, by the President of the INSTITUTE, Professor Edwin J. Houston.

It was voted that the reading of the minutes of the previous annual meeting be dispensed with, and, on motion, they were approved as printed in the TRANSACTIONS.

THE PRESIDENT:—Before proceeding to the next order of business, which will be the report of the Council, the duty of the meeting is to appoint tellers. The rules call for two tellers. Will some gentleman make a nomination?

Messrs. Metcalfe and Upton being nominated, they were appointed by the President as tellers.

Mayor Stewart, of Philadelphia, was then introduced by the President, who welcomed the INSTITUTE to the city, with appropriate remarks.

THE PRESIDENT:—Mr. Mayor, I desire to express, on behalf of the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS, the body I have the honor of presiding over at the present time, their thanks for the warm and cordial manner in which you have welcomed us to the city of Philadelphia, for the flattering things you have said about us, and for the kind invitation you have given us. I assure your honor. that we shall hold this meeting in Philadelphia in pleasant remembrance.

MR. JOHN C. TRAUTWINE, JR., President of the Engineers' Club, of Philadelphia, upon invitation, addressed the meeting as follows:—I trust that I may be able to give expression to the sincere pleasure it affords me to bid you, in the name of our club, a hearty welcome to our house. where I trust that you will be able to make yourselves comfortable during your brief stay in our city.

Our club, as must almost of necessity be the case in a local engineering organization, numbers among its members engineers of all branches of the profession, and among these are many worthy and active representatives of your own branch, and members of your INSTITUTE.

His honor, the mayor, has already pointed out to you the fitness of selecting, as the place for holding your tenth annual convention, the city of Franklin's electrical researches, the city where his remains were laid to rest. But Philadelphia claims also the honor of being the birthplace of your still young, and already vigorous, organization, and in this way we see another claim to your attention.

I congratulate you upon the excellent weather which your local committee has selected for the occasion, and I trust that your stay with us may be in every way enjoyable.

The following reports of the Council and Treasurer were then read.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

REPORT OF COUNCIL FOR THE YEAR ENDING APRIL 30th, 1894.

In compliance with the rules of the Institute your Council begs leave to submit the following report covering the period since the Annual Meeting, May 16th, 1893.

Ten regular meetings and one special meeting of the Council have been held at which the average attendance was 9.

As stated in the report of last year, there has been a desire manifested by the membership for the adoption by the Institute of an official badge, and a certificate of membership. Many designs were submitted and the matter was given careful attention by a special committee. Both the badge and certificate were prepared for distribution about the date of the last annual meeting. Your Council has also instructed the Secretary to print upon all publications of the Institute the emblem thus officially adopted.

In view of the probability of a large number of visitors at the World's Fair and Electrical Congress, who would be glad to avail themselves of office facilities, your Council recognized the necessity of establishing at the Electricity Building, a suitable rendezvous, which was placed under the direct charge of the Secretary. As no fund existed which could be properly drawn upon for this object, it became necessary to issue a call for voluntary subscriptions for this purpose. There was a general and hearty approval of the plan and as will be seen by the financial report, the receipts were $1,148.14. This amount was not quite sufficient to cover the entire expenses which were $1,331.047, leaving a deficit of $183.33. The beneficial results accruing from the carrying out of the plan under the supervision of the special World's Fair Committee may be learned from their final report submitted February 21, 1894, from which the following is an extract.

"The wisdom of establishing official headquarters at the World's Fair "has been conclusively proven, and your Committee believes that the Institute may well congratulate itself upon the results achieved. The Institute "register shows the memorandum of 612 visitors, among whom were many "of the most distinguished electricians of the day. Eighty-one new asso-"ciate members have been elected this season; 15 come up for election on "January 17th, and 21 are posted for the February meeting. A total of "117. Although but few meetings have been held this season, this num-"ber is already in excess of the increase in membership during the entire "years of 1891 and 1892. A large proportion of this increase can be traced "directly to the World's Fair headquarters, and without doubt our member-"ship will continue to be largely augmented by the addition of many of "those identified with the electrical profession who have been led to ap-"preciate the importance of the Institute through information received "at the official headquarters at Chicago."

It may also be stated that the expense of maintaining independent offices was less than would have been necessary had the Institute joined in the maintenance of the general engineering headquarters at Chicago which would have required an assessment of $2 per member, or a total expenditure of $1,380, with far inferior results.

The record of the preliminary work of the Institute in preparing for the International Electrical Congress of 1893 has been very fully placed before the membership in the printed TRANSACTIONS, and it is satisfactory to know that it was generally appreciated, especially in the deliberations of the Chamber of Delegates. In order to provide for the publication of the proceedings of the Congress, an arrangement was entered into between the regularly appointed Publication Committee and your Council, by which the Institute has undertaken to secure a sufficient number of subscriptions to guarantee the publication of the Congress records without loss. The limit of 400 subscribers having been reached, the work has been started and it is expected that the book will be issued by September next. Should any surplus accrue from this undertaking, it has been recommended by the World's Fair Committee that it be applied to cover the deficit in the World's Fair fund as stated. Owing to the lack of time, several matters were left unfinished by the Congress, some of which were deemed of such importance that a committee has been appointed of which the President is Chairman, to carry on the necessary investigations and report upon a standard of illumination. This matter, which was considered the most important item of incompleted work, is in the hands of various sub-committees, having laboratory facilities at their disposal in various parts of the country, and is under the direct supervision of members of the Institute.

The plan of printing and distributing to the entire membership the advance copies of the papers read before the Institute meetings, has been carried out as thoroughly as possible during the year. The distribution of the revised papers with their discussions at the end of the year in a bound volume, has been generally appreciated, and while this plan has added somewhat to the cost of publication, the total expense incurred was less than for the volume of TRANSACTIONS issued for the previous year.

Among other results of the World's Fair year, the increase of membership in Chicago and vicinity, led to a desire upon the part of western members for an opportunity to discuss the various papers brought before the Institute. At the November meeting in New York City, a plan was authorized by which upon the petition of 20 members such meetings might be held either simultaneously or following the meeting at which the paper was originally read. Two meetings under this plan have already been held at Chicago, at which the papers read in New York City on March 21st and April 18th were also read by proxy, and discussed. Letters received by the Secretary from western members indicate a hearty approval of the plan and it seems likely that it will become a permanent practice of the Institute, and lead to a most important extension of its influence. Every movement of this kind which has been undertaken with a view to extending the more general knowledge of the work of the Institute seems to lead to a substantial growth of membership

Without going into the detailed statistics of your affairs during the past 10 years, the following statement is of interest, showing the number of members who have been elected and actually qualified for membership during that period:—

1884	154	1889	113
1885	14	1890	116
1886	11	1891	100
1887	152	1892	96
1888	70	1893	165
			991

The lesson to be learned from this statement is, that there must be yet a large number of electrical engineers who may be expected to become members in the near future, and it has been the policy of your Council to maintain the standard of membership in order that such membership become year by year of constantly increasing value. The ten volumes of TRANSACTIONS already issued form in themselves a very complete record of electrical progress during the life of the Institute. Many of the most important inventions and researches have first been made public under the auspices of this organization,

The total membership at the close of last year's report was 673, classified as follows:

Honorary Members		3
Members		206
Associate Members		464
Total		673
Honorary Members elected during the past year	1	
Associate Members elected	179	180
Total		853

The following have resigned during the year:

S. V. HOFFMAN,	CHAS. TAYLOR,
CHAS. L. POOR,	E. Z. BURNS,
L. H. LAUDY,	E. J. WESSELS,
H. B. CRAM,	G. E. FISHER,
WM. GRUNOW, JR.,	EDW. BLAKE,
J. D. GAINES,	P. P. BEALS,
H. H. HOSFORD,	S. W. HUFF.

Total resignations ... 14

The following have died during the year:

DR. JAMES B. WILLIAMS,	ANTHONY C. WHITE,
GROSVENOR P. LOWREY,	DR. F. SCHULZE-BERGE,
ANTHONY RECKENZAUN,	DR. CHAS. E. ZETZSCHE.

Total deaths	6	
Dropped from the roll on account of non-payment of dues	20	
Elected, but failed to qualify	13	53
		800

Leaving a total membership of 800 on April 30th, (a net gain of 127,) classified as follows:

Honorary Members	3
Members	235
Associate Members	562
	800

The reports of the Secretary and of the Treasurer show in detail the financial affairs of the Institute at the close of the fiscal year:

SECRETARY'S BALANCE SHEET,

For The Fiscal Year Ending April 30, 1894.

Dr.		*Cr.*	
To balance from 1893	$ 23 34	By cash to Treasurer	$10,363 70
Receipts for the year	10,385 26	Secretary's Balance on hand	44 90
	$10,408 60		$10,408 60

ITEMIZED STATEMENT OF RECEIPTS AND EXPENSES OF THE INSTITUTE,

For Fiscal Year Ending April 30, 1894.

General Account.

Receipts.		*Expenses.*	
Treasurer's Balance from previous year	$ 90 90	Office Expenses	$138 92
Secretary's " " " "	23 34	Office Fixtures	12 30
Sundry Receipts	5 40	Telegrams	5 72
Entrance Fees	825 00	Stenography and Typewriting	429 50
Life Membership (E. J. Houston)	100 00	Stationery and Miscellaneous Printing	673 56
Past Dues	229 15	Postage	296 52
Current Dues	5921 50	Messenger Service	39 90
Advance Dues	181 66	Salary Account	1999 95
Electrotypes Sold	153 57	Meeting Expenses	123 93
Transactions Sold	345 31	Rent of Office and Auditorium	900 00
Transactions Subscribers	141 45	Express	11 25
Advertising	254 72	Engraving and Electrotyping	406 03
Received for Binding Transactions	37 14	Publishing Transactions	2337 17
" " Badges	532 70	Advertising	7 50
" " Certificates	158 42	Binding Transactions and Periodicals	246 99
" " World's Fair Fund	1148 14	Paid for Badges	644 30
" " Congress Book (Advance Subscribers)	351 10	Paid for Certificates	337 75
		World's Fair Quarters and Expenses	1331 47
		Subscription to Electrical Congress	50 00
		Expenses Congress Book	47 55
		Copyright	5 00
		Duties	1 75
		Secretary's Balance to next year	44 90
		Treasurer's " " " "	407 54
Total,	$10,499 50	Total,	$10,499 50

The outstanding current bills against the Institute, April 30, amounted to	$1,764 46
Due from members	1,185 13
Due from advertisers and others	277 71

The financial depression of the past year has been severely felt in electrical circles, and has had its effect in retarding collections. Fortunately, however, this has not interfered with the ordinary administration of your affairs, and has certainly not checked the growth in membership, which is in excess of any previous year.

Respectfully submitted by direction of Council,

RALPH W. POPE.

Secretary.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

New York, May 1, 1894.

TREASURER'S REPORT,

FROM MAY 1, 1893 TO MAY 1, 1894.

GEORGE M. PHELPS, TREASURER, in account with

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

Dr.

Balance from May 1, 1893	$ 90 90	
Received from Secretary, May 1, 1893 to May 1, 1894..........	10,363 70	$10,454 60

Cr.

Payments from May 1, 1893 to May 1, 1894, on warrants from Secretary, Nos. 429 to 521, inclusive..........................	10,047 06	
Balance to new account..........................	407 54	$10,454 60
Balance on hand, General Fund, May 1, 1894		407 54

BUILDING FUND.

Balance as per last report..........................	$ 850 00	
Interest accrued to May 1, 1894, 3 per cent., to May 14, 1892 and 2 per cent. thereafter..........................	55 49	905 49

Cash book and warrants herewith for audit. Vouchers are in the hands of the Secretary, to whom they are returned for filing after payment.

GEO. M. PHELPS,

Treasurer.

New York, May 5, 1894.

Messrs. Hammer and Willyoung were appointed by the Chair to audit the accounts of the Treasurer.

THE PRESIDENT:—There are various items of business which ought properly to come before the INSTITUTE to-day; among others are reports from the Committee on Units and Standards, and other matters which probably will suggest themselves to you.

It may not be amiss now to give one or two notices. The Reception Committee, to whom is referred the matter of the informal reception tendered to our INSTITUTE by the Engineers' Club, of Philadelphia, and the Electrical Section of the Franklin Institute, to be held to-night at half-past eight at the Manufacturers' Club, 1409 Walnut Street, wish me to say that they have sent no formal invitations to the members, but that they desire that all shall consider this as an invitation to attend that reception to-night. I may also state that an invitation has been sent to those who care to avail themselves of it, to visit Girard College. Those having in charge matters of interest to the ladies I think might note that. Girard College is a beautiful institution located within ready access to various lines of cars, and would well repay the visiting ladies as well as the members, if they could find the time to visit it. Also the Athletic Club of the Schuylkill Navy. Its clubhouse is open to the gentlemen members, and they will be admitted at the door on presentation of their badge or card, or on the mere statement that they are members of the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS. You will find there a very pleasant clubhouse where you can have all the advantages of an ordinary club including a very good pool of filtered, heated water for swimming. The Franklin Institute of the State of Pennsylvania, the Historical Society and the University of Pennsylvania also invite our members to visit them. There are also invitations from the new power station of the Philadelphia Traction Company, the station of the Edison Electric Light Company, the factory of the Chloride Accumulator Company, the plant of the Germantown Electric Light Company, and other invitations which I do not recall at the present time.

The INSTITUTE is now open for the transaction of business. Is it your wish to take up the reports of the Committee on Units and Standards? If there is no objection we will take them up in the order in which they have been presented.

The Secretary read the following reports:

NEW YORK CITY, MAY 15, 1894.

To the President and Council AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS, New York City.

GENTLEMEN:—Your Committee on "Units and Standards" desires to submit the following report:

In accordance with instructions from your Committee "On Uncompleted Congress Work," a circular letter was addressed to—

1st. The Institution of Electrical Engineers, London.

2d. La Société Internationale des Électriciens, Paris.

3d. The Physikalisch-Technische Reichsanstalt, Charlottenburg, Germany.

4th. The Elektrotechnischer Verein, Berlin.

requesting the favor of their opinion as to the meaning which should conventionally attach to the word "inductance."

This word "inductance" has unfortunately been employed by different writers in two senses, and has, therefore, given rise to confusion of meaning and perplexity among students. In Great Britain, Messrs. Oliver Heaviside (who originated the term) Andrew Gray[1], Dr. J. A. Fleming[2] and others, have employed the term in their writings as representing a coefficient of induction conventionally symbolized by the dimensional formula L, and the same meaning has been employed by a number of writers in this and other countries. On the other hand, several of the French writers, Professor Silvanus Thompson[3] in Great Britain, followed by Mr. Steinmetz[4] and others in this country and abroad, have employed the word in their writings as signifying the product of a coefficient of induction and an angular velocity, conventionally symbolized by dimensional formula $\frac{L}{T}$.

It was hoped that the delegates at the Chicago International Congress would render a decision as to the meaning which should conventionally attach to the word, but as this hope was not realized and the confusion still continues, the above mentioned circular letter was addressed to four of the most prominent electro-technical associations of Europe in the hope of ascertaining by the favor of their replies, the consensus of opinion upon the subject. We beg to append herewith the replies so received.

It would seem that in the opinion of the Société Internationale, "inductance" should be regarded as the product of a coefficient of self-induction and angular velocity.

The London Institution of Electrical Engineers considers that the signification of the term should be that which its originator, Mr. Oliver Heaviside, attached to it, viz., "The coefficient of self-induction."

The Physikalisch-Technische Reichsanstalt gracefully accepts the meaning offered by Dr. Fleming and others, viz., "A Coefficient of Induction."

The Elektrotechnischer Verein expresses no opinion.

In expressing our indebtedness to these institutions for their courtesy in according to us their views, your committee desires to recommend the opinions expressed by the British and German bodies that "Inductance" should mean "A Coefficient of Induction," and while we regret that this opinion is not universal, and that the members of so eminent a body as La Société Internationale of Paris should have taken an opposite view of the subject, yet we believe that the majority of the opinions obtained entitles us to recommend to the INSTITUTE the adoption of the word "Inductance" as a "Coefficient of Induction" conventionally symbolized by L.

Yours respectfully,

F. B. CROCKER,
WILLIAM E. GEYER,
W. D. WEAVER,
A. E. KENNELLY,
GEO. A. HAMILTON,
Committee.

1. "Abs. Meas. in Electricity and Magnetism" vol. ii., pp. 344 and 345.
2. "The Alternating Current Transformer."
3. "Dynamo Electric Machinery," 4th edition, p. 629.
4. Steinmetz "TRANSACTIONS A. I. E. E." vol. ix., No. 1, p. 61, January, 1892, also vol. x., p. 231, April, 1893.

Paris, le 13 Decembre, 1893.

Société Internationale des Électriciens, Reconnue d'utilité publique,
44 Rue de Rennes, 44 (place St. Germains-des-Prés,) Paris.

A Monsieur A. E. KENNELLY, Chairman
AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS, á New York.

MONSIEUR:—Par votre lettre du 10 Novembre dernier, vous avez bien voulu demander à la Société Internationale des Electriciens d'exprimer son opinion sur la signification conventionnelle qu'il conviendrait d'adopter pour le terme "Inductance."

Le désir a été transmis à la Société dans sa dernière Réunion Mensuelle.

Considérant qu'une Commission spéciale de la Société a délibéré sur ce point en Mai dernier, et que ses conclusions, approuvées en séance, ont été publiées dans le *Belletin Mensuel* de Juin, 1893 (page 285), la Réunion a été d'avis de maintenir ses conclusions précédentes à l'égard de la locution visée.

Ye vous adresse, d'antre part, le fascicule du Bulletin contenant la délibération de la Société.

Veuillez agréer, Monsieur, l'expression de mes sentiments les plus distingués.

Le Secretaire du Comitt,
(Signature)

TRANSLATION.

SIR:—In your letter of the 10th ult., you have expressed a request that the Société Internationale des Électriciens should express an opinion upon the conventional signification that should attach to the word "Inductance."

This request has been laid before the Société at its last monthly meeting. In consideration of the fact that a special committee of the Société considered this point in May last, and that its conclusions, approved in session, were published in the *Bulletin Mensuel* of June, 1893, page 285, the meeting decided to maintain its prior conclusion in regard to the said term.

I beg to forward to you a copy of the Bulletin containing the views of the Société, and remain, etc.

The following extracts from the record forwarded by the Société appear to be the only ones bearing upon this question.

La Commission estime que les expressions *de coefficient de self-induction* et *de coefficient d'induction mutuelle* sont employées depuis longtemps sans ambiguïté et qu'il n'y a pas de raison de les changer.

"La Commission propose de donner un nom à la grandeur dont le carré ajouté au carré de la résistance d'un circuit traversé par un courant périodique donne le carré de sa résistance apparante. Le nom de réactance pourrait convenir à cette grandeur."

A full translation of the report has appeared in the Transactions of the AMERICAN INSTITUTE, Vol. X., page 419.

From the official record, therefore, the views of the Société might remain in doubt. The views expressed by prominent members of the Société upon "inductance" appear, however, in discussions upon this subject published in preceding numbers of the *Bulletin Mensuel*, and which appear in abstract in the TRANSACTIONS of the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS, Vol. X., pages 407 and 411. From these opinions which are not controverted in the official report, the above stated conclusion has been reached concerning the attitude of the French Institute in the matter.

ELEKTROTECHNISCHER VEREIN.

Berlin, den 14ten December, 1893.

In beantwortung ihres geehrten schreibens vom 10 v. mts. theile ich ihnen ergebenst mit, dass der ausdruck "Inductance" in deutschen wissenschaftlichen werken nicht gebrauchlich ist.

Ich bedaure daher sehr, Ihnen mit der gewunschen auskunft nicht dienen zu können.

Hochachtungsvoll,

NORDMANN, Schriftführer des Elektrotechnischer Vereins.

An das Cômité on Units and Standards,
AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS,
per A. E. KENNELLY, Chairman, New York.

TRANSLATION.

In reply to your favor of the 10th ult., I beg to inform you that the expression "Inductance" is not customary in German scientific works.

I therefore regret that I am unable to give you the desired information.

NORDMANN, Secretary of the Elektrotechnisher Verein.

PHYSIKALISCH-TECHNISCHE REICHSANSTALT. ANTHEILUNG II.

Charlottenburg, den 22ten December, 1893.
Berliner Str. 151.

Auf the aufrage ihres Committee on Units and Standards vom 14 v. Mts. beehrt sich die Reichsanstalt folgendes zu erwiedern.

Wie ihnen bekannt sein dürfte, wird in Deutschland der Ausdruck "Inductance" überhaubt nicht gebraucht. In rein wissenschaftlichen Abhandlungen und der wechselseitigen induction haüfig die Ausdrucke "Potential einer spule auf sich selbst" und "Potential einer spule auf die andere," während in technischwissenschaftlichen arbeiten, und neuerdings auch in rein wissenschaftlichen aufsatzen die ersteren Bezeichnudgen bevorzugt werden.

In der englischen Litteratur wird wenigstens von massgeben den Autoren das Word "Inductance" stets im sinne eines Induction Coefficienten angewandt und hat somit die dimensionen einer lange (vgl. Z. B. Fleming, The alternate current transformer, Vol. I, p. 51 und Hospitalier. Bericht über die verhandlungen des Internationalen Electrotechnicher congresses zu Frankfuft a M. 1891, p. 64)

In dem ohm'schen gesetz für sinusoisale ströme tritt der inductions coefficient L bekanntlich im nenner $\sqrt{R^2 + p^2 L^2}$ multiplicirt mit einer Winkelgeschwindigkeit p auf, so dass das Product pL die dimensionen eines widerstandes hat. Zur Underscheidung von dem ohm'sehen widerstand R wird deshalb auch in der englischen litteratur die grösse pL, den man in Deutschland keinen besonderen namen gegeben hat, als "inductive resistance" bezeichnet (Fleming loc. cit p. 116); durch diesen ausdruck wird die von ihnen erwähnte zweideütigkeit im bebrauch des wortes, inductance vermieden.

Physikalisch-Technische Reichsanstalt,
Antheilung II. Hagen.

To the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS,
New York City, U. S. A.

P. T. R., II., 5524.

TRANSLATION.

To the question of your committee on "Units and Standards" of the 14th ult., the Reichsanstalt has the honor to reply, as follows:

As you are doubtless aware, the term "Inductance" is not used at all in Germany. In purely scientific works the notion of a coefficient of self-induction and of mutal induction commonly finds expression as "the potential of a coil upon itself" or as "the potential of a coil upon another," while in technical scientific works, and recently also in purely scientific works, the first mentioned expressions are preferred.

In English literature, at least by prominent authors, the word "inductance" stands in the sense of a coefficient of induction, and has, therefore, the dimensions of a length (see for example Fleming, the Alternate Current Transformer, vol. i., p. 51, and Hospitalier Bericht uber die Verhandlungen des Internationalen Electrotechnicer Congresses zu Frankfurt, a M, 1891, p. 64).

In Ohm's law for sinusoidal currents, the coefficient of induction L is known to enter the expression $\sqrt{R^2 + p^2 \ L^2}$ multiplied by an angular velocity p, so that the product $p \ L$ has the dimensions of a resistance. To distinguish this from the ohmic resistance R, the quantity $p \ L$ which has not received in Germany a special name, has, therefore, in English literature been denoted by the term "inductive resistance" (Fleming, loc. cit., p. 116); through these expressions the ambiguity you have pointed out in the use of the word "inductance" may be avoided.

Physikalisch-Technische Reichsanstalt,
Section II, Hagen.

THE INSTITUTION OF ELECTRICAL ENGINEERS. Founded 1871. Incorporated 1883.
Victoria Mansions, 28 Victoria Street, London, S. W.,
November 28, 1893.

MY DEAR SIR:—I beg to acknowledge receipt of your letter of the 9th inst., which I will take the earliest opportunity of communicating to the Council of the Institution.

I am, my dear Sir, yours faithfully,
F. H. WEBB, Secretary.

Chairman on the Committee of Units and Standards,
AMERICAN INSTITUTE ELECTRICAL ENGINEERS.

THE INSTITUTION OF ELECTRICAL ENGINEERS. Founded 1871. Incorporated 1883.
Victoria Mansions, 28 Victoria Street, London, S. W.,
December 15, 1893.

DEAR SIR:—Referring to your letter of the 9th ult., the receipt of which I acknowledged on the 28th, I beg to inform you that the Council have appointed a special Committee to consider the question which you have put to them, and as the new Council to whom the Committee's report must be submitted do not meet until next month, it will be some little time before I am able to send you a definite reply.

I am, dear Sir, yours faithfully,
F. H. WEBB, Secretary.

Chairman on the Committee of Units and Standards,
AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

THE INSTITUTION OF ELECTRICAL ENGINEERS
(late the Society of Telegraph Engineers and Electricians).
Founded 1871. Incorporated 1883.
Victoria Mansions, 28 Victoria Street, London, S. W.,
April 18, 1895.

DEAR SIR:—In reply to your request for the opinion of this Institution as to the meaning that should conventionally attach to the term "Inductance," I am now instructed to inform you that the Council consider that the signification of the term should be that which its orginator, Mr. Oliver Heaviside, attached to it, viz., "the Coefficient of Self-Induction."

I remain, dear Sir, yours faithfully,
F. H. WEBB, Secretary.

Chairman on the Committee of Units and Standards,
AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

The President:—You have heard this report of the Committee on Units and Standards. What action will you take on it?

Mr. Hewitt:—Would it not be well, before taking action, to ask Mr. Steinmetz to give us some reason for using the term as he has in his writings.

The President:—The Chair would be happy to call on Mr. Steinmetz.

Mr. Steinmetz:—I hardly think I shall be able to give here a complete statement answering these questions.

I did not originate the use of the term "inductance" in this meaning as a wattless resistance, but adopted it, following the precedence of others, as S. P. Thompson.

It was in telegraphy that the effect of self-induction was first observed in practice, and denoted by the term "time constant of the circuit."

With the advent of alternating current engineering, self-induction became of importance in electric light and power distribution and transmission, as a phenomenon, consuming E. M. F. proportional to the current strength, analogous to resistance; with the distinction, however, that the E. M. F. consumed by the resistance is in phase with the current, and thereby represents consumption of energy, while the E. M. F. consumed by self-induction is in quadrature with the current, and thus does not consume energy, but merely influences the flow of current.

It became necessary when dealing with alternating currents to introduce a term analogous and of the same dimension as resistance, and for this wattless resistance as apparently the best suitable word, the name inductance offered itself and was applied frequently, while the quantity L retained its former name "coefficient induction," which seems to me no drawback, since L is used very little in practice.

The necessity for a proper name for $2 \pi N L$ was universally felt; and a great number of different names were proposed and abandoned again; in short, it was a groping for a proper term. "Magnetic momentum," "inductance speed," "ohmic inductance," etc. were used. Since, however, the name "inductance" appeared to be the best suitable, and though introduced for L, seemed to me very little needed for this quantity, I saw no objection to the use of "inductance" for $2 \pi N L$ or rather for the wattless resistance which, as you know, may be due to other causes also, as to capacity.

In circuits containing capacity, we find a wattless resistance of the value $\frac{1}{2 \pi N K}$ which, however, is of opposite direction to the inductance of self-induction, hence, is to be introduced as negative inductance and has been called "condendance." But outside of self-induction, capacity and polarization which, as producers of quadrature E. M. F.'s, give rise to a true wattless resistance;

other phenomena exist which cause a phase displacement between current and E. M. F., as mutual induction, live E. M. F.'s of synchronous motors, parallel running generators, or induction machines.

Thus in dealing with the general alternating circuit, the E. M. F. can be dissolved into two components, one in phase with the current and representing consumption of energy, and one in quadrature with the current, or wattless.

The former is due to the ohmic resistance, hysteresis, etc., as more fully discussed in my paper before the Electrical Congress, at Chicago, and may be represented by the term "equivalent resistance," while the latter is due to self-induction, capacity, etc.

While the name "equivalent resistance" for the coefficient $\frac{\text{E. M. F. in phase with the current}}{\text{current}}$ appears to be quite appropriate, the name "inductance" for the term $\frac{\text{E. M. F. in quadrature with the current}}{\text{current}}$ is really not very appropriate, since this does not well apply to capacity, inductance, etc.

As a more appropriate name, the term reactance has been proposed for wattless resistance, and the "reactance" called "inductance" when positive, as when due to self-induction, and "condensence" when positive, as when due to capacity or polarization.

DR. FREDERICK BEDELL:—I would heartily endorse the remarks of Mr. Steinmetz, but would not give to "inductance" quite such a general signification; that is, I would limit it simply to the term including self-induction, being equal to the coefficient of self-induction multiplied by $2\pi \times$ frequency. To the term including capacity as well as self-induction, the name "reactance" has been given and might be retained, although that is not the point in question. But when it comes to the question of using "inductance" as the coefficient of self-induction, or as the product of this coefficient and the angular velocity, it seems to me that, for practical convenience and for making mathematical and other writing clear, we should choose the latter.

MR. A. E. KENNELLY:—Mr. President and gentlemen,—This matter is a vexed question, because there are two completely opposite views in the case. If the question were whether the word "inductance" should or should not be applied to a "wattless resistance," that would be one thing; or whether it should or should not be applied to a coefficient of induction, that would be another thing. There might be two views to either of those questions. But having the word "inductance" in use and in frequent application, the question is, should it be accepted as meaning a "wattless resistance," or as a "coefficient of induction"? Here is the great confusion, and if we look through the writings of this country and the writings of Europe, we frequently find these two different meanings employed for the same word inductance ; so it is very important that some convention should be made to decide which meaning to retain, and when we have once selected that meaning we should adopt it thereafter, no matter what our own particular views may be. I think all of us are ready to sacrifice our own personal views for the benefit of electrical engineering, if we can only have a settlement of the matter once for all. We all hoped that the Chicago Electrical Congress would settle it ; that they would place their dictum on this matter and say we want "inductance" to mean so and so. But the Chicago Congress unfortunately had many other things to attend to and they left the matter absolutely untouched. So in the hope that we might settle this question before the next Congress convenes and thus avoid further confusion, these letters were addressed, under the direction of the committee on Congress work, to the Institutions of Europe asking for their opinions. We have received four opinions from Europe, and two of those opinions are in favor of using "inductance" as the coefficient of induction. The members of the French Institute, on the other hand, have shadowed their belief that it should be used to mean a "wattless resistance." Our own committee was in favor of using it as the coefficient of induction. So we submit that the general opinion is in favor of using "inductance" as the coefficient of induction, and not as a resistance. One strong argument is that the man who introduced this term, Mr. Oliver Heaviside, introduced it as the coefficient of induction, and the burden of proof should be on those who want to change the meaning that he gave, to show why it should be changed. We submit, therefore, that the INSTITUTE should adopt the term as meaning, either a coefficient of self-induction or of mutual induction.

THE PRESIDENT:—What action will you take in this matter?

MR. JAMES HAMBLET:—I move that the decision of the INSTITUTE be as the committee has recommended.

MR. STEINMETZ:—I fear that the adoption of such a definition of the term "inductance" will tend to increase the confusion by inducing writers to use the term without definition, while now, where the term "inductance" is known to be ambiguous, it is usually defined when used, for instance, by the addition of "ohmic," etc.

How difficult it is to get universal adoption of such a name, especially by those who before used a different name, has been shown by the fate of the term "effective." Several international congresses adopted the name "effective value of the alternating wave" for the square root of the mean square, since this represents the effect on the power of the wave. Before the adoption of this term, S. P. Thompson had introduced for this quantity the term "virtual," and has never accepted the term "effective," and the result is, great confusion, especially among those very people for whom S. P. Thompson's book is written—

the students of electrical engineering. European bodies are in general not in sufficiently close touch with the practice to be able to say whether the one or the other definition is more convenient, so that their opinions in this case are not as important as in strictly scientific matters; consequently I would rather like to see the question unsettled until a future time when practice will have decided which is the more convenient.

I am inclined, however. to prefer the term "reactance" and to restrict the term inductance to the reactance of self and mutual induction.

MR. KENNELLY:—Does Mr. Steinmetz mean that we should try to sustain the confusion that exists? Surely if confusion does exist, as he admits, should you not try to come to some convention that shall put an end to that confusion?

MR. STEINMETZ:—Have we the best?

MR. KENNELLY:—We have adopted the best means that we knew of.

THE PRESIDENT:—Mr. Hamblet's motion is before the house. Does any other gentleman wish to speak? It is moved and seconded that the INSTITUTE adopt the recommendation of the Committee on Units and Standards as to the meaning that should be attached to the word "inductance."

[The motion was carried.]

MR. STEINMETZ:—The general consensus of opinion is that a term is badly needed for the "wattless resistance," that is, the "quadrature component of impedance," for which I intended to use the term "inductance." Inductance has been taken now for the coefficient of self-induction. Therefore I propose that the INSTITUTE adopt as the term for the ratio:

$$\frac{\text{quadrature component of E. M. F.}}{\text{current}}$$ the name "*Reactance*."

MR. KENNELLY:—I second the motion.

THE PRESIDENT:—It is moved and seconded, as you have heard. If the Chair may express an opinion, it would be that this is a very good settlement of the question.

[The motion was carried.]

THE PRESIDENT:—There is another recommendation of the Committee on Units and Standards.

The Secretary read the second recommendation of the Committee favoring the legalization by the United States Congress of the electrical units adopted by the Chicago Electrical Congress of 1893, and also the following extract from the report of the New York meeting.

PROF. CROCKER:—I move that it is the sense of this meeting that the passage of the bill legalizing the electrical units adopted at the Chicago Electrical Congress of 1893, be recommended.

MR. GEORGE M. PHELPS:—Mr. Chairman, if you will permit me I will suggest a slight addition, to make the action possibly stronger, and that is, that this meeting recommend to the general and annual meeting of the INSTITUTE to be held in May, a more formal endorsement and recommendation of the measure. I offer this because one or two speakers suggested that this meeting give its sense on this subject, and that the general meeting in May might take a more formal action.

PROF. CROCKER:—I accept the amendment.

THE PRESIDENT:—Gentlemen, it is moved and seconded, as you have heard. Are you ready for the question?

[The motion was put and carried.]

THE PRESIDENT:—What action will you take on this recommendation?

MR HEWITT:—Would it not be in order to specify in the resolution the manner of presenting it to Congress?

THE PRESIDENT:—I think it would be well for the INSTITUTE first to determine if it wishes to adopt this resolution. The question is on the adoption of the resolution. Afterwards you can determine how it is best to carry it out.

MR. CARL HERING:—I move that the resolution be adopted.

[The motion was carried.]

THE PRESIDENT:—I think it would be proper for the INSTITUTE to appoint a small committee to see that this matter be carried out.

MR. HERING:—I suggest that the Committee on Units and Standards constitute this body.

THE PRESIDENT:—It is moved by Mr. Hering that this matter be referred to the Committee on Units and Standards.

[The motion was carried.]

THE PRESIDENT:—There is another announcement I wish to make. Mr. Edwin J. Hall, of the American Telephone and Telegraph Company, says that the company desires to extend the use of the long distance lines to the members of the INSTITUTE while they are in this city. The office of the company is at 114 South Fourth Street, Philadelphia. The secretary has some announcements to make.

THE SECRETARY:—At the meeting of the Council held this morning the following associate members were elected:

Name.	Address.	Endorsed by.
BERRESFORD, ARTHUR W., *B. S., M. E.*	Electrician, Brooklyn City R. R. Co., 197 Van Buren St., Brooklyn, N. Y.	Samuel Sheldon. Frederick Bedell. Aug. Treadwell, Jr.
BIJUR, JOSEPH	Student in Electrical Engineering, Columbia College, Residence, 41 West 53rd St., New York City.	F. B. Crocker. George F. Sever. M. I. Pupin.
CHADBOURNE, HENRY R., JR.	Electrical Engineer, Troy City Ry. Co., Troy, N. Y,	T. C. Martin. E. G. Bernard. J. B. Cahoon. Chas. D. Shain.
CHILDS, SUMNER W.	Supt. of Line Construction, J. G. White & Co., 1425 Maryland Ave., Baltimore, Md.	C. G. Young. J. G. White. Wm. C. Burton.

CLARK, LEROY, JR.	Post-Graduate Student in Electrical Engineering, Columbia College, 350 West 30th St., New York	M. I. Pupin. H. A. Storrs. F. B. Crocker.
DOOLITTLE, CLARENCE E.	Manager and Electrician, Roaring Fork Electric Light and Power Co., Aspen, Colo.	F. B. Badt, A. H. Cowles. Wm. A. Anthony.
DORR, FRANK H.	Engineering Dept. General Electric Co, 425 Baker St., San Francisco. Cal.	T. C. Martin. Jos. Wetzler. L. Stieringer.
GLADING, FRANK W.	Lehigh University, 2005 East York St., Philadelphia, Pa.	Edwin J. Houston. A. E. Kennelly. George E. Wendle.
HARTWELL, ARTHUR,	Electrical Engineer. Westinghouse Electric and Mfg. Co., Pittsburg, Pa.	Chas. A. Terry. Albert Schmid. Charles F. Scott.
HAVILAND, FOSTER L.	With Clark Electric Co., 192 Broadway; Residence, 163 St. Nicholas Ave., New York City.	E. P. Clark, Ralph W. Pope. R. N. Baylis.
HEWITT, WILLIAM R.	Special Student, Electrical Dept. Columbia College; 130 E. 60th St., New York City. Residence, San Francisco, Cal.	M. I. Pupin. H. A. Storrs. C. T. Rittenhouse.
HOLBERTON, GEORGE C.	Electrical Engineer, General Electric Co., 15 First St., San Francisco, Cal.	A. L. Rohrer. H. F. Parshall. Wm. E. Geyer.
MORSE, GEORGE H.	Assistant Instructor in Electrical Engineering, University of Minnesota, Minneapolis. Residence, Excelsior, Minn.	G. D. Shepardson. Morgan Brooks. M. H. Gerry, Jr.
NICHOLSON, WALTER W.	General Supt. Central N. Y. Telephone and Telegraph Co., 73 Howard Ave., Utica, N. Y.	F. A. Pickernell. U. N. Bethell. Hammond V. Hayes.
PHILBRICK, B. W.	Electrician, in charge of Electrical Plant, Levi P. Morton, Rhinecliff, N. Y.	T. C. Martin. Chas. D Shain. F. Saxelby. E. G. Bernard.
RICKER, CHARLES W.	Expert Electrical Engineer, 109 White Building, Buffalo, N. Y.	Chas. R. Cross. Edw. E. Higgins. Henry H. Wait.
SHARP, CLAYTON H.	Instructor, Department of Physics, Cornell University, 122 University Ave., Ithaca, N. Y.	Edw. L. Nichols. Harris J. Ryan. C. P. Matthews.
STINE, PROF. WILBUR M.	Director Electrical Dept., Armour Institute, Chicago, Ill.	Edw. Caldwell. Joseph Wetzler. H. A. Foster.
TOWER, GEORGE A.	Electrical Engineer, The Sherwood Land Co. and The Jefferson Hotel Co., 109 S. First St., Richmond, Va.	M. B. Leonard. C. E. McCluer. George Hill.
WHARTON, HUGH M.	Electrical Engineer, Westinghouse Electric and Mfg. Co., 29 Plane St., Newark, N. J.	A. E. Kennelly. Edwin J. Houston. L. A. Osborne.
WINCHESTER, SAMUEL B.	Supt. and Electrician, Holyoke Water Power Co , 9 Laurel St., Holyoke, Mass,	C. P. Steinmetz. W. L. R. Emmet. Theodore Stebbins.

Total 21.

The following associate members were transferred to full membership:

LORRAIN, JAMES GRIEVE	Norfolk House, Norfolk St., London, W. C., England.
WIENER, ALFRED E.	Electrical and Mechanical Engineer, General Electric Co., 24 Yates St , Schenectady, N. Y,
CRAIGIN, HENRY A.	Engineer, Westinghouse Electric and Mfg. Co., 15 Charles St., Boston Mass.
HASSON W. F. C.	Consulting Engineer, 104 Sutter St., San Francisco, Cal.
IVES EDWARD B.	Lieutenant U. S. A., Electrical Engineer, 11th and Colona Sts., Philadelphia, Pa.

Total 5:

The Secretary also read invitations from the following: A. Falkland, Girard College, The Historical Society of Philadelphia, The La Roche Electric Works, Queen & Co., Dr. William Pepper, The Germantown Electric Light Company, The Engineers' Club, Dr. Wahl, Secretary Franklin Institute, Edwin J. Houston, on behalf of the Athletic Club of the Schuylkill Navy, and the Drexel Institute.

THE PRESIDENT:—Gentlemen, there is a matter which was laid on the table at one of our meetings which you may wish to bring up. It is my duty, as presiding officer, to call your attention to the fact—the report of the Committee on Revision of the Rules, do you wish to take it up now, or is there any other business that you would like to take up?

MR. HEWITT:—Mr. President, we have a communication from a member in Chicago in reference to certain matters which concern the INSTITUTE. It seems to me this would be a good time to consider it. Although it is stated that there is a memorial which is to be presented later it may come at some time when we are busy with something else and we should not be able to give it attention. I move that this matter be brought up now.

[The motion was seconded and carried.]

The Secretary read a letter from Mr. Caldwell, local honorary secretary, stating that a memorial had been prepared by a committee appointed for that purpose, asking that the expenses of the meeting at Chicago, for reporting, postage etc., be paid from the general funds of the INSTITUTE.

MR. GEORGE M PHELPS:—In order to have some action to discuss I would move a resolution as follows:

Resolved: That the expenses of reporting the Chicago meetings up to a sum not exceeding $10 per meeting, be borne by the general fund of the INSTITUTE until further notice.

I offer that resolution because, like many other members, I have been exceedingly impressed by the success of the recent meetings at Chicago and the merit of the discussions that have taken place there. It is practically out of the question for members in that vicinity to attend a meeting in New York oftener than once a year or so, and they have, I think, amply justified their proposition to have local meetings and have done useful work. It seems very proper that we should bear so very reasonable a portion of the expense as suggested in the communication from Mr. Caldwell.

MR. HEWITT:—I second that motion, Mr. President.

THE PRESIDENT:—Mr. Phelps' motion has been seconded. Does any other gentleman wish to speak on the subject?

MR. PHELPS:—I think, Mr. Chairman, that it is a matter that had better be well discussed and I would like to hear expressions from other members.

THE PRESIDENT:—This is a matter, gentlemen, that you cannot be too careful about deciding properly. It is not the matter of an expense of $10 for a single meeting, but it is the very grave matter of precedent. The INSTITUTE should, I think, properly consider the future as well as the present, and while I would be equally desirous with any member of doing all in my power to obtain the freest discussion, yet I think we ought to consider carefully as to whether or not in the establishment of such a precedent we are not in danger of increasing the annual dues of membership. I think that should have a full discussion.

MR. HAMBLET:—Do I understand Mr. Phelps' motion to limit the expenditure to the reporting of the discussions at the Chicago meetings?

MR. PHELPS:—The only request that we have before us is to do exactly that thing. The letter is quite specific and a resolution or some remarks at the end of the printed report of the Chicago meeting is to the same effect; that it was the sense of the meeting that the general fund of the INSTITUTE should meet the expense of reporting their meetings. There was in addition some little charge for stationery mentioned by Mr. Caldwell, but he states that it is their opinion that the estimate of $10 per meeting would more than cover the expense contemplated. The expenses contemplated, I believe, are merely a stenographic report and such stationery as is required by the local honorary secretary in conducting the correspondence.

MR. HEWITT:—I think that with respect to a great many papers, the discussion is of as much value, if not more value, than the paper itself, and anything that this INSTITUTE does to bring forth increased discussion, redounds to the interest of the INSTITUTE and to the benefit of the members, and anything that we may do in order to aid this wider discussion is to our benefit. I do not sympathize with the President exactly in the fear of increased dues, because I believe that the encouragement of these meetings in Chicago and other such cities will so increase our membership as to offset the light extra expense. I well know the President's fears from his expression of them elsewhere, but I must say that I cannot feel in sympathy with him on this question.

MR. HAMMER:—I would like to ask for the reading of the resolution in the letter again.

The Secretary read the part of the letter referred to.

MR. HAMMER:—I think it is very reasonable that the Chicago people should expect to have a certain amount of the funds of the INSTITUTE placed at their disposal. I think it ought to be done only, however, after careful consideration of just how it should be done. I think at the time this question of local meetings was threshed out in the different Council meetings this very point came up among others, and it was suggested then that if local bodies were organized, and after their membership attained a certain number, the INSTITUTE would assume a certain portion of their expenses. It seems to me that if this matter was referred to a committee they could consider it very carefully and recommend some plan that would be fair to Chicago and to other cities that would take the matter up hereafter. Supposing, for instance, it was decided that when Chicago gets fifty members, each of which members paying annual dues of $10, the amount would represent $500. Now the INSTITUTE would receive that $500, and in view of the fact that there are fifty members of the INSTITUTE residing in Chicago a certain percentage of the receipts could go to that organization, and for any expenses which they would incur over and above that amount the local organization would have to be responsible. I merely give this as a suggestion. It seems to me, as the President says, we ought to be careful in deciding this matter, because it is more readily corrected now than hereafter. If the matter is referred to a committee, I think they could formulate some plan which would give satisfaction to Chicago and other cities which would form local organizations hereafter. I, therefore, make a motion that this matter be referred to a committee, say, of three appointed by the Chair.

MR. PHELPS:—Mr. President, I tried to express the resolution with some particularity, so that it should not be too broad, nor establish a too serious precedent. The resolution, if it was taken down as I meant to express myself, was confined strictly to the request in the letter of Mr. Caldwell, namely, to have the expense of reporting the meetings and the stationery paid. Now that resolution, if passed, I think, would not necessarily commit the INSTITUTE to incur the general expenses of local bodies or sections at any time. It meets this specific request, on the ground, in my mind, that the work done has already been useful and is likely to become more important; that it has led already to the acquisition of five members, there is $50; and the expenditure under the resolution I have offered is limited to $10 a meeting. There are not likely to be more than a half dozen meetings in a year, and I think it would be well to determine the point here and now, rather than put it in the hands of a committee to report at some subsequent period. If the committee propose to report at this meeting, I do not think they can do much better than the meeting itself on that very distinct proposition, which I do not regard as a general permission to all and sundry meetings to charge the INSTITUTE with all and sundry expenses.

THE SECRETARY:—I would like to make a few remarks for the information of the meeting regarding the expense. The Armour Institute at Chicago has very kindly placed at our disposal a hall with all necessary facilities, without charge. The notices for the meetings at Chicago have been included, as you are aware, in the regular postal notice which is sent out to all members which adds nothing whatever to the outlay. The cost would be the same either with or without the Chicago meetings. This reduces the amount simply to the communications of the local honorary secretary with the New York office, or such communications as he may make with the members there in arranging for meetings; so that there is really very little additional expense beyond the reporting of the discussion. We have had the discussion of two of their meetings, and that of the last meeting was printed in the May number, copies of which are here for distribution; that, of course, involved, in addition to the fee for reporting, the necessary proportion of the cost of printing the TRANSACTIONS. But, as Mr. Hewitt has very properly said, everything of the kind that is done adds to the value, of the INSTITUTE to the membership generally, and I have it from the gentleman who read the last paper that he considered the discussion at Chicago, so far as the question of practical experience was concerned, as of more importance than the New York discussion; that is to say, the western men had done certain work in the line of the paper, and gave their experience. Now, if that discussion is of any value to anybody here, it has been brought out by this Chicago meeting. I presume that if that meeting had not been held, these facts would not have been brought out in the TRANSACTIONS. It has been the policy of the management to make the TRANSACTIONS as valuable to the members as possible, so that each one would feel that he was getting a full return for the money spent, and, personally, I have never considered the INSTITUTE a concern for accumulating a fund, and I do not believe that technical societies should be so considered, but rather that they should expend their money, as the rules say, in "the reading and discussion of professional papers, and "the circulation, by means of publications among its members "and associates of the information thus obtained."

That is the principal reason for the existence of such an institution as this.

There is just one other point I wish to bring up in connection with the work of the secretary and the work of these local meetings. Any one who has anything to do with running a local organization, such as has been suggested, knows that the collection of dues is one of the most annoying things in connection with it. By the plan we are pursuing, or which is being discussed, the local secretary is entirely relieved of that burden. The members simply carry on the meeting, and this is done for the benefit of the national body as well as themselves, and they pay their dues in one lump. This removes the probability of any irritation on the part of members in the vicinity who might say, "Why here we are paying $13 a year for our membership, "and in the vicinity of New York they pay but $10 per year, and "they get just as much benefit as we do." That is the way I have to look at this matter from the standpoint of members at various points, because I am in communication with them. This matter has been brought up by the Chicago members, who practically ask us "Why should not the INSTITUTE pay for steno-"graphic reports of our discussion of the papers, as well as the "discussion of these papers elsewhere?"

MR. PHELPS:—I wish to say a word more to back up my resolution. A committee was appointed last autumn to consider this matter of local meetings and spent quite a good deal of time and care in presenting a plan. That plan was thought at the time by some people proposing local meetings, perhaps by some in Chicago, to have been somewhat restrictive of the field of such local meetings, because it required that such local meetings should discuss only such papers as were accepted by the INSTITUTE to be read at its meetings here. The plan has been accepted at Chicago in perfect good faith. The same papers have been read there, and, if possible, simultaneously, and have been discussed in both places. In other words, the western members have been doing part of the monthly work of reading and discussing papers. They have done it in Chicago while we were doing it in New York. It seems to me they were doing precisely the same work that we are doing, and simply by their distance they are debarred from doing it with us here. If they were running an entirely distinct sort of meeting, having their own papers which were not necessarily a part of the TRANSACTIONS of the INSTITUTE, the case would be quite different.

MR. HAMBLET:—The matter of the accession of five members seems to me some sort of compensation to the INSTITUTE for the expense. It is said five members would bring in $50, but for the first year it is $75. The cost of each meeting in Chicago, merely limiting it to the stenographic report, would amount for the eight meetings to about $80. By allowing that expense to the Chicago meeting, are we not binding them more closely to us, and also getting the advantage of the educational value of their discussions in the TRANSACTIONS, as well as the increase of membership? It has been said that this is an entering wedge, and rather a dangerous plan for us to adopt, to expend money on these meetings. I acknowledge that there may be some doubt about the policy of doing so. But it seems to me at present, in the aspect of the work hat has so far been done in Chicago, we are warranted in allowing them some small expense.

MR. STEINMETZ:—While it seems to me quite fair that something should be done to help the Chicago local meetings, at the same time we should proceed very considerately in this matter.

It is a happy fact, that the Chicago meetings begun under very favorable circumstances, and their expenses are very low. But with the same right that Chicago members request us to share the expenses, other local meetings can, and *will* do the same, and should be treated in the same way, and other local branches may have a good deal larger expenses. Therefore, we ought, at least, to specify what we intend to pay, and thus I make the amendment to the motion that the INSTITUTE pay the cost of reporting, but nothing else; but not only of the Chicago meeting, but any other section which may be established. I think that would restrict the possibility of having too large expenditures at such meetings, and at the same time I think it fair to pay the expenses for that in which we are mostly interested—improving the TRANSACTIONS by improving the discussions.

MR. KENNELLY:—I second the amendment.

MR. HERING:—That puts no limit on the amount which can be expended, and I, for my part, think it would be much better to limit the amount to $10, as Mr. Phelps suggested. When the meetings become more and more important, and the reporting more expensive, there is nothing to prevent us from granting a greater amount. To promise to pay for the expense of reporting I think would be imprudent, because that is too indefinite a quantity; it would be much better to fix upon a certain sum.

PROF. ANTHONY:—It seems to me we had better confine ourselves here to this specific case that is brought before us, and when some other meeting wants its expenses paid, we can take that up as a separate case. I do not see the need of crossing that bridge until we come to it. I do feel that in the case of the Chicago meeting the discussions have been very valuable. They will form a valuable feature of our TRANSACTIONS. The Chicago meetings have been of advantage to the INSTITUTE at large. They are not simply little meetings, of advantage only to those attending them, and, therefore, it seems to me perfectly proper that we should bear that expense. The discussions are of just as much value to the INSTITUTE at large as those in New York. For this reason I am heartily in favor of paying for the reporting, and whatever expense there may be in connection with obtaining these discussions for our TRANSACTIONS. But I am not in favor of passing any resolution that will bind us to do anything in particular in the future in reference to any other meeting or in reference to this one. We pay up to $10 now. By-and-by if we find that the reporting is going to cost more, we can change that resolution and pay more if we think it desirable.

THE PRESIDENT:—The Chair will now put the vote on the amendment of Mr. Steinmetz to Mr. Phelps' motion.

If you will pardon me, the Chair would like to make a few remarks. The President does not wish to be misunderstood in his position, as he fears, from some remarks made in the discussion, that he may be. The President is in hearty sympathy with any movement which will increase the value of the INSTITUTE and the value of its TRANSACTIONS by offering free discussion to its members wherever they may meet. The Chair has expressed an opinion that it is the part of wisdom of this INSTITUTE to be careful lest in any legislation here it establishes what might prove to be a dangerous precedent. The Chair particularly wishes its position to be understood. It inclines to the opinion that the broad question raised in the Chicago meeting is not met by Mr. Phelps' motion. As the Chair understands it, the particular question raised in the Chicago meeting is this: That the expenses incurred in connection with the local meetings shall be paid out of the general fund, and the Chair has expressed its opinion that that is a dangerous precedent unless it is carefully considered. I rejoice with any member in the growth of our INSTITUTE. I am not quite able to see that the five members who have joined the INSTITUTE since the date of the Chicago meeting would not have joined had that meeting not taken place. That has by no means been proved. Your President certainly does not look on the INSTITUTE as a money getting organization. Certainly it is not desired that the funds of the INSTITUTE shall be simply accumulated for the sake of being accumulated. The President does not think the society is in any immediate danger of being ruined by too large sums for that or other purposes. But while I have the honor of occupying your Chair, I shall never hesitate, even though I may be in a hopeless minority of one, in expressing to you decidedly what my views are on any subject that may involve the good name and success of the INSTITUTE. Therefore, before putting this question, I would like to suggest to you that this is not meeting the issue raised in the Chicago meeting.

MR. PHELPS:—Mr. Chairman, if you will pardon me, I wish to point out that the purpose of my resolution was not to meet that broad question, but to meet a much more specific and narrow one; to satisfy that particular demand or request, but not to go to the length of accepting the proposition of the Chicago meeting that we were responsible for all their expenses. Mr. Steinmetz's motion would create a much broader action and be more in the direction of meeting that large issue. I was very glad that Professor Anthony expressed himself as he did in respect to those points.

THE PRESIDENT:—The Chair has nothing to add, except that it simply desires to express the opinion that as this question has come up it is preferable that the INSTITUTE shall meet it. I will call for a vote on Mr. Steinmetz's motion.

[Mr. Steinmetz's amendment was lost.]

THE PRESIDENT:—The vote will now be taken on Mr. Phelps' resolution.

[Mr. Phelps' resolution was carried.]

The President:—What other business will you present?

Mr. Hammer:—I would like to report on behalf of the committee for examining the books of the Treasurer, that they have been audited and found correct.

The President:—Mr. Secretary, will you please inquire of the tellers what probability there is that a report will be had from them shortly.

The Secretary made the desired inquiry of the tellers and reported, as follows:

Mr. President, the tellers inform me that they will probably not be able to make a report for an hour or more, and they wish me to bring before the meeting for instructions this question: According to the rule which they refer me to, that is Rule V., paragraph 4, "on this outer envelope the member shall add his signature and a postage stamp." They hand me these envelopes, upon which the name is either typewritten or imprinted with a rubber stamp; in one case embossed. It answers the purpose of showing who the envelope is from, but it is not a signature.

The President:—This is a matter for you to decide, gentlemen.

Mr. Hammer:—I move that they be accepted upon the endorsement of the committee.

[The motion was carried.]

Mr. Phelps:—Would it be in order to call up now the report of the Committee on Revising the Election Rules? If anything is to be done, I think this would be a suitable opportunity. I move that it be taken up.

[The motion was carried.]

The President:—I will ask the Secretary to read this report.

The Secretary read the following report:

REPORT OF COMMITTEE ON REVISION OF RULES.

At a meeting of the Committee, duly held, it was unanimously resolved to advise that the rules relating to elections be immediately changed as set forth below.

In pursuance of this resolution and of the provision in Section VIII., controlling the manner in which amendments may be made, the prescribed written notice is hereby given by the Chairman on behalf of the Committee.

At the next or at some subsequent regular meeting of the Institute the following separate amendments of the rules will be brought up:

Resolved, that Section V. be amended as follows:

1st change—After "a" in line 13, add the words "second list headed."

2d change—Line 18, after "choice" add "opposite the name of each nominee in each list shall be printed a number indicating the number of nominations received by him, and a suitable explanation of these numerals shall be placed on the sheet."

3d change—Lines 33 to 36, shall be changed to read "sealed unmarked and unidentified 'Inner envelope' of any suitable character, to be in its turn enclosed either in the 'Voting envelope' (received from the Secretary) or in any other envelope marked on its face 'Non-official Voting Envelope-Enclosing a ballot only.' The outer envelope of either class must be identified by the signature of the member on its face, and must be sealed and mailed to the Secretary.

Respectfully submitted,

Jan. 17, 1894. Signed, { F. BENEDICT HERZOG, Chairman. T. C. MARTIN. F. R. UPTON, Assenting.

[For existing Rule, see Member's pocket year-book, edition 1893, p. 52, also TRANSACTIONS Vol. ix. p. 460.]

MR. PHELPS :—I wish to offer a slight addition, or an amendment, so to speak, not changing anything in this rule as it would read amended by the committee, but as an amendment to the report of the committee, if you please; an addition at the end of the second paragraph, the last sentence of which is: " This sheet, " together with an envelope, on which shall be printed the ad- " dress of the Secretary, and the words ' Voting envelope—en- " closing a ballot only,' shall, not later than the 15th of April, " be mailed by the Secretary to every member in good standing."

I would add there "Provided that members elected after April " 15th, and who shall have paid their dues shall be supplied with " ballots on request at any time before the election." I offer that for this reason. It appears that a considerable number of members now in good standing, elected at the Council meeting immediately following April 15th, who have paid their dues, find, under a recent interpretation of the rule, that they are debarred from voting, although they are members in good standing and have been so for some weeks. This would meet a case of that kind. I hardly think that this was intended to exclude members from voting, but was intended for convenience—that the Secretary should on that date mail these tentative ballots to all the members then in good standing, but this would exclude any doubt on that subject.

[The motion was seconded.]

THE SECRETARY :—I would suggest to the gentleman proposing the amendment, that the Secretary should send these ballots to *all* members who pay their dues who are elected subsequent to April 15th, instead of sending them upon request. That is to say, I think the members who were elected should be entitled to these ballots whether they request them or not. Not being familiar with the *modus operandi*, they may not ask for them.

THE PRESIDENT :—Does Mr. Phelps accept that change?

MR. PHELPS :—Yes, sir. If you will permit me, I will word that a little differently to meet the Secretary's suggestion.

THE SECRETARY :—There is nothing in the language of this rule fixing a limit to the time of receiving ballots. I had ballots handed to me this morning by the tellers who asked whether they should be counted. So far as I know, there is no limit, provided they are handed in before the tellers finish their work. If a person should hand his vote to the tellers now, while the count is proceeding; as the rule is construed, there would be no reason why they should not count it.

THE PRESIDENT:—Pending the preparation of Mr. Phelps' resolution, in pursuance of the suggestion of the Secretary, the Chair wishes to be advised by the INSTITUTE what action shall be taken on those ballots. Unless the INSTITUTE determines to the contrary, the Chair will request the tellers to accept them as *bona fide* votes. Will you take some action on that matter?

It was voted that the ballots be accepted, and the tellers were instructed accordingly.

The following amendment to be added to the second paragraph of the report of the Committee on Revision of the Rules, introduced by Mr. Phelps. was adopted:

"Provided that the Secretary shall also mail such ballots, "sheets and envelopes to members qualified after April 15th "before the annual meeting, and that any member not having "ballots and envelopes shall be entitled to obtain them from the "Secretary at any time before the calling to order of the annual "meeting."

On motion of Mr. Carl Hering, it was also voted to amend the committee's report by making the following change at the end of the fourth paragraph. The proposed rule which now reads "and must be sealed and mailed to the Secretary," shall be changed to read "be sealed, and must reach the Secretary prior to the hour of the actual opening of the annual meeting."

The annual meeting then adjourned, pending the preparation of the tellers' report.

The annual meeting reassembled, and was called to order by President Houston at 2.30 P.M.

THE PRESIDENT:—Mr. Secretary, is the tellers' report ready?

MR. UPTON:—We have not made a full report. The canvass of the votes showed that the entire Council ticket was re-elected. We will make a full report to the Secretary, giving the items showing the number of votes cast for the various persons. The majority is quite large and full for the entire Council ticket.

Philadelphia, May 15, 1894.

We find the result of the balloting as follows:

For President, EDWIN J. HOUSTON 236
" " T. D. LOCKWOOD 48

The balance of the Council nominees were also elected, each having a majority of the 365 votes cast.

GEO. R. METCALFE.
FRANCIS R. UPTON. } Tellers.

The vote in detail is as follows:

FOR PRESIDENT.

E. J. Houston	236	Chas. R Cross	5
T. D. Lockwood	48	Louis Duncan	3
E. L. Nichols	16	Louis Bell	2
Nikola Tesla	12	Wm. Stanley	2
F. B. Crocker	11	T. A. Edison	2
A. E. Kennelly	11	C. F. Brush	1
Elihu Thomson	8	M. I. Pupin	1
Elisha Gray	6		

FOR VICE-PRESIDENTS.

W. A. Anthony	293	C. L. Clarke	3
F. B. Crocker	272	C. E. Emery	3
Jas. Hamblet	240	E. E. Higgins	3
D. C Jackson	28	H. W. Leonard	3
Louis Bell	15	T. D. Lockwood	3
H. J. Ryan	15	T. C. Martin	3
C. O. Mailloux	14	R. W. Pope	3
C. P. Steinmetz	13	L. B Stillwell	3
A. L. Rohrer	11	Brown Ayres	2
Wm. Stanley	10	F. L. Pope	2
R. H. Pierce	9	G. D. Shepardson	2
Elisha Gray	8	L. L. Summers	2
Carl Hering	8	Chas. Wirt	2
Elihu Thomson	8	A. J. Wurts	2
T. A. Edison	7	A. G. Compton	1
G. A. Hamilton	7	A. G. Bell	1
P. Benjamin	6	J. S. Brown	1
Louis Duncan	6	C. F. Chandler	1
W. F. C. Hasson	6	S. D. Field	1
E. L. Nichols	6	C. J. Field	1
W. L. Robb	6	H. A. Foster	1
W. E. Geyer	5	S. D. Greene	1
E. W. Rice, Jr	5	H. V. Hayes	1
Jos. Wetzler	5	J. W. Howell	1
Townsend Wolcott	5	W. Maver, Jr	1
B. J. Arnold	4	G. C. Maynard	1
C. F. Brush	4	F. A. Pickernell	1
J. J Carty	4	C. F. Scott	1
E. J. Houston	4	O. B. Shallenberger	1
M. I. Pupin	4	G. H. Stockbridge	1
A. Schmid	4	E. P. Thompson	1
Edward Caldwell	3		

FOR MANAGERS.

A. E. Kennelly	323	Carl Hering	7
W. D. Weaver	279	Jas. I. Ayer	6
C. S. Bradley	264	R. O. Heinrich	6
W. B. Vansize	250	Jos. Wetzler	6
N. W. Perry	18	A. C. Crehore	5
H. J. Ryan	15	I. H. Farnham	5
G. A Hamilton	14	R. H. Pierce	5
W. F. C. Hasson	14	W. A. Anthony	4
Brown Ayres	13	R. N. Baylis	4
Nikola Tesla	11	Fred'k Bedell	4
C. O. Mailloux	10	Morgan Brooks	4
H. F. Parshall	10	Louis Duncan	4
Alfred S. Brown	8	W. J. Hammer	4
Samuel Sheldon	8	E. E. Higgins	4
C. P. Steinmetz	8	J. R. Lovejoy	4
C. C. Haskins	7	T. C. Martin	4

FOR MANAGERS.—Continued.

Thorburn Reid	4	R. B. Owens	2
F. A. Scheffler	4	F. A. C. Perrine	2
F. J. Sprague	4	B. F. Thomas	2
Wm. Stanley	4	E. P. Thompson	2
J. J. Carty	3	S. S. Wheeler	2
F. B. Crocker	3	J. G. White	2
R. Eickemeyer	3	F. B. Badt	1
H. A. Foster	3	C. H. Davis	1
C. E. Emery	3	A. de Khotinsky	1
C. D. Haskins	3	S. D. Greene	1
F. B. Herzog	3	H. V. Hayes	1
C. T. Hutchinson	3	F. V. Henshaw	1
F. W. Jones	3	A. S. Hibbard	1
E. Merritt	3	W. Hochhausen	1
E. L. Nichols	3	E. J. Houston	1
F. A. Pickernell	3	J. W. Howell	1
E. W. Rice, Jr.	3	D. C. Jackson	1
C. D. Shain	3	F. P. Little	1
O. B. Shallenberger	3	L. B. Marks	1
C. Thomson	3	W. Maver, Jr.	1
B. J. Arnold	2	G. M. Phelps	1
Brown Ayres	2	E. P. Roberts	1
J. B. Cahoon	2	A. L. Rohrer	1
C. R. Cross	2	L. B. Stillwell	1
C. Cuttriss	2	G. H. Stockbridge	1
T. A. Edison	2	F. R. Upton	1
C. J. Field	2	E. G. Willyoung	1
Elisha Gray	2	A. J. Wurts	1
T. D. Lockwood	2		

FOR TREASURER.

Geo. M. Phelps	302	Geo. A. Hamilton	47

The President:—The business of the adjourned meeting being now completed we will proceed to the regular business of the general meeting—the reading of papers.

Gentlemen, I notice the President's name is down on the programme for the first paper. Before beginning I desire to assure you that I appreciate the very high honor you have conferred upon me in again electing me to the Presidency of the Institute. I beg to assure you that it will be my earnest endeavor faithfully to discharge the duties thus imposed upon me. I will give you, in the way of a brief inaugural address, the Progress of our Institute During its First Decade.

[The President then read his address, as follows:]

A REVIEW OF THE PROGRESS OF THE AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

INAUGURAL ADDRESS.

BY EDWIN J. HOUSTON, PH.D., PRESIDENT.

The AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS is in no sense a local organization. It has in view the interests of no particular section of country, but, on the contrary, is a national body. It represents the electrical profession in all parts of our great land, and welcomes into its membership bright and progressive men in the electrical profession wherever they may be located.

But while the INSTITUTE is in no sense a local body, so that no city can properly claim as a right the high privilege of having the annual meeting held in it, yet there is, perhaps, at this time, a special fitness that the annual meeting, which witnesses the closing of the first decade of our association, should be held in the City of Brotherly Love, where the INSTITUTE had its birth.

The International Electrical Exhibition, held in 1884, in Philadelphia, under the auspices of the Franklin Institute of the State of Pennsylvania, was called together at an exceedingly favorable moment. Eight years had elapsed since the Centennial Exhibition of 1876, in Philadelphia, had sown broadcast the germs of public interest in electricity, and thus laid the foundation for a belief in the bright promises of the electric future. These germs, carried to all parts of the land, were beginning to bear fruit, and a body of earnest and intelligent workers had sprung up on all sides, so that our comparatively limited knowledge of electrical science was markedly increased, although in an extremely irregular and unsystematic manner.

Between 1876 and 1884, nearly a decade, the work done in the electrical field was necessarily of a pioneer and independent character. The great principles of the science, already discovered and announced, were but vaguely understood, and needed the practical man to carry them into actual commercial use. To a great extent, each investigator trod the path of discovery alone, gropingly penetrating into the regions of the unknown, unaccompanied by his fellow investigator, and often, indeed, unconscious of his existence. Had this early work been properly organized, much of the labor expended in going over ground already trodden might have been saved, but it is by no means clear that this labor was in vain for the weal of the electric future; for, truths thus repeatedly wrested from nature and established again and again by independent investigators, cannot be too highly prized.

In our nineteenth century activity, events move rapidly. In less than a decade from the time of the Centennial Exhibition of 1876, namely, in 1884, the time had come when the advantages of congregation as opposed to segregation were to be demonstrated; when the lonely investigator was to be brought into contact with his brother toiler and taught the advantages of organized work and a free exchange of ideas.

Happily, the International Electrical Exhibition in Philadelphia of 1884, already alluded to, brought together the workers in electricity both in this country, and, to a certain extent, in other parts of the world, not only during the Exhibition itself, but especially during the completion of the buildings and the arrangement of the exhibits. The varied exhibits thus brought together from all sides were a revelation to these hitherto independent workers, and showed them, from what had already been accomplished by electrical science, what might reasonably be expected in the near future. The stimulus, so excited, culminated in the organization of the distinguished body I have now the honor of addressing.

At the same time, the U. S. Government appointed a United States Electrical Commission authorizing it to conduct a National Conference of Electricians in Philadelphia during the progress of the International Electrical Exhibition. Fortunately for the cause of electrical science, the Commission after due deliberation determined to appoint as members of this Conference not only those investigators in the physical laboratory and lecture room, the college and university professors, whose labors have always proved of such great value to the world's weal, but also those equally important investigators, the inventor and actual worker in the commercial electrical field, whose knowledge of principles is based on actual experience; a class that proves the correctness of its ideas by subjecting them to the test of actual trial on a commercial scale.

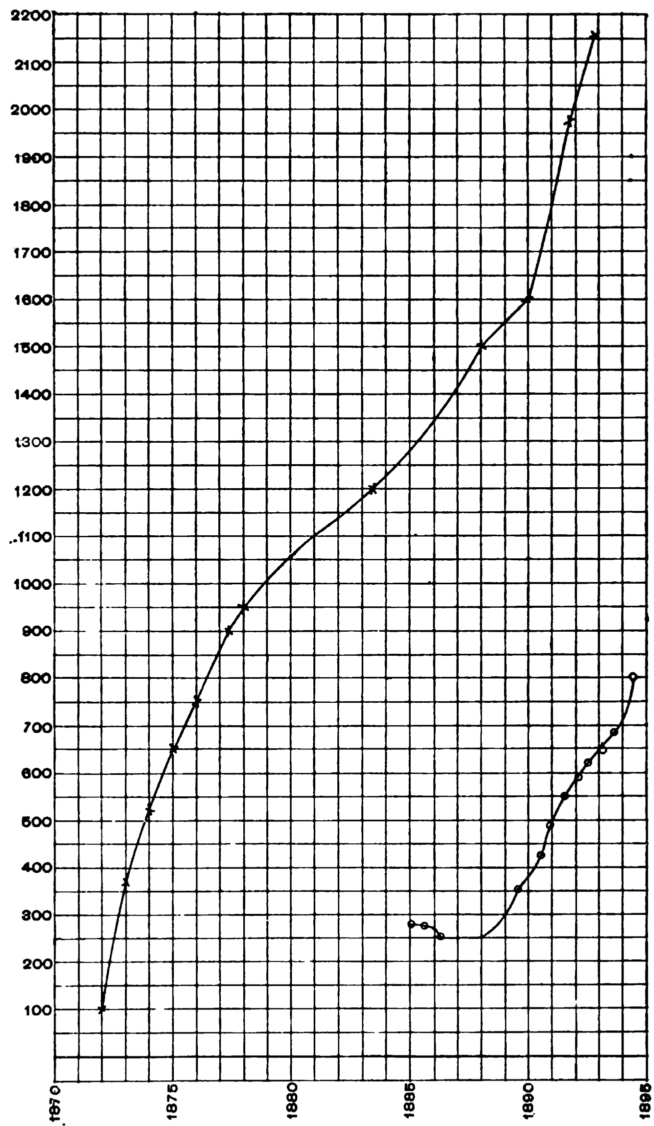
There was thus convened in 1884, in the city of Philadelphia, a notable gathering of men who had long toiled in the electric field, both in the so-called pure sciences and in the applied sciences, and I feel sure that each class recognized the fact that it learned much from the other.

In this notable assembly of electrical students, our AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS originated. I may be pardoned if I briefly review the facts attending its inauguration.

The first step was the circulation in April, 1884, by Mr. N. S. Keith, of a paper asking for signatures for the purpose of organizing a National Electrical Society, for affiliation with sister societies; for the possession of a library, the institution of original research; protection from unfavorable legislation; the settlement of disputed electrical questions, and the exchange of volumes of its TRANSACTIONS with foreign and other electrical scientific societies. A preliminary meeting was called on April 15th, 1884 in the city of New York, at which a series of resolutions were passed, and a Committee of Organization appointed to call a meeting, which was subsequently held on May 13th, 1884, when rules of order were adopted and officers elected. The first regular meeting of the INSTITUTE was held in Philadelphia, October 7th and 8th, 1884, in one of the Exhibition Buildings in West Philadelphia.

From this small beginning our INSTITUTE has assumed its present proportions. Its growth was, at first, uncertain, but its vitality was undoubted, and its present rate of increase is fully equal to that of our English cousin, viz., the Institution of Electrical Engineers. I append a curve showing the membership of both bodies at different dates, and although the British Institution had the start, and has the advantage of us in membership, yet I look forward in the near future to a membership in our body that will be fully on a par with theirs.

I think it would be difficult properly to estimate the good that has accrued to electric science, not only in this country but also in the world at large, from a properly organized association of specialists in a practical branch of science like that of electricity.



Total Membership at different times of the
British Institution of Electrical Engineers, and
American Institute of Electrical Engineers.

If we can properly trace, from the circumstances attending a single electrical exhibition and series of conferences held in

Philadelphia in 1884, a great awakening in the field of electricity, what must have been the influence for good, exerted by a body like ours, which I think I am correct in saying includes all the distinguished practical electricians in this country.

In order to inquire what has been the nature of this influence, let us briefly examine the history of the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS during the ten years that have elapsed since its foundation, and see whether, in the first decade of its existence, it has duly availed itself of its great opportunities. Let us inquire what great inventions and investigations have been made by its members. I think that as a result of these inquiries, you will agree with me that our INSTITUTE has nobly fulfilled the expectations reposed in it, and that electricity is much further advanced than it would have been had the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS never been organized.

A glance at the TRANSACTIONS of the INSTITUTE will show the extended and valuable character of the work of its members. This work embraces notable inventions, extended commercial applications, and valuable researches; as, for example, researches in high frequency discharges; the development of alternating current apparatus for electric welding, and for the transmission of power; improvements in continuous current apparatus; improvements in the practical applications and control of electric motors for traction, mining, manufacturing and other purposes; improvements in telephony and telegraphy; improvements in the application of electricity to various chemical processes; improvements in designs for electric machinery; improvements in electric lighting apparatus of various descriptions, and developments in electro-therapy.

The work of the INSTITUTE as a body has also been of a broad and valuable character. I have already pointed out to you, in my inaugural address of last year, the valuable contribution the INSTITUTE made to the Chicago Congress and Exhibition of 1893. Since that time, as you are aware, organized work, under the auspices of the INSTITUTE, has been and is being carried out in different parts of the country, as well as in England, for the completion of some of the work the Chicago Congress was obliged to leave incompleted; viz., the determination of suitable standards of light and of illumination.

Another action of which I think the INSTITUTE may be proud, has been its provisional adoption of the well known names of gilbert, oersted, gauss and weber, for the most important quantities in the magnetic circuit, thus filling a well defined void in the practical development of the dynamo, motor, and magnets generally. These names have already been favorably commented on in Europe, where they have been embodied in at least one standard text-book.

Up to this date there has been much uncertainity as to the meaning which should properly be attached to the very important term "inductance." It was hoped that the Chicago Congress would decide this question, but, as this hope was not realized, the INSTITUTE, by appealing to the prominent affiliated institutions in Europe, has been enabled to ascertain the consensus of opinion upon this matter among electrical engineers all over the world, and has to-day adopted the meaning of "inductance" as a "Coefficient of Induction," this being the world's majority verdict, so far as has been possible to obtain it without the aid of an International Congress. It has also adopted the word "reactance" for that quantity in alternating current circuits, whose square added to the square of the resistance is the square of the impedance.

It would be ungenerous in me in thus reviewing the causes which have led to the development of electrical science in this country, to fail to mention another potent factor. I refer to the electrical press. I recognize its power and the good it has accomplished in spreading broadcast over the country, not only to the members of the INSTITUTE, but to all interested in electrical progress, the knowledge of every great advance made in electric science. In a certain sense, however, the electrical press supplements the influence of the INSTITUTE, because the press, unlike the INSTITUTE, cannot bring electrical workers together, but can only guide and disseminate the conclusions they have reached.

The growth of the electrical press has kept pace with the growth of electrical science. In 1876 the power of the press was comparatively feeble. The Exhibition of 1884 caused, perhaps, as great an increase in the power and influence of the press, as it did in the devolopment of the science of electricity itself, and, great as has been the marked improvement in electrical science, as demonstrated by the Chicago Exhibition of 1893, I think close observers will agree with me that such progress has been fully equalled by the wonderful improvement in the electrical press of our country.

There is another association of electrical engineers of the same high standing, and governed practically by the same principles as those of the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS, and this is our affiliated association, the Institution of Electrical Engineers, with its headquarters in London, England. Like our association its membership contains the leading electrical engineers and experts both in the country in which it is located, and in the surrounding countries.

France has established a somewhat similar body in her Société Internationale des Électriciens, located in Paris. This society has the same general characteristics as the American and English societies, and, like them, publishes regular transactions of its proceedings. In Germany, there is the Elektrotechnischer Verein and the Physikalisch-Technische Reichsanstalt.

Although there are electrical societies in other parts of the world, notably in Italy, and Belgium and Australia, yet in none of these countries is to be found that organized effort and concentration in one central body of the electricians from all parts of the country, as is so markedly seen in the United States, England, France and Germany.

It is, I think, a significant fact, that the countries in which there has been so marked a progress in electrical invention and engineering, are those which possess the advantages of this combined effort on the part of all its electricians. The reason is, I think, evident; under these circumstances, there exists the enthusiasm which comes from properly organized effort; the rapid progress which is encouraged by friendly rivalry and the incentive to increased and continued effort, bred of healthy competition. I think I can safely assert that America, England, France and Germany owe much of their marked advance in electrical science to the existence of their organized bodies of electricians, such as is found in the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS, the Institution of Electrical Engineers, the Société Internationale des Électriciens, and the Elektrotechnischer Verein, and I feel sure from the great number of able electricians of Italy, Switzerland, Belgium, Russia, India and other parts of the world, that the progress made in these countries, a progress which is confessedly great, would be still greater if they but tried the advantages of electrical work conducted on the co-operative plan.

It may be advantageous here to review some of the advantages of membership in such learned associations, as, for example, the INSTITUTE in which we are the most interested. Among the many advantages are the following: concentration of effort; increased mentality excited by generous rivalry; systematic explorations into the domains of the unknown; a tacit agreement as to what shall be regarded as the standard of good work; the practical establishment of a high court of last resort by whom all disputed technical questions in electrical engineering shall be finally settled; the removal of electric work from the region of guesswork to that of certainty, permitting results to be as surely predicted as in other sciences, and, consequently, an increased stimulus to the successful investment of capital in electrical enterprises; the reduction of misdirected effort by the promulgation of information concerning what has been attempted or achieved in any direction; and last, but not least, the means of establishing a rapid intercommunication of ideas between different parts of the country to others.

As to the privileges of membership in our association, a member in any part of the country, whether in Maine, Florida, Illinois, or elsewhere, can, after submitting a paper to a committee appointed by the INSTITUTE for that purpose, have it read simultaneously at the New York and Chicago meetings, and thus not only derive the advantages which come from the broad dissemination of his ideas over the country, but can also have those derived from criticism by those best adapted to judge and discuss them. Instead of being obliged to wait and wonder if his results are valuable or correct, or instead of being forced to endeavor to solve such questions for himself, he is now, by means of the powerful machinery of our association, enabled to hear in a very short time the opinion of those best suited to sit in judgment on his work.

We are naturally and properly proud of the progress shown by our INSTITUTE in the first decade of its existence. I ask you now in all seriousness, how has this progress been assured? Clearly by the establishment of a central, organized body, as distinguished from separate, independent, and possibly antagonistic bodies; by the establishment of a central body which derives its authority from a membership extending over the entire country. Is it credible that independent, disconnected, and possibly antagonistic societies, located in as many separate cities as there are groups of members sufficient to form separate societies, can hope to accomplish as much good in so short a time as has been accomplished? Would not the disintegration of our INSTITUTE prove to the electrical engineers of this country little short of a calamity? Might not the establishment of separate organizations result in mutual jealousies and intense sectional feeling, and, consequently, in a tendency to the continuance of errors once contracted? Partisanship and intelligent scientific work, in the nature of things, have nothing in common. The true scientific instinct is shown in the desire to know the truth for the truth's sake, and the true electrical engineering instinct is to accomplish the best work in the most economical manner possible. I feel sure you will agree with me that to ensure the greatest success, there must of necessity be a central governing body, viz., the Council of the INSTITUTE, deriving its authority from a membership extending all over the country, and vested with the power of speaking authoritatively for the INSTITUTE between the periods of its recognized official meetings.

In a country like ours, in which distances are so great, a difficulty exists in all our members attending the meetings of a central body, no matter where such meetings might be called. This difficulty is real, and like all geographical difficulties, cannot readily be solved. I think our association has, however, to a great extent, partly solved it by encouraging simultaneous meetings in all parts of the country where the same paper can be read and discussions had thereon, yet at the same time, holding the governing body, the Board of Managers or the Council of the INSTITUTE, responsible for the proper direction of its work. That all local meetings must be amenable to the organic law of the INSTITUTE, be that law what you may choose to make it, I think needs no discussion. I am glad to say that already, under due authority of the INSTITUTE, local meetings have been established in the city of Chicago, and I trust there may soon be other similar meetings held in all other great centers of population where our membership will warrant it.

Such, I think, are some of the advantages of organization under a central body as opposed to organization under separate, independent bodies. They are, briefly, the advantages of concentration as opposed to those of diffusion; of directed, organized effort as opposed to unorganized, undirected effort. To argue in favor of the latter would be, I think, to deny the advantages of a central government, like our national government at Washington, with its representation from all the various States of the Union, and to revert to the condition of states sovereignity, an un-American and altogether untenable position.

As I look over this assembly of distinguished electricians, I am particularly impressed with this thought; viz., our average member though old in actual experience, is, nevertheless, seldom hoary in years. There must be something in electricity, though what it is I would not venture to say, which attracts the younger and more vigorous members of our race to its study. Perchance it may be that in this mysterious force, there exists some lingering traces of the long sought for "fountain of youth;" but, be it what it may, I find in the fact that such comparatively young men have been able to do so much for the world's weal in a special science, a bright promise of what they may be able to accomplish before their tasks are completed.

Such is the record of the past ten years of our INSTITUTE! What will be the history of its next ten years? I look forward confidently to a still greater and more marked progress than that which has characterized it during the first decade of its existence. I believe that during the next decade its standing will be such that all notable achievements and discoveries in the electrical field in this country will either originate in this body, or be carried out under its direction, and that the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS will be the acknowledged center of the industry and art it now so ably represents.

But while I believe I see so bright a future for our AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS, I must not be unmindful of the fact I have so earnestly endeavored to point out, viz., the advantages to be derived from co-operation, and that our INSTITUTE is only one of several such organizations in different parts of the world, and that the highest purposes of the science and art in which our interests are so closely centred, can only be best realized by the most cordial sympathy and hearty co-operation with all associated societies and their members wherever they may be.

THE PRESIDENT:—The next paper "On the Subdivision and Distribution of Artificial Sources of Light," will be read by Professor Anthony.

A paper presented at the eleventh General Meeting of the American Institute of Electrical Engineers, Philadelphia, May 15th, 1894. President Houston in the Chair.

ON THE SUBDIVISION AND DISTRIBUTION OF ARTIFICIAL SOURCES OF ILLUMINATION.

BY PROF. WILLIAM A. ANTHONY.

It is a well recognized principle that to illuminate evenly a given area by means of an artificial source, it is necessary that this source should consist of numerous small sources distributed over the area. In carrying out this principle it is usual to divide the area to be lighted into squares, and place a lamp in the center of each square as shown in Fig. 1 where each lamp is represented by the sign ×. In order to study the distribution of light by this arrangement of lamps I have computed the illumination at the central point of the figure due to the lamps situated upon the boundaries of each of the squares represented by the dotted lines, the illumination produced by the lamps on the smallest square be taken as unity. The following table gives the values up to the twelfth square, twice the number represented in Fig. 1, and corresponding, therefore, to an installation in which four times as many lamps are used.

I.	II.	III.	IV.	V.
1	1.000	1.000	4	4
2	.511	1.511	12	16
3	.312	1.823	20	36
4	.223	2.046	28	64
5	.174	2.220	36	100
6	.143	2.363	44	144
7	.121	2.484	52	196
8	.104	2.588	60	256
9	.092	2.680	68	324
10	.083	2.763	76	400
11	.075	2.838	84	484
12	.067	2.905	92	576

Column I gives the designating number of each square counting outward from the central point. Column II gives the illumination at the center due to the lamps located on each boundary. Column III gives the total illumination at the center due to the lamps included within and upon each boundary. Column IV gives the number of lamps situated upon each boundary. Column V gives the total number of lamps.

It is seen from the table that the twelfth series which consists of 92 lamps gives at the center less than 7 per cent. as much light as the first series of four lamps, and contributes only about two and one-half per cent. to the total illumination at the center. This arrangement of the lamps does not give an even distribution of light over the entire area, as will be evident from a considera-

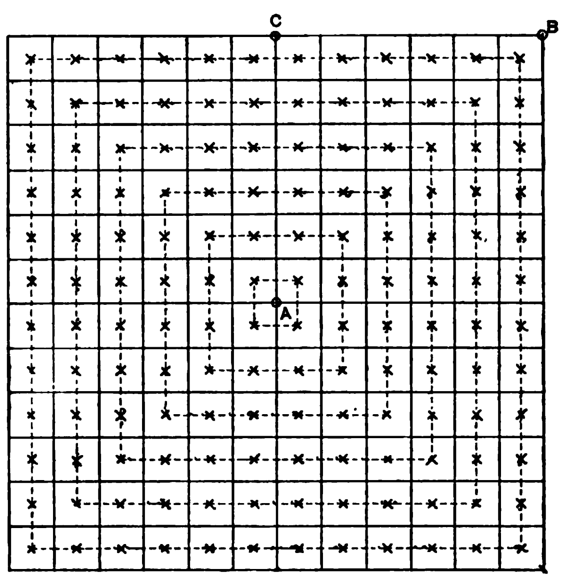


FIG. 1.

tion of the illumination upon the outside boundaries of the space. It is evident that the point B at the corner of Fig. 1 receives one-fourth as much light as it would do if it were the center of an area four times as large, and lighted by four times as many or 576 lights. But the illumination at the center of such an area as seen by the table is 2.905. The illumination at B is, therefore, 2.905 ÷ 4 or .726, while the illumination at A from the sixth line of the table is 2.363. The illumination at B is then less than one-third that at A. At C the illumination is evidently but little more than half that at A. Points located between the center and outside would be illuminated to an intermediate degree, but after leaving the outer boundary the illumination would rapidly approach that at the center of the figure. I am considering always points of least illumination in any region, that is, points situated as far as possible from any lamp. It is evident, therefore, that to obtain a uniform distribution of light, the lamps must be concentrated toward the outer boundary of the space instead of being placed at equal distances throughout, as in the figure.

But the most interesting question connected with this matter is: what is the effect upon the uniformity of the illumination of grouping the lamps in clusters or using larger lamps at fewer points? It is evident that whatever the candle power of the lamps or their distances apart in a distribution like that represented in Fig. 1, the *relations* represented in the table will remain unchanged. It is evident also that the illumination at the central point will be proportional to the intensity of the individual sources (if lamps are placed in clusters each cluster is to be considered as a source) and inversely to the square of the distance between the sources.

This illumination is, therefore, given by the formula

$$K\frac{s}{d^2}C$$

where K is a constant depending on the units employed, s the intensity of the individual sources, d the common distance between them, and C the quantity in column III of the table corresponding to the number of sources as found in column V.

Compare the illumination at the center of an area lighted by 576 lamps with that at the center of the same area lighted by the same lamps arranged in 144 clusters of four lamps each. If s and d represent the intensity and distance in the first case, $4s$ and $2d$ will represent the corresponding quantities in the second case. C in the first case is 2.905 corresponding to 576 sources. In the second case, for 144 sources C is 2.363. The relation sought is, therefore,

$$\frac{K\frac{4\,s}{4\,d^2}\,2.363}{K\frac{s}{d^2}\,2.905} = .813$$

The light at the central point with the fewer sources, is only 81 per cent. that obtained under the first arrangement. Other points of minimum illumination will suffer in like manner. On the other hand, points near the clusters of lamps will be much more strongly illuminated than corresponding points near the single lamps of the first arrangement.

If it be required that the illumination at no point shall be less in the second case than in the first, it will be necessary to increase the intensity of the sources nearly 25 per cent., or make the clusters consist of five instead of four lamps.

Again, suppose an area lighted by 64 sixteen C. P. lamps distributed as in Fig. 2. It is required to substitute 16 lamps of larger candle power distributed as in Fig. 3, and fulfilling the condition

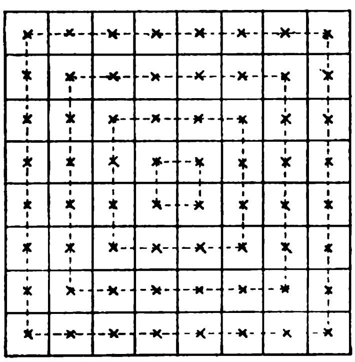


FIG. 2.

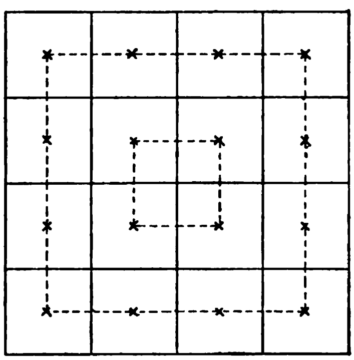


FIG. 3.

that the minimum illumination shall be no less in the latter case. If x be the candle power of the larger lamps, we have:

$$\frac{x}{4\,d^2} \times 1.511 = \frac{16}{d^2} \times 2.046$$

$$x = 64 \times \frac{2.046}{1.511} = 86.7$$

From which it appears that the total candle power must be increased 35½ per cent. If the 16 candle power lamps consumed 3.5 watts per candle, the larger lamps to compete with them should consume less than 2.6 watts per candle.

It will be noted from the two examples given, that the loss from reducing the number of sources in a given ratio is less when the number is large. In other words, large lamps, or clusters of lamps, can be more economically used for large, than for small areas.

An inspection of the table will show, that while a given area may be lighted satisfactorily by 16 clusters of four lamps each, an area one-fourth as large, lighted by four clusters at the same distance apart, would not be as well lighted with six lamps to the cluster. It will be seen also that the larger the area, the further apart may lamps of the same candle power be placed. For example a room 40 feet square with 16 lamps 10 feet apart, is fully as well lighted as a room 16 feet square with four lamps eight feet apart.

Let us compare arc and incandescent lights on the same basis, that the minimum illumination shall be the same under both systems. First, I must say that the efficiency of arc lights has been greatly overrated. Instead of being ten times, it is rarely three times, and often only one and a half times that of an incandescent lamp.[1] This is for the naked arc. For indoor illumination, ground or opal globes are nearly always used, and these cut off fully half the light.[2] This leaves the efficiency at the most 1.5 times that of the incandescent lamp. The power required for a 16 candle incandescent lamp is 50 watts, and for a full arc lamp 450 watts. The arc lamp must, therefore, replace nine incandescents. Assuming that the lamps are distributed as described in this paper, the table shows that if four arcs take the place of 36 incandescents, their efficiency as compared with the incandescents must be 1.823, and with a relative efficiency of only 1.5, the arc lamps would not light the space as well as the incandescents. If 16 arcs take the place of 144 incandescents, the ratio of the efficiencies required is 1.56, and arcs at 1.5 are still not equal to the incandescents. If 64 arcs take the place of 576 incandescents, the ratio of the efficiencies becomes 1.42, and arcs at 1.5 would be an improvement on the incandescents. It comes then to this, that unless the area to be lighted is so large as to require about 500 incandescent lamps distributed uniformly over it, the use of full arc lamps requiring the same power, will leave some parts of the area less brilliantly lighted.

If we could make small arc lamps of the same efficiency as the full arc—by full arc I mean the so-called 2,000 candle lamp consuming about 450 watts—we could improve somewhat upon

1. See "Efficiency of Artificial Methods of Illumination." Dr. Nichols: TRANSACTIONS, vol. vi., p. 171.

2. "Loss of Light from Use of Globes with Arc Lamps." George D. Shepardson: *Electrical World*, vol. xxiii., p. 287.

the result as obtained above. But the fact that the mechanism of a small arc lamp costs just as much as that of a large one, and that *each* small arc will require as much care and attention as a large one, would place a limit upon the subdivision of the arc even if the efficiency could be maintained. And here I wish to enter a protest against the assumption that I often find in discussions of this subject, that the *so-called* 1,200 and 2,000-candle power arc lamps *are* 1,200 and 2,000-candle lamps. There is no 450 watt arc lamp in use that will measure 2,000-candles in the direction of greatest intensity, and compared with other artificial sources for general illumination, that is measured as other sources are measured, there is no 450 watt lamp that will give, when surrounded by a plain glass globe, 500 candles.

In a linear distribution, as in street lighting, the arc is at a still greater disadvantage. Remembering that nine 16-candle incandescent lamps can be run with the power required for one 450-watt arc, it is seen that the arcs must be nine times as far apart as incandescents consuming the same power, and to give the same minimum illumination must be 81 times as intense, or about 1,300 candle power. But in no arc lamp as used for street lighting, do the rays proceeding toward the most distant points to be illuminated, reach more than one-fourth this intensity. In my way of thinking, the location of arc lamps at intervals of 1,000 to 1,600 feet as they are often seen in pretentious country villages and suburban places is an entire waste of money. A little spot 50 to 100 feet in diameter under each lamp is brilliantly lighted, while the more distant points are in darkness all the more profound from the loss of sensitiveness of the eye when in the strong light. Incandescent lamps at intervals of 100 to 200 feet, which could be run by the same power, would give a far better illumination.

Of course, in deciding between the use of arc and incandescent lamps in any special case, there are other questions to be considered besides that of the power required to operate. Practical questions of installation may outweigh all others. The considerations of these is foreign to this paper. The question I have considered is purely one of efficiency, and in treating that question I have assumed that every part of a space to be lighted, *needs* the same amount of light. I have omitted from the consideration all effects of reflection from ceilings, walls, or reflectors purposely provided, as these are too various and depend too much upon the conditions in each special case to be introduced into a general discussion. My main object has been to point out to just what extent the general illumination of a space is affected, other things being equal, by the use of large in the place of small sources using in the aggregate the same power, and I trust the figures and illustrations I have given may be found useful in considering the special cases that may arise.

Vineland, N. J., April 28th, 1894.

DISCUSSION.

MR. NELSON W. PERRY:—I would like to ask in regard to the statement that no 450 watt lamp will give, when surrounded by a plain glass globe, 500 candles, what it means? There have been so many thousand measurements of the intensity of arc lamps that have almost universally shown an intensity in the plane of maximum illumination between 750 and 1250 and even more, that it would seem that they could not all be wrong. Most of these measurements were made with lamps without globes probably. Now, if we assume 1000 C. P. as an average of all of these determinations without globes, we must assume that Prof. Anthony's statement if true must mean that clear glass globes cut off at least 50 per cent. of the light in order to bring it down to 500. This of course is absurd.

PROF. ANTHONY:—I think you misunderstood the statement there. The statement is in the direction of the most distant point to be illuminated.

THE PRESIDENT:—Is that made by the short arc or the long arc?

PROF. ANTHONY:—The long arc 450 watt lamp. That is in the direction of the farthest point to be illuminated. It would be not more than ten degrees from the horizontal line. The illumination of any arc lamp in that direction is very small.

MR. KENNELLY:—While the figures that Prof. Anthony gives are no doubt true in the open air, without moon or star light; that is to say where there is no reflection; yet within doors the circumstances of reflection from walls and ceilings, would so far modify the practical conditions of the problem, that the conclusions to be drawn from the paper will surely undergo great modification in consequence; and while no doubt the general proposition is true that to get the most uniform and the least minimum illumination at any point, you should place a thousand candles in 1,000 candles and not have it in one lamp, still the effect of reflection from walls and ceilings, will so far modify any preconceived notions that we may have upon what should be the best luminous distribution, that everything would largely depend upon the particular circumstances of each case.

THE PRESIDENT:—I must confess to considerable surprise at the figures reached by Prof. Anthony. I do not doubt but that as a careful scientific man he has the facts of actual measurement to warrant the correctness of these conclusions. I would say, though, that such a very small economy of light for an arc lamp is quite at variance with the experience I have had in the matter, and is absolutely inconsistent with our ideas of the relations existing between temperatures and the power of emitting light. The temperature of an arc is immensely higher than that of an incandescent filament at any temperature at which it would be practicable to run the filament, and to get so astoundingly small an economy out of an arc lamp shows that there must be either some peculiarity of measurement, or something wrong in our preconceived ideas as to the relation which ought to exist between candle power and temperature. I do not think it fair to measure the light giving power of an arc in a horizontal direction and base conclusions on that. It is fair enough to take that in an ordinary gas flame or in an incandescent light where this direction is far from being its position of least efficiency. I do not think it is fair to take it in the case of an arc light. I was particularly induced to ask whether you were running with the long arc or the small arc since it struck me at first that possibly the error might come from the shading of the positive crater by the projection or nipple on the negative carbon. The figures surprise me very much. I feel sure that it will be quite a surprise to those interested in arc lighting in the country to learn, if I am correct in my understanding, that you do not get more than one and one-half times out of the arc than out of the incandescent. Still, if true, it is very well worth learning.

Mr. Steinmetz:—I think the figures are not very far from true. When the 2,000 candle power arc light got this name, it was given to it because if everything is adjusted carefully, as it can be done in the laboratory, then the maximum intensity is about 2,000 candles, but the spherical intensity is only little more than one-third as high, that is, 700 candles, and taking into consideration, that a part of the light is shaded off by the glass globe, we get not very far from 500 candles as the mean intensity of the covered arc. Still from another side you can approach the same result:

The brilliancy of the arc is that of boiling carbon. The brilliancy of the incandescent lamp filament, that is, the candles per watt, is less. But, increasing the temperature by raising the voltage, increases the brilliancy, until the carbon filament evaporates. Now, immediately before this, the brilliancy must be about the same as that of the arc. Photometric tests of incandescent lamps at seven to eight times their rated candle power give brilliancies of from 1 to 1.2 candles per watt, and seem to point towards a maximum value of about 1½ c. p. per watt. The spherical brilliancy of very large arcs was observed as 1.5 to 1.7 candles per watt.

The above stated data of 2,000 maximum or 700 spherical candles for the ordinary 450 watt arc gives a brilliancy of 1.55 candles per watt, so that, estimating the light absorption of the glass globe as 30 per cent. we get the figures given by Prof. Anthony.

Hence the name "2,000 candle power arc" only refers to the maximum intensity of the naked arc in the most favorable direction. A more proper way to rate arc lamps would be, not by the candle power, but by the watts consumed. I think that is quite extensively done now.

Mr. Clayton W. Pike:—I understand Prof. Anthony's figures are about 500 candles spherical when the glass globe is on. I remember making a number of experiments on 1,200 candle power lights without any globe, of the spherical intensity, and in every case we ran below 450 candles. If we change from the 1,200 to the 2,000 and also bring in the correction for loss from the globe, we shall find those figures correspond very closely with Prof. Anthony's. I have done that on a sufficient number of arc lights to enable me to feel sure that those figures are correct.

PROF. ANTHONY:—If I may put a figure on the blackboard:—(making a sketch, Fig. 4) measuring the intensity of the light from an arc lamp at various angles from the horizontal downward, we obtained results which when plotted on a system of

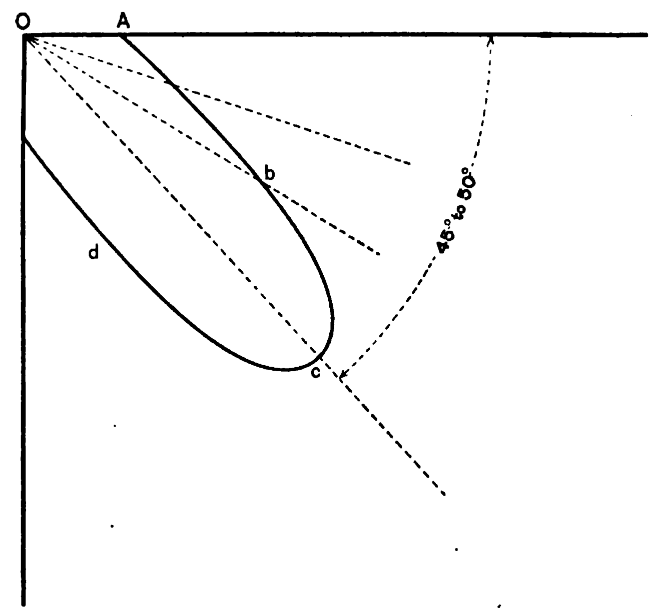


FIG. 4.

polar coördinates gives a curve like A, *b*, *c*, *d*, where O A, O *b*, O *c*, etc., represent the intensities in those directions. The maximum intensity occurs at 45 or 50 degrees below the horizontal, and from that point toward the horizontal it decreases very rapidly, until along the horizontal the 450 watt arc will rarely measure more than 300 candles. It is usual to take as the mean intensity of the arc light the mean intensity in the lower hemisphere because it is the lower hemisphere that we wish to illuminate in all ordinary use of the lamp. Since the light in the upper hemisphere is extremely small, the mean intensity for the lower hemisphere is much greater than the mean intensity for the entire sphere, but even in the lower hemisphere the mean intensity is not above 500 candles.

THE PRESIDENT:—At what height did you estimate the average height of the arc?

PROF. ANTHONY:—These are candle power measurements made on a photometer. The horizontal intensity would be a horizontal line.

THE PRESIDENT:—You are not considering then the question of illumination at all?

PROF. ANTHONY:—In this case, in determining the candle power, we simply measure it on a horizontal line as so many candles and so on as you go down. At 45 or 50 degrees there is a maximum, and the measurements will sometimes run to 1,500 or 1,700 candles. On good 450 watt long arc lamps I never obtained but one measurement out of thousands I have made, as high as 2,000 candles.

THE PRESIDENT:—That probably is near the crater, say, on one side.

PROF. ANTHONY:—Very likely.

THE PRESIDENT:—I do not yet see why you call that the maximum intensity. That is not the maximum intensity; else I do not understand. The horizontal line—did I understand you to call that the maximum intensity?

PROF. ANTHONY:—No. At some 45 degrees below the horizontal, is the line of maximum intensity. At the horizontal line it would not be more than 300 candles.

THE SECRETARY:—Then the practical efficiency of the lamp would depend very largely on what purpose that lamp was intended for. If it was intended for the illumination of a park or any large open space, it might be well to have it up at a certain height, while for practical purposes, if the illumination was required on a horizontal plane opposite the arc it would be comparatively inefficient. So that the practical result will have to depend largely on the purpose for which the lamp is designed.

MR. FRANCIS R. UPTON:—A little apart from the subject of arc lamps, I would like to say, that I am very glad to see that Prof. Anthony has made an effort at the distribution of light, which, as I understand, is the title of this paper. A number of times I have had that subject brought before me, and I know that it is one of the most puzzling subjects which can be presented. The conditions vary so, as Mr. Kennelly mentioned, about ceilings and walls, that the question of proper distribution of light is one which it is very difficult to answer in general terms. There is one point in distribution which I have noticed, which probably many of you have observed: that is the fact, that in the illumination of a given space, the whole aim should be not to spot the lighting, so that the eye will not lose its sensitiveness by being dazzled by a bright spot. It is well known to those who have to do with lighting, especially with the incandescent lamp, that rooms can vary two or three-fold in their rate of illumination; that is the absolute rate of illumination without being noticed if the eye has only darkness to compare the light with. I wish Mr. Stieringer were here, as he is a master of the art of illumination. He put in practice one of the most successful illuminations in this country; that was at the exposition at Louisville some years ago. At that place he distributed his lights with the power to prevent anybody from putting an arc lamp in the building, or from using any light, grouped in clusters, near the ground. The result was that he was able to illuminate a large exhibition building there, so that it looked brilliantly lighted with a very low grade of absolute illumination, because there was no one spot which your eye looked at which made the rest look dark. I feel that there is great room for some good means for determining illumination in distinction from determining the candle power of the light giving body. There appears to be no good unit for illumination that is thoroughly reliable for this purpose, and I think that a discussion of the general lines that Prof. Anthony has made in this paper, adds to the knowledge of what is meant by the distribution of light, and I find it very interesting. I think, probably many of you have perceived, in going along a street at night and looking into the various stores and seeing how much the illumination varies, by reason of the mode of placing the lights in those places, how much more use can be made out of lamps well placed than out of lamps poorly placed.

THE PRESIDENT:—I suppose the photometer you used absolutely prevented any reflection from the ground. You simply measured the horizontal ray. You had probably a shield or screen or something to prevent any other light from coming in except from that direction.

PROF. ANTHONY:—Those measurements such as I have shown on the board are made in the photometer room—a blackened room.

THE PRESIDENT:—Consequently you would lose all possible advantage of what you get in actual practice—illumination of objects from light thrown from the ground. I cannot believe in only one and a half times the efficiency of the arc light over the incandescent. It is contrary to my experience. I think there is something wrong in the method. However, I certainly shall know in a little while whether I am wrong or not.

MR. R. O. HEINRICH:—It is rather surprising that so little attention has been paid to the very important and eminently practical questions considered by Professor Anthony. If we would simply make a distinction between luminosity of a source and illumination produced, such perplexing questions as the actual candle power of a "2000 candle power" arc would be of very minor importance.

On account of the complication of conditions the illuminating effect of a combination of sources of light, such as met with for artificial illumination, is almost beyond mathematical computation. A practical and successful solution of such problems can be obtained only through a vast number of observations and measurements.

The requirements of actual practice are two-fold: First: A uniform illumination should be attained, one which comes nearest to the ideal illumination of diffused daylight. Second: A definite intensity of illumination should be assumed as necessary either for the performance of certain work, such as reading, writing, drawing, sewing, etc; for general indoor illumination in theatres, halls, etc., and for outdoor and street illumination.

It matters little for these considerations what the "candle power" of a source of light amounts to; for the conception of a definite illumination, this expression is without meaning. It is necessary under the above considerations to express the intensity of illumination in its own distinct unit.

Little has been done towards the universal adoption of such a unit, although the "metre candle" was proposed for the purpose more than ten years ago by Prof. Leonhard Weber, W. H. Preece, and Mr. Wybaw, the latter proposing the name "lux" for this unit.

On the authority of Dr. H. Cohn (Breslau) 50 metre candles are sufficient to permit reading with the same facility as in diffused daylight. He considers 10 metre candles as a minimum illumination from a hygienic standpoint, for the purpose of reading. Wybauw (Mésure et répartition de l'eclairement. *Bull. de la Soc. Belge d'Electr.*, 1885) considers 15 to 25 metre candles or "lux" as necessary to permit a fluent and prolonged reading of a newspaper, and holds that a minimum of one metre candle should be required for street illumination.

Assuming such or similiar values based upon a unit of illumination, all controversy as to candle powers would be at an end. Contracts for the illumination of reading rooms, halls, streets, squares, etc., should specify a required minimum illumination in some such unit as above referred to, and it would then rest with the expert engineer to accomplish this with the least possible amount of mechanical energy converted into light.

Actual measurements would soon convince us in what bungling way illumination is generally carried out. Prof. Anthony's remarks in reference to the lighting of pretentious country villages and suburban places are very pertinent; they would apply equally well to the illumination of our city streets and squares if it were not for the illuminated shop windows which somewhat mitigate the contrast between superabundance of light and darkness.

In connection with this matter I may be allowed to make reference to Professor L. Weber's portable photometer, which is especially adapted for making measurements of the intensity of illumination. This photometer seems little known on this side of the Atlantic, and I therefore give a sketch of its general arrangement. See Fig. 5 and 6.

The apparatus consists of a tube A about 30 cm. long, which can be moved up and down, and swung in a horizontal plane on the upright O. The standard light S, a benzine lamp, is contained in a lantern fastened to the right end of the tube A. Within the tube A a circular plate of opal glass can be moved from or towards the light S; its distance from S is read in centimetres on the scale s by means of an index fastened to the pinion P. At right angle to tube A a second tube B is fastened. This tube can be rotated in a vertical plane, and its position in reference to the horizontal is read on the graduated circle C. A rectangular prism contained in tube B in its axis of rotation receives light from the opal glass plate in tube A and reflects this light towards

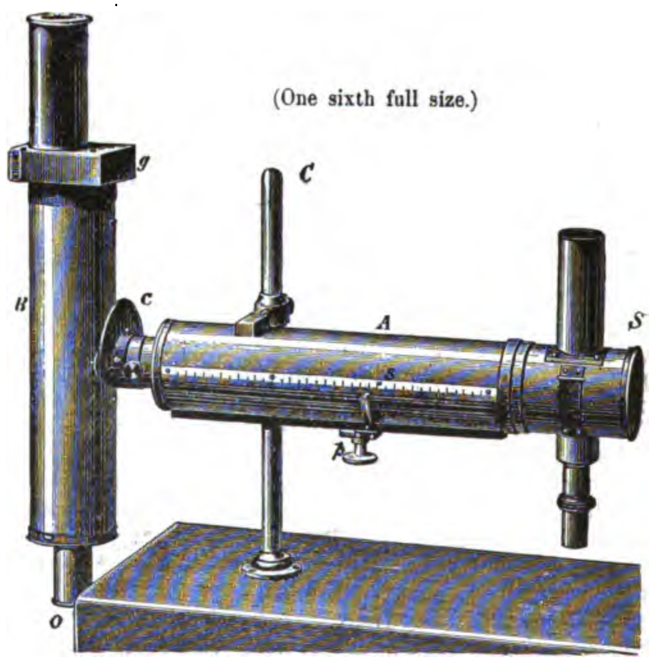


Fig. 5.

the eye piece O, so that the right half of the field of vision is illuminated by this light, the left half is illuminated by the light entering the tube B at *g*.

In making measurements, the tube B is pointed towards the source of light to be measured. This light has to pass through a square box *g* in which may be inserted one or more opal glass plates, in order to diminish the intensity of the light and thus to make it comparable with the standard light. The apparatus permits the measurement of light in the shape of a flame as well as the measurement of diffused light.

Since the measurement of diffused light interests us most at present, a short description of the method will not be out of place.

A white screen, the surface of which is absolutely without lustre, furnished as part of the apparatus, is placed in a convenient position, either horizontal or vertical, or at any desired inclination towards the source of light.

The photometer having been located at a convenient distance from the screen, the tube B is pointed to the center of the screen. The distance of the photometer from the screen can be varied within very wide limits, the only restrictions being that the field of vision receives no other light than that emanating from the screen. The necessary precautions for adjustment having been observed, the opal glass plate in tube A is moved until both halves of the field of vision appear equally illuminated. The distance r of this glass plate from the standard light at the

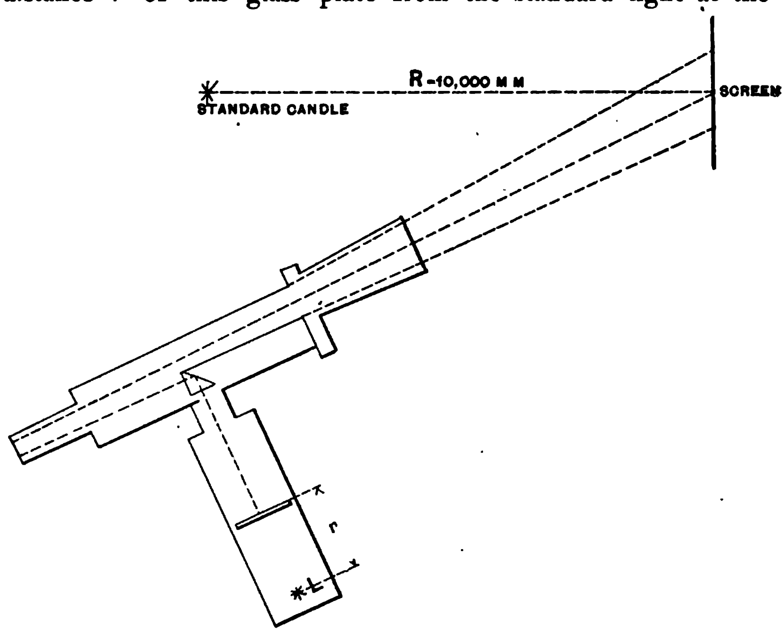


FIG. 6.

moment of equal illumination is read on the scale on tube A in millimetres, and the intensity of illumination on the white screen is calculated from the formula.

$$I = \frac{10000}{r^2} K.$$

The constant K is previously determined as follows:

A standard candle is placed exactly one metre distant from the white screen and the tube B of the photometer is pointed towards the screen, so that the center of the screen, which is marked by a cross, is seen in the center of the field of vision. As indicated in the sketch, the photometer must be so placed that the eye looking through the eye piece sees nothing but the white screen. The angle of inclination under which the screen is observed may be varied within wide limits without influencing the result, it should however not exceed 60° from the normal to the screen.

Equal illumination of both halves of the field of vision having been obtained by means of adjusting the opal glass plate in tube A, the constant c is found by calculation;

$$K = \frac{r^2}{R^2}.$$

Since r is read in millimetres and R is made 1 metre or 10000 millimetres, 10000 instead of 1 must be taken in the formula for calculating the intensity of illumination in metre candles.

A second method permits of measurements of diffused light without the intervention of the screen, but for further details I must refer to the description of the apparatus by Prof. Weber, *Elekrotechnische Zeitschrift*, vol. v. p. 166.

Since the whole apparatus can easily be taken apart and packed in a box about 24 x 8 x 12 inches, it recommends itself extremely well for out-of-door work. In this case the benzine lamp might well be replaced by a small incandescent lamp, provided this lamp is standardized before and after each set of experiments. I have found such miniature lamps very convenient, and quite sufficiently constant in candle power for several hundred observations.

THE PRESIDENT:—If there is no further discussion, we will go on with the next business. I understand that Mr. Wurts has notified the Secretary that it would be impossible for him to read his paper on "Discriminating Lightning Arresters and Recent Progress in Means for Protection against Lightning," to-day. Meanwhile, Mr. Hammer has asked the privilege of the floor to explain a matter in connection with the consideration of rules adopted by the National Electric Light Association for electrical construction and operation.

MR. W. J. HAMMER:—Mr. President and gentlemen—Secretary Porter of the National Electric Light Association has sent me for distribution some of the copies of the Standard Rules for electrical construction and operation recently adopted at the convention at Washington of the National Electric Light Association, and on behalf of that Association and as chairman of the Committee on Standard Rules I wish to bring these rules before the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS, and I make the motion that the chair appoint a committee, preferably of five, who will examine these rules with a view of recommending their endorsement by the INSTITUTE. And in connection with this motion I would like to ask that this be taken up as a special matter on Thursday.

I wish to say one word in this connection, and that is that these rules are the result of a very large amount of work by gentlemen connected with a number of committees which have had this matter in charge for years past. Some years ago there existed an infinite variety of rules issued by the different insurance companies, boards of trade, electric light companies, experts and others, which rules have gradually disappeared or been incorporated in a set of rules issued by the National Electric Light Association. These rules have been issued with very slight modifications by the International Board of Fire Underwriters, the National Board of Fire Underwriters, and the Local Board of Fire Underwriters without, however, giving any credit to the original source, which is the National Electric Light Association. Various efforts have been made to have one single set of rules go out. It is my hope and that of others that before this year expires some action will be taken which will bring about this long sought for result, and as these rules that are issued by the National Electric Light Association are to all intents and purposes the same ones which are issued by the Board of Fire Underwriters, with very slight modification, I would recommend that this committee be authorized to examine into these rules with a view to recommending their endorsement by the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS. This will be a step in the right direction. There will be undoubtedly certain things open to criticism. But these rules are in the hands of a permanent committee that are intending to make them the standard rules and keep them up to date, and the endorsement of the INSTITUTE, as a representative body of scientific men interested in matters of this character, will assist this good work and I feel sure that with this endorsement by the INSTITUTE, before the year is out or before another year comes, there will be but one set of rules which will be satisfactory and which will receive the endorsement of the National Electric Light Association, the Boards of Fire Underwriters, the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS and all other bodies. It is with this end in view that I have asked the privilege of bringing the matter before the INSTITUTE, and I make the motion that the chair appoint five gentlemen to report upon this matter and bring it up before the meeting on Thursday.

[The motion was carried.]

THE PRESIDENT:—The Chair appoints as the committee, Mr. Hammer, Mr. C. P. Steinmetz, Mr. A. E. Kennelly, Mr. Edward Weston and Mr. N. W. Perry.

The Secretary made some announcements respecting invitations and the meeting then adjourned until the following day.

Tuesday evening the members attended an informal reception given by the Engineers and Manufacturers of Philadelphia, under the auspices of the "Engineers' Club" and the "Electrical Section of the Franklin Institute," at the Manufacturers' Club.

A paper presented at the Eleventh General Meeting of the American Institute of Electrical Engineers, Philadelphia, May 16th, 1894. President Houston in the Chair.

SOME STORAGE BATTERY PHENOMENA.

BY W. W. GRISCOM.

It was with great hesitation that I ventured to accept your invitation to read this paper before a body of distinguished men, who are more or less masters of their own time and have devoted it, and dedicated themselves, to science and research, and it was finally with a view to enlisting your interest and assistance in the storage battery problem that I decided to lay before you some of the phenomena which have in turn baffled and instructed me, occupied my thoughts, and kept my faith from flagging during the ordeal through which the storage battery interests have passed.

The study of a complete curve of discharge of a storage battery (Curve III) discloses three rather sharply defined changes in P. D., and after allowing for the effect of internal resistance, we find that the changes are those of the E. M. F.

What is the reason for these changes? Which plate is responsible for them?

A microscopic examination of the negative (spongy lead) plate, disclosed metallic lead and what appeared to be one, or possibly two sulphates. The positive (peroxide) plate, however, showed spongiform crystals of very dark color, known as electrolytic peroxide; other comparatively large crystals of brilliant red, probably Frankland's red sulphate; others of yellow, probably yellow sulphate; and finally the better known white sulphate of lead.

The production of these diverse chemical forms must be attended by production of diverse potentials, and the E. M. F. of the battery is possibly a resultant with one or more chemical reactions predominating at various parts of the charge and discharge.

That there should be any change of E. M. F. during the charge or discharge of the battery, shows the complexity of the chemical actions, and as the changes are at times sudden, and at times gradual, uniform and invariable, it would seem to point to the conception that the E. M. F. is the resultant of three or more sets of chemical actions.

The material on the charged positive plate of the battery is

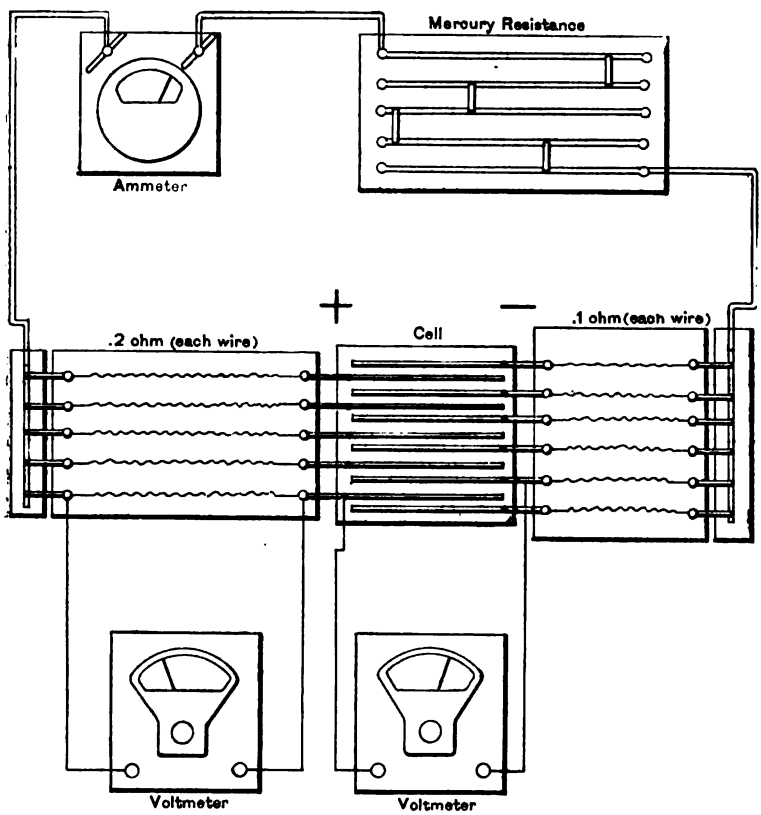


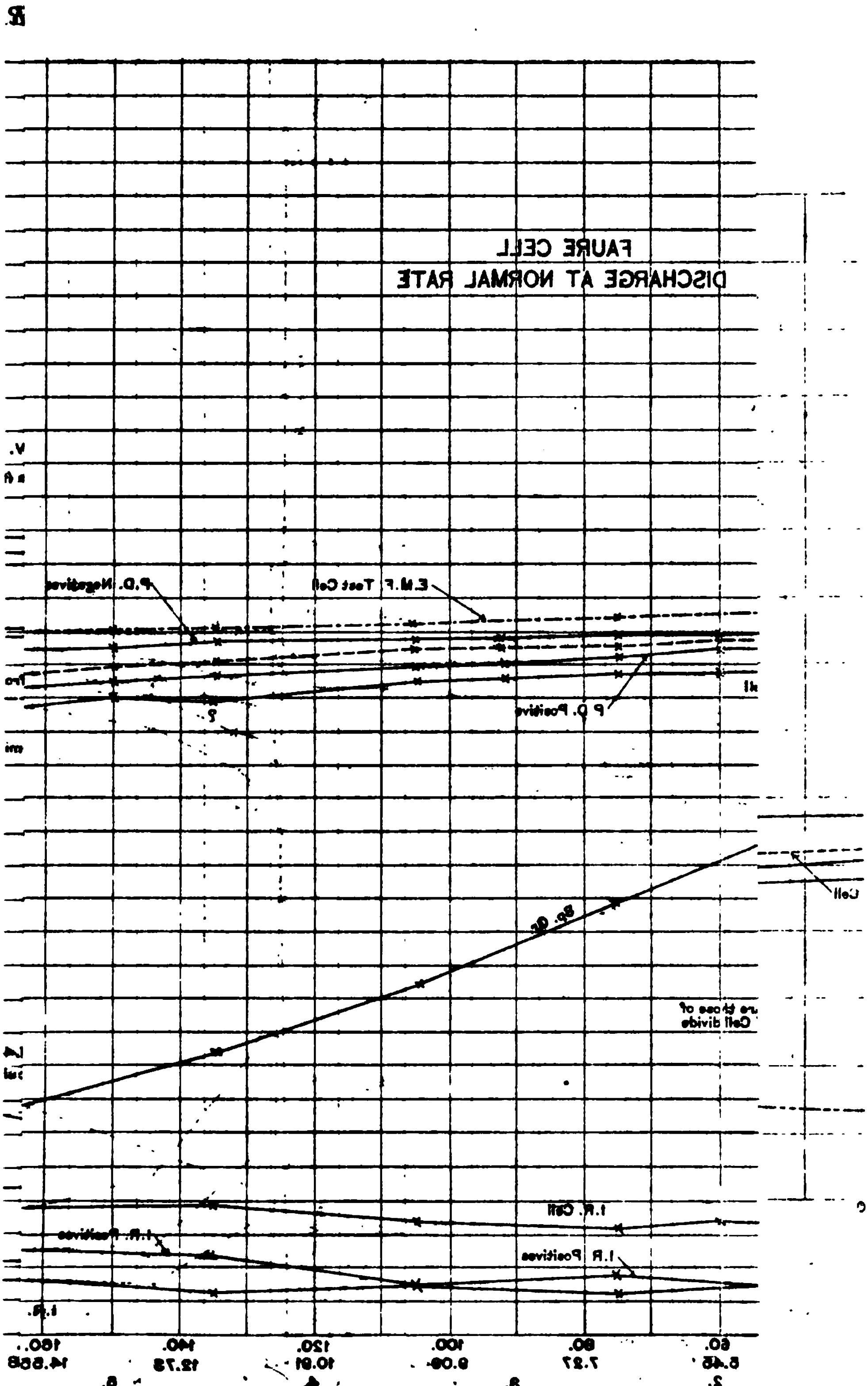
FIG. 1.

commonly called peroxide of lead, but it certainly differs from it both in its ability to generate electromotive force, and in its appearance, and Fitzgerald has pointed out that its composition corresponds to the hydrated peroxide of lead, $H_2Pb_2O_5$. He further intimates that a higher oxide of lead may be present, such as perplumbic acid, $H_2Pb_2O_7$. McLeod has told us how peroxide of hydrogen, ozone, and persulphuric acid are produced. Then there are the two new sulphates of lead and the various compounds of sulphuric acid and water. With this array of chemical products to assist our imagination, the wonderful curves of E. M. F. of a storage battery on charge and discharge become comprehensible. And the fact observed by Gladstone and Tribe that thirty-four per cent. more of oxygen was absorbed by the positive plate than could be accounted for by the production of PbO_2 becomes explicable. It has probably been used in converting $H_2Pb_2O_5$ into $H_2Pb_2O_7$. Their suggestion that it was absorbed by local action between the grid and the peroxide during charge is utterly untenable. There is no such action. And if there were, the grid would not last through a dozen charges.

The conversion of $H_2Pb_2O_5$ into $H_2Pb_2O_7$ would account for the abnormal rise of E. M. F. at the end of charge, and if it be assumed that the $H_2Pb_2O_7$ is not stable, but yields ozone gradually, thus accounting for the odor of a freshly charged positive plate, it would account for the steady fall of E. M. F. on interrupting the charging current. The chemist will easily see the relation between these reactions, and the presence of peroxide of hydrogen and the continual evolution of oxygen from the positive plate, and the fact that a charged cell gradually loses its charge, maintaining for days a higher temperature than the air.

I am sorry that the time allotted me for the preparation of this paper did not admit of the preparation of curves showing the differences in temperature between certain plates of a cell and even between different parts of the same plate. Of course these changes, many of them, are very minute, and they are due to at least two causes, viz., the liberation or combination of sulphuric acid on the one hand, and $C_2 R$ on the other.

In order to study the progressive changes on either plates and to plot them out in a curve, it is merely necessary to choose a substance which produces a measurable electromotive force with the plates, independently of the electromotive force which is being produced by the two plates appertaining to the battery, but this substance must be one which is neither modified by the electrolyte nor by the minute current which it is required to produce for the voltmeter. The last is of more consequence than is generally understood, as a storage battery which has been over-discharged and which has been allowed to recuperate will, even if it be of large size, say of 350 ampere hours capacity, produce a deflection on a Weston voltmeter (of about 300 ohms),


FAURE CELL
DISCHARGE AT NORMAL RATE
P.D. Negative
E.M.F. Test Cell
P.D. Positive
Sp. Gr.
I.R. Cell
I.R. Positive
I.R. Positive
I.R.
160.
140.
120.
100.
80.
60.
14.558
12.73
10.91
9.09
7.27
5.45
6.
4.
3.
2.

CURVE I.

duc
var
of
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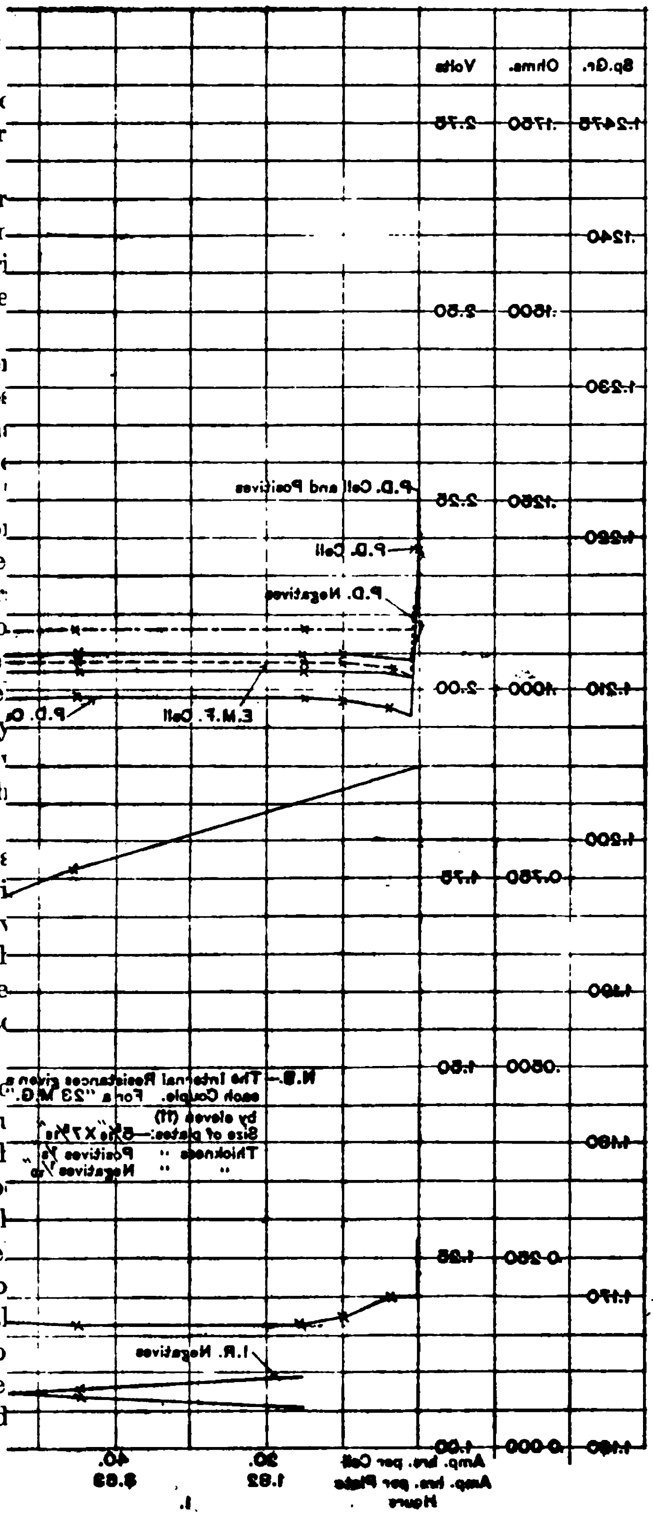
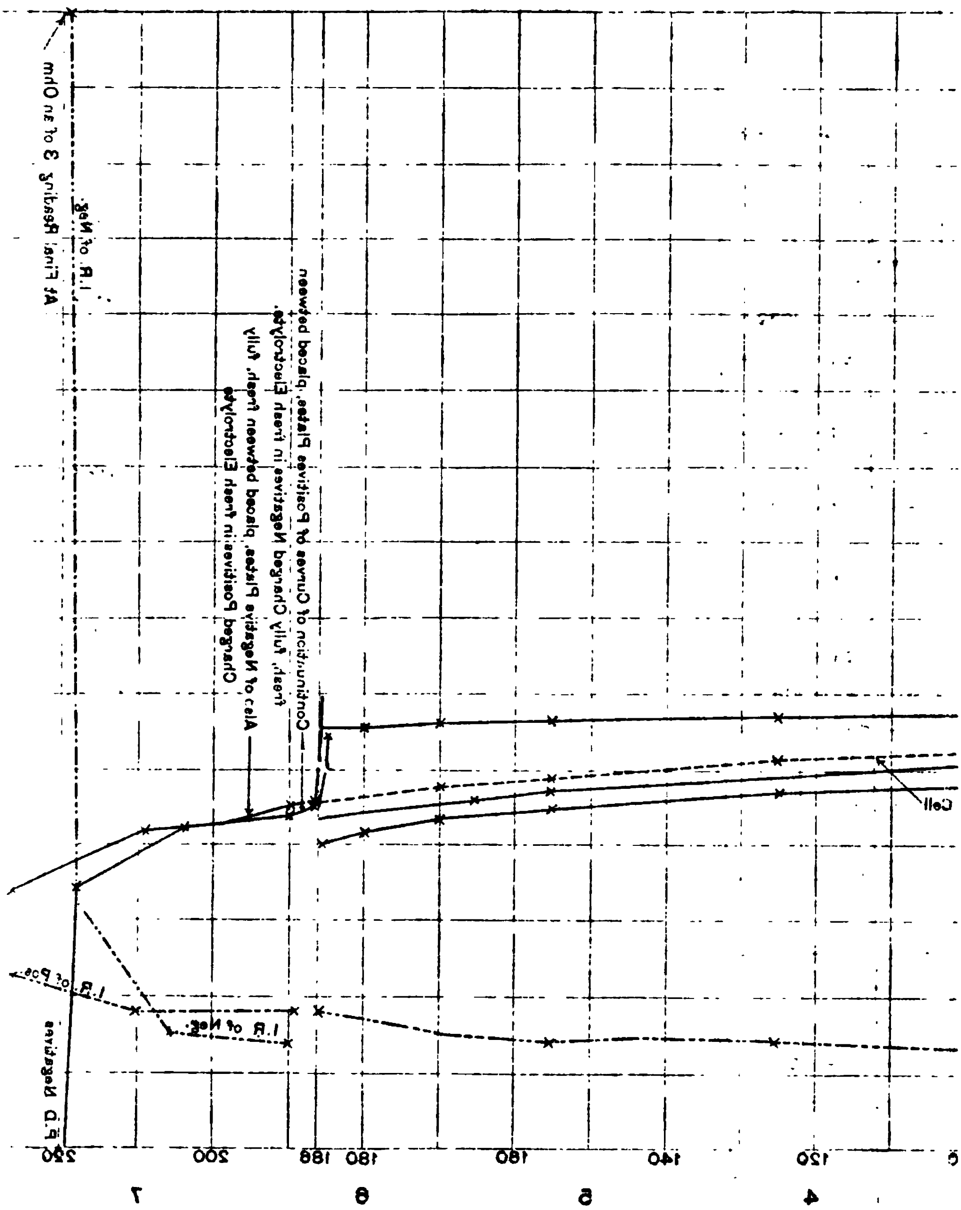
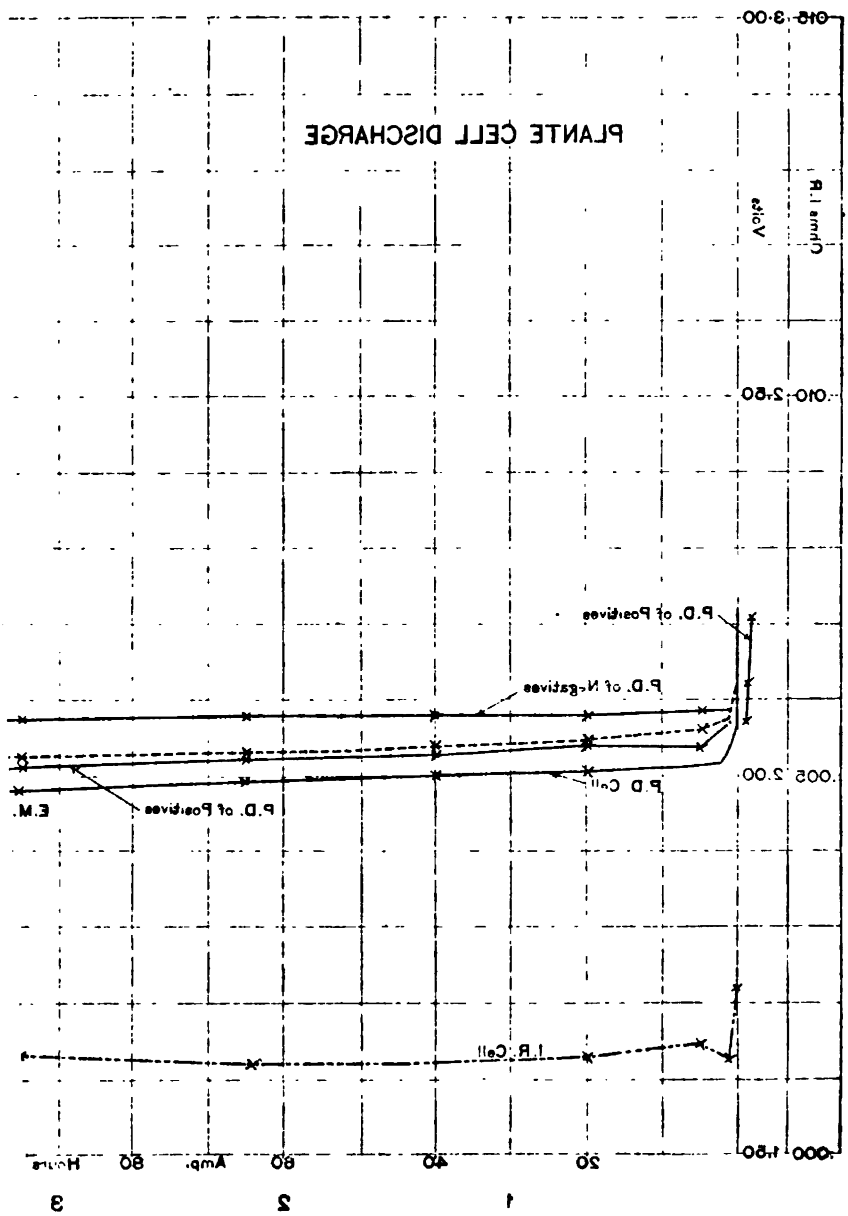



At Final Reading 3 of an Ohm
I.R. of Neg.
Continuation of Curves of Positive Plates, placed between fresh, fully Charged Negatives in fresh Electrolyte.
Also of Negative Plates, placed between fresh, fully Charged Positives in fresh Electrolyte.
Cell
I.R. of Pos.
I.R. of Neg.
P.D. Negatives
220
200
188
180
160
140
120
7
6
5
4

CURVE III.



PLANTE CELL DISCHARGE
Volts
Ohms I.R.
3·00
2·50
2·00
1·50
·015
·010
·005
·000
P.D. of Positives
P.D. of Negatives
P.D. Cell
E.M.
I.R. Cell
20
40
60
80
Amp.
Hours
1
2
3

which at first may be 1 75-100 volts and which will immediately begin to fall with a velocity quite appreciable to the eye. Many substances have been tried for such a test plate, as for example, zinc, carbon, platinum and copper, but nothing seems to be better than a well charged Faure or Planté couple of considerable dimensions. The use of such plates was first published by Mr. Crompton, although they have been used for years in my laboratories, and give concordant results.

It might be desirable, but it is not necessary to ascertain accurately the exact fraction of the total E. M. F which is due to the plate under test. It is important, however, to know the rate of its change of E. M. F. while the cell of which it forms an individual part is discharging or charging.

Referring to the Curve III entitled Planté Cell, it will be observed that the negative plate maintains a nearly horizontal line, until the P. D. of the couple on discharge has fallen to 1.9 volts, while the positive plate maintains a curve almost parallel to that of the cell, showing that the characteristic curve on discharge of a storage battery with plates of nearly equal capacity is due mainly to the reaction in the positive plate.

At the end of the charge, however, the characteristic curve of E. M. F. is due to the negative, the rapid rise of its curve being nearly parallel to that of the battery, while the rise of potential of the positive is nearly a straight line, which, however, is gradually rising.

It should be noted that this curve is the true E. M. F. and is not dependent on the internal resistance of the cell, which reaches the maximum shortly after the 14th hour in this case, and remains quite constant during the remaining seven hours of the charge. But while the E. M. F. is not dependent on the internal resistance, the sudden changes in each curve are usually simultaneous, indicating a common cause.

It may be considered proven, therefore, that in a storage battery with plates of nearly equal capacity, the changes in the positive plate determine the characteristic curves of potential on discharge, and that the changes in the negative plate determine the characteristic curves of potential at the end of the charge.

A curious feature in charge is the intersecting of the curves of the positive and negative plate at several points. At the beginning of charge (Curve III) the two plates are at nearly the same potential,—the positive potential rises rapidly until it reaches

2.22 volts, when it begins to rise in a straight line strictly proportional to time. The negative on the other hand rises gradually until it reaches 2.18 volts, then it rises rapidly, intersecting the positive curve in 14½ hours and at 2.24 volts, and continuing to rise until it reaches 2.40 volts in 16¾ hours, whereafter it remains constant for the five hours which the positive requires to reach the same potential.

To trace the history of a negative during discharge. (*vide* Curve I) it fell in five minutes .135 volts, in the next six hours. .035 volts, in the following hour .250 volts and in 15 minutes more, 1.500 volts, showing less capacity than the positive plate. In half an hour after stopping discharge it recovered to 1.9 volts—jumping instantly on charge to 2.08 volts, and in 12 hours of charge arose only .05 volt. Of course the rate will not modify the general characteristics of the curve.

Within the working limits of charge or discharge, the negative did not vary over 2 per cent. of potential difference.

The history of the positive in this curve is not so satisfactory. Falling rapidly to 2.04 in ten minutes it fell thereafter in a gradual curve .115 volt during the normal time of discharge, a fluctuation of about 6 per cent. Ultimately the positive shows more capacity than the negative.

The total fluctuation of the cell during the six hours after the first five minutes was 6 per cent. in discharge down to 1.9 volts. This was the fluctuation of the P. D., that is to say it was the fluctuation which would be noticeable to the engineer in practical service and included all changes due to internal resistance.

The usual construction of storage batteries—all the plates of one name being permanently fastened to one heavy conductor—has heretofore interfered with the study of the individual plates of a cell. To the end, therefore, of facilitating this investigation, the author constructed some cells with independent plates, connecting all those of one name to a common mercury trough, either directly or through intermediate resistances as in Figure 1.

The resistances were made such, that one ampere would give ten divisions on a dead beat galvanometer, and as each division could be divided by the eye into tenths, the current passing could be read within one-hundredth of an ampere. An independent ammeter and an adjustable mercury resistance were inserted and the total current kept constant. The cells tested in this way were of three types. The pasted cell of the Accumulator company, the chloride cell and the Planté cell. The object in testing a chloride cell was to ascertain whether the exceedingly good contact (produced by casting the grid around the active material while the latter was hard, and allowing the molten metal to contract upon it) would operate to lessen the somewhat remarkable variations in the behavior of all storage battery plates, but this was not the case. Neither was there any marked difference in the behavior of the Planté cell.

The following characteristic readings were obtained from the three types of cells at their normal discharging rates:—

PLANTE CELL.

	AT START.		1 HOUR AFTER START.		2 HOURS AFTER START.		3 HOURS AFTER START.		6 HOURS AFTER START.	
	Amp.	Volts.	Amp.	Volts.	Amp.	Volts.	Amp.	Volts.	Amp.	Volts.
1	4.73	2.00	4.72	1.995	4.72	1.99	4.70	1.965	4.67	1.90
2	4.72		4.72		4.71		4.71		4.64	
3	4 71		4.71		4.70		4 70		4.72	
4	4.69	1.95	4.71	1.99	4.70	1.985	4.69	1.955	4.67	1.90
5	4.71		4.70		4.69		4.63		4.42	1.85
0	4.73		4.74		4.72		4.79		4.91	
7	4.75	2.005	4.77	2.003	4.74	1.995	4.81	1.985	4 96	1.956

CHLORIDE CELL.

Positive.	AT START.		1 HOUR AFTER START.		3 HOURS AFTER START.		4 HOURS. AFTER START.		6 HOURS AFTER START.		8 HOURS AFTER START.		Negative.
	Amp.	Volts.	Amp.	Volts.	Amp.	Volts.	Amp.	Volts.	Amp.	Volts.	Amp.	Volts.	
1	5.30	2.03 2.065	5.75	2.01 2.04	5.75	1.94 2.00	5.80	1.90 1.965	5.80	1.735 1.89	5.60	1.80 1.78	1 2
2	5.43	2.035 2.045	5.40	2.015 2.03	5.45	1.965 1.98	5.40	1.935 1.94	5.45	1,86 1.88	5.80	1.80 1.81	2 3
3	5.40	2.035 2.035	5.35	2.02 2.02	5.35	1.975 1.96	5.30	1.94 1.95	5.35	1.865 1.87	5.80	1.80 1.80	3 4
4	5.43	2.045 2.03	5.40	2.03 2.015	5.40	1.985 1.965	5.40	1.955 1.93	5.40	1.88 1.86	5.85	1.80 1.64	4 5
5	5.70	2.055 2.02	5.65	1.99 1.925	5.70	1.99 1.925	5.70	1.96 1.87	5.55	1.86 1.66	3.60	1.56 1.26	5 6

PASTED CELL.

AT START.		8 HOURS AFTER START.	
Amperes.	Volts.	Amperes.	Volts.
6.00	2.035	6.00	1.77
5.93	2.025	6.15	1.79
5.90	2.024	6.125	1.705
5.89	2.021	6.05	1.78
5.92	2.025	6.025	1.78
5.92	2.02	5.85	1.74
5.95	2.022	6.10	1.80

The curious phenomenon was presented of variations of current amounting to 30 per cent. in plates manufactured rigorously alike, kept in parallel and subjected to like treatment during their previous life. In cases where the discharge is pushed below 1.8 volts I have observed even more serious differences amounting to a variation of about 225 per cent., one reading being 2.8 amperes, another 6.4 amperes for perfectly good new plates carefully treated.

And perhaps a still more curious fact was the differences of E. M. F. of the plates in the same cell discharging through equal resistances and connected finally in parallel to the same circuit—the E. M. F. ranging from 1.60 volts to 1.85 volts for neighboring plates in parallel in the same cell, at the same time. On interrupting the circuit at the end of the discharge of a cell, a considerable flow of current as might be expected passed from one plate into another and it was hours before the batteries reached equilibrium after the external circuit was open.

The discharge of one positive into its neighbor, was a rather unexpected result. It had been thought that if one plate had less capacity than its neighbor it would simply stop discharging at a certain point, but that its E. M. F. would always be equal to that of its neighbors and that therefore, no current would flow.

The explanation of the phenomenon appears to be that the deficient plate keeps on discharging at a lower rate than the perfect plate, and finally reaches a much lower point of discharge. On interrupting the current, the plate which has not been discharged so far, rapidly recovers a higher voltage than its neighbor and, therefore, discharges into it.

This effect must also take place in the different parts of any one plate, and may be a cause for the formation of peroxide on the surface of a negative plate after a discharge, a phenomenon which I have never noticed, but which has been remarked by too many observers to be ignored.

The two outside negatives give more than their proportional amount of current on discharge, the current being actually less than on the other plates, the potential remains higher, and the discharge is therefore a little greater. This, in turn affects the positives next to them and these positives are usually the first to disintegrate in a carefully used cell. This fact was most noticeable in batteries used on the Eckington and Soldiers' Home Railway at Washington, where out of 45,000 positive plates, there did not occur a single instance of buckling, and yet the outside positives always showed greater disintegration than the others.

The tendency of one part of a plate or one portion of peroxide to discharge faster or slower than its neighbor, is one of the reasons which induced us to adopt the equi-potential methods of connecting up the several plates of the storage battery. It is to be noted that these phenomena occur to a serious extent only when the batteries are discharged below 1.95 volts of potential difference per cell.

The above tables afford the most complete proof of the irregularity of the chemical actions which produce the electromotive force of the battery. Now one plate is giving current, now another. And most remarkable of all, the different sides of the same plate exhibit differences of potential at their terminals which I can only attribute to differences of internal resistance, both in the electrolyte within the plates and in the porous active material itself.

The active material during charge and discharge is undergoing chemical change irregularly, not merely in the different plates, but in the different sides of the same plates, and as the active material is made up of large numbers of little pellets isolated from one another by the grid, the conclusion seems inevitable that one side of a given pellet is active to a different degree from the other side. It is not necessary to conceive that the E. M. F. generated on one side of a pellet is as different from the E. M. F. of the other side as would be indicated by the potential difference at the plate terminals. The more reasonable conception appears to be that the internal resistance of one side of a pellet is sometimes greater, sometimes less than that of the other side, varying with the unequal chemical action. But I cannot escape the conclusion that there must be some real difference of potential and consequently local action—not merely between the parts of the grid, but between opposite sides of the same pellet or paste. This would account for the fact observed by Ayrton that a working cell is always above the temperature of the air—even when its own temperature is falling in discharge.

In 1890 Prof. Ayrton in his most valuable contributions to storage battery literature independently noticed the fall of temperature in a discharging cell, and published a curve which is reproduced in Fig. 2.

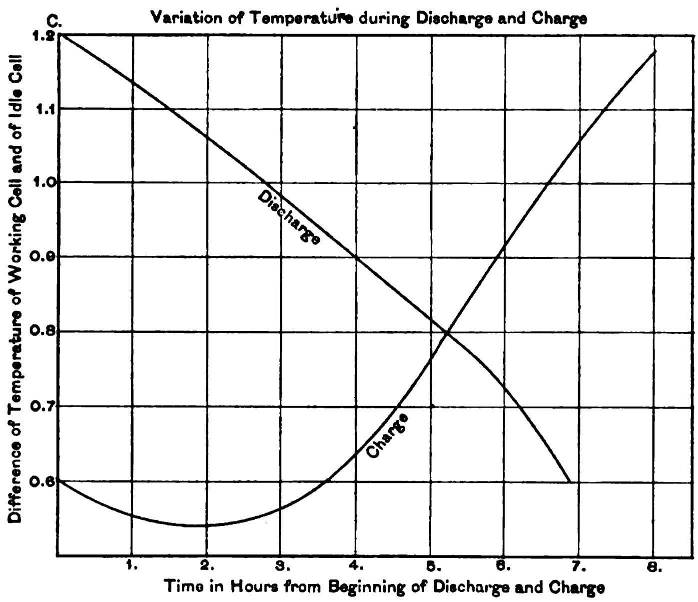


FIG. 2.

The explanation for at least a part of this phenomenon is simple. If, as we all know, the addition of sulphuric acid to water raises the temperature, it is natural to infer that the removal of the acid from the water will lower the temperature. The former occurs on charge, the latter on discharge.

This fall of temperature on discharge was first brought to my attention in 1887, when some of the cells of the Julien cars on Fourth Avenue, New York City, were reported to heat unduly, and the explanation offered was, that they were called on for increased currents on certain grades with unfavorable conditions of the tracks. As some of the cells did not heat I was not satisfied with the explanation, especially as the resistance of the cells, about .002 ohms, would only account for about 1¼ heat units even on 100 amperes discharge which the cells produced at moments. I then investigated the question and found that the temperature actually fell even with heavy current, when the cells were in good order.

The potential of a cell is partly due to the degree of charge of the positive, and partly to that of the negative, and partly to the electrolyte. If a negative plate, taken from a fully charged cell indicating say 2.65 volts on the normal charging current, is coupled with a positive from a partly discharged cell, indicating say 1.9 volts on its normal discharge current, the E. M. F. of the combination will lie between the two. If the couple be removed to a stronger or weaker electrolyte, the E. M. F. will rise or fall accordingly. So that a measurement of the P. D. at the terminals of the cell is not an infallible indication of its condition of charge. If both plates are equally charged, the indication is most useful, but when as frequently happens in actual service, one plate is further discharged than the other the P. D. is deceptive.

The variations of specific gravity of the electrolyte are practically proportional to the ampere hours of useful charge or discharge—barring local action, short-circuiting, change of temperature, and a gradual sulphating of discharged positives when idle.

The variations of the internal resistance of a cell affords a valuable indication of the condition of its active material, and therefore of its degree of charge. These variations have been attributed to the varying porosity of the active material—the electrolyte becoming more and more excluded as the pores become clogged. But this explanation does not account for the odd but invariable nature of the characteristic curves of the internal resistance. If this were the true explanation, why does the curve fall rapidly in the early part of the discharge instead of rising? Why should it remain constant for the greater part of the useful discharge? Why should it suddenly rise to a great degree, and then fall in the middle of a prolonged charge? Why is the internal resistance less, instead of more, on a higher rate of discharge? These facts are not easily reconcilable with the clogging theory, and our knowledge of the chemical reaction is not sufficient as yet to afford a convincing explanation.

Experience shows that it adds greatly to the life of a cell, and brings other advantages to stop the charge at between 2.3 and 2.45 volts, and to stop the discharge at 1.90 or 1.95 volts, while a comparison of the accompanying curves of a cell will show that these potentials are reached shortly before, or during a sudden rise of the internal resistance.

A very curious misapprehension prevails among some battery people (in spite of the airing the subject has again and again received) to the effect that the negative plate has more capacity than the positive. If a cell be discharged at a high rate to 1.85 volts for example, the positive plate will show a much lower potential than the negative, and might therefore be deemed to be more discharged. If then the negative element be transferred to a freshly charged positive element, the negative will usually show a considerable additional capacity and this has been cited as a proof of the erroneous statement. But if the positive be similarly treated, it usually shows considerably greater capacity than the negative. This is especially the case with the Faure cell in Curve I*a* in which the positive actually has 25 per cent. more volume of active material than the negative, and shows 45 per cent. more capacity; and is even so on a Planté negative which has as much active material as the positive, and yet the latter has nearly 10 per cent. more capacity.

The positive plate is usually made of greater capacity than the negative in order to prevent it from ever becoming discharged, to allow for its gradual loss of active material, but there are grounds for doubting the propriety of so doing. A negative plate ought never to be discharged so far as to drop after the first ten minutes, more than 1 or 2 per cent. in voltage, yet the temptation to do so with 45 per cent. excess of capacity in the positive is very great—with the result of causing shrinkage of the negative paste and a serious loss of capacity in the cell.

There is another action which goes on in storage batteries and presumably in other forms of electrolyte chemistry, which I have not seen explained, nor in fact described.

When the chemical reactions in an electrolytic cell are simple, as for example, decomposing sulphate of copper or sulphate of zinc, the amount of action is proportional to the ampere hours. But when the possible chemical reactions are complex as in a storage battery, the changes are governed by the potential difference as well as by the current, probably because one of the components requires a different electromotive force to break it up from that required by another. A curious result ensues. Perhaps the current governs the amount of chemical action and the potential the kind. Our factory department reports that if a plate pasted with red oxide of lead is opposed to a plain lead grid in a "forming" bath, the formation of peroxide proceeds evenly and uniformly in the well known way. If it be opposed to a pasted negative the same result follows. But if it be opposed by another positive plate, a different action ensues, and the unformed plate cannot be properly converted to peroxide until the other positive has been completely reversed, and converted into a negative, thus raising the potential difference of the cell. In the Curve II it will be noted that at the time the potential of the piles suddenly rises, due to the sudden increase of the potential of the negative plate. there is at the same time a sudden increase of the internal resistance of that plate. This is, however, accompanied by a sudden and apparently sympathetic perturbation of the internal resistance of the positive plate, showing that some chemical change occurred in it, quite different from the usual action. This perturbation ceased as soon as the negative potential ceased to rise.

The purple color which is frequently observed in the vicinity of the peroxide plate has been variously attributed to the presence of gold, iron, manganese, etc. But as the phenomenon is observed anywhere and everywhere on the face of the globe, now in one cell, now in another, it seems more likely to be some unusual form of lead. The persulphide of lead is purple, and it may even be that the unstable elusive per-plumbic acid—if it really exists at all—is the source of the evanescent but beautiful tint. A freshly charged plate has a purplish slate color, very different from one that has been idle for a long while.

There are two other phenomena to which I will refer briefly in the hope that some members may be able to throw some light on their cause, viz: The sudden spontaneous discharge of a fully charged positive and the tendency to buckle away from the light, *i. e.* with a concave face toward the light. Wherever the tendency to buckle is great, as in treating Planté plates in some of the more rapid methods, I have observed that when a single positive is suspended freely a great distance from a negative in the center of a large jar, the buckling is invariably away from sun light. The plate may be turned around, it may be moved quite close to one negative or the other, yet the buckling repeats itself invariably from the light. The plate may be straightened as you will, it may be left with a slight curve in the other direction, yet neither the natural differences of tension in the two sides of the plate, nor the increase or diminution of electrolytic action on either side has any influence in buckling compared, apparently with the action of direct sunlight.

It will be remembered that Prof. Ayrton in one of his valuable contributions to storage battery literature, commented on the theory of Mr. Crompton regarding the effect of light on the sulphating of the negative plate, and detailed an interesting experiment which although not conclusive, tended rather to confute this theory. Sir David Salomons remarked upon the effect of light on the glass cells causing them, he said, to crack. Altogether it would seem that storage batteries are devices which engineers would do well to keep in the dark.

The automatic discharge of the positive plate is a comparatively rare phenomenon. In my experience I have secured accurate data in only three instances. I am under the impression that Sir David Salomons, when by his courtesy I was enabled to examine his splendid private plant in 1886, mentioned this phenomenon, but I do not find it described in his book, and as it may not have recurred in his experience he may have felt some hesitancy in publishing it.

On one occasion a cell was reversed experimentally, and the reversing charge continued until 470 ampere hours or three times the normal capacity of the cell had been passed through it. The potential attained 2.41 volts on a low charging rate. The positive had the healthy dark color of electrolytic peroxide of lead and was freely gassing, showing that it was as fully charged as possible. The charging current was interrupted and the E. M. F. fell gradually to 2.12 volts in a perfectly normal way. In 35 minutes a curious seething sound attracted my attention to the battery, and I found it in a state of violent, almost explosive ebullition. The gas given off was pure oxygen in immense quantities. The temperature at the bottom of the cell had risen from 68° F. to 85° F. and at the top of the cell it reached 109° F. Fifteen minutes later the cell was quiescent, the voltage had fallen to .24 and the specific gravity from 1.157 to 1.123,—the lowest point which it had reached 470 ampere hours before. Two or three days later the E. M. F. was zero.

During the progress of the phenomenon and afterwards, the cell was examined carefully with the most minute care for any defect, short-circuit, or other irregularity and it was found to be in perfect order. A further charge of 876 ampere hours or about six times the normal capacity was given at a normal rate. On this charge as well as on the first reversal charge another curious effect was observed—the voltage rose in 15 minutes from zero to 0.18, in one minute more it jumped to 2.63. It then gradually fell to 2.01 in about 7 hours and thereafter gradually rose to 2.48. The discharge was normal to 148 ampere hours and presented a peculiarity of a fully charged cell, that is to say, the E. M. F. fell in two minutes from 2.05 on 30 amperes to 1.935 and then rose in a few minutes more to 1.94 volts, and then gradually fell the usual way.

Here, then,is the story of a complete automatic discharge of positive plates from the beginning of the preceding charge to the end of the following normal discharge. Was it because the peroxide formed only to a certain depth in a spongy lead and so densely as to exclude the electrolytes for a time, so that when the liquid at last penetrated the spongy lead a violent local action ensued? This would account for the heat, and for the fall of potential and of specific gravity, but would it account for the liberation of immense volumes of oxygen? I have frequently noticed the presence of considerable quantities of peroxide of hydrogen in electrolytes. Gladstone and Tribe have also remarked this substance, but intimate that they found it in minute quantities only. In some cases, however, I have observed it in very considerable amounts. Is it possible that this substance was present and exercised its well known property of liberating one atom of oxygen from peroxide of lead,and another atom of its own oxygen at the same time? This would account for all the phenomena, but what accounts for the peroxide of hydrogen in such an enormous amount, and why does it not always discharge oxygen from the peroxide? And finally is it possible that the continuous evolution of oxygen on the positive plate while the cell is idle is sometimes due to the presence of peroxide of hydrogen in minute amounts, and not merely to what has been called local action. I trust some of the gentlemen present will enlighten us on this point.

Perhaps the most striking peculiarity about the modern storage battery is the diversity of opinion among professional electricians, as to its utility and commercial value. Men of the highest rank as electricians and engineers, are ranged on either side of the question. Men of affairs who have put them to a commercial test exhibit a like divergence of views. Men who have tried the storage battery for a year or two, have written about it in the most flattering terms, and have discarded it later. Men who have used accumulators ever since their introduction when they were much less efficient machines than now, continue to use them, and would under no circumstances be induced to part with them. Unmitigated praise on the one hand, and unmitigated contempt on the other. Broadly stated the European consensus of opinion, both technical and commercial, may be said to be in favor of storage batteries. The American view until now has been mainly the opposite. What is the ground for this wide discrepancy? Why is cautious conservative Europe so far ahead in the race? Why is America a laggard in the running?

The answer is not far to seek. Storage batteries are almost always an economical success abroad, while here they have been too often an economical failure in the past. And the reason is that the Europeans always demand a margin for safety, while the Americans, with less capital and keener competition, are tempted to sail too close to the wind.

A storage battery continually worked to its commercial rating is a commercial failure. A storage battery worked sufficiently within its capacity is invariably a commercial success.

It has been said of Watt that he pursued careful and exhaustive experiments upon the power of horses for all day work, and that he ascertained that the average power which they could maintain for ten hours was 22,000 foot pounds per minute, but in rating his engine he added 50 per cent. and called the horse power 33,000 foot pounds per minute, so as to make his new pumping devices more than satisfactory substitutes for horses. Would that storage battery people had been equally wise.

A battery's discharge should be stopped after its E. M. F. has fallen to 2 volts or at furthest to 1.9, unless it be desirable to draw upon its reserve. It should be understood that a full discharge, that is, to 1.8 volts is working a battery to the danger limit and is unadvisable for the following reasons:

a. Regulation is troublesome.

b. Efficiency is low.

c. Dangerous molecular changes take place (as indicated by changes of internal resistance and changes of electromotive force as well as occasional buckling.)

d. Uneven plates discharge into one another after the circuit is interrupted.

e. The life of the battery is diminished.

The writer has had occasion to watch with the closest scrutiny a considerable number of plants, aggregating perhaps some millions of plates. These batteries were rated like all reputable makes, well within their capacity in ampere hours measured by charging the battery to about 2.65 volts, and then discharging it until the potential difference was 1.8 volts per cell. Some batteries have been spoiled by bad management and neglect, and some by false economy or bad engineering at the very start; by eliminating these, there remain a very large number which were successful or were failures for no apparent reason. The utmost care was exercised at the factory. The constituent materials were analyzed as soon as purchased. Able engineers watched the processes of manufacture. but while the quality of the battery was improved until no flaw, mechanical. electrical or chemical seemed to remain, still an occasional failure occcured in actual practice and the cause eluded our search.

Finally by classifying the failures and successes, the truth dawned upon us. Wherever the battery was exhausted to its full capacity daily, its life did not exceed 500 discharges, but wherever it was worked within two-thirds of its capacity, complaints were unknown.

Exhaustive tests were undertaken and some curious phenomena heretofore unknown to the writer, were disclosed, and after some hesitation he concluded that they might be of interest to this society.

It is natural to ask why the rating should not be changed so that the owner of the battery would not be tempted to work it to the danger limit. It ought to be done, but in these days of close commercial figuring it would be difficult to sell a battery which appeared *cæteris paribus*, to cost 50 per cent. more than its competitors. However, the present rating is strictly accurate and has the sanction of custom the world over. It is only necessary for the engineer to remember to add 50 per cent. of the capacity as a factor of safety to his maximum load, just as he allows several hundred per cent. in calculating the strength of a bridge, or an axle.

Discussion.

Dr. Louis Duncan:—In a paper which I had the pleasure of reading before the Institute some years ago, I described some experiments on some of the points brought out by Mr. Griscom, and I would like to say a few words about them. In the first place, with respect to the local action taking place in the plates, Mr. Weigand and myself made some investigation on this point, and found that the local action between parts of the same plate was very considerable under certain conditions. We found that when the cell had been very heavily discharged, that the chemical condition of different parts of a plug of active material was different, and a local action took place, which tended to make the plug uniform in its constitution, and this accounted for a part of the increased loss of energy when the discharge rate in a battery was high. Again I see that Mr. Griscom states that Professor Ayrton discovered the cooling of a storage cell on its being discharged. Mr. Griscom could not have read Professor Ayrton's paper carefully, or he would have noticed that Professor Ayrton himself attributes this discovery to myself and Mr. Weigand. We tried to explain the cooling effect by the taking of sulphuric acid from the solution, but this did not account for all of the cooling, and there must be some other cause for it. There is a considerable amount of energy given to a cell which is never given back again, but which results in the formation of compounds which are not reversed on discharge.

I see that Mr. Griscom states that the theory held by some people that the decrease of the porosity of the plugs changes the resistance of the cell, is untenable. In the paper by Mr. Weigand and myself to which I have referred, we made some experiments on the porosity of plugs both when completely charged, and when discharged, and we found that they were much more porous when charged. We wanted to find why it is that a high rate of discharge is injurious to the battery, especially if it has been partly discharged, and also why the electromotive force of a cell drops faster when the battery is partly discharged than when it is fully charged. We found that when the discharge rate is rapid, acid is taken from the solution inside of the plug, thus, of course weakening the solution, which only gains acid again by diffusion from the outside. The diffusion decreases greatly as the battery becomes discharged, and when a certain strength of current is taken from it, the acid in the plug becomes greatly impoverished, and a phenomenon occurs which is mentioned by Gladstone and Tribe in their work on the storage battery. They found that when the solution reached a certain dilution the chemical action on the lead plates changed, and a different compound than peroxide of lead was formed, the plate being rapidly corroded. We came to the conclusion then, from our work, that a large part of the fall of electromotive force, and the deterioration of batteries at high discharge rates was due to the weakening of the acid in the plug, and from our experiments on the rate of diffusion in the plug, we saw that both of these effects would be exaggerated as the battery was discharged, and the rate of diffusion was lowered. As the chemical action when the battery is partly run down takes place on the inside of the plug, the resistance will naturally be increased both by the greater distance from the support plate, and the decreased conductivity of the material through which the current must flow to the support.

There is one thing that seems to be particularly interesting, and that is the fact that plates in the same cell, after the discharge has taken place, will give a current between one another. Although the fact that local action takes place in the plate, has been observed before, yet I do not know of any experiments that have been made on the loss of energy due to currents between plates of the same name in the same cell.

There is another very interesting point in the paper, and that is the wonderful phenomenon of automatic discharge described by Mr. Griscom. I have never seen it take place, and I am very certain I cannot explain it.

Mr. Griscom:—I would like to correct two misconceptions on the part of Dr. Duncan. I did not say that Ayrton whose unrivalled researches I have not only read but studied, was the first discoverer of the cooling effect of discharge, but that he independently discovered it and published the first curve showing the history of cooling on discharge. Nor did I mean to convey the impression that the clogging action of discharge had no effect whatever on the resistance of the cell, but that it was not the only influence at work. For the resistance varies in some parts of the charge and discharge in exactly the opposite way from that which would result from the clogging theory.

As to the irreversible electrolytic actions remarked by Drs. Duncan and Weigand, I can only say that I have heard of such actions very often since the Faure discoveries were published, but if the word "irreversible" be used in its absolute sense I have failed to find them—at least during the proper and legitimate use of storage batteries.

I entirely concur with Dr. Duncan regarding the importance of the researches made by himself and Dr. Weigand on the varying diffusivity of acid in the paste. Such researches give precision to our thoughts, and make us feel that some of our hypotheses, at least, are builded upon the rock.

[COMMUNICATED AFTER ADJOURNMENT BY MR. TOWNSEND WOLCOTT.]

Mr. Griscom's paper is a valuable contribution to the literature of the storage battery. In my opinion it is the best storage battery paper ever read before the INSTITUTE. In the paper Mr. Griscom offers explanations for a number of phenomena before unaccounted for, but he also presents a large number of phenomena for which he asks explanations from the other members. I am afraid that the desired information is not forthcoming. I say this because the theory of the storage battery is full of anomalies such as the following:

Take a strip of ordinary sheet lead which has been exposed to the air and become somewhat tarnished, cut it in two and place the halves in the ordinary battery acid and connect with a low reading voltmeter. In the majority of cases there will be a small E. M. F. shown, due to the minute differences in the oxidation (tarnish) of the two halves of the same strip. Now by carefully scraping the positive piece, the voltmeter may be brought to zero, after which a little more scraping will produce a deflection in the opposite direction.

This sensitiveness would seem to indicate that there was an exceedingly great propensity to local action in the storage battery, and it would also seem that a lead grid filled with peroxide would constitute a short-circuited couple with an E. M. F. of two volts or thereabouts and that when placed in the acid it would completely discharge itself in a short time.

That is not the case, however, as every one knows. In fact, if we take two perfectly clean pieces of lead which give no deflection on the voltmeter, and coat one with peroxide, even on one side the couple will give a considerable voltage, although it may be below two volts. That is to say, there is, apparently at least, no E. M. F. between the peroxide and the lead with which it is in direct contact, while between the peroxide and the lead with which it is connected through the voltmeter there is a very decided E. M. F.

The explanation of this appears to be that there is a strong tendency of the metallic lead and the peroxide to come to the same potential as soon as current begins to flow between them. There is probably a momentary current on immersing a peroxide grid, but it is probably only momentary, even if the grid remains apparently unoxidized. The fact that the voltmeter in the experiment just mentioned remains deflected for some time with two small strips of lead, would seem to show however, that an appreciable quantity of electricity passed before equilibrium is attained.

I am somewhat surprised at the results obtained by Mr. Griscom, in connection with the discharge of one plate into another which was at a low potential. So far as my experience goes, it is rather contradictory to this. I have frequently seen an attempt made to divide the charge between two equal sets of cells, one of which was fully charged, and the other pretty well discharged. The result was always unsatisfactory. The tendency of the E. M. F. of a discharged cell to rise quickly above two volts as soon as recharge begins, would soon reduce the current to a trifling amount, so that the charges would not be even approximately equalized in any reasonable time.

The presence of peroxide of hydrogen in some cells but not in others, is another of the unexplained phenomena which are so frequently met in storage battery work. It may be possible or even easy to construct hypotheses to account for such irregularities, but what is wanted is demonstration and not speculation.

[REPLY TO MR. TOWNSEND WOLCOTT'S COMMUNICATION BY THE AUTHOR.]

Is Mr. Wolcott confident that the experiment which he details proves the existence of E. M. F. due to differences of oxidation? Is it not possible that it is due to occlusion of gases? I have found considerable differences of potential between two plates of chemically clean platinum, one of which had been exposed to the air, after being heated red hot, rather longer than the other.

If two pieces of platinum about one inch square be chemically cleaned, heated red hot and immersed in dilute sulphuric acid, they may be brought to the same potential by a little manipulation. If then one of them be removed again. cleaned, heated red hot and allowed to cool for a few minutes and then immersed in the electrolyte it will at the moment of immersion show a powerful deflection equal to over a milli-ampere. If, now, the same plate be taken out of the electrolyte again, cleaned, heated red hot and plunged while red hot in the electrolyte a very curious phenomenon will be noticed—the first deflection will be negative and immediately afterward the current will reverse and the platinum will resume its positive polarity.

It is hardly credible that the potentials are due to oxidations of the platinum. I am rather inclined to think that it occludes oxygen from the air and possibly it occludes an extremely minute portion of hydrogen when immersed red hot in the solution. It might, perhaps, be worth while for chemists to remember that platinum always absorbs oxygen from the air after being heated red hot and really to an appreciable extent—enough in fact to produce several milli-ampere seconds per square inch under the above circumstances. I have not given careful examination to the behavior of lead under these circumstances, but I must confess that I have not found so great deflections while using lead as I obtained with platinum.

I do not think that there is as much local action in a storage battery as has generally been supposed, if by local action is meant an electrolytic effect due to the paste and grid. That local action must ensue with plain lead, coated with peroxide of lead when immersed in sulphuric acid is of course unquestionable, but the amount of action is limited to the oxidation of the metallic lead surface and that oxidation prevents further action to such an extent that positive plates which have been in use for eight years are not oxidized to a greater depth than $\frac{1}{100}$ of an inch, provided the lead was of good quality and not full of minute holes. My own impression is that the loss of energy in a storage battery is due to a different kind of local action, to wit: that which is caused by the actual differences of potential of different parts of the paste and acid. This difference of potential may be due to different densities of acid in the pores of the active material, or it may be due to different degrees of oxidation of the active material, or it may be due to both factors combined.

[COMMUNICATED AFTER ADJOURNMENT BY SIR DAVID SOLOMONS, OF LONDON.]

I have been favored with an advance copy of Mr. Griscom's interesting paper, in which he does me the honor of referring to me in regard to one or two matters. I will, therefore, confine myself to saying a few words on my experience since the time alluded to in the paper.

It is quite true that the glass cells are very apt to crack in the sunlight. At first I thought this must be due to some chemical action upon the glass, but such, it would now appear, is not the case, for empty glass cells crack in the same way. Therefore, I conclude that the cause must be the unequal expansion of the glass, for I was unable to make pots, which had been very carefully annealed, crack in the manner described.

There can be no doubt that the positive plates in a section discharge themselves, if left at rest. There appears to be conclusive evidence upon this point. There are two causes for this: First, by the slight leakage which exists in every installation; and, secondly, a leakage in the cell itself apart from any local action which may take place, in consequence of the materials employed in building up a section, and this circumstance cannot be avoided, for all substances conduct in a greater or less degree. With due care and by the addition of caustic soda or sulphate of soda, I have been able to reduce the slow discharge in a very great degree. In all cases, where the battery is likely to be left at rest for considerable periods, great attention should be given to the materials employed for building up the section. Although this seems to be a common-sense proceeding, it is one often neglected.

[COMMUNICATED AFTER ADJOURNMENT BY MR. FREDERICK RECKENZAUN.]

While I had just missed the presentation of Mr. Griscom's interesting paper, on arriving at the Philadelphia meeting, I have since carefully perused its contents. Besides various striking points and suggestions, I note some features therein which appear to warrant discussion. First in their order are the curves and data giving observations on the P. D of the positive and the negative plates, wherein the individual plates of another cell, which the author terms "test cell," as distinguished from the cell under test, have been used as the basis of measurement. Why Mr. Griscom should have adopted two separate standards, one for each plate under test, is not convincingly explained, but it is apparent, that this selection was not a fortunate one. Owing to the inconstancy of the standard cell (test cell) itself, as shown by its E. M. F. curve, the P. D. curves obtained for the separate plates actually represent the resultant values of two unknown and varying factors and not, as they purport, the specific potential variations of the plates under test. Nor could the latter be definitely deduced by allowing for the deviation, from a straight line, of the E. M. F. curve of the "test cell," because there is no evidence that the individual plate potentials of the latter varied alike. Such being the case, the characteristics obtained, necessarily become somewhat problematical.

In reference to the "continuation" discharge, (Faure cell) to which Mr. Griscom attaches special significance, it may be noted that the negative curve is decidedly odd, and the context rather remarkable for its omissions. The negative P. D. curve preceding the "continuation" discharge, if the compensation above referred to were made, would undoubtedly be nearer the horizontal than shown, and would be far from indicating a sudden, abrupt break to one volt, within the first five minutes of the continuation, and to almost zero in five minutes more; indeed, even taking the curve as it is, which represents the *sum* of the potential losses of both the standard and the plate under test, it forms an angle with the "continuation" curve which it would be idle to anticipate in an uninterrupted continuation of the discharge, and still more so in an interrupted one, as was the case, unless some very untoward accident or interference befell the plate. We are told that for the continuation test, the plates were separated and each opposed to freshly charged plates in fresh electrolyte. We are not told the specific gravity and temperature of the fresh electrolyte, which factors influence to some extent the E. M. F.; we are not told why fresh electrolyte has been used instead of the old one or one like it; nor are we told the period of rest that intervened. However, that their combined effect was considerable, and favorable to the positive plate, is shown by the curve for that plate, in view of which the negative continuation curve appears utterly incredible. Such a state of the negative, one might expect if it had been allowed to discharge, as by a short-circuit, or exposure to air, (followed by a momentary recuperation in the electrolyte) during the intermission. It may also be observed, by the way, that the "enlarged" continuation curves do not coincide with the smaller ones.

The charge curve (Faure cell) cannot lay claim to being illustrative of normal conditions, owing to the abnormal discharge (continuation) immediately preceding the charge, and which, of course, must have left its impress upon the plates, producing a corresponding influence upon the observations following. The electrolyte, too, differs here again, rising above its original specific gravity owing to the acid absorbed from the "fresh" electrolyte in which the discharge "continuation" had been effected. In fact, this gain in the electrolyte invites a comparison with the output of the continuation discharge. It should be approximately proportionate thereto, but it is not. Allowing equal formation of $PbSO_4$ for positive and negative plates per ampere-hour of discharge, the amount of acid absorbed corresponds to one half of 11 + 108 ampere-hours (the respective extra discharge credited to the positive and negative plates) namely, 59.5 ampere-hours. The gain shown by the specific gravity curve, however, corresponds to 87 ampere-hours if the acid temperature had remained constant, but, as the temperature rose during the charge (temperature readings are omitted, only a rise of 8° F. for a certain period being quoted), and even if this had been all the rise occurring between the beginning of the discharge and the end of the charge, the gain in specific gravity would be equivalent to over 100 ampere-hours. The assumption that this difference between 59.5 and over 100 ampere-hours is due to a greater ratio of $PbSO_4$ formation during the accounted-for extra discharge is not admissible, and the shape of the specific gravity curve of the discharge, as far as it goes, (for the extra discharge no specific gravity curve is given) would not in the least support it. Nor would it seem reasonable to suppose that the "fresh" electrolyte was so excessively strong that the portion of it that may have been conveyed by the plates made such a difference. So that this reminds again of the queer behavior credited to the negative plate on the continuation discharge. Altogether the curve tables seem but an imperfect basis for the conclusions built thereon in the context.

In regard to standards for taking the individual plate P. D.'s., it would seem beyond question that the use of a *single* standard, which is either electro-positive or electro-negative to *both* plates under test, would yield curves that bear a definite relation to each other, irrespective of any possible changes in its own potential. It would also facilitate the checking-off of observations, in-as-far as the distances between the two (P. D. positive and P. D. negative) curves obtained must correspond to the independently measureable values of P. D. of the cell; and similiarly with the respective E. M. F. values. Dr. Streintz, of Graz, Austria, in his extensive storage battery investigations (*Zeitschrift fuer Elektrotechnik*, Vienna, Vols. IX and XI.), has used zinc as a standard in observing the potentials of individual plates during discharge, and his results indicate that under proper conditions that metal is very satisfactory. This being the case, it is manifest that it will also lend itself to a variety of other lines of observations upon lead accumulators, including relative capacity tests of individual plates,—even to observations upon a complete reversal and its concomitant characteristics,—yet leaving a fair working margin between itself and the plates throughout all phases. And what is of no small advantage, the zinc standard affords a striking graphical illustration of the characteristics observed.

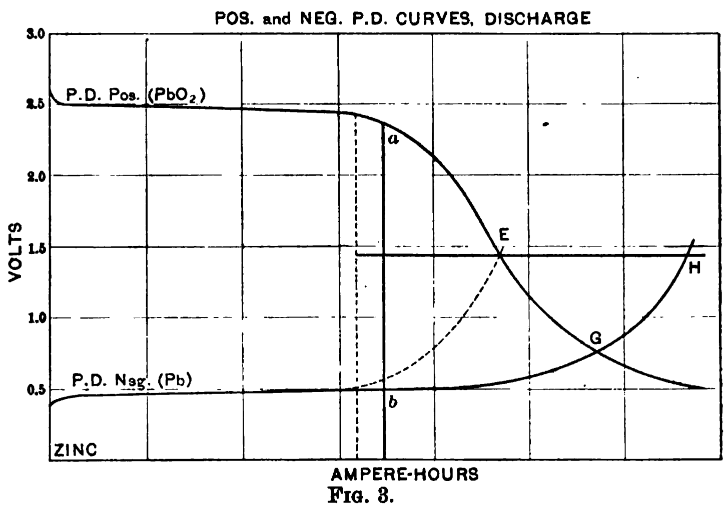


FIG. 3.

In testing the relative capacity of the plates, it is only necessary to discharge the cell in series with a larger cell (or two in an extreme case), in order to assist in overcoming the external resistance (wires, connections, ammeter, etc.) when nearing zero P. D. of the cell, as in the case of an ordinary zero test under a constant current, and beyond that point when the weaker plate or section, owing to its sluggish action, sets up a counter P. D. in the cell. The entire test can be made *without* removing the plates or electrolyte, or in any way interrupting the discharge or interfering with the proper conditions of continuation, and, consequently, without mutilation of the curves which are to tell the tale of the characteristics sought.

The accompanying Fig. 3 furnishes an example of the general shape of curves obtained by this method to and beyond the point of the cell P. D. zero. The upper curve indicates the P. D. values of the positive plate ($Zn—PbO_2$) and the lower curve those of the negative ($Zn—Pb$), zinc being the zero or base line common to both. The two curves approach each other as the discharge proceeds, finally cross (as at G,) and deviate, if the discharge is forced toward reversal. Now, if a practical working limit of cell P. D. is prescribed or chosen (which should be well inside of the steep curvature of the weaker plate,) let special note be taken of the $Zn—PbO_2$ value a, and the $Zn—Pb$ value b, on *passing* that limit, because here really begins the comparison of the relative plate capacities. Then, if the positive and negative plates have the same capacity, zero cell P. D. (crossing of curves) will occur at a point (E) corresponding to $(a + b): 2$, whereas, if either plate has an excess of capacity, the intersection will occur above or below that point; in the example given, the cell zero P. D occurs at G. Thence the discharge is forced along until the P. D. of the stronger plate reaches the value $(a + b): 2$, indicated by the letter H. *The difference between the relative capacities is expressed by the linear dimension E—H.* Upon reducing the capacity of the stronger plate accordingly, the new curve would more or less alter its shape at the assumed cell P. D. limit, and the latter for the same value would come somewhat further inside, as suggested by the dotted lines. The curves (for charge, discharge and complete reversal) obtained by this method will prove instructive in various ways, and their differences may suggest further investigation into structural and other features.

The drop in temperature of a discharging cell, alluded to by Mr. Griscom, is a result dependent upon very low internal resistance and moderate current. Under opposite conditions the opposite result, namely, a rise in temperature, will occur. Mr. Griscom's explanation of the cooling effect may help to explain the heating effect also. There are two opposing functions in discharging a storage battery, which do not bear a fixed relation to each other; the chemical (or cooling) effect being *proportional* to the current (as is also the absorption of sulphuric acid, referred to by Mr. Griscom), whereas the heating effect varies as $C^2 R$. Thus, while the cooling effect may predominate under favorable conditions, upon increasing C, a point will be reached where the two factors balance and beyond which heat will ensue. This balance point, of course varies with different types and sizes of storage batteries. In Prof. Ayrton's temperature tests, alluded to by Mr. Griscom, the cell temperature, though remaining above that of the surrounding air, fell slightly below that of an idle cell, wherein the heating was due to some local action (possibly the same two functions, with their relative magnitude reversed. Dr. Duncan and Mr. Weigand[1] mention

1. TRANSACTIONS vol. vi., p. 217.

having observed a temperature reduction in some cases, and a rise in a number of others, the data on which indicate favorable conditions in the latter direction. My own observations were also of both kinds, and numerous; especially have I noted a marked rise when discharging at such an excessive rate as to produce lively gassing at the plates, which was accompanied by a marked increase in the internal resistance. Again, in cases where a number of cells were confined within a close space affording little or no ventilation, (as in an electric launch, etc.,) even a slight initial rise would have a cumulative effect, to a noticeable extent. These observations were made on cells in good condition.

On page 312 the author refers to "a very curious misapprehension" prevailing "among some battery people" to the effect that the negative plate exceeded the capacity of the positive, and then proceeds to prove the contrary by reference to tests, the shortcomings of which have been pointed out above. As a matter of fact both conclusions are unwarrantably broad. In view of the diverse types that are on the market, it must be observed that the relative capacity of the plates is as the manufacturer chooses to make it. It depends upon structural features, upon the relative size and thickness of the plates, the relative proportion of "active material" contained therein and the quality, condition and distribution of the latter, each of which factors, if varied itself, will produce different results. In some accumulators the positive and negative plates are made exactly alike up to the point of final "formation," and in most of these, when in good working order, the negative plates have a considerable excess in capacity, owing, no doubt, to the spongy lead being capable of more thoroughly undergoing the chemical change than the peroxide. Dr. Frankland (Royal Society, 1890) remarked that "only half as much material seems to be necessary for the negatives as for the positive plates." Messrs. Gladstone and Tribe made similar observations. Prof. Ayrton[1] suggests an explanation why in the E. P. S. (pasted grid) type "it is necessary to employ nearly twice as much lead peroxide as is actually needed for the chemical action." Some manufacturers have as much as ten years ago recognized the difference and accordingly made the positive plates thicker than the negatives, and, although Mr. Griscom doubts the propriety of so doing, his company also follows that practice. I have myself made careful tests of the comparative capacities of plates and materials, extending over some years, and found a considerable difference in the oxides of different makers, their capacities ranging (with grid plates ¼ inch thick), under like conditions, from 1.4 to 2.5 ampere-hours per ounce of red lead in positive plates, and (with similar grids) from 2.75 to 3.25 ampere-hours per ounce of litharge contained in

1. *Inst. Elec. Eng.*, London, 1890.

negative plates, in all these cases putting double their amount of active material opposite them, to ensure maximum output of the samples with the usual working P. D. limit, at a uniform current. Among a variety of tests to determine the relative capacity of plates working together, I have made a series of experiments calculated to yield the most direct results for practical purposes, as follows: I prepared 3 cells, each containing 4 negatives and 3 positives; size $7\frac{3}{4}$ x $9\frac{3}{4}$ inches; thickness, all positives $\frac{1}{4}$ inch; negatives, in one cell $\frac{1}{4}$ inch, second cell $\frac{3}{16}$ inch, third cell $\frac{1}{8}$ inch; $\frac{1}{4}$ inch grids being planed down to the respective thicknesses for the second and third cell. The oxides used were of the best quality available (the litharge being specially manufactured for storage battery purposes.) The cells were worked daily for several weeks, under normal conditions, and the net results, briefly stated, were, that the cell in which the negatives were reduced to one half their thickness and the amount of litharge therein to 38 per cent. of that contained in the $\frac{1}{4}$ inch negatives, yielded an output only 16 per cent. less than cell 1. (The reason for only 38 per cent. and not 50 per cent. of litharge being in the $\frac{1}{8}$ inch plates was in the perforations being tapered.) The object of this experiment was to find how thin a negative would answer for a given positive with the materials used, without undue sacrifice in the capacity of the cell, and the results obtained approximately correspond with theoretical deductions. Generally speaking, the results do not vary exactly with thickness, volume and weight, but many other minor points, as above indicated, necessarily contribute in determining them; the subject is too intricate to be broadly covered by either Mr. Griscom's implied conclusions or the one he undertook to refute.

I concur with Mr. Griscom in his views on the success of storage batteries in Europe, and their lack of success in this country. Other points of comparison suggest themselves, among which are the generally greater day load of central stations in this country, due largely to a more general use of motors, which tends to make the use of storage batteries a somewhat less pressing object, and further, the entire absence, until recently, of large, substantial and durable types of cells suited for central station purposes. While in Europe central station types have been given the utmost attention by manufacturers, with corresponding success, American storage battery manufacturers, on the other hand, have largely endeavored to cope with that alluring subject: storage battery traction—unfortunately with but little success—and neglected to cater to the central station field with types especially adapted to that class of work.

[REPLY TO MR. FREDERICK RECKENZAUN, BY THE AUTHOR.]

Some of the points made by Mr. Reckenzaun show that my paper requires a little further elucidation:—

First. Mr. Reckenzaun says that the text is rather remarkable for its omissions with respect to the curves. I did not pretend to give a complete analysis of the curves, which, by the way, are perfectly normal, but only to use some features of them to illustrate my paper.

Second. He states that I adopted two separate standards, one for each plate under test, and states that owing to the inconstancy of the standard cell, (test cell), the P. D. curves for the separate plates actually represent the resultant values of two unknown and varying factors.

Now this appears to me to show a little tendency—

> " * * * * to divide
> " A hair 'twixt north and north-west side."

The total discharge of the "test cell" during the 300 or 400 readings represented by the curve did not exceed $\frac{1}{2000}$ part of its capacity. Therefore, there was no change appreciable to my instruments in the E. M. F. due to the amount of discharge. Similarly, the current was about $\frac{1}{500}$ of the ordinary discharge rate of the plate, consequently there was no appreciable modification in the readings due to the discharge rate. The total variation of the "test cell" in discharge is, as shown by the curve, about $\frac{1}{70}$ of the total variation of the cell under test, say .03 volt, of which about .01 volt was due to the negative and .02 volt to the positive. And this variation, which Mr. Reckenzaun is pleased to call "inconstancy," is due entirely to the specific gravity and temperature of the electrolyte. The advantage of using this plan is evident. Inasmuch as the temperature and density of the electrolyte affect the "test cell" and the cell under test equally, and inasmuch as the "test cell" and the cell under test are of the same nature, this plan enables us to eliminate the effect of the change of temperature and of average density from those curious changes going on in the plates which it was my endeavor to lay before the INSTITUTE.

Third. Later on in Mr. Reckenzaun's communication he appears to recommend the use of a single standard and refers to the investigations of Dr. Streintz, of Gratz, Austria, who used zinc. I have also used zinc in past years, but abandoned it on account of its solubility in the electrolyte, its tendency to deposit during charge on the negative plate and the false effects due thereto and for other reasons. It is true that by using separate cells and by taking precautions to prevent the intermingling of the electrolytes, it is not impossible to obtain important results with the use of a single standard, but unfortunately it is necessary to make an exhaustive and complete research into the various behaviors of the single standard in the presence of a varying electrolyte and a varying storage-battery plate, before it is possible to be sure that curves with any single standard represent anything but the misleading resultants of unknown and varying factors.

On the other hand, by using for the test cell, well-charged storage-battery plates, which have been allowed to stand until they have reached a stationary E. M. F. you obtain fixed and invariable and scientific results because you obtain the loss of potential which is due purely and simply to the plate under test. It is as though you measured the difference of potential due to the changes in that one plate while the opposing plate had undergone no change from the beginning.

The use of single standards will not do this. They give simply a comparison which is not only uncertain in its nature, as explained above, but which has no relation to the real use of storage batteries.

Fourth. It does not appear whether Mr. Reckenzaun or Dr. Streintz is responsible for the method which he advises for testing the relative capacities of the plates of a storage-battery. The idea of forcing a current for this purpose through a battery which has ceased to be active whether through the exhaustion of one or both of the plates, is, to say the least, open to criticism. If the negative plate, for instance, becomes exhausted first, as usually happens, forcing a charge through it means charging it in the reverse direction so that you have the anomaly of a negative plate charged, or still worse, partially charged as a positive opposed to a positive in the same cell. The experiment, of course, is interesting; in fact, I have no doubt that this very thing frequently occurred in former practice, in the dark ages of storage-batteries, when cells frequently got exhausted by short-circuits and buckling, were straightened and cleaned again, and put in service only half charged. Buckling and short-circuiting are happily things of the past, but Mr. Reckenzaun's method would yield an interesting study of the behavior of a battery when treated in the best manner to ensure its destruction.

Fifth. Mr. Reckenzaun says in reference to the continuation of the discharge of a Faure cell, that, "if the compensation above referred to were made," the curve would undoubtedly be nearer the horizontal than shown. I think there is no reason for this conclusion of Mr. Reckenzaun. The continuation of the discharge was made with fresh plates of the *L* type, the capacity of which was many times in excess of the remaining capacity of the cells under tests,—so much so, in fact, that there was no appreciable falling off in potential on the part of the larger plates, and the curve of the test cell was therefore omitted as superfluous.

Sixth. The fall of temperature on discharge is a difficult thing to explain. Unfortunately for Mr. Reckenzaun's explanation, the cell will sometimes indicate a rise of temperature and sometimes a fall under what appear to be, externally, at least, exactly the same conditions. I am not inclined to believe that such an eminent scientist as Professor Ayrton could have made an error due to local action in an idle cell, as Mr. Reckenzaun insinuates. And in this connection, as against his theory that there is a marked rise of temperature whenever there is "a marked increase in the internal resistance," I will remind him of the tests which were made under his supervision in our Newark laboratory in 1887. In one of these tests, the cell was discharged at the rate of 40 amperes to 1.8 volts. The records show that the temperature of the cell fell .9° F., and at the time when the cell reached 1.8 volts, its temperature was actually .4° below that of the air, and yet, as everyone knows, the internal resistance of the cell under those circumstances was at least double its average internal resistance, and the current remained constant at 40 amperes. This seems to be in direct contradiction of Mr. Reckenzaun's theory.

To be sure, such a test, crudely made in a factory laboratory, has not the weight of a test made by Professor Ayrton, assisted by competent electricians and checked by all the skill and resources of a trained scientist for the express purpose of instructing the scientific world. But when the test conforms to Professor Ayrton's results, it seems entitled to at least as much weight as some other crude tests which differ from these results.

In Professor Ayrton's experiment he found that the heat produced by $C^2 R$ amounted to 3,456 calories, whereas the heat loss due to the cell cooling down amounted in some cases to 12,000 calories—three or four times as much. Furthermore, the heating due to $C^2 R$ in a given cell does not vary directly as the square of the current, because, the internal resistance diminishes as the current increases, and what is still more curious, the E. M. F. itself appears to increase.

Mr. Reckenzaun talks of experiments in which an excessive discharge rate increases the I. R. He must have been using cells of extremely faulty design, or else he must have made his measurements after the cell had exceeded its proper working capacity for the discharge rate, or else he based his statement on guesswork instead of experiment. I have never known of such a case.

Seventh. Mr. Reckenzaun devotes some space to what he calls the "queer behavior" of the negative plate in the continuation of the discharge of the Faure curve and proves that it is "queer" by an argument based upon certain apparent behaviors of the electrolytes as shown by the curves. The facts are not as Mr. Reckenzaun assumes. The relative proportions of electrolyte were not the same in the charge and discharge, and there is consequently no relation between the actual measurements of the specific gravities in charge and discharge. The specific gravity curves are absolutely independent of one another, and if used for purposes outside of my paper should be studied independently.

The "queer behavior" which so impressed him, is what always takes place in the complete discharge of a negative plate of good construction and the fact that some electricians did not know of this "queer behavior" was perhaps a sufficient reason for me to mention it. The fact is only "utterly incredible" to those who are too credulous of outworn theories, and who will not venture to subject them to actual test.

His explanations, that there might have been short-circuits, or unsuitable electrolyte, or undue exposure of the negative to air, display a singular idea of scientific tests. It may be proper to inform him that such infinite precautions hedge about laboratory work of this nature that none of these things could occur. Moreover, the negative, even if exposed to air, does not lose any capacity whatever until it begins to heat; and after it has heated, it does not recuperate to two volts, or more, but behaves like an entirely different material with a much lower electromotive force. I think all the voltage readings are correct within two-tenths of one per cent.; any possible errors in any of the curves are in one direction, so that what I particularly desired to set forth, to wit, the relation between the curves, is accurately shown.

Eighth. Mr. Reckenzaun takes it very hard that I should have said that a very curious misapprehension prevailed among some battery people to the effect that the negative plate exceeded the capacity of the positive, and he then points out alleged shortcomings of the curve upon which he appears to think I based my statement. Of course, I did not rely on any one experiment, but upon many hundreds of tests. If Mr. Reckenzaun wants to attack them by repeating the tests under the same conditions, I shall, of course, have no objection; but I must protest against having my curves demolished by faulty dialectics based on errors of fact.

I am surprised that Mr. Reckenzaun should not have known that the negative plate in all usual and commercial storage-batteries of good design has less capacity than the positive, and that the curve at the end of discharge for both plates is for a considerable space a nearly vertical line,—the loss of a volt in five or ten minutes being of usual occurrence. His method of testing the relative capacities of the plates is roundabout and the results have evidently misled him. The simplest ways are the best. My usual method is to place one plate between two others of several times its capacity and discharge it to exhaustion. In this way its curve is purely its own curve, practically unaffected by anything but the electrolyte, and the insignificant loss of potential of the larger plates. Care is taken to maintain the electrolyte at the proper density. A similar curve is made of the other plate and any changes of proportions which may be desirable are calculated from the curves.

Ninth. The citation which Mr. Reckenzaun attributes to Dr. Frankland in support of what appears to be his theory, to wit: "only half as much material seems to be necessary for the negative as for the positive plate," was made by an anonymous writer in the *Electrical Review*, of London, of August 22d, 1890, in an article referring to Dr. Frankland and not by Dr.

Frankland himself. It would not be astonishing, however, if Dr. Frankland had assumed this to be true, as did many other scientific gentlemen at that time; but experiments of Crompton, Anthony Reckenzaun, Drake and Gorham, which I have confirmed myself, show that it is not true. The original idea obtained currency, I believe, because in the original formation of a battery it took twice as much energy to form a given weight of negative element as it did of positive element, and it was then supposed that on discharge after formation, the chemical processes were reversed; but such is not the fact.

Tenth. His reference to the *Journal of the Institution of Electrical Engineers*, 1890–1891, containing Professor Ayrton's papers, is fortunate, for it contains a refutation of the very theories which he is endeavoring to support by it. In the discussion following one of Professor Ayrton's papers, Mr. Reckenzaun's brother gave a clear and logical explanation of the causes which made it difficult, if not impossible, to furnish a durable negative plate with more capacity than the positive, and told why the positive plate in the E. P. S. form of battery always showed after the cell was discharged, the presence of a large amount of lead peroxide. The reason he gives for the latter effect is that the negative plates become exhausted too soon, in other words had too little capacity, and so left the positive plates only partially exhausted and consequently with a large excess of peroxide. This explanation, I think, is at least partially true, inasmuch as the negative plate in the principal batteries of the world, when new and in good condition always becomes exhausted first.

I ought, perhaps, to add that I referred to commercial batteries of usual types and in good condition, and not to the abnormal types which occasionally make their appearance in the market with excessively large pellets of active material, the centers of which gradually grow inactive.

In this connection, I am sorry that Mr. Reckenzaun was not more specific in alleging an error in the "enlarged" continuation curves. I fail to find it. Both curves were made directly from the notes of the tests.

Eleventh. The experiments which Mr. Reckenzaun performed for us and which he relates in his paper, viz: the ones where he reduced the thickness of the negative plates and obtained 84 per cent. output with 38 per cent. of negative active material, are also explained by Mr. Reckenzaun's brother in the discussion before the Institution of Electrical Engineers three or four years ago; in a word, a greater proportion of the spongy lead was active in the thin plate than in the thick plate.

Twelfth. Mr. Reckenzaun's curve, which he says has the "general shape of curves obtained by" his "method," reached me after writing the above. I am at a loss to tell from his cautious language, whether the curve was purely hypothetical, or whether it was the result of the actual measurements of a roundabout method. In either case, it suits his argument in appearance, and is an admirable proof of the danger of using any but direct methods of ascertaining scientific facts. There is only one safe way of ascertaining the relative capacities of positive and negative plates and that is by measuring them independently. Any other way until checked by a direct method may involve misleading and unknown factors.

Mr. Reckenzaun's curve, (with zinc standard), shows an easy slope for one-fifth of the total discharge to zero on the part of the negative, and for one-third on the part of the positive. Such a thing never occurred in a normal storage battery discharge. During the latter part of complete discharge, the E. M. F. of each plate is always on a mad rush to perdition—measured not in hours, but minutes or seconds. His curve is not representative of the behavior of a well designed cell in normal discharge, nor even of the worst commercial cell which has ever come under my notice. Nevertheless, the curve is of a novel nature, and if not purely hypothetical may open the field in curious and unexplored directions.

I do not recall at present any other point of importance in my paper which Mr. Reckenzaun has attacked. I would like to add, however, that such frank, direct and pointed criticism as he has made, is always useful as tending to give precision to our ideas. Eventually, of course, the truth on whichever side it may be, will prevail.

A paper presented at the Eleventh General Meeting of the American Institute of Electrical Engineers, Philadelphia, May 16th, 1894, President Houston in the Chair and at Chicago, May 23d, 1894, Mr. Bion J. Arnold in the Chair.

DISCRIMINATING LIGHTNING ARRESTERS, AND RECENT PROGRESS IN MEANS FOR PROTECTION AGAINST LIGHTNING.

BY ALEXANDER JAY WURTS.

PART I.—AN EXPERIMENT WITH LIGHTNING ARRESTERS ON A 3,000-VOLT ALTERNATING CURRENT CIRCUIT.

Lightning storms in the West, particularly among the mountains where water power abounds, have up to the present time proved a serious drawback to the successful operation of electric light and power plants. In some instances these plants are, by reason of frequent and violent electric storms, practically inoperative during the afternoons of the summer months, and now that such far reaching progress has been made in the construction of high potential alternating current power transmission apparatus, the subject of protection against lightning has become of vital importance. The ordinary devices which provide automatic circuit interrupting attachments, and which are still used with doubtful success on circuits of low potentials, had signally failed to do the work required of them on these high potential circuits, and it had become evident that something radically new was needed. During the winter of 1892 and 1893 I made a searching investigation of this subject, experimenting with disruptive discharges and various kinds and combinations of apparatus which might promise advantageous results, and since that time have spent nearly six months in the State of Colorado—a land of thunder storms—testing the various forms of apparatus which I had designed as a possible protection against lightning, and have also made a careful study of such phenomena as presented themselves. This work was carried out with the determination to construct, if possible, some form or combination of apparatus which should practically shut out lightning discharges from the vital parts of a system, and thus greatly enhance the possibilities of electric power transmission.

The general requirements of efficient lightning arrester apparatus are: First. To provide discharge circuits which shall operate automatically and repeatedly, and which shall with certainty avoid dynamo short-circuits or interruption of the system;

Fig. 1.

Second. To provide discharge circuits, or so install them that they shall invariably offer a certain path to ground for disruptive discharges in preference to any other part of the system. It follows also from this last, and as a matter of practical experience, that ground discharge circuits should be short and straight, and that ground connections should be of the most approved construction.

The difficulties in the way of meeting the above requirements on circuits of high potential seemed at first somewhat serious, in view of the fact that many of the arresters already used, had not only been instantly destroyed by the short-circuiting which follows each discharge, but, in many instances, the arresters had been entirely ignored by the lightning, the discharges preferring an armature or converter to the spark gaps of the arresters. The correct method for overcoming this latter difficulty is to equip the system with a liberal distribution of line arresters, which, by thus affording frequent opportunities for discharge along the line, will greatly lessen the probabilities, or rather necessity, for

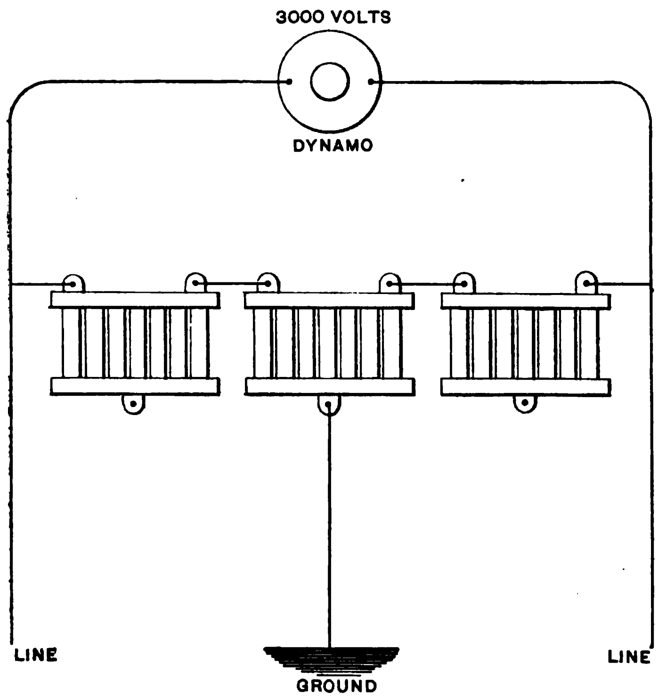


FIG. 2.

discharging in the station. But the country over which most of these power transmission circuits are run is of such a dry, barren and rocky character as to make it practically impossible to secure proper ground connections along the line. Fortunately, however, parts requiring protection in these power transmission plants are confined to the ends of the circuits, so that it became possible to adopt the following general plan, namely: to provide, first, an abundant discharging capacity from the line immediately before entering the station or power-houses; and second, to interpose between this point and the parts to be protected, such a resistance to disruptive discharges as should make it a matter of necessity rather than of choice, for these discharges to pass to earth over the discharge circuits provided for them.

The only apparatus which seemed at all available for this work was the non-arcing metal lightning arrester, and some efficient form of choke coil which might be connected in the circuit between the arresters and the apparatus to be protected. As is well known, the 1,000-volt non-arcing metal lightning arrester,

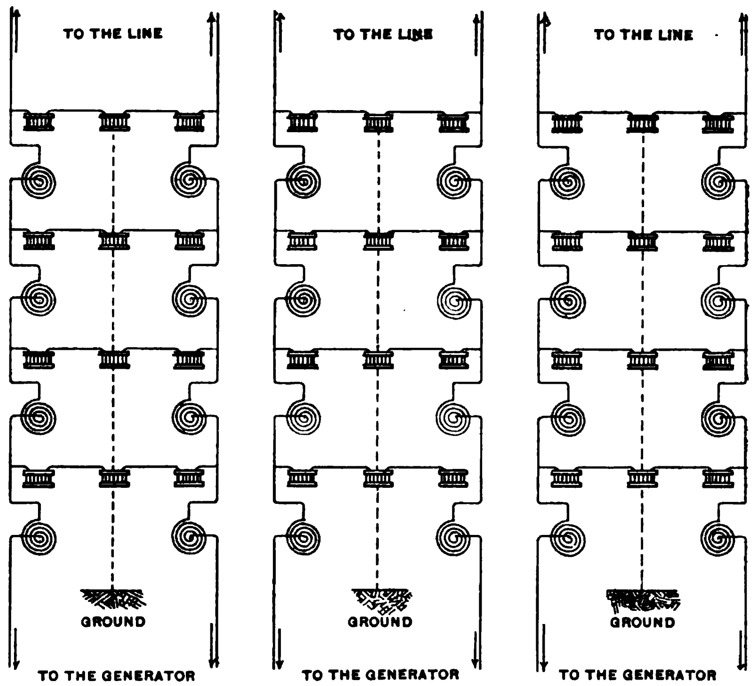


FIG. 8.

which is now almost universally used on alternating current circuits (see Fig. 1), consists essentially of seven non-arcing metal cylinders arranged side by side with 1-64 inch spark gaps intervening. The two outside cylinders are connected to the respective sides of the circuit and the middle cylinder to the ground, thus forming a double pole arrester. For higher voltages, however, the number of cylinders must be increased in order to prevent the dynamo current from following in the path of disruptive discharges. Experiments were, therefore, made to determine the number of non-arcing metal cylinders and spark gaps which would be necessary to interrupt a short-circuit on a 3,000-volt alternator with the potential raised to 3,300 volts. Nineteen cylinders, or 18 gaps, were found to offer ample margin, and the breaking down E. M. F. on half this number of gaps, which would intervene between line and ground, was found to be about 70 per cent. of the E. M. F. required to break down insulation ordinarily used in a 3,000-volt generator. Three 1,000-volt non-arcing metal arresters were, therefore, selected as offering a convenient means of installing this apparatus (see Fig. 2). It is also well known that coils, and even sharp turns in a wire offer a high inductive resistance to disruptive discharges. Various forms of choke coils were, therefore, constructed and tested with the idea of determining those proportions which, for present requirements, should offer maximum impedance with a given

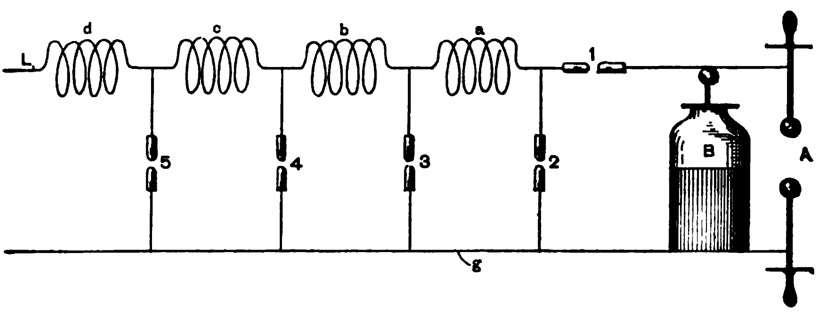


FIG. 4.

length of wire. These experiments will be more particularly described in Part II. The form finally adopted was that of a flat coil about 18 inches in diameter, and wound with 17 turns of wire; the size of the wire varied, of course with the carrying capacity of the particular circuit into which it was to be connected. After further experimenting with various combinations of spark gaps and choke coils, it was decided that the trial apparatus should consist of eight choke coils and twelve 1,000-volt non-arcing metal arresters for each end of each circuit; that is, four choke coils should be connected in series in each leg of each circuit, with discharge circuits intervening. The relative positions of these parts are clearly indicated in Fig. 3, which represents one end of each of three circuits.

The theory upon which the selection of this apparatus was based will be more generally discussed in Part II.; suffice it to say here, that disruptive discharges form nodal points in the system, that is, points where there will be a minimum tendency to discharge; hence, to avoid these with any degree of certainty a multiplicity of arresters, preferably line arresters, should be used. Choke coils form points of reflection, or points where there will be a maximum tendency to discharge—hence a discharge spark gap connected directly in front of a choke coil is more likely to receive discharges, than if placed at some other point in the system. Properly constructed choke coils also offer a very high resistance to the passage of disruptive discharges—so that if one or more such coils be connected between a discharge circuit and the apparatus to be protected, and if the discharge circuit or ground wire be short and straight—the probabilities of safe discharge to earth will be vastly increased. Further, disruptive discharges are liable to divide and follow

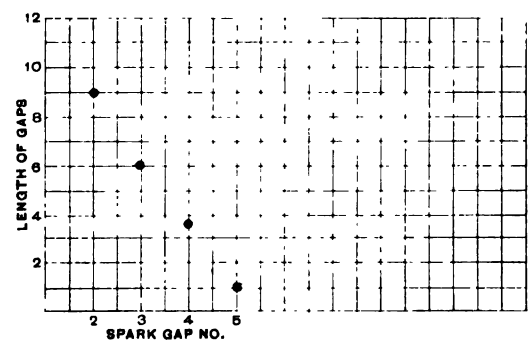


FIG. 5.

several paths, being governed by a complexity of ever varying circumstances. For this reason it was decided to connect several choke coils in series, so that should only a portion of a discharge pass across the first arrester, the balance passing through the first coil. a second opportunity for discharge would be found at the second arrester, which was also connected in front of a coil, and, therefore, at a point of reflection. Should this remaining portion of the discharge again divide, a further opportunity for discharge would be afforded at the third arrester, and so on, so that by the time the fourth coil was reached, it was presumed that the discharge would have spent itself.

Some of the many experiments made to substantiate this theory are exceedingly interesting as well as instructive. Referring to Fig. 4, A are the terminals of a powerful influence machine, B is a battery of Leyden jars, G a wire which may represent the ground, and is connected to the outside coating of the jars, L is a second wire which may represent one leg of an electric circuit; *a*, *b*, *c* and *d* are choke coils connected into the line L, and in series with each other; 2, 3, 4, and 5 are intervening discharge circuits containing spark gaps; 1 is a $\frac{13}{32}$ inch spark gap separating line L from the inside coating of battery B. If now the battery becomes charged from the influence machine A, a large and violent disruptive discharge will take place across gap 1 and suddenly charge line L. This discharge will then pass to earth G through one or more of the spark gaps

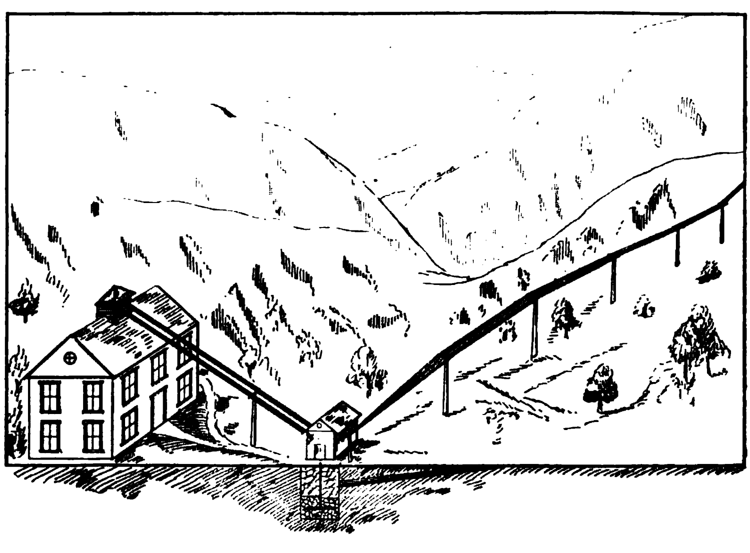


FIG. 6.

2, 3, 4, 5, according to circumstances. It will be noted now, that this arrangement of choke coils and discharge circuits is similar to that shown in Fig. 3. The spark gaps were made of rounded half-inch brass rod adjustable with a 1-32-inch screw thread. The coils were wound with No. 0000 wire, were 3 inches in diameter, 6 inches long and contained 11 turns each.

The following table indicates the very interesting results obtained by varying the lengths of spark gaps 2, 3, 4 and 5. The signs "—", "*" and "o" indicate, respectively, a heavy discharge, a feeble thread-like discharge and no discharge.

The Spark Gap Numbers	1	2	3	4	5
The Spark Gap Lengths	12	14	1	1	1
Results, 1st	—	—	*	0	0
" 2nd	—	0	—	0	0
The Spark Gap Lengths	12	14	2	1	1
Results	—	0	—	*	0
The Spark Gap Lengths	12	14	2	2	1
Results	—	0	—	0	*
The Spark Gap Lengths	12	14	2	2	2
Results	—	0	—	0	0
The Spark Gap Lengths	12	14	3	2	2
Results	—	0	—	*	0
The Spark Gap Lengths	12	14	3	3	2
Results	—	0	—	0	*
The Spark Gap Lengths	12	14	3	3	3
Results	—	0	—	0	0
The Spark Gap Lengths	12	14	4	3	3
Results, 1st	—	—	0	*	0
" 2nd	—	0	—	*	0
The Spark Gap Lengths	12	14	4	4	3
Results	—	0	—	0	*
The Spark Gap Lengths	12	14	4	4	4
Results, 1st	—	—	*	—	0
" 2nd	—	0	—	0	0
" 3rd	—	0	0	—	0
" 4th	—	0	—	*	0
The Spark Gap Lengths	12	14	5	4	4
Results, 1st	—	0	—	0	0
" 2nd	—	—	0	0	*
" 3rd	—	—	—	—	0
" 4th	—	0	0	—	0
" 5th	—	0	—	0	*
The Spark Gap Lengths	12	14	5	5	4
Results, 1st	—	—	—	0	*
" 2nd	—	—	0	0	*
" 3rd	—	0	—	0	*
The Spark Gap Lengths	12	14	5	5	5
Results, 1st	—	—	—	0	0
" 2nd	—	—	0	0	0
" 3rd	—	0	—	0	0
" 4th	—	—	0	0	*
" 5th	—	—	0	0	*
" 6th	—	0	0	—	0
" 7th	—	—	—	0	0
Spark Gap Lengths	12	16	5	5	5
Results	—	0	—	0	*
The Spark Gap Lengths	12	16	6	5	5
Results, 1st	—	—	—	—	0
" 2nd	—	—	0	—	0
" 3rd	—	0	—	0	0
" 4th	—	0	0	—	0

The Spark Gap Lengths	12	16	6	6	5
Results, 1st	—	0	—	*	0
" 2nd	—	0	—	0	*
" 3rd	—	—	—	0	*
The Spark Gap Lengths	12	16	6	6	6
Results, 1st	—	0	—	0	*
" 2nd	—	0	—	0	0
" 3rd	—	0	—	0	*
The Spark Gap Lengths	12	16	9	6	6
Results, 1st	—	—	—	—	0
" 2nd	—	—	—	0	*
" 3rd	—	—	*	0	0
" 4th	—	—	0	—	0
The Spark Gap Lengths	12	16	10	6	6
Results, 1st	—	0	*	—	0
" 2nd	—	—	0	—	*
The Spark Gap Lengths	12	16	11	6	6
Results	—	0	0	—	*
The Spark Gap Lengths	12	16	10	10	10
Results, 1st	—	—	—	—	0
" 2nd	—	—	—	0	*
" 3rd	—	—	—	0	0
The Spark Gap Lengths	12	20	10	10	10
Results, 1st	—	0	—	—	0
" 2nd	—	0	—	0	*
" 3rd	—	0	—	*	0

When 1 = 12, 2 = 9, 3 = 5½, 4 = 3¾ and 5= 1, the sparks were about evenly divided, except that 5 never sparked alone and always gave thread-like sparks when 2 sparked. These results are represented graphically in Fig. 5.

Glancing now at the above table we notice: First. The difficulty that disruptive discharges experience in passing the coils, jumping as they do large air-gaps in preference to the more circuitous but infinitely better conducting paths afforded by the coils. Second. The tendency for discharges to divide into two or more paths, part passing across the first or second gap, and the remainder through one or more coils to a succeeding gap, showing thereby that a single choke coil does not offer certain security against the passage of these discharges. Third. The uncertainty of the paths that will be selected during a series of tests where the conditions are maintained as nearly constant as possible; in fact, it seems almost impossible to produce the same results twice in succession. It is, however, to be noticed that with these four choke coils in series and spark gaps intervening, the discharges are so thoroughly sifted out that only an occasional thread-like spark finds its way across the last gap. With laboratory results such as these, it seemed fair to presume that results more or less similar might also be expected in practice.

The plant selected for the trial of this apparatus was that of the San Miguel Consolidated Gold Mining Company, of Telluride, Colorado, which is equipped with a 3,000-volt alternating current synchronous system, operating stamping mills, and furnishing current to the Telluride Electric Light Company. These points are situated among the mountains at distances varying from three to ten miles from the power-house. Three separate circuits leaving the power-house extend over a wild and rocky country and in some places rise above timber line. In previous years every attempt to protect this plant from lightning had failed. During the summer months two horses were kept con-

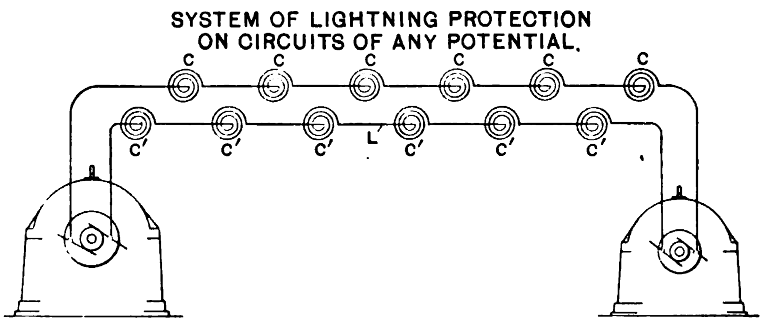


FIG. 7.

stantly saddled ready for emergencies consequent on lightning discharges, and at the motor and power-house it was common practice on the approach of a thunderstorm to lay out, ready for instant use, an extra armature coil, with all the necessary tools for handling the same. In one of the former types of arresters used in this plant forty fuses were blown inside of 60 minutes.

The location of each bank of arresters was selected with particular reference to securing permanently damp earth for the ground plate, the lines being led to this spot. The circuit was thus made to accommodate itself to the arresters rather than the arresters to the circuit. The apparatus was then installed in a specially constructed and weather-proof lightning arrester house (see Fig. 6) the arresters and choke coils being mounted on thoroughly dried wooden frames and every precaution taken to insulate these from the ground and from each other. In this manner also it was expected that the lightning discharges would be kept entirely out of the station.

The connections inside the power-house lightning arrester house are all clearly indicated in Fig. 3. One main B. and S. No. 3 ground wire was used for all the arresters in each bank, unnecessary kinks and bends being studiously avoided.

FIG. 8.

The grounds which I used were of the most approved construction, and in this respect were in marked contrast to ground connections commonly found in practice. An old rusty casting or an abandoned pulley hooked onto the end of a tangled piece of scrap wire, and then thrown into a neighboring creek or the tail race of a mill, or a small iron spike driven conveniently into dry earth or sand, form ground connections not infrequently found in practice. But my work being only of an experimental nature, a thoroughly good ground connection was thought to be one of the essentials. The ground connections were therefore made as follows: First, a hole six feet square was sunk directly under each bank of arresters until permanently damp earth was reached; Second, the bottom of this hole was covered with two feet of crushed charcoal (about pea size)—crushed coke would have answered equally well. Third, over this was laid 25 square feet of No. 16 copper plate; Fourth, the ground wire was then

FIG. 9.

firmly soldered all the way across the ground plate; Fifth, the plate was now covered with two feet of charcoal; and sixth, the hole was filled in with earth, using running water to settle. In one instance permanently damp earth could not be reached; water was therefore brought from some considerable distance through an iron pipe and run into the upper earth of the ground hole. In this manner the entire ground construction was maintained constantly damp.

Observations were taken by competent men at each bank of arresters during the entire lightning season, and the results obtained indicate that the discharges occurred most frequently over the second arresters; many passed over the third arresters; very few, however, over the first or fourth. The writer personally watched one of these banks of arresters through severe thunderstorms and in every instance the discharges noticed by him were seen to pass across the second series of spark gaps. The discharges followed each other across the cylinders with great rapidity, making peculiar and often startling sounds, very similar to the cracking of a teamster's whip, but in no instance was there

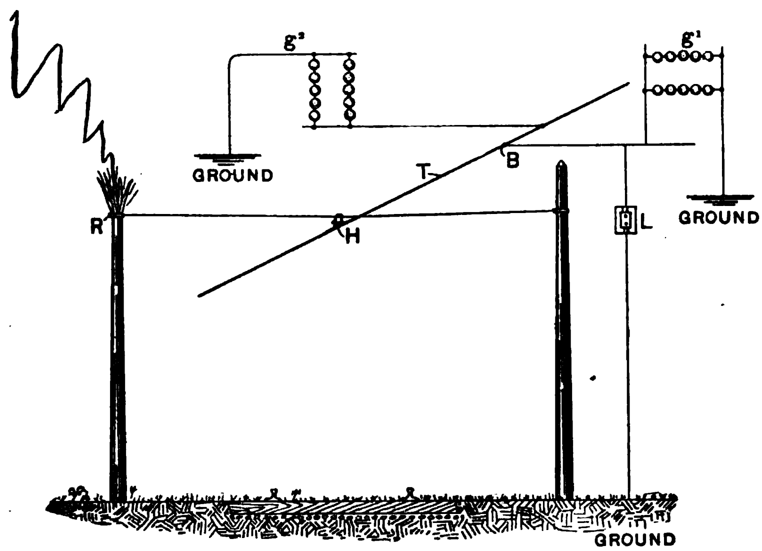


FIG. 10.

a fuse blown, or damage done to the arresters. Now, these facts are not only of great practical importance, but are also exceedingly interesting when studied in connection with the results already mentioned as having been obtained with choke coils and spark gaps in my laboratory experiments. At the end of the lightning season the non-arcing metal cylinders of the arresters were carefully inspected and the confronting sides found to be covered with minute black spots where the discharges had passed from cylinder to cylinder. These spots were often centered with very small, smooth exposures of the metal, but in no instance was there any indication of beading or fusing together of the adjacent cylinders.

The net results of this lightning arrester experiment are most satisfactorily summed up in the following letter written by Mr. P. N. Nunn, Electrical Engineer of the San Miguel Consolidated Gold Mining Company. I feel greatly indebted to Mr. Nunn for the kindly assistance he has rendered in carrying out these tests, and take this opportunity to again thank him for his valuable suggestions and the pains-taking care with which the installation was made.

THE SAN MIGUEL CONSOLIDATED GOLD MINING CO.

TELLURIDE, COLO., October 12th, 1893.

Mr. Alexander Jay Wurts,
Pittsburgh, Pa.

My Dear Sir :—In reply to your inquiry respecting the results obtained from your lightning arresters in the operation of our electric power transmission plant, we take pleasure in making the following statement :

At the present time, as you are aware, our plant consists of a power station at Ames, equipped with Westinghouse alternating current generators of 1100 horse power capacity, which supply current for synchronous motors in three stamp mills at distances of two, three and ten miles respectively, and for lights in the town of Telluride about eight miles distant. The pole line runs over the mountains in altitudes varying from 8,800 to 12,000 feet above the sea level, traversing at different places bare ridges of mountain divide, and large tracts of magnetic mineral. It is thus peculiarly exposed to lightning in a district where lightning is of extraordinary frequency and force at certain seasons of the year.

The difficulties of the situation are further aggravated by the fact that we generate and utilize without transformation, except for lighting purposes, an alternating current of 3,000 volts.

Despite the use of an overhead grounded wire on our pole line, and the best arresting apparatus we could procure, we suffered continual interruptions during the lightning seasons of 1891 and 1892. On one occasion five armature coils were burned out in a single storm. With any other form of armature this would have meant five burned out armatures ; but with the toothed armature of your machines it represented the cost of five new coils and a day's delay. The frequency of the discharges of lightning was so great that the insulation in the highest grade of cables and cores throughout the entire apparatus became so honeycombed with perforations as to give rise to continual leakage, grounds and short-circuits, requiring constant and expensive repairs and causing prolonged delays.

Since putting in your non-arcing metal arresters in connection with your system of specially designed choke-coils, we have passed through the lightning season of 1893 without a particle of lightning having passed to our knowledge beyond the arresting apparatus, without a shut-down, without the loss of a coil, and without a single perforation of insulation. The experience of this season has given us the greatest confidence in the efficiency of your apparatus as a protection against lightning.

(Signed) P. N. NUNN,
Electrical Engineer.

PART II.—PROTECTION AGAINST LIGHTNING ON CIRCUITS OF ANY POTENTIAL.

A system of protection against lightning discharges has been designed by the writer, which is applicable to circuits of any potential, and although this system has not yet received a practical test, the indications are that it will prove efficient.

The proposition is to connect in series with the circuit, and at frequent intervals, a system of properly constructed choke coils, the expectation being, that the energy stored in the circuit in the form of static electricity will, at moments when there is a tendency to a disruptive discharge, dissipate itself into heat through the electrical surgings which will be set up between and among the several choke coils. In Fig. 7 there is represented a power transmission circuit provided with this system of protection against lightning.

The theory upon which these predictions are based, is more or less familiar to every electrical student. There exist, however, differences of opinion regarding some of the points involved. Many of my impressions are derived from personal observation, and through these I have been led to believe that, in many cases, electric circuits become statically charged by contact with the neighboring charged atmosphere, that is, by conduction from it. The charge on the line, no doubt, leaks to earth, so does the charge in the atmosphere, but the two are maintained at practically the same potential.

The potential of the atmosphere surrounding the wires of overhead electric light and power circuits is not very high. At the top of Washington monument, Washington, D. C., the difference of potential between the atmosphere and the earth during the thunder storms is about 3,000 volts. If then the potential of an aerial wire is the same as that of the atmosphere intimately surrounding it, the immediate source of danger from the static strain will be inconsiderable. When, however, a lightning discharge occurs from the clouds, this charged condition of the wire becomes unbalanced, the atmosphere has been discharged and its potential has suddenly sunk to zero. The static charge in the wire having lost the support of the previously charged atmosphere now seeks an equilibrium, and in so doing sets up electrical waves which travel to the extremities of the system, or to points of great resistance, and are there reflected to other parts to be again reflected, and so on. At these points of reflection there is an enormous strain, and a consequent tendency to "side flash" so often causing the damage to insulation which we are seeking to avoid. Such electrical disturbances consequent upon a lightning discharge can be observed in wire fences and in tramway rails. In high altitudes even the wet rocks are seen to become luminous, giving off brush discharges at moments of electrical disturbance, and the human body will often feel that peculiar sensation familiar to those who handle influence machines, namely, a sudden and cool draught when an existing condition of electrical strain is broken down.

It is then at these points of reflection that the damage is done, and that we get the impression of a very high potential. An armature insulation is pierced by the discharge, the dynamo current follows, there is a great flash accompanying the short-circuit, several fuses let go and the superintendent is informed that lightning entered the station. As a matter of fact, it is probable that the discharge has made a hole in the insulation no larger than the prick of a pin or perhaps that of a pin head, which in most cases is greatly enlarged by the dynamo current.

There are other methods, however, by which circuits probably become charged; namely, by static induction from clouds and by dynamic induction from cloud discharges. It is probable that these also set up electrical surgings in the circuit and that the danger to insulation occurs at points of reflection. I am also inclined to believe that, although a line is seldom, if ever, struck by a direct lightning discharge, it occasionally becomes charged by some of the ramifications which are often seen to accompany a lightning stroke.

Fig. 8 is an example of what might be called a direct discharge, and to the best of my knowledge there is no evidence to show that such discharges strike electric wires. In Fig. 9, there is represented a lightning discharge accompanied by ramifications which wander off in a seemingly aimless manner, but which, no doubt, find their way to bodies or strata of air variously electrified. I am inclined to believe that at times, some of these ramifications find their way into electric circuits.

A curious freak which came under my observation last summer is illustrated in Fig. 10. T represents an overhead trolley wire, on either side of which are wooden poles. H is a bell-shaped insulator made of compressed mica and shellac. These insulators easily withstand an electric strain of 12,000 volts. R is an iron ring holding the span wire to the pole. B is a branch circuit feeding current to a group of lamps G^1, and L is a lightning arrester in its discharge circuit. G^2 is a second group of lamps. The distance between H and B is about 50 feet. After a violent thunder storm it was noticed that one of these poles had been shattered from the top, down to the iron ring, the remaining portion being uninjured. This had been done by lightning, and in the opinion of the writer, by one of the ramifications to which allusion has already been made. In any case, this discharge, whatever it may have been, passed over the span wire to the bell insulator H, piercing it and breaking it into three pieces, then travelled along the trolley wire to B, where it apparently divided, one part passing to the group of lights G^2, breaking them all, ten in number, and the other part to earth through the arrester L, without in any way interfering with the group of lights G^1. It is quite remarkable to note that none of the parts damaged by this discharge showed any indications of heat.

During the past summer I have watched many discharges enter a generating station over idle wires and pass to earth across simple carbon spark gaps provided with No. 18 copper wire fuses. The discharges gave a crackling sound similar to that of discharges from a Leyden jar, and in some instances where I had placed pieces of tissue paper between the electrodes of the spark gaps, the papers were perforated with small holes. In one instance only which came under my observation did the copper wire fuse melt. An apparently prolonged discharge passed over one of the spark gaps with a hissing sound, raised the tips of the carbon electrodes to a white heat and fused the copper wire cutout, the time taken being approximately two seconds, which would indicate in the neighborhood of 150 amperes. I believe this discharge to have been part of a true lightning discharge—one of its ramifications—but in the great majority of cases the discharges are small, and in this respect bear no comparison to cloud discharges.

Now, if the "electrical surgings" theory be correct, I believe that these dangerous points of reflection can, by means of properly distributed and effective choke coils, be confined to long lines and made to wear themselves out, as it were, between the coils. In this manner frequent points of reflection would be distributed over the system. Primary waves set up during electrical disturbances would be broken into smaller waves, which, surging back and forth between the coils or points of reflection, would finally become dissipated and their energy pass off into heat.

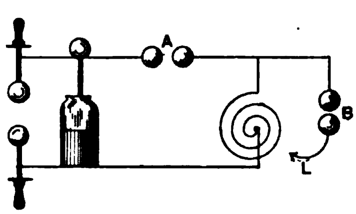


FIG. 11.

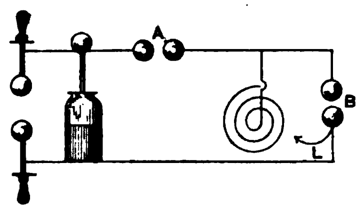


FIG. 13.

The number of coils which it would be necessary to use for a given length of circuit has not yet been determined, but the writer would suggest placing four to each mile of single wire. For convenient accomodation on the poles these can be arranged alternately on the two legs of the circuit, thus avoiding two coils on any one pole.

Should such a system of protection against lightning be found efficient, its many advantages over the discharge circuit method would be obvious, particularly in a country where good ground connections are costly, and often impossible to construct.

The general form and dimensions of these choke coils is a matter of considerable importance. Dr. Lodge states, I believe, that for disruptive discharges a flat spiral will offer maximum impedance with a given length of wire. To the best of my knowledge, however, Dr. Lodge makes no mention of the number of turns

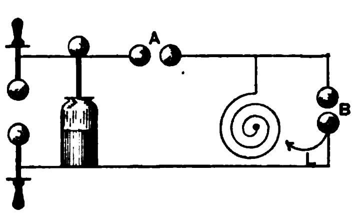


FIG. 12.

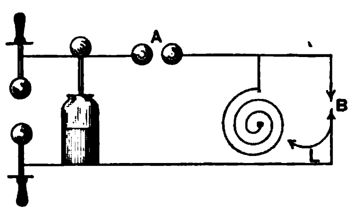


FIG. 14.

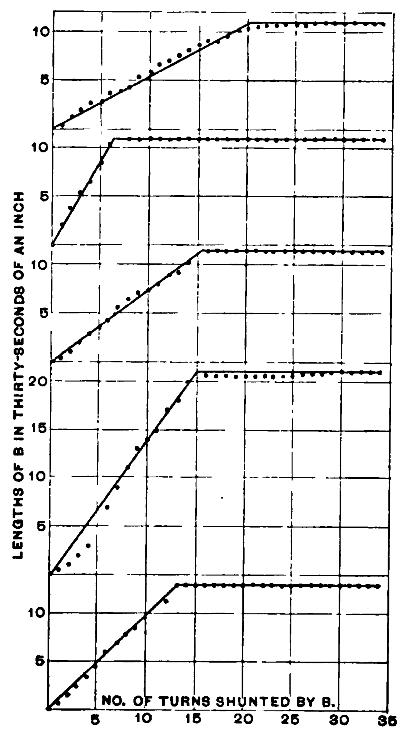
which may be advantageously used. I have found by experiment that the impedance or resistance to the passage of disruptive discharges does not continuously increase with the number of turns, but that a critical point is rapidly reached, beyond which additional turns do not add appreciably to the impedance of the coil.

The critical point was determined in the following manner. A large flat coil was constructed, having the following specifications:

Size of wire	No. 0
Number of turns	34
Outside diameter	30"
Inside "	3"

The wire was carefully insulated so that the discharges could not pass between consecutive convolutions.

This coil was then connected up in the usual manner for determining impedance, the general arrangement of the apparatus being shown in Figs. 11, 12, 13 and 14, where B is a variable shunt spark gap, one terminal of which is permanently connected to either terminal of the coil, while the other is provided with



FIGS. 15 16 17 18 19.

a sharp metallic point fastened to a short flexible cord. The sharp point was used to pierce the insulation of the coil, thereby reaching the copper of any convolution, and also in this manner providing a means for shunting any desired portion of the coil by the spark gap B. Spark gaps A and B were varied by means of a thirty-second of an inch screw thread, so that the dimensions of these gaps are given in thirty-seconds of an inch.

Each determination was made by shunting with the gap B that portion of the coil whose impedance was desired, and then varying the length of B until the discharges were about evenly divided between the coil and the gap. In the following table are given the values of B and the number of convolutions shunted by the gap in each determination.

No. of Turns.	1st B.	2nd B.	3rd B.	4th B.	5th B.
1	¼	2	¼	⅛	¾
2	1	3¾	1	1	1½
3	1¾	5¼	2	2	2½
4	2½	6½	3	3	3½
5	2¾	8½	4	4	4½
6	3¾	10½	5	7	6
7	3¾	11	5½	9	6¾
8	4¼	11	6¼	11	7½
9	5¼	11	7	13	8⅛
10	6	11	7¾	14	9¾
11	6½	11	8	15	10½
12	7	11	9	17	11¼
13	7½	11	9½	18	13
14	8	11	10	20	13
15	8¾	11	11	20½	13
16	9	11	11½	20½	13
17	9	11	11½	20½	13
18	9⅛	11	11½	20⅛	13
19	10¼	11	11½	20½	13
20	10¼	11	11½	20½	13
21	10½	11	11½	20⅛	13
22	10½	11	11½	20½	13
23	10½	11	11½	20½	13
24	10¾	11	11½	20⅛	13
25	10¾	11	11½	20½	13
26	10¾	11	11½	20¾	13
27	11	11	11½	20¾	13
28	11	11	11½	20¾	13
29	11	11	11½	21	13
30	11	11	11½	21	13
31	11	11	11½	21	13
32	11	11	11½	21	13
33	11	11	11½	21	13
34	11	11	11½	21	13

In the first column are given the number of convolutions shunted by the spark B.

Column 1 B gives the values of B under conditions shown in Fig. 11, with A equals 12 and with rounded electrodes at B.

Column 2 B gives the values of B under conditions shown in Fig. 12, with A equals 12 and with rounded electrodes at B.

Column 3 B gives the values of B under conditions shown in Fig. 13, with A equals 12 and with rounded electrodes at B.

Column 4 B gives the values of B under conditions shown in Fig. 14, with A equals 12 and with sharp electrodes at B.

Column 5 B gives the values of B under conditions shown in Fig. 13, with A equals 15 and with rounded electrodes at B.

For convenience, these results are graphically represented in Figs. 15, 16 17, 18 and 19, and we note, first, that in each case a positive maximum impedance is reached, beyond which additional turns do not affect the results; second, that with the exception of the results shown in Fig. 15, the impedance does not increase beyond about the 17th turn; third, that with both terminals of the coil permanently connected to the discharge circuit (see Fig. 15), the increase of impedance is more gradual than when only one terminal is so connected; fourth that when permanent connection is made to the outside of the coil, Fig. 16, the increase is more rapid than when permanent connection is made at the center, Fig. 17, in other words, a single turn on the outside offers more impedance than a single turn nearer the center; for equal lengths of wire, however, the turns near the center offer greater impedance: fifth, that increasing A slightly decreases the number of turns, Fig. 19; sixth, that using points

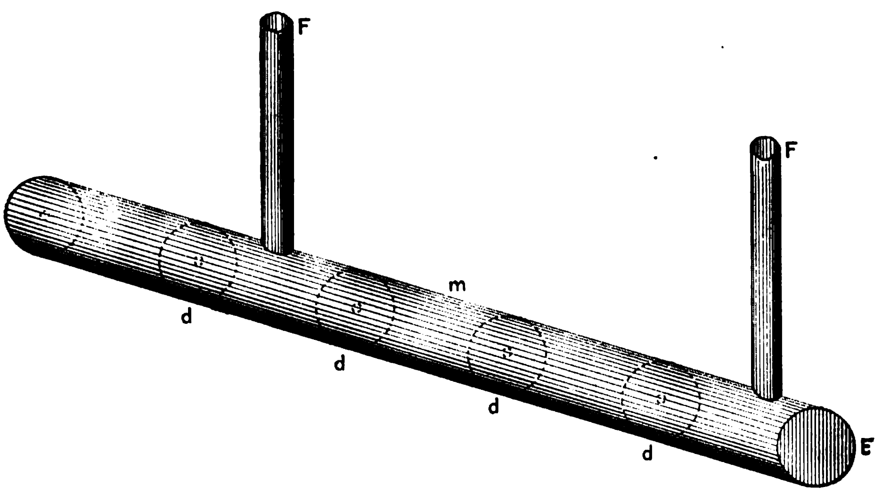


FIG. 20.

at B, instead of knobs, does not materially change the results, other than to make the path selected by the discharge somewhat more uncertain, as will be noticed by the irregular curve shown in Fig. 18. The coils which I have constructed have been wound with reference to the critical point.

Dr. Lodge has also experimented with such coils in connection with iron cores, and found that the impedance is not appreciably affected by their introduction. I have also experimented in this line, and have naturally surprised myself by obtaining quite different results. It is, however, to be supposed, in view of Dr. Lodge's masterly study of this subject, that my conditions have in some way differed from his. I found that upon the introduction of a small bundle of iron wires into the core of a coil, the impedance to disruptive discharges was reduced nearly 20 per cent. The cause of this was at once made apparent by the beautiful sparks which were seen to appear among the iron wires of the core upon the passage of each disruptive discharge. These iron wires, of course, form among themselves closed secondaries to the primary coil, and in this manner also very beautifully illustrate the oscillatory character of the discharge.

A convenient mechanical analogy for this electrical surgings system may be found in hydraulics. Referring to Fig. 20 let M represent a water main closed at either end; F F a supply and outlet; *d d* etc. perforated elastic diaphragms If now an impulse be imparted to the water at E, as by a blow, a wave will

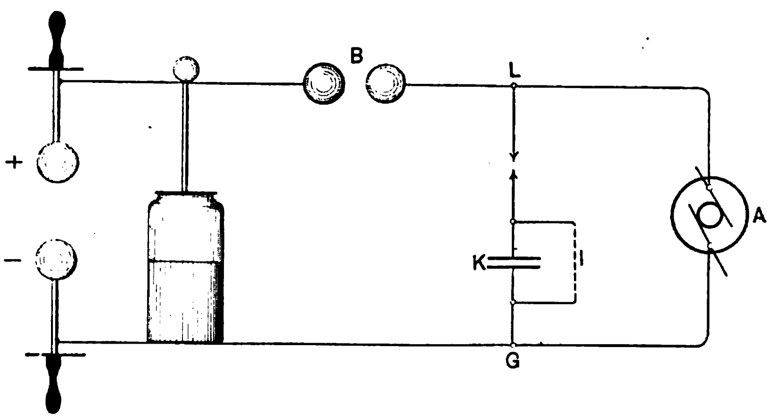


Fig. 21.

tend to travel from one end of the main to the other, and if it were not for the diaphragms, this wave would either burst out the opposite end of the pipe or be reflected back and forth until the energy of the wave had become transformed into heat. The presence of the diaphragms, however, will tend to break up the initial wave and confine the surgings to comparatively small portions of the pipe. At the same time the perforations in the diaphragms allow of a continuous flow of water under pressure. In the electrical system the choke coils are supposed to break up the electrical surgings, much as these diaphragms break up the water surgings, and at the same time the coils do not materially interfere with the passage of the dynamo current. The coils cause points of reflection to occur where an excessive static strain can do no harm. The ends of the circuit, which are the vital parts of an electric power transmission system, are thus shielded by the coils and the energy of the static charge is harmlessly transformed into heat.

It has, however, been mentioned in Part I. that coils are not absolutely impervious to disruptive discharges. A discharge entering a coil meets with continuous resistance and is finally all or partially reflected; in other words, a coil is not a stone wall, and

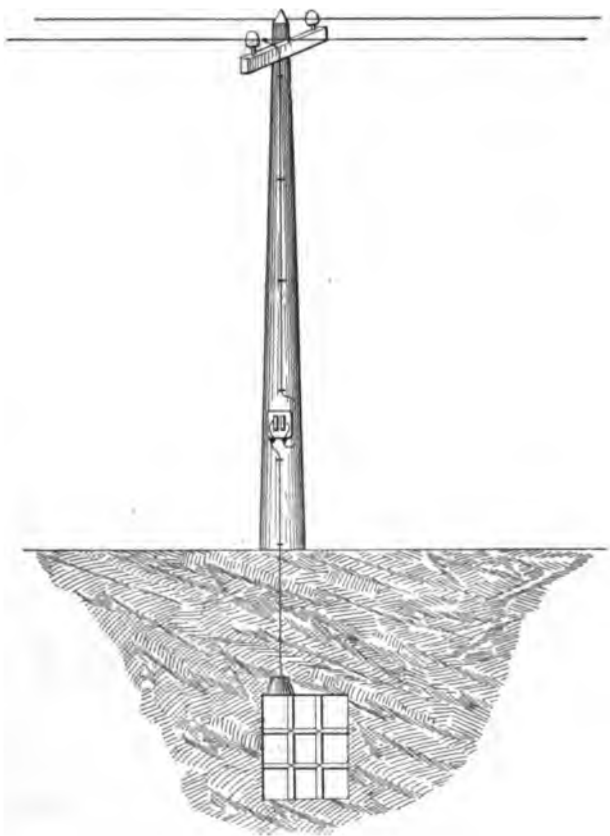


FIG. 22.

does not necessarily confine the point of intense electrical strain to the first inch or two of the coil. The discharge penetrates the coil and often causes "side flash" to occur well within its interior. In my mechanical analogy therefore I have represented this yielding opposition, as it were, by elastic diaphragms.

PART III.—DISCRIMINATING LIGHTNING ARRESTERS.

The Condenser Lightning Arrester.—The discovery and practical application of non-arcing metal to alternating current circuits about two years ago,[1] has indicated the possibility of constructing a discriminating lightning arrester, as it were, for direct current circuits; that is, an arrester which should not require the usual automatic circuit interrupting attachment, but which, by virtue of its material or construction, should allow static electricity to pass and prove an effectual barrier to the dynamo current.

The first step taken in this direction was to carefully analyze the conditions, and the general conclusions arrived at were as

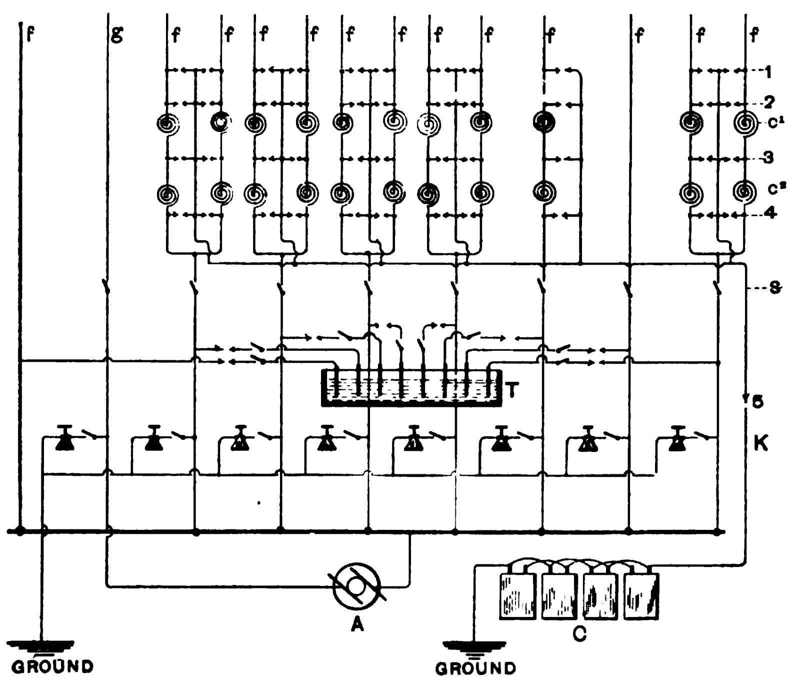


FIG. 23.

follows: A circuit becomes charged in various ways with static electricity tending to earth. These charges are, in a majority of cases, small and of no considerable intensity. The surgings in the system find points of reflection which also become points of greatly increased tension. The earth is the great reservoir (to use common language) for these discharges. This reservoir is, however, unnecessarily large for the accommodation of line discharges. Might not smaller reservoirs be made—little earths insulated from the mother earth? If the circuit were connected

1. TRANSACTIONS, vol. ix, p. 102, 1892.

directly or through spark gaps to such little earths, these dangerous surgings might be broken up and the line safely discharged without a possibility of the dynamo current following. Each spark gap in each of such discharge circuits would then form a discriminating lightning arrester, but these little earths would be nothing more nor less than one coating of a condenser—the mother earth a common coating to them all. Why not then use

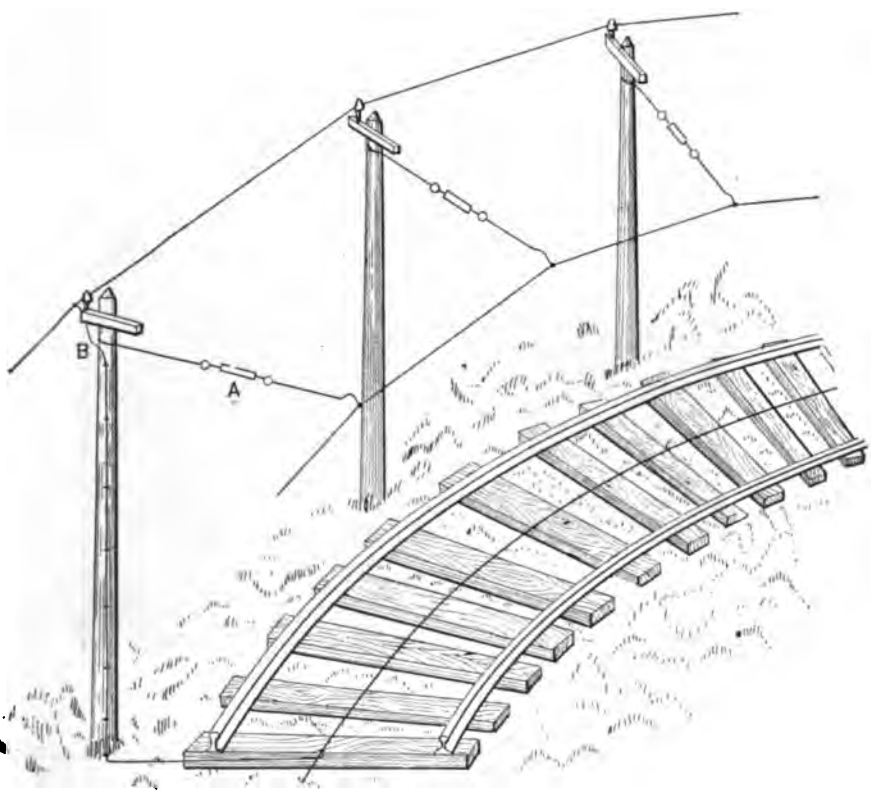


FIG. 24.

condensers, connecting one side to line, the other to earth through a spark gap?

One difficulty immediately presented itself. After the condenser had become charged how was it to become discharged? To be successful it must be self-discharging, and not only this, but the discharge must take place in such a manner that no dynamo current should follow. Therefore, to discharge the condenser disruptively was at once out of the question. Experiments in this line led first to the application of a wet string to the terminals of a condenser, the idea being to leak out the charge through an ohmic resistance sufficiently great to prevent an appreciable flow of dynamo current. A two M. F. condenser provided with a wet string leak was treated with violent disruptive discharges from a battery of Leyden jars, and upon immediately applying the tongs to the terminals of the condenser after the crack of the discharge, not the smallest trace of a charge could be detected. When the string was removed or had become dry the charge was retained. Evidently a wet string would not answer as a permanent leak. Various compositions

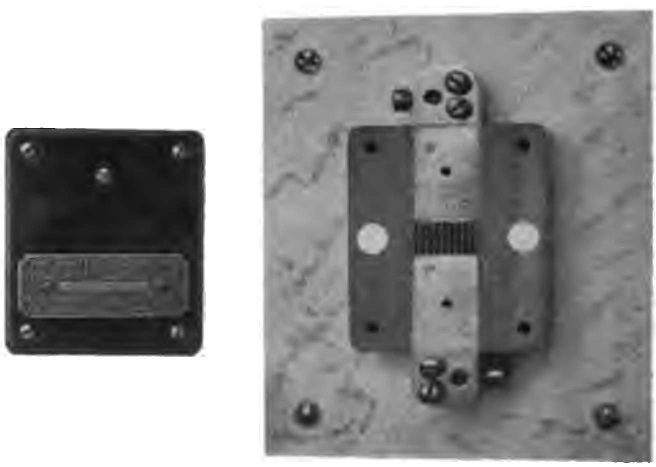


FIG. 25.

of plumbago with moulding sand, red lead and plaster-of-paris were pressed into a tube and used, but these all proved unsatisfactory, being unreliable. In many instances this composition leak would, before testing with the condenser, have a resistance of 30,000 ohms, and after a single discharge it would be found that this resistance had increased to many hundred thousand ohms. This was, perhaps, due to the flying apart of the plumbago particles under the influence of the discharge, and so breaking the continuity. Finally, a pencil mark over ground glass was suggested and this was found to work admirably. A medium pencil was rubbed back and forth over a strip of ground glass, making a narrow shining streak having a resistance of from 40,000 to 50,000 ohms. Rubbing with the hands did not seem to appreciably change this resistance. Broad black pencil marks were made on the ends for better contact, and connection to the terminals of the condenser was obtained through aluminium foil, both glass and foil being protected on the bottom by the wooden case of the condenser, and on top by a small inverted wooden trough. A condenser provided with such a pencil mark leak,

FIG. 26.

was now connected to a 500-volt direct current circuit and to apparatus, as shown in Fig. 21. A was a 500-volt direct current generator; L one leg of the circuit, which may represent a trolley wire; G the other leg of the circuit, and which may represent the ground return: K the two M. F. condenser with its high resistance leak *l*; C a small spark gap in series with the condenser, and in the discharge circuit; B was a $\frac{12}{32}$ inch spark gap over which disruptive discharges would pass from the battery of

Leyden jars (11), and in this manner suddenly charge the line L. The most violent disruptive discharges that could be obtained in the laboratory were unable to damage either the generator or the condenser. A small spark gap, a trifle larger than c, connected either in series or in shunt to the generator failed to take any of the discharges, thus demonstrating the ease with which the discharges were received by the condenser. Such tests were continued for half an hour at a time, the discharges following each other in rapid succession, but the condenser was ever ready, being kept constantly discharged by the high resistance leak. Of course, the resistance of the leak was sufficient to prevent the formation of an arc at c. The terminals of the leak were also too far apart to permit the condenser to discharge disruptively. This device ensures all the requirements of a discriminating lightning arrester, and in this form seemed admirably adapted for station and indoor use on 500-volt direct current circuits.

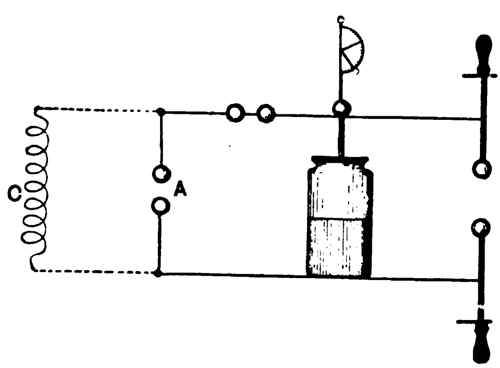


FIG. 27.

For line or outdoor use the condenser was placed in a suitable cast-iron box, one terminal of the condenser being grounded to it, while the other was led out through a specially designed and water-tight bushing. If then this iron box were buried in damp earth, as a ground plate, so to speak, the outer terminal connected to the lower electrode of a small spark gap placed on a pole, and the upper electrode of the gap connected to the line, the combination would constitute a discriminating line lightning arrester. See Fig. 22.

Practical tests have been made during the past summer with both station and line condenser lightning arresters, and the results obtained have been most gratifying. The plant in which the apparatus was installed was that of the Denver Tramway Company, Denver, Colorado, in a locality where lightning storms are particularly severe and of frequent occurrence. This plant extends over a vast amount of territory and no attempt was made to protect it as a whole with these arresters. The experiences of this company with automatic lightning arresters on the line had

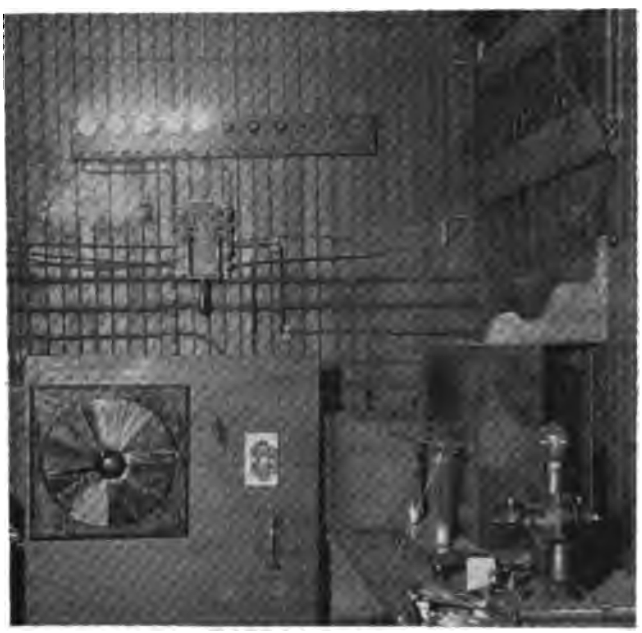


FIG. 28.

been extremely unsatisfactory, and as a final measure they had resorted to simple carbon gaps liberally distributed over the system, depending upon the station circuit-breakers to interrupt the short-circuits. This combination, however, worked satisfactorily near the station only; at any considerable distance therefrom, the drop on the line was such, that even a short-circuit across these spark gaps failed to trip the breakers. The arc therefore continued until it had burned itself out, which in many cases was not accomplished until the top of its wooden pole had been burned off.

The installation of these condenser lightning arresters consisted of five arresters on the line and 44 spark gaps with 11 condensers in the station. The station spark gaps were also provided with two specially constructed choke coils for each feeder. The original arrangement of this apparatus is shown in Fig. 23. A is the generator; F F etc. the feeders; 1, 2, 3, 4, etc. the 44 spark gaps alluded to; C 1, C 2 the two choke coils in series with each feeder; S represents the circuit-breakers and K Keystone lightning arresters; C is a bank of four 2 M F condensers, making a common ground for the 44 spark gaps; T is a tank lightning

FIG. 29.

arrester; G is an overhead ground return; 5 a spark gap in the ground wire of the 44 spark gaps. A tell-tale piece of tissue paper was placed in this gap, which, becoming punctured, would at once indicate and register the passage of a discharge across any of the 44 spark gaps.

The first thunder storm which occurred after this apparatus had been installed gave unlooked for results. The tell-tale paper at 5 failed to indicate any discharge whatever—the spark gaps 1, 2, 3 and 4, which were placed in the cupola were examined and many of these found to be badly burned. At first thought, this indicated some path to earth other than that provided through gap 5 to the condensers. Tests for a ground, however, failed to prove this to be the case. A succeeding storm reproduced these same strange results. A more careful study of the problem, now disclosed the fact that as the circuit-breakers were frequently thrown open during thunder storms, the feeders which were thus opened became ground wires, being grounded through their respective motors. Consequently, live feeders dis-

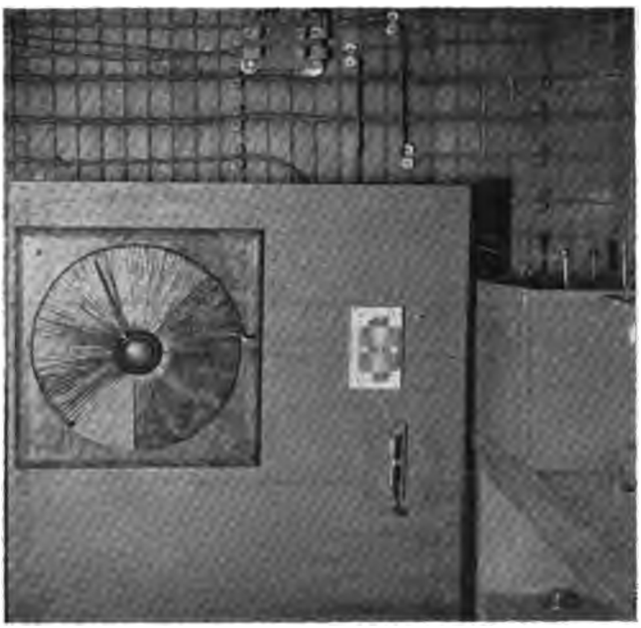


Fig. 30.

charged into these dead or grounded feeders and *vice versa*, forming arcs which burned the carbon electrodes of the several spark gaps according to the amount of current taken by the respective feeders.

Fresh carbon electrodes were subsequently provided, and a single condenser connected to each group of spark gaps on the individual feeders, which arrangement would prevent the possible re-occurrence of arcing between feeders.

The line arresters had, however, in the meantime been carefully watched, and after every storm the tell-tale papers were collected and found to be perforated with a greater or less number of small holes, each hole being slightly discolored around the edges.

The sensitiveness of these condenser arresters was particularly noticeable, in that the tell-tale papers were found to be punctured at the slightest provocation, when in fact no considerable thunder storm had passed over the lines, and yet, sensitive as they were to slight charges on the line, it is remarkable to note that the simple carbon spark gaps belonging to the local company, which have already been mentioned, and which provided a gap $\frac{1}{8}$ inch between electrodes, frequently received discharges in preference to the minute spark gap of a neighboring condenser line arrester. Such occurrences coupled with many others of a similar nature point

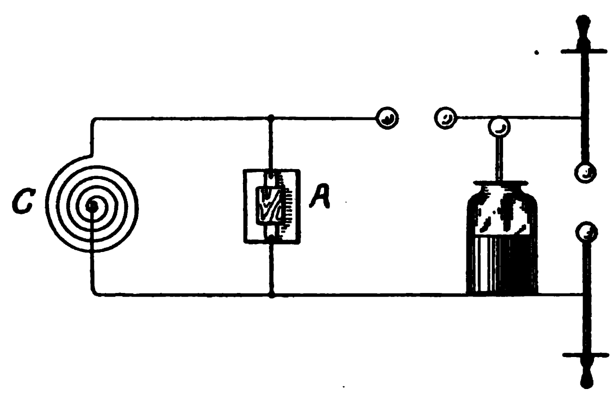


Fig. 31.

to the existence of nodal points, and the necessity of distributing line arresters at frequent intervals over the system.

In fact a lightning arrester connected directly to the terminals of a generator or motor offers no absolute guarantee of protection to this particular piece of apparatus. The presence of the arrester simply offers an opportunity for discharge. The discharge may occur in the armature, or it may occur through the arrester, according to the particular conditions existing at the time of each discharge. The arrester could have a comparatively large spark gap, and the breaking down strain of the armature insulation be low, and still the discharge might prefer the large gap to the weaker insulation, while on the other hand the arrester could have a very small spark gap and the armature be provided with the highest grade of insulation, yet in some cases this insulation would be pierced by the discharge in preference to the small spark gap of the neighboring arrester. A striking example of this possibility is illustrated in Fig. 24. A is a type of strain-wire insulator which is ordinarily capable of withstanding from 15,000 to 20,000 volts. B shows a wire connecting an overhead return to the ground wire return. The distance between wire B and the iron hoop holding the strain wire is about one inch. During a violent thunder storm the insulator A was punctured by a discharge which passed from the trolley wire through the insulator

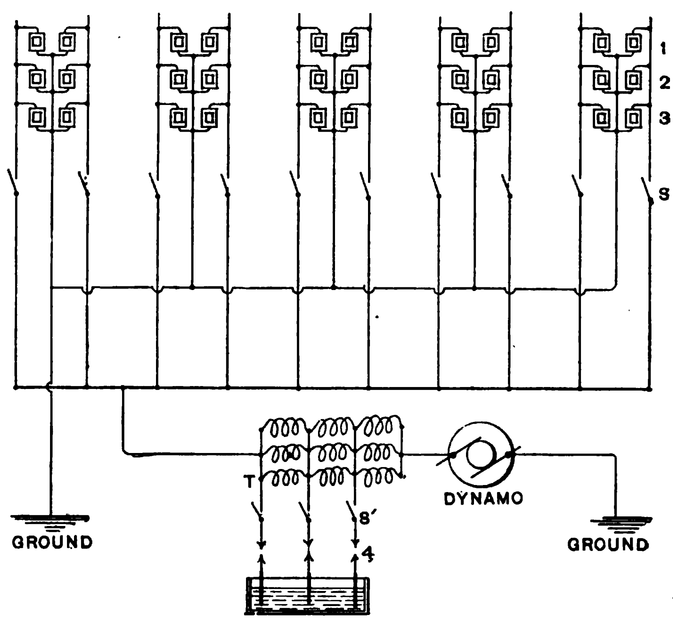


FIG. 32.

to the iron hoop, and thence across the one-inch air space to the grounded wire B, and this very high ohmic resistance path was taken in preference to a neighboring line arrester having a small spark gap. Had an arrester been connected at this point, there is no question but that the arrester would have taken the discharge, but by reason of the nodal points which are formed along the line by the electric surgings, a lightning arrester 100 yards away, although it is liable to take a large proportion of the discharges, does not by any means offer an absolute guarantee of protection to a neighboring piece of apparatus. A large number of line arresters, that is, a large number of opportunities for discharge, is the surest means of securing efficient protection.

The results obtained with these condenser arresters have exceeded my expectations, and in my opinion have thrown considerable light on the intensity and volume of discharges commonly found in electric light and power circuits. During my two months' stay in Denver not a single condenser was disabled, nor were any of the tell-tale papers burned. I have with me a few of these perforated papers which I shall be glad to have you inspect. I was not present in Denver after the burned out spark gaps in the station had been replaced, and am therefore not in possession of any further data from that source. But, in Colorado Springs, where I spent the next following six weeks, I connected three of these condenser arresters in the station of the Colorado

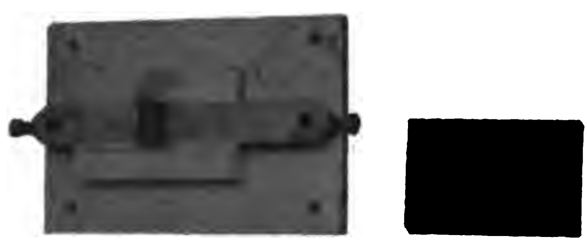


Fig. 33.

Springs Rapid Transit Railway Company, and was treated to many violent thunder storms, through all of which, these arresters showed a wonderful capacity for repeatedly and rapidly discharging the line. Through many of these storms the discharges passed the spark gaps as many as twenty times a minute, completely riddling the tell-tale papers.

I may, therefore, say with emphasis, that the condenser lightning arrester has proved itself most efficient in points of sensitiveness, durability and general reliability. In actual service it has demonstrated the successful construction of a truly discriminating direct current lightning arrester.

The Non-Arcing Railway Lightning Arrester.—I have, however, designed a lightning arrester which is much smaller, cheaper and, perhaps, in every way more desirable than the condenser arrester. It is also a discriminating lightning arrester.

I had frequently noticed what is probably familiar to many of you, namely, that a disruptive discharge will leap over a non-conducting surface much more readily than through an equal air space. The non-conducting surface seems to form an entering wedge, as it were, through the air, so that this being already partly split or ruptured, the discharge has but to further separate the air from the non-conducting surface instead of boring its own path through it. A pencil mark over a rough piece of

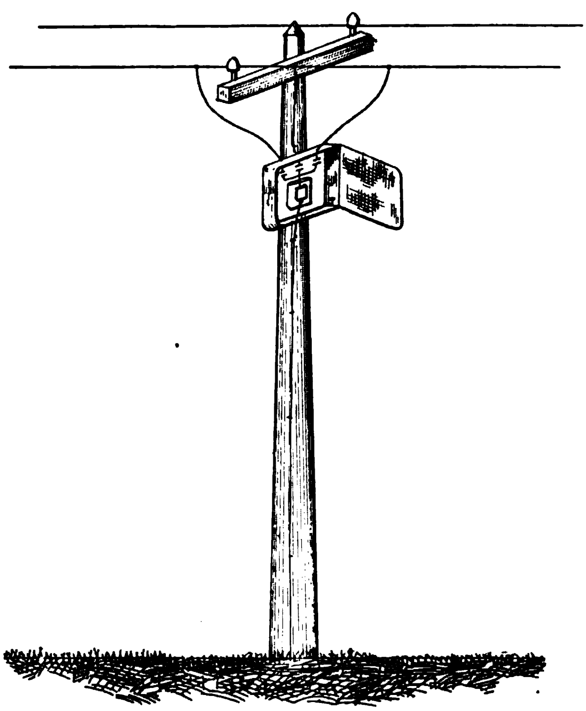


FIG. 34.

glass or unpolished marble still further reduces the breaking down strain.

But, to avail myself of this fact in the construction of a lightning arrester was a problem over which I pondered for some time. My design was to bring electrodes located in a discharge surface, near enough together to form a lightning arrester spark gap, and at the same time to avoid the passage of dynamo current when the electrodes were connected to the terminals of a

500-volt direct current generator. I reasoned that a dynamo arc to be maintained must be fed by the vapors of its electrodes. To prevent an arc, the formation of such vapors must be suppressed. My first attempt in this direction proved successful. I drew a pencil mark about two inches long over an unpolished piece of marble, covered this with a second piece similar to the first, and, having previously slipped between the two marbles, pieces of aluminium foil as terminals to the pencil mark, I bound the whole together with twine. These terminals were now connected to the terminals of a five hundred-volt direct current generator and disruptive discharges caused to pass be-

FIG. 35.

tween the marbles and over the pencil mark. A one-ampere fuse was connected in the dynamo circuit. No current following these discharges, the terminal foils were brought successively nearer together, testing each time for the passage of dynamo current. When the electrodes had reached a distance of a quarter of an inch from each other, the fuse was blown. The terminals were now placed a half-inch apart, and oft repeated tests failed to establish a short-circuit. The dynamo was now cut out, and upon resting my hand on the upper marble, while the discharges were still passing, I noticed a considerable mechanical shock, and when the twine was removed, the upper block was thrown off. In consequence of this, it seemed advisable to provide more space for the discharge. A small groove was, therefore, cut in the lower marble from one electrode to the other, and well blackened with a lead pencil mark. Discharges now failed to produce the above mentioned mechanical shock, or to throw off the cover. It was, however noticed, that after several discharges had passed, this pencil mark disappeared, having been dissipated and apparently scattered over the surfaces of the two marbles. To overcome this difficulty a piece of wood was laid into the lower marble between the electrodes, and into this a shallow groove was burned. This construction, which is shown in Fig. 30, seemed to possess some advantages over the one already described—it suggested more lasting qualities. Various materials were now tested into which one or more discharge grooves might be burned, such as fiber, felt, leather, ivory, boxwood, celluloid and others, but most of these proved unsatisfactory for various reasons. With fiber, the charred surface was quickly worn away, ivory chipped off in small pieces, both leather and felt crumbled away. Lignum vitæ, however, proved to be a most excellent material. In the final form adopted for this arrester, both upper and lower blocks were made of this material, thus enabling the discharge grooves to be burned into the lower block itself, and avoiding the necessity of inserting a discharge piece between the metal electrodes. This arrester for station use, is illustrated in Figure 25; for line use in Figure 26. The blocks are 3 inches wide by 3½ inches long and 1 inch thick. Two brass electrodes each 1 inch wide are laid into the lower block, flush with its surface, the distance between electrodes being ½ inch. The charred grooves are nine in number, and about $\frac{1}{16}$ inch wide by $\frac{1}{32}$ inch deep.

Another and more simple form of this arrester was also constructed. A hole was bored through a solid block of marble, the center of which was then filled with a cylindrical block of hard maple, having grooves burned in on the sides. Solid brass cylindrical electrodes were then inserted in either end, making a snug fit. But in practice, these marbles were in many cases split open by the expansive force of the discharge, thus demonstrating the necessity of the vent which is obtained by clamping two surfaces together, and between which the discharges may pass.

This device, like the condenser arrester, constitutes a strictly discriminating lightning arrester. In regard to its action I have been asked why a discharge should find an easier passage across a conducting film, than through a non-inductive conductor having the same ohmic resistance as the conducting film. My conception of the case is as follows: When a discharge passes through a conductor of more or less ohmic resistance the time of discharge is considerable, there is a great strain from all parts of the charged surfaces during the time of passage, and there will be a tendency to discharge or "side flash" along paths normal to the conductor. The passage of the discharge may be likened to the passage of tangible matter through a dense fluid —there is a gradual yielding of the opposing resistance, but nothing is broken. When, however, a discharge passes over a conducting film as described, there is a sudden breaking down or giving way of the dielectric, and this is aided by the presence of the conducting film. The film, however, does not act in the sense of what is commonly called an electric conductor, but as a wedge, splitting the dielectric preparatory to the passage of the disruptive discharge.

The difference between these two cases is very clearly illustrated by the discharge of a Leyden jar provided with a pith ball electrometer. Take the arrangement shown in Fig. 27; while the machine is charging the battery of jars, the increasing charge can be observed by the deflection of the pith ball, until finally a disruptive discharge takes place across the arrester or spark gap A, and the pith ball is seen to fall back suddenly, indicating in this manner complete discharge. If, however, the gap A be removed and the discharge be caused to pass either through the inductive coil C, or in its stead a non-inductive high ohmic resistance, the pith ball at the moment of discharge will fall part way only, indicating thereby an incomplete discharge. The sound of the discharge in the latter case is also very different from that in the former—it is more like a thud, suggesting opposition to the passage, whereas in the former case the sound is that of a crack, indicating something instantaneous and complete.

The non-arcing property of the arrester is easily understood; the conducting vapors which are necessary to the formation and maintenance of a dynamo arc are suppressed by the cover which fits tightly over the metal electrodes. In Fig. 28 the arrester is seen in operation. The terminals of the arrester are connected to the terminals of a 500-volt direct current generator. The cover is of glass so that the discharges may be seen. In the upper part of the figure, five 100-volt lamps in series indicate the dynamo pressure; just below this and to the left is a long horizontal 5-ampere fuse; to the left of the arrester is a spiral choke coil connected in the dynamo circuit, and interposed between the arrester and the generator. This coil is very similar to those which I used in my Colorado experiments. The disruptive discharges immediately below the arrester, represent the means used for suddenly charging the dynamo circuit. The discharges across the arrester are distinctly seen through the glass cover. When, however, this cover is removed, the first discharge establishes a short-circuit and the fuse is instantly blown, as seen in Fig. 29.

The relative ease with which a discharge will leap over such a surface in preference to a few turns of large copper wire, is illustrated in Fig. 30, which is taken from a photograph. Fig. 31 shows the arrangement of the apparatus more clearly. A is an arrester such as I have been describing and is provided with a clear glass cover so that the discharges may be seen. C is a choke coil connected in parallel to A, it is 16 inches in diameter and has 17 turns of No. 0 copper wire. Discharges from a battery of Leyden jars invariably pass at A in preference to the path C, as shown in the photograph.

A thorough test of these arresters has been made in both Denver and Colorado Springs during the past summer. Fig. 32 represents the equipment used in Denver. There were ten feeders, each feeder being provided with three arresters placed three feet apart. These are indicated at 1, 2 and 3. The object of using three arresters to each feeder was to avoid nodal points. S represents the circuit-breakers; T is a tank arrester; S 1 the discharge circuits of the tank arrester and 4 represents spark gaps in these discharge circuits. None of the arresters were provided with tell-tale papers, and although they were carefully watched during thunder storms, no indications were observed of passing discharges. After the first storm following their installation the writer removed the covers of the upper ten, and of these, six showed marks of discharge; the remaining twenty were constructed on the "solid marble" pattern and were consequently difficult and unsatisfactory to inspect. One of the upper ten arresters was subsequently dismounted and photographed, and Fig. 33 is an excellent copy clearly showing the smudge formed by the discharge over the marble surfaces.

The only mishap which occurred to any of these arresters was the explosion of one of the "solid marble" type. No experimental line arresters of this type were used on the Denver plant. In Colorado Springs, however, 25 were installed on the line and 24 in the station. Some of these were of the divided kind (see Fig. 25), others of the solid marble pattern. Nearly all of the latter were split open by the discharges, while of the former none was damaged in the slightest degree. One of the line arresters was provided with three connections to one feeder, with a spark gap and tell-tale paper in each discharge circuit. This arrangement is shown in Fig. 34. The connections to the feeder were made about two feet apart. After thunder storms each tell-tale paper was found to be perforated, but whether the discharges occurred simultaneously or successively is not known. The perforations shown in Fig. 35, were taken from this arrester.

This non-arcing railway lightning arrester is eminently suited for the protection of direct current circuits up to 1,000 volts. On 1,000-volt alternating current circuits from smooth body armatures it also operates satisfactorily, but on similar circuits from toothed body armatures, the arresters break down after a few discharges, and a short-circuit is established. However, it is not impossible that two or three of these arresters might not be used to good advantage in series, on circuits of high potential.

The especial advantages of this arrester may now be summed up as follows:

1. It offers a direct and non-inductive path to earth.

2. It is absolutely non-arcing and consequently requires no attention after being once properly installed.

3. It has no moving parts and there is nothing about it to get out of order.

4. It is small, and therefore easily installed under a car.

5. It is cheap, and can therefore be used in large numbers on the line, on the cars, and in the station.

6. Its non-arcing property avoids danger from fire, which property also ensures the non-interruption of the system due to blowing of fuses and constant throwing of the circuit-breakers.

7. Its simplicity and reliability will commend it to every one.

In my closing remarks I wish to answer the now old and oft repeated question "How many, and what kind of arresters would you advise us to use on our line?" as follows: The proper number of line arresters, is that number which will prevent discharges from entering the station. Station arresters, if used at all, should be installed with the one idea of providing a final opportunity for discharge, but as the tendency to discharge occurs on the line, often several miles away from the station, a sufficient opportunity should be provided *there* where the tendency to discharge originates, and in this manner advantage may be taken of the large inductive resistance afforded by the circuits leading back to the station. If discharges enter the station it is a sure indication that they have done so for lack of sufficient opportunity to discharge from the line—for lack of a sufficient number of line arresters.

It is well to protect dynamos with lightning arresters—it is better to avoid the necessity of this protection by discharging the line.

It is even well to use a lightning arrester having a circuit interrupting attachment. It is better to avoid the necessity of a circuit interrupting attachment by using a non-arcing arrester. In fact—

An ounce of prevention is always
Better than a pound of cure.

Discussion in Philadelphia.

Mr. Joseph Sachs:—Mr. President, I would like to ask Mr. Wurts if he has ever tried the choke coils without the arresters upon the comparatively low potential circuits in Colorado; and whether the action upon a low potential circuit would not have been just the same as the proposed action on a high potential—that is, leaving out the arresters altogether and only using choke coils on the line.

Mr. Wurts:—I have not made any such experiments, but should expect to find the action of these coils the same on a low potential system as on a high potential system. On a low potential circuit, however, the insulation of the apparatus being inferior to that found on a high potential circuit, it is possible that a system of choke coils might not sufficiently protect the armatures and other translating devices from the electric strains, which would be set up by the surgings. In other words, it is expected that with very high potential systems, such as are now being talked of in connection with long distance transmission, the surgings may be broken up and rendered harmless to the very high grade of insulation which would be used in such systems. On the other hand, with low potential systems, it might be that the number of choke coils found necessary for efficient protection would be prohibitive. This system of protection seems to be more particularly adapted to long distance power transmission circuits, where there are no translating devices intervening between the extremities of the system. Referring to my tests with a combination of choke coils and spark gap discharge circuits, it is interesting to compare the results which I obtained in Telluride and in Denver. In Telluride I used four choke coils with most satisfactory results. In Denver I used two coils, and found that these did not altogether protect lightning arresters which were connected nearer the dynamo, and these results, obtained in Denver with two choke coils, were verified by the results obtained in Telluride with four choke coils, inasmuch as with the four coils in series, some of the discharges reached the third and fourth discharge circuits; that is, passed beyond the first, second and third coils.

Mr. Sachs:—It would seem to me, that if we had enough choke coils in the line, the effect of the disruptive discharge could be chopped down so low that it would not do any harm to the insulation of the machine at all, and that there would not be potential enough to jump any gaps. As I understand the theory of the use of the choke coil used here, it simply acts as a buffer against the discharge. The discharge comes along and strikes the buffer; part of it passes on, and the part that passes on is very much less than that which initially struck the buffer, and so on until you get practically a very small amount. Now, if you went on far enough and had choke coils enough, you would get down to an amount that would not hurt your armature at all. You would get down to practically nothing, and in a case like that, you would not need any spark gap whatsoever. But another question arises: the use of these various inductances in the line upon a system of alternating currents would, in my mind, have some effect, perhaps very small, upon the line, and the question I think to be decided would be, what the relation would be between the point where you got no discharge, by putting in enough choke coils, and the effect upon the impedance of the line.

MR. WURTS:—I have not tested in practice this system of protection against lightning discharges, but am hopeful that such a system will prove successful. In regard to the self-induction of these choke coils, I do not think that they will interfere seriously with the operation of an alternating current system at 7,200, or even at 16,000 alternations per minute, because no iron core is used and the number of turns is small. Referring to my paper, it will be noticed that I employ only seventeen turns.

MR. SACHS:—The point, however, would be, if the 17 turns were multiplied by the number of choke coils, you would have that many more turns, that is, if you put in enough choke coils to kill your discharge entirely. Your 17 turns might increase to five, six, seven and eight, etc., times 17. In reference to the iron, it would appear to me that if the iron core could be laminated and insulated to a very fine point so that the induced currents in this secondary were brought down to a very small current, that your iron would really help along the impedance of the choke coil. The actual results that you obtain now, would appear to me inferior, because the iron is not sufficiently laminated and insulated, and the currents set up in the iron bring down the induction in your primary, and, therefore the result is not as good as without the iron. If, however, your iron wires could be made sufficiently small, and insulated so that the current induced therein would be infinitesimal, why, then, I should think that the iron would actually help you. It may be, however, that this cannot be practically attained on account of the very high potential induced in the core, and the necessity, therefore, of extremely small and well insulated wires.

MR. WM. STANLEY:—I have been greatly interested in this paper, particularly because of the logical demonstration that Mr. Wurts has given us. It seems as though the paper were one continuous line of logical reasoning from one end to the other. It seems to me that he has shown to us more new and interesting experiments with lightning discharges than we have ever known before. The subject of lightning discharges must be viewed from the lightning discharge standpoint. In working with lightning discharges we are not using alternating currents that we know very much about. We are familiar with currents of, perhaps, 75 periods, or something of that sort, but the lightning discharge currents are of enormous frequency. Of course it is true that with the ordinary frequency the self-induction of the coil would be increased by winding on it additional turns of wire, or by adding to it an iron core. But if the frequency be increased 10 or 50 or 100 times, the iron core may be considered as consisting of iron wires 10, 50, or 100 times larger. In other words, the local currents in these iron wires are 10, 50, or 100 times more important, and the self-induction of the coil may be largely impaired by the foucault series. It seems to me, however, that Mr. Wurts might have given us a little clearer statement regarding the discriminating properties of the apparatus on page 341. It is a charming diagram to me, for the arrangement shows that each one of these by-paths is tuned to a particular rate of alternation; that if the lightning discharge be surging on the line, the first by-path will be tuned by its spark gap, self-induction and capacity, to one frequency of the discharge, the second to another frequency, the third to another frequency; and, so it seems to me, Mr. Wurts takes through each one of these by-path spark gaps, a certain amount of the discharge current. That is to say, he takes the surging current and splits it up into a number of different frequency currents, and then carries each of them off to the ground in its particular path, if I correctly understand the diagram.

The self-induction induced into the line by the choke coil practically amounts to nothing. I have not calculated it, but I should imagine it would be on the coil shown, perhaps, a couple of volts with 50 or 60 amperes of current passing or something of that sort, with the ordinary frequency of 72 periods, and the self-inductive E. M. F. with a high potential system of three or four thousand volts would amount to nothing at all.

Another point that interested me particularly was the diagram at page 355. I do not think that the straight lines drawn on that diagram, are a fair exposition of Mr. Wurts' work, for they seem to show that one could wind a choke coil, adding turns to it and increase the self-induction of the coil up to a certain point, and then that the self-induction could not be farther increased by adding more current. Now the dotted lines on that diagram show that that is not so. It shows that that knee at the top of the angle is really a curve, and that really there is an increase of self-induction by adding more turns. Is not that so?

MR. WURTS:—I think it is to a slight extent.

MR. STANLEY:—If a straight line is a correct demonstration of the results, then we have a saturation for air. If the curved line is the correct demonstration, then we have, I think, the ordinary results found before. I would like to ask Mr. Wurts one question. In 1885 or 1886 I built an alternating line, and found I had to protect it from lightning; so instead of putting my wires underground, I put my ground over my wires, and strung a ground above my line, grounding it every pole or two; and obtained very satisfactory results. We had no trouble whatever. Now, is it not true that in this part of the country, leaving out of consideration Colorado and high altitudes, is it not true that very excellent results in line protection may be obtained by simply stringing a wire above the line, and so forming a belt of earth potential above the wire without any further apparatus.

MR. WURTS:—I cannot say that I have had any considerable experience with the use of overhead ground wires as a means of protecting aerial lines against lightning. I am, however, familiar with the results obtained in two places; one on Staten Island, where the overhead ground wire has been used successfully; the other at Telluride, where it has proved a total failure. Upon making inquries at the Staten Island plant, I learned that previous to the use of the overhead ground wire, discharges entered the station frequently and with violence, but that after the overhead wire had been erected, although the discharges continued to enter the station, they were less frequent and much less violent. In Telluride the overhead ground wire failed; the reason may possibly be this: If the wires become charged, as I am inclined to believe they do, by contact with the surrounding charged atmosphere, and if the wind be blowing across the length of the wires, then both wires will tend to drain the atmosphere of its chárge, but the drained portion of the atmosphere belonging to each wire will have a cross-section in the shape of a wedge, the direction of the wedge being that of the wind and more or less horizontal, so that the drained portion of the atmosphere caused by the overhead ground wire will fail to include the electric wire. If a charged atmosphere be kept rapidly moving over a well-insulated electric wire, it is probable that the charging from the atmosphere will exceed the loss due to leakage.

MR. CHARLES HEWITT:—Some years ago the Edison companies made experiments with overhead grounded wires, using them as guard wires. They were put up primarily as guard wires, but the intention afterwards became to use them also as lightning protectors, and a certain gentleman took out a patent, using a barbed wire, similar to barbed fence wire, in the hope, I suppose, that the points of the barbs would pick the current out of the air and carry it off. Mr. W. J. Jenks has given a good deal of study, I think, to that very question. I hope he will say something before the discussion closes. I would like to ask Mr. Wurts, however, whether it is necessary to connect a lightning arrester to each one of these coils on page 341. What would be the effect of connecting, to say one arrester, connecting one arrester between each coil? We have here in Philadelphia, on the traction company's plant, not only the question of protecting generators, but the question of protecting a great many miles of valuable lead cable, which is quite as important, if not more important than the question of generators, and the problem is simply this: We have miles of lead cable coming out of the ground, say, at one or two points. On one traction company's line we have a cable coming out at four points, but that is unusual. Most of the cables come out at two points. As I understand the problem from Mr. Wurts' paper, we must get the lightning arresters between the trolley and that cable if we want to protect it. I may be mistaken about this. It may be we can place our lightning arresters on the poles along the trolley line and in that way avoid these nodal points; but it seems to me that it will be necessary for us to get our lightning arresters in between the cable and the trolley wire. In that case we would need four coils and four lightning arresters on one pole, and on an iron pole it would be quite unsightly and a rather difficult thing to put up, as we haven't much space.

Mr. Wurts:—I think the gentleman has correctly understood my ideas in this matter. If the system of protection, illustrated in Fig. 3, is to be used, the spark gaps and choke coils must be connected between the trolley and the underground cable. Ordinarily, and with a reasonably high grade of insulation, I believe that electric systems can be protected by a liberal distribution of line arresters; that is, a large number of opportunities for discharge should be provided, so many, in fact, that the few opportunities for discharge afforded by the translating devices shall be small, in comparison with the number provided by the lightning arresters. It is, however, well known that underground cables are particularly sensitive to the electrical surgings set up during thunder storms, and that they are liable to be punctured, so that where cables are to be protected, I unhesitatingly recommend the use of four choke coils in series between the overhead and underground system, together with four lightning arresters, connected as shown in Fig. 3. My experiences in Telluride and in Denver have led me to believe that a fewer number of coils would be insufficient. For the proper protection of cables it may even be possible that five or six coils will be found necessary; for it must be borne in mind that the insulation which I protected in Telluride was of a very high grade, much higher than is ordinarily found in underground cables.

Mr. Hewitt:—What would be the effect of connecting the one arrester? Has it ever been tried? Or what would you suppose would be the effect?

Mr. Wurts:—I think the result would be that the discharges would frequently enter the dynamo armature. In Telluride I had four choke coils in series, together with four opportunities for discharge, as indicated in Fig. 3, and as the majority of discharges passed across the second discharge circuit, it is reasonable to think that if there had been only one discharge circuit, many of the discharges would have reached the generator.

Mr. Hewitt:—Don't you give four opportunities? You have four coils.

Mr. Wurts:—Yes; I provided four opportunities for discharge, but I understand your question to be: What would happen if only one opportunity were provided.

MR. HEWITT:—What I mean is, if there were four coils and those coils were connected to one arrester, what would be the effect? Wouldn't you practically get four opportunities?

MR. WURTS:—That arrangement would provide only one opportunity, and would at the same time short-circuit the choke coils.

MR. SACHS:—I would like to ask Mr. Wurts another question with reference to the iron wires. I would like to ask whether he tried various sizes of iron wires, and whether the iron wires were in intimate contact or insulated. I think these features make quite a difference. From what I could see of the wires, they seemed to be of rather a fair size and not insulated from one another. In reference to the idea proposed by Mr. Hewitt, it would appear to me that if you had choke coils enough to tone down the potential of the lightning discharge sufficiently, one gap between the machine and the first coil would take care of all the lightning that would be left.

MR. HARRINGTON:—Gaps are cheaper than choke coils, and it has been my practice, covering the last four years, to use a large number of gaps placed at many intervals on the line. In fact I place them at every tap of the feeders to the trolley wires and use the ordinary spark space with a fuse wire in series with it. I never had any trouble at all from burn-outs at the station using this method.

MR. WURTS:—Referring to the matter of introducing an iron core into these choke coils, the E. M. F. per foot of wire in the coil is so high, that no matter how finely sub-divided the iron may be, discharges will take place between the iron particles, unless, of course, these be thoroughly insulated from each other. The bundle of iron wires I have used as a core, is not made of very fine wire, but has served to illustrate what I wished to show; namely, that secondary currents are set up, and that these secondary currents cause the coil to offer less resistance to the passage of disruptive discharges. Dr. Lodge has shown conclusively, I think, that the mere presence of iron in a coil does not at all affect the resistance of that coil to the passage of disruptive discharges. His experiments were made by using a very finely sub-divided iron core boiled in paraffin.

MR. A. E. KENNELLY:—There are so many points of interest in this paper that one can scarcely do justice to it on first perusal. But some information Mr. Wurts might give us upon those points would, I think, have important bearing upon their application. For example, on pages 354 and 355, Mr. Wurts alludes to experiments made in the laboratory, with such choke coils as those now before us, and where he is experimenting upon the number of turns that are desirable to produce the best effect, and he says he has found by experiment that the impedance does not continuously increase with the number of turns. Diagrams are then given on page 355 to supplement that view. The method of testing is a very ingenious and a very pretty one. But I think he means impedance there, in the practical sense of what takes place in that particular circuit under the particular conditions, and he does not mean that the impedance of the coil as actually represented in ohms did not continuously increase. I think there is no question that the impedance, as measured in ohms of such a flat coil, would very continuously and very markedly rise, as the number of turns was continuously increased upon the outer edge; but the experiments seem to show conclusively that the practical impedance, or that the effect of arresting the discharge through them, did not appreciably increase with the electrical impedance of the coil beyond a certain point; that is to say, the sparks approached a certain ultimate rate by an approximately straight line. Of course, we might expect that if the conditions of the experiment had been varied, if the Leyden jar had been altered in size, or if the resistance or inductance of the path from the Leyden jar had been altered in any way, that that condition of practical impedance might have been affected, and it would be interesting to know how far those changes were attempted or carried out by Mr. Wurts.

Secondly, I differ from Mr. Wurts in his view concerning the reflecting points on such wires carrying currents, for power or other purposes. If I understand the matter correctly, when any electrical impulse, whether it be a discharging impulse at very high pressure from a lightning disturbance, or whether it be a telegraphic signal, or whether it be an alternating current, whatever the impulse may be, it must travel continuously along to the end of that wire, unless it is met by some change or want of uniformity in the wire itself; a change in the size of the wire, for example, or the insertion of some resistance or apparatus; if there is no discontinuity in the wire, no change in its uniformity, the wave must, unless totally absorbed, run on until it reaches the terminal and be reflected there, and that when it is reflected there, if I understand the matter correctly, there is no piling up of pressure in the act of reflection. We know that when water is running through a pipe and is arrested at the end of the pipe in the manner mentioned by Mr. Wurts, that an accession of momentum is reached at the end, and that the stress of the walls of the pipe at a closed end may be very considerable; but since electricity is not matter and cannot posses inertia, and the reflection of an electric wave does not, therefore, embody the idea or effect of inertia, there ought to be, if I understand the matter correctly, no greater stress upon the insulation on the end of the wire than at any other intermediate point. On that account I should interpret the results as due to the effect of the combination of a great number of wave systems, and where these waves are superimposed upon one another at different parts of the line, their crests will meet at some places, and oppose at other places, and where they happen to coincide, their pressure will be superposed, and those points Mr. Wurts has, I think in mind, when he speaks of reflecting points, which might be called anti-nodes, because at the nodes there would be an absence of pressure.

One point mentioned by Mr. Wurts, which is a very interesting one, is that that particular line in Colorado, in spite of the fact that they have an overhead wire erected for the purpose I presume, of carrying off electrical disturbances from lightning, failed to be protected thereby, and it was necessary to apply some of these interesting remedies here in order to give adequate protection. It is the first occasion that I have noticed of the influence of an overhead wire being insufficient for the purpose of lightning protection. I had an opportunity of examining statistics last year from different stations in this country and also in Europe, and in every case where an overhead wire had been employed either directly for the purpose, or even incidentally—as, for example, where a power circuit ran underneath telegraph wires continuously all the distance—no electric damage and no electric troubles from lightning were found in the circuits so protected. But, of course, Colorado is an exceptional country for lightning, and discharges occur there, I believe, when there is no cloud in the sky, and no rule such as might ordinarily apply to lightning protection would apply to Colorado. But it would be interesting if Mr. Wurts would tell us a little further about that overhead wire, whether it was grounded at frequent intervals, how far it was from the wires protected, etc., because I had the belief very firmly in my mind, from the statistics before mentioned, that an overhead wire, when frequently grounded, was an adequate protection against lightning. Furthermore, I do not quite catch Mr. Wurts' view that the danger from lightning is due to picking up electricity from the air. Mr. Wurts himself shows, from experimental evidence, that only about a hundred volts or so in pressure could be so picked up from the air in the vicinity of an ordinary aerial conductor.

If it is 3,000 volts at the top of Washington Monument, and 10,000 at the top of the Eiffel Tower, it cannot be much more than 100 volts at the top of an ordinary pole. That is not an alarming pressure. Furthermore if the wire is grounded through the apparatus continuously at one or both ends, the tendency will be to carry off any charge so accumulated. Surely the trouble from lightning is due to the accumulation of the static charge upon the surface of the wire, which is very suddenly liberated when the original lightning disturbance in the clouds or in the neighborhood passes by, when the spring, as Mr. Wurts characterized it, is released. So I should not be inclined to suppose that the wind blowing over the surface of the wire would render it any more liable to trouble from lightning than if the air were perfectly quiet. At least if it is so, I do not know of any evidence to that effect.

Mr. Wurts:—Mr. Kennelly has brought to the surface so many interesting points that I am at a loss where to begin, but first I wish to thank him for explaining so much more clearly than I was able to do the results of my experiments with choke coils as illustrated on page 355.

In regard to the increased tension at the end of a wire, I would say, that I have obtained many of my ideas on this subject from Dr. Oliver Lodge, whose brilliant researches teem with novel and unexpected results. Dr. Lodge, distinctly states that he has found a much greater sparking distance at the ends of wires than at any intermediate point.[1]

The overhead ground wire at Telluride, as I remember it, was suspended three feet above the circuits to be protected. The circuits were run on either side of a single cross-arm and the ground wire was grounded at every other pole. The lengths of the circuits ranged from three to ten miles. The overhead wire was of course not connected to ground plates, which would be impracticable in a case of this kind, but ground connection was made by means of a bare galvanized iron wire run down the pole and then forced into the earth as far as possible. This was the method adopted by the system I referred to on Staten Island. There, according to their statement, the overhead ground wire

1. Pertinent to this matter of increased tension at the ends of wires, I quote from Dr. Oliver Lodge's work on "Lightning Conductors and Lightning Guards," Chapter 10, under the sub-title "Experiment of the Recoil Kick" as follows:

"These electrical oscillations are of considerable interest, and have sundry practical bearings; let us proceed to make them more conspicuous. Fig. 35

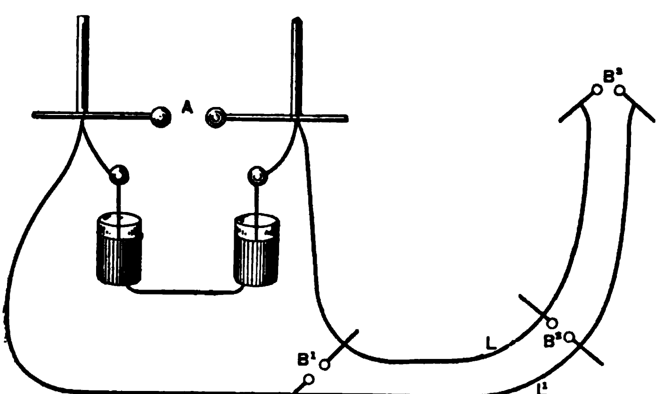


Fig. 35.

shows a couple of long leads, L and L′, reaching round the room (No. 18 wire in two 95-feet lengths was actually employed), insulated from one another and seemed to work satisfactorily, but here it proved, to my certain knowledge, a total failure. What was the other point you mentioned?

MR. KENNELLY:—Did you vary the conditions with that choke coil?

MR. WURTS:—I did, yes, but the range over which I was able to work with my apparatus was so small that I could detect very little difference in the results. When I increased my spark gaps to any considerable extent the discharges failed to pass, due to the unavoidable leakage of my lines, jars and influence machine. I believe you asked me one other question.

MR. KENNELLY:—I think you have covered it.

MR. STEINMETZ:—Mr. Chairman and gentlemen, I think it is very gratifying to see how much progress we have made in the last years in the protection of our electric circuits from lightning; from the old lightning rod, which was expected to have the kindness to take off all lightning discharges, to the modern well improved forms of lightning arresters.

The main difficulty we had to labor under, and have still, in protecting lines from lightning, is that we really know comparatively little, as to what lightning is. In the name "lightning" we probably include quite a number of different electrical phenomena of the atmosphere, phenomena which act differently and consequently cannot be guarded against by the same means.

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from the earth, but attached to the two poles of a machine; the machine having also a couple of Leyden jars attached to it in the given arrangement of main discharge circuit, A, the customary manner when supplied by the maker. A discharger, B, can be arranged to bridge the gap between these leads, either near the machine, as B_1, or about the middle, as B_2, or at the far end, as B_3. Now, of course, sparks can be obtained either at A or at any of the B knobs, and all about the same length; but supposing the A knobs to be brought nearer than the B knobs, the spark would be expected to occur at A only. Nevertheless on trying the experiment, one finds that every time a spark occurs at A, a longer spark occurs at B; it is as it were, precipitated with a rush; and the longest spark of all is obtainable at the far end, viz. at B_3.

"Here are some figures, in the obtaining of which, however, for convenience of manipulation, the B length remained constant in each position, and the least length of the determining spark, A, was the thing observed:

Nearest	Spark length, A	3.20	4.15	5.12
position.	Corresponding spark length, B_1.....	3.22	4.80	6.18
Middle	Spark length, A	1.92	2.37	2.70
position.	Corresponding spark length, B_2	3.22	4.80	6.18
Furthest	Spark length, A	1.60	2.2	2.45
position.	Corresponding spark length, B_3	3.22	4.80	6.18

The electricity in the long wires is surging to and fro, like water in a bath when it has been tilted; and the long spark at the far end of the wires is due to the recoil impulse or kick at the reflection of the wave."

I have also made some interesting experiments bearing on this question, not with the idea, however, of discovering anything new, but rather to familiarize myself with these curious and interesting phenomena already pointed out by Dr. Lodge. The following is an extract from an article which I published a year or two ago, and which will serve not only to emphasize the results obtained by Dr. Lodge, but will also lay stress on some of the remarks I have already

First you have the direct lightning stroke: A disruptive discharge takes place between the cloud and the ground, and your electric circuit, or the power station, is in the direct path of the disruptive discharge. I am glad to agree with Mr. Wurts, that this is a very infrequent phenomena. I am inclined to think, however, that if it takes place, it matters little whether you are protected by lightning arresters or not; the only thing you can do is to trust in Providence and repair what is left of the station.

But there is a second form of lightning, that is the return stroke or secondary stroke. If a lightning discharge takes place, or any other disruptive change in the electro-static condition of the atmosphere, its electro-static field is unbalanced, hence, readjusts itself, and independent secondary lightning discharges take place everywhere in the denser part of the electro-static field.

Whenever lightning strikes, in all conductors parallel to the stroke, secondary currents of very high potential are induced, which may do a lot of damage if not properly guarded against by lightning arresters.

But there is still a third and very different form of lightning, which is frequently overlooked. Consider an electric trans-

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made in connection with the best means of protecting electrical apparatus from the damaging effects of electric discharges:

"It is well known that disruptive discharges form nodal or neutral points along the line, and that at these points there will be little or no tendency to discharge. If a single arrester be connected to the line it may, or may not, discharge, according to circumstances; a certain discharge may take place across the arrester, or it may pierce the insulation of an armature, or it may even do both at the same time. In fact these nodal points are not fixed, but

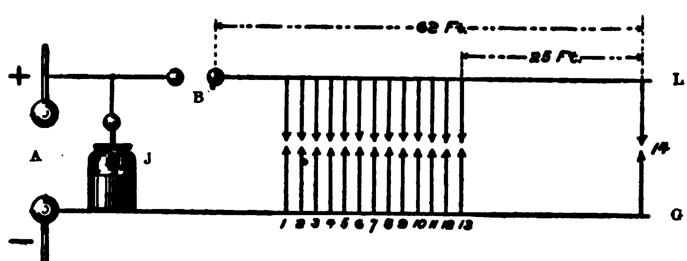

FIG. 36.

are constantly changing their positions. Referring to the accompanying diagram, (Fig. 36), let A represent the terminals of an influence machine; J, a powerful battery of Leyden jars; B, a large spark gap; L, is a wire which may represent a trolley line; G, a second wire which may represent the ground; 1, 2, 3, etc., up to 13, are $\frac{1}{32}$ of an inch spark gaps placed 12 inches apart and connected between wires L and G. These may represent as many lightning arresters connected to the line. The connections and distances are all clearly indicated. If now the influence machine, A, be set in motion, the battery, J, will become heavily charged, till finally a violent disruptive discharge will occur mission line. With regard to lightning potentials, this line is connected with the earth, because there is no perfect insulation at these voltages.

If, now, a thunder cloud whose potential is very different from the potential of the earth, moves over the ground, the cloud and the ground will form the two coatings of a condenser, and thereby, accumulate very high opposite charges. As often as discharge takes place between a cloud and another cloud, or the ground, the cloud is suddenly discharged, and thus, the ground also. The charge of the line, however, which forms a part of ground, can not disappear suddenly, due to the high insulation between the line and the ground, and the difference of potential between line and ground thus suddenly rises enormously, the charge in the line not being balanced now by the opposite charge of the cloud. It is the same phenomenon as known from the electrophorus of the physical laboratory, where by withdrawing the charged metal cover from the resin plate, the potential of its charge is raised.

Now, this phenomenon of a lightning stroke, due to the sudden release of the gradually accumulated electro-static charge in the line is by no means infrequent, and it is the case where

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at B, and thus suddenly charge the line, L, with a different potential from line, G, so that there will now be a tendency for a disruptive discharge to take place between lines L and G, and in fact this discharge will pass at one or more of the spark gaps, 1, 2, 3, etc. These discharges occur in a great variety of ways, the following being a few examples: A single brilliant spark may pass at any one of the first ten gaps. Two, three, and occasionally four simultaneous sparks, variously located, will pass between one and ten; sometimes these occur at successive gaps, but they are more often scattered. In some instances the discharges are of equal brilliancy, in others one or two intense sparks would pass one or two gaps, together with a much fainter discharge or scarcely perceptible spit at any of the other gaps included in the first ten. A discharge never occurs across any of the last three gaps. Four of the first ten gaps are then increased to $\frac{10}{32}$ of an inch, with the result that the discharge frequently selects one of these larger gaps in preference to any of the others. Occasionally, however, sparks will jump one of the larger gaps, together with one or two of the smaller ones, and so an indefinite number of combinations can be produced with results emphatically demonstrating that with several spark gaps there is no certainty that the discharge will take place across any particular one of them.

"A $\frac{1}{32}$ of an inch gap is now connected at 14, and with every discharge at any one of the first ten gaps a faint, thread-like spark will pass at 14. It can hardly be called a disruptive discharge, and is not of a character likely to do damage. This minute spark is due to what Dr. Lodge calls the "recoil kick," that is, the electric waves strike the ends of the wire and recoil with increased intensity. When the gap at 14 is increased, these thread-like sparks become less and less frequent, till finally, when the gap equals $\frac{17}{32}$ of an inch, they cease altogether. However, when only 1 and 14 are connected to the lines, brilliant discharges frequently occur at either one or the other, or at both.

"Now, assuming that the above conditions and results correspond in many respects to those found in actual practice, the lesson to be learned is a simple one: Electric light and power circuits, should be provided with spark gap lightning arresters at frequent intervals along the line. Four or five to a mile of wire would seem to be sufficient in a majority of cases; in others a fewer number might suffice. A spark gap arrester should certainly be connected at either end of each line. An electric circuit well equipped with properly constructed line arresters should have little to fear from disruptive discharges."

the grounded wire fails to protect, because the grounded wire is nothing but a parallel line which will be charged by electro-static influence at the same time, and will discharge simultaneously with the main line, without in any way relieving the main line.

Against the direct or secondary lightning stroke, however, the grounded wire will protect more or less.

I think this is the reason that such different experiences have been recorded with the grounded wire, and indeed with any means of lightning protection.

Thus, before trying to protect lines from lightning, we first have to learn what lightning is, and then to see how we can guard against secondary stroke, how against the line discharge, the direct lightning stroke, etc.

If a disruptive discharge enters the line, electric oscillations will be set up therein. The frequency of these oscillations depends upon the capacity and the inductance of the line; and the number of cycles which take place before the energy of the discharge is dissipated, that is, the rate of decay of the electric oscillations, depends on the resistance of the line. We can thus easily determine what will be the result of introducing choke coils, that is, inductance, or resistance into the line.

If you put in a choke coil, it will increase the impedance of that branch of the circuit passing through the choke coil, and therefore offer a certain inducement to the lightning stroke to take the other path. But even if you have a very large choke coil, the impedance is not infinite. Consequently a part of the stroke will go through, and even if you take 99 per cent. of the lightning stroke off safely, the last fraction which is left may be quite enough to cause any amount of damage. But there is another action of these choke coils: the frequency of oscillation of the circuit is lowered, and hence the danger reduced, because the foremost danger of lightning is its secondary action, and by reducing the frequency by the insertion of self-induction you reduce this, which in itself is a protection.

If an oscillating current passes through the line, the space around every coil of wire becomes an oscillating magnetic field of force, and this magnetic field of force will induce in the same wire, or in any other wire in the neighborhood, electromotive forces, and there you have a second secondary current produced, which may do harm, while the lightning proper, or the primary current may have gone over the spark gap and disappeared harmlessly. So the choke coil, instead of protecting directly, occasionally produces a return stroke.

With regard to the way of protecting from lightning by introducing resistance, that is, by increasing the rate of decay of the electric oscillations as far as possible, or to make the current altogether a periodic, that is allow no oscillations to be set up at all, I think it is only just to call attention here to an old paper read by Mr. Hodges, before the AMERICAN INSTITUTE OF ELEC-

TRICAL ENGINEERS, at the meeting of April 21, 1891, where he obviously starts from the same idea of offering the lightning work to do, by the interposition of a very high resistance, and thereby to dissipate the energy of the lightning.[1]

For merely sending the lightning stroke to the ground by the old lightning rod does not offer complete protection. An oscillating current is produced which goes down to the ground, and up again, until the energy is expended.

The experiments of Mr. Wurts on the increase of impedance by the increase in the number of turns of the choke coil up to a certain number, and the constancy of impedance reached then, I do not think conclusive. If I understand Mr. Wurts rightly, he had an electric oscillator consisting of Leyden jars discharging through these choke coils, and measured the E. M. F. by spark electrometer. Let us see now, what will take place in such a system, if we increase the number of turns. The coefficient of self-induction—the inductance since yesterday—of the coil, will increase with the number of turns, there is no doubt about that; the inductance increases, but with increasing inductance the frequency in circuit decreases, because the frequency depends on the inductance of the circuit. Consequently the reactance as the product of the inductance into the frequency, will not increase proportionately to the inductance. But if you take another circuit of a different capacity, you will get again a different number of turns from the last. The increased number of turns does not offer an increased reactance, because the increased inductance is counteracted by the decrease of frequency.

Thus, increasing the inductance of the coil by increasing the number of turns, increases the reactance proportionately, only as long as the coil inductance is small compared with the total inductance of the circuit, and thus does not much affect the frequency.

In circuits of larger capacity, you will probably find the best inductance, much larger than in a circuit of comparatively low capacity, as the Leyden jar.

With regard to the phenomena described in connection with discriminating lightning arresters, I have made a few experiments, also at very high voltages. I used two brass plates with a sheet of mica between. The mica was projecting some inches over the brass plates. I exposed this apparatus to a voltage of 30,000 volts. At 30,000 volts, beautiful sparks were flowing over the edge of the mica, from brass plate to plate, but no current following. Then I reduced the voltage and reduced the distance from plate to plate over the edge of the mica. The phenomena was the same; brilliant, white sparks were passing, but no current was following down to about 5,000 or 6,000 volts, where the distance between the plates was still about one inch. But below

1. TRANSACTIONS, vol. viii, p. 159.

this voltage it failed to protect, but the spark was followed by the arc, and the transformer short-circuited. Here you find the same protection, a creeping discharge, as you may call it, protecting for very high voltages where the arc does not follow, but not protecting for low voltages where the arc does follow.

MR. WURTS:—In regard to the use of discriminating lightning arresters on high voltage circuits, I would say that I have used these lightning arresters on arc circuits up to 4,000 volts; that is, arresters very similar to the one I have just tested, except that for these high voltages I use two gaps in series instead of one. I have connected such an arrester to the terminals of a 4,000-volt arc machine, and then sent disruptive discharges through the arrester, across the terminals of the machine, as I have already shown in connection with the 500-volt lightning arrester, without any dynamo current following. The 500-volt lightning arrester operates successfully on a 1,000-volt alternator with smooth body armature, but with a toothed armature the arrester will break down after two or three discharges. With this 500-volt arrester connected to the terminals of a street railway generator, and sending disruptive discharges through the arrester, exactly as I have shown you, I have counted the discharges in a number of cases as high as 5,000. In some instances, however, the arrester will break down after 2,000 or 3,000 discharges; but, of course, it would require a great many thunder storms to reach even that number.

Mr. Kennelly, I remember now the point you brought up, and which had slipped my mind; that is, about the charging of the wire by conduction from the atmosphere. Many of my ideas are obtained through observation, and in connection with this point, would say that I have known circuits to become charged during perfectly clear weather, so that the discharges across a single lightning arrester occurred at the rate of 100 to 140 a minute. It seems to me that the charging of the wire in this case, must of necessity be due to conduction from the atmosphere.

MR. W. J. JENKS:—In the course of Mr. Kennelly's remarks, he expressed the opinion that the potential developed by a lightning discharge or a similar current from laboratory apparatus, was no greater at the end of the circuit, where the discharge found difficulty in escaping, than at other points on the line. I would like to secure his idea of what happened in a case which occurred in Massachusetts some years ago, where the facts are vouched for by Captain Wm. Brophy, the well known insurance inspector. In 1882 there was in connection with the telephone exchange, in Worcester, a line running from the central office, some three miles to the residence of a prominent citizen. There were five instruments on the wire, but the discharge manifested itself strongly at the lightning arresters of only two of these stations. The fourth from the central office showed decided blackening of the arrester plates. At the end farthest back into the country, and perhaps one mile from the point where the bolt appeared to fall, the great force of the charge was apparently expended; the telephone with its bell and bracket was torn from the wall and thrown several feet into the room. The inside wiring and ground connections were left intact, but the iron line wire, No. 12 B. W. G., absolutely disappeared for a distance of four stretches, the three poles nearest the house being entirely torn to pieces by the shock. One was splintered from top to bottom into kindling wood; the next was burst as from a charge of powder at its heart, and cut off squarely some five feet above the ground; the third, where the remaining current had evidently made its way to earth, had a deep channel cut in it, following the grain of the wood from top to bottom.

It would appear that a rush of electrical energy, finding no adequate outlet, exercised upon this extreme end of the line, an action similar to that of a mass of water rushing down a pipe closed at one end, the arrested energy being sufficient to utterly destroy the conducting wire at the point where it failed to find an escape; also that a certain amount may have surged back with a sharp recoil, blackening the lightning arrester next towards the central office. In this case, perhaps one of the branches or ramifications of the lightning flash which Mr. Wurts has described in his paper, may possibly have struck the line, and it would seem that as the wire apparently carried the entire current for a considerable distance without any sign of excessive heating or without any escape to earth which left a trace, the peculiar phenomena at the end of the line may have been the result either of an intensified potential, or an oscillatory effect by means of which a portion of the energy was dissipated in vaporizing the wire. It is also my belief that there are special local spots or lines of attraction within the crust of the earth, possibly corresponding to underground water courses, to which lightning discharges often make their way in preference to following what would naturally be considered easier paths through small air spaces and heavy ground wires, which may communicate with subterranean regions of inferior conductivity. I witnessed some years ago some experiments with the ancient and much-derided divining rod or forked twig, which strengthened my belief in these special lines of attraction, and I am not aware that the theory to which such phenomena point, has ever been disproved.

MR. KENNELLY:—I should be very sorry to try to explain what happened in the interesting case mentioned by Mr. Jenks, and in fact, as all admit, our knowledge must be very much more complete than it is now, before we can attempt to bring lightning or its effects under exact rule. But I think we may fairly infer that if there existed at the end of the wire the condition which exists at the end of a water pipe when water rushes through the pipe, we should expect that that last spark gap here before us, should indicate far more spark discharges than any of the middle points. During the continuance of this experiment just now, I counted the number of sparks at the different gaps, and I did not find any preponderance during that time of any one gap over another. I think if Mr. Wurts were to repeat that for us again, that would be again the case, whereas if the theory were true that any reaction existed at the end of the wire whereby the pressure was greater there than at any intermediate point, there should be a large preponderance of discharges at the last gap. In regard to Mr. Jenks' case, it is certainly from his observation a matter of fact, that in this particular case the discharge was far more violent at the ends of the line than at the middle point.

In regard to Mr. Wurts' interesting observation that 140 or 144 discharges took place on that line in fair weather, all I can say is that we are glad that we have not to deal with anything of that kind in this more favored land. But in regard to any deduction to be made from that state of things, surely if the charge were being accumulated by soaking, as it were, out of the air, there would be a steady current produced. Why should it accumulate and jump every third of a second when it has a chance of running steadily through? If it is a gradual soaking in from the layer of air, why should it wait half a second and make a jump, and why shouldn't there be a continual stream instead of an oscillatory disturbance, which argues oscillatory effects taking place somewhere in the upper regions of the atmosphere.

Mr. Wurts:—The circuit to which I referred was not a trolley circuit; it was a high voltage metallic circuit; was therefore well insulated from the ground, and the charge, having no opportunity to leak to earth, accumulated until discharge took place across the arrester.

In regard to the preponderance of discharges at the end of a line, I can hardly agree with you that the charges would pass more frequently at the end by reason of the increased tension there, for the reason that there is an enormous impedance in the line, and if a spark gap arrester be connected at an intermediate point, and at a point were there is a maximum tendency to discharge, the discharge will take place there, rather than follow through any considerable length of wire in order to discharge through an arrester placed at the end. Had time permitted, I might have illustrated that point by actual experiment.

Before the discussion closes, I should like to make a few remarks upon the *method* of applying insulating material to electric apparatus, and particularly electric street railway apparatus.

The fundamental principle of this non-arcing railway lightning arrester which I have just shown you, is based upon the fact that disruptive discharges pass more readily over surfaces than through insulating media, such as air, or the well-known insulating materials used in electrical apparatus. For example, an electric spark will jump five or six inches across a glass, marble or wooden surface, where it would not pierce through $\frac{1}{32}$ of an inch of insulating material. Further, if flat pieces of marble, wood or other insulating material be laid together, exactly as is done in the construction of the non-arcing railway lightning arrester, the discharge spark will readily pass *between* these flat surfaces, in fact quite as easily as before, and nothing will be broken or ruptured. Similarly, if a discharge spark tends to pass from a copper wire wound with insulating tape, to the iron body or core of an armature, this spark will find an easy path *between* the convolutions of insulating tape, and will pass between these convolutions without piercing, or in any way damaging the insulation. In practice, however, the dynamo current immediately follows this discharge spark, and the insulation is then and thereby damaged.

I have had a wide experience in making observations of this kind, and it has been my unvaried observation, that armatures insulated in the above manner, and in general, surface wound armatures, are very sensitive to atmospheric electrical disturbances. I have also observed that street railway apparatus insulated with homogeneous insulation, which I am pleased to note is now used in the best grade of street railway apparatus, is rarely damaged by lightning when protected with a reasonable number of lightning arresters.

Mr. Steinmetz:—I think this very instance Mr. Wurts mentioned of a lightning stroke out of a clear sky, is such a case as I have referred to; because there the electro-static field in the atmosphere, changing constantly and rapidly by changes taking place in the higher regions of the atmosphere, causes rapid variations of the charge of the ground, and the wire being part of the ground due to its charge or discharge, sparks must constantly cross the air-gap.

The circumstances described, are just the most favorable for this phenomenon: A clear sky in parts of the country where thunder storms are frequent, and large difference of electro-static potential exist in the atmosphere. A layer of hot air, the best insulator, separates the ground as one condenser coating, from the higher regions of the atmosphere as the other condenser coating. Any change in the electrical conditions of the atmosphere causes corresponding charges and discharges between the ground and the wire. You can see the same in Leyden jars occasionally. If you have the outer coating of the Leyden jars cut into small squares of tin foil, then by charging the jar you see brilliant sparks passing over the outer surface to the hand.

Meeting of Western Members at Chicago, May 23d, 1894.

A meeting of the western members of the Institute was held on May 23d at which Mr. Wurts' paper on Discriminating Lightning Arresters was read by the author. The meeting was held at the Armour Institute, Chicago, where special preparations had been made by Professor Stine and his assistants, in arranging apparatus for the experiments accompanying Mr. Wurts' paper. About 140 members and guests were present. Mr. B. J. Arnold was chosen Chairman for the evening.

Discussion.

The Chairman:—I am sure we have all been benefited by the excellent paper which we have just heard, and the many interesting experiments shown us to-night. I wish personally to express my thanks to Mr. Wurts, for the knowledge I have gained. I have found, contrary to my former instructions, that single lightning arresters *do not arrest*. I was instructed by my firm several years ago when I was engaged in the selling business, to the effect that their lightning arresters were absolutely reliable, and to guarantee them so. The result was that when lightning entered the station and caused damage, I was forced to give the parties new lightning arresters, and otherwise make apologies and amends. I now find that I made a serious mistake, and that instead of giving them new lightning arresters without charge, I should have *sold more* lightning arresters.

Mr. Farnham and others have complimented the last Chicago meeting upon the capable way in which it handled the discussion of his paper, thus paying the western members of the Institute a high tribute. The paper before us to-night is one of value; it has been delivered in a very excellent manner, and is worthy of the highest consideration at our hands, and I hope the members present will avail themselves of the opportunity presented, and endeavor to maintain the standard which has been set for these meetings. I will call on Mr. A. V. Abbott, Chief Engineer of the Chicago Telephone Co., to open the discussion.

Mr. A. V. Abbott:—I have been very deeply interested in the instructive paper which Mr. Wurts has just presented, the subject bringing vividly to my mind former experience with lightning arresters in electric railway work. I can fully substantiate Mr. Wurts' experience with lightning in Colorado and the freaks which it plays with Colorado street railways. I do not know of any other place in the world where there is so much lightning to the square inch as in Colorado, nor where lightning storms come with such suddenness and regularity. Out of the Cheyenne Canon, one may always look for thunderstorms during the months of June, July and August at two o'clock in the afternoon, with the greatest regularity; in fact, I have heard it asserted that some of the inhabitants of the Springs set their clocks by the first flash.

Mr. Wurts has called our attention to the advisability of providing each circuit with a sufficient number of lightning arresters.

Some three or four years ago I had the pleasure of doing considerable electrical railway work in the West. At that time the street railway of Salt Lake City was suffering severely every summer from the destruction, both of their motor and generator armatures, due to lightning. At that time the station of the Salt Lake Street Railway Company was protected by lightning arresters, but few or none were introduced into the line itself. A recommendation was made to equip the entire line with lightning arresters, in the hope of lessening the trouble experienced, but I believe, owing to the expense involved, this recommendation was not carried into effect. At about the same time I completed the Ogden City Street Railway, and there arranged to place a number of lightning arresters in the station, and also to protect the line itself, by lightning arresters located on the poles themselves, at intervals of at least every mile. Ogden is so near Salt Lake City, that it would seem the atmospheric electrical conditions would be nearly the same in both cities. So long as I continued in direct contact with the Ogden City railway, no trouble whatever was experienced with either the motors or the generators from lightning, although many severe storms occurred after the completion of the road, and so far as I am aware, no difficulty has since been experienced.

In the case of another road in the South, where thunderstorms are nearly as severe as those of Colorado, a succession of generator armatures were damaged during the first summer of the road's operation.

The introduction of lightning arresters along the lines, spacing from 2500 to 3000 feet upon the poles, removed the difficulty as far as I am informed.

In constructing the Lake Roland Elevated Railway of Baltimore, the practice was adopted of placing lightning arresters on every twentieth pole; practically every half mile of the road. Baltimore is also subjected to very severe thunderstorms in the summer time, and this number of lightning arresters seems to have afforded a security against injury from lightning.

The experiments of Mr. Wurts and Dr. Hertz have shown very conclusively that in electrical circuits, nodal points will be formed at different places along the line, depending upon the varying conditions of the electrical state of atmosphere. If the position of the nodal points could always be predicted, lightning arresters placed at the anti-nodes would be sufficient to protect the lines, but as the location of these points changes from time to time, it is essential to place a sufficient number of lightning arresters, to be sure that the anti-nodal points are always reasonably guarded. It is, therefore, advisable to be liberal in the use of lightning arresters.

Attention has been called to the desirability of securing good and adequate grounds for all lightning arresters. A good ground certainly does no harm. In the instances previously alluded to, the station grounds were made by bringing the lightning arresters to the station machinery and especially to the engines, for as the engines were compound condensing, they were always located in direct connection with good water supply. It has also been my custom to connect station lightning arresters to gas and water mains, as an additional protection. The circuit lightning arresters are always connected to the return circuit of the street railway, in addition to being grounded by means of a long iron rod, driven so deeply into the ground as to reach permanently moist strata, and soldering the lightning arrester wire to the projecting end of this rod.

Dr. Lodge, however, has called our attention to the fact that a lightning arrester grounded through considerable ohmic resistance will apparently work sufficiently well, the extremely high rate of oscillation of the lightning discharge causing any circuit to present such an amount of impedance as to render the ohmic resistance a very small proportion to the effective circuit resistance, in anything but the poorest kind of grounds.

Mr. Ludwig Gutmann:—I have been very much pleased and interested in Mr. Wurts' paper and experiments, and having had some little experience in that line, I wish to call attention to some points which are not sufficiently brought out, or not touched at all in this very timely and instructive paper.

To begin with, as Mr. Wurts states clearly, that the lightning arrester (so called) is a device which does not arrest lightning, I wish to say that this name is not only misleading but is wrong. What we wish to accomplish is, to conduct these high tension and dangerous currents through circuits especially prepared for them to ground, to avert destruction of property. We therefore "deflect" the lightning from our circuits, through other more favorable ones, to ground; and therefore a more appropriate name for such device would be "lightning deflector."

He further criticizes the central station men for their action on this question under discussion, and their wrong views, and while this criticism may be well founded, I believe that manufacturers and constructing engineers are more to blame for this state of affairs. They never or seldom have given any instructions.

In 1890 I was sent to Guthrie (Oklahoma Ter.) to investigate and repair a dynamo which was struck by lightning. On examining the "ground," I found the lightning deflector connected with a copper wire to a two foot iron rod ⅜" in diameter driven into the dry stone wall of the building. The central station proper and this rock wall were situated below the street level. To assure good connection between this rod and the deflector, the wire was twisted round the rod some six or eight times. I substituted this supposed ground by a sheet of copper 1½ feet wide and 3½ to 4 feet long. The same was closed up to form a cylinder and was provided with ½ in. holes through which several conductors of No. 8 copper wire were threaded the entire length of the cylinder and then soldered. The object of this was, that should a discharge melt the solder, the connecting wire could not be detached from the cylinder but would remain in metallic contact with this large earth plate. A hole was made below the floor some four feet deep, and a layer of coke spread on the bottom, then the cylinder was placed on edge and filled up, surrounded and also completely covered with coke. Finding while digging, that there was little or no moisture in the ground, I ordered it to be soaked, by throwing several buckets of water on it; and further, after completing this work, the floor was not nailed down, but a trap door provided, so as to afford convenient means for inspecting the ground plate and also for keeping the ground moist.

Mr. Wurts is perfectly correct when he criticizes the action of central station men for condemning a lightning deflector which apparently did not live up to their expectations. There are a number of good devices on the market, but unless installed properly, and systematically arranged, no good results can be expected. I believe that the present arrangement is poor practice, which I have criticized frequently, but never on any occasion like the present. I refer to the proper location of lightning deflectors. I have always held that their place is on the line, and in front of the central station, but not *in* the station.

In my opinion it is rather short-sighted policy to place a well-finished lightning deflector on the switchboard, so as to enhance the appearance. It is a fact and a practice that is generally indulged in. Lightning deflectors can be found on many switchboards or at least in the central stations without, I dare say, a single exception.

This device, however nice looking, is not a voltmeter or other necessary switchboard instrument, and should not be placed on the switchboard. Its place should be outside the station, within a few feet of the building and located conveniently to the circuits. We find a central station with 6 to 12 or more miles of overhead wires "protected" (it is said) by a single lightning deflector in the station. What is the result of this illogical practice? All high tension currents within this distance, caused to flow by direct communication or induction from lightning discharges in these wires *have to travel all the distance to the station for the only purpose of again being conducted out of the same, into the ground.* Why permit them to come to the station at all? Why not keep them out by leading them to the ground or dissipating them before they reach the station.

Sometimes lightning once conducted to the station prefers to go to ground through a dynamo, which owing to its large surfaces of armature and pole-pieces is a far more efficient lightning deflector, especially as generally it has a far better and larger ground plate than the deflector proper. This will be clear, if we remember that the concrete foundation and anchor bolts are directly connected to the dynamo base plate. If no favorable path to ground was furnished in the station there would be no tendency for the lightning to affect the electrical machines.

Another point of importance to which I wish to call your special attention is the necessity of a good ground, because it belongs to the "system," and none of the deflectors will work unless this ground is in proper condition.

Now let us look at present practice. When a station is in course of construction, much importance is given to the ground plates, and generally they are well made because the cost of the plant is, at this stage, ever present before the eyes of the constructing engineer. The ground plate is made and buried, and because it is buried, it is soon forgotten and relegated to the dead. We find superintendents test daily for grounds and leakage, where they do not want to find them, but I have never heard of an instance where a test has been made on a ground where it ought to be. The former test is no doubt much the more important, as it refers to additional consumption of coal which may amount to several dollars a day, while the latter would mean but an occasional loss of several hundred or thousand dollars.

On only one occasion have I been in doubt as to which name is the more appropriate one, "lightning arrester" or "lightning deflector." This doubt was caused by the performance of some two dozen or more lightning arresters nicely arranged on a special board just below the cupola in a New York central station, whose feed wires connected to circuits extending as well over down-town as up-town districts. Lightning discharges could be observed and heard without fail as soon as there was a thunderstorm in that city. These discharges were frequently heavy ones, and sounded in the spacious station like the report of a shot-gun. On one of these occasions these lightning arresters very effectually arrested the lightning, for it did not leave the station but burnt up the arresters, the cupola, feeders and switchboard so completely, that the company had to supply its consumers by the aid of other central stations.

This particular station had a most excellent ground terminal, which consisted of a large iron pipe about two feet in diameter extending into the river, but as to the conditions of the deflector terminals to this pipe I do not know.

To sum up; a central station can be effectually protected: 1, by locating lightning deflectors on and along the lines and provided with proper grounds; 2, by locating some deflectors near the station and providing means for conveniently inspecting the terminals and the state of the ground, especially as the earth changes in conductivity at various seasons; 3, by artificially providing means to moisten the ground manually or otherwise where necessary; and 4, reducing fire risks by having no lightning deflectors inside the station itself.

The following communication from Mr. C. C. Haskins was read by the local honorary secretary.

Mr. Haskins:—I regret being unable to attend this meeting. I have been much interested in studying Mr. Wurts' paper, and it brings back the old days when our telegraph instruments were at the mercy of the disruptive discharges of heaven's artillery, and our only lightning arrester was a lightning rod—our only refuge of safety was to open the keys all along the line, and thus extemporize an air break at every office. Those were the days when there were no binding posts on the telegraph desk, before which the operator stood (because it was deemed unhealthy to sit down), and mercury cups bored into the hard-wood desk served for connections.

Then this was improved upon by cutting out the office, using the abbreviation, s. f. l., as a notice that such action was about to be taken. After this came the ground plate terminal with saw-tooth edges brought extremely close to two plates connected with the line on either side of the office. This was improved by making one-half the edge of each plate saw-toothed, and the other half plain, to provide for discharges from, as well as to the earth. This was varied so that points were made with adjusting screws, that the distances might be regulated at the air-gap, reducing or increasing this at will; and, besides, these points sometimes fused and required separation after a discharge had made a permanent ground. Mr. C. H. Summers, the Western Union electrician, invented a lightning arrester for telegraph purposes which proved a good one, save that after every successful protection it was necessary to rearrange it. A fine wire, silk insulated, was wound several times around an upright brass cylinder which was connected to ground. This wire was connected to the line. Any discharge sufficiently energetic to do mischief to the instruments, burned the insulation next the cylinder, and welded the line to ground, or at least discharged the wire to earth.

The condensers, introduced into duplex and quadruplex work, to some extent played the part of lightning arresters in telegraphy, and the gradual weakening of the discharge is forcibly illustrated in its action upon the plates of foil.

I was assisting Professor Gray with his way-duplex, in 1877. We were successfully operating the line from Chicago to Dubuque, transmitting through business between the two terminals, while between 20 and 30 offices were doing local business on the same wire at the same time. Condensers formed part of the equipment at each office. A severe thunder storm, lasting half a day, grounded the line, and I started for the break. I found not one, but several troubles, all in the condensers. In some three or four of these, the current had bored its way through many sheets of the tin foil and paraffined paper, the hole growing gradually smaller as it passed down, until it was entirely lost. The largest hole was nearly or quite a sixteenth of an inch in diameter.

To remedy the difficulty I took the condensers apart and turned the sheets of foil so as to make the holes break joints, and, by riding on all sorts of trains, and doing a deal of hurrying, I managed to get the line working in a comparatively short time. The condensers, fortunately for us, were not so firmly pressed together as to prevent their being overhauled in the way described.

While the slopping over of a disruptive discharge, if I may use the expression, is probably chargeable with much mischief, I think more credit is due to induction than it generally receives in these disturbances. A case in point here, some years ago, will, perhaps, have some interest. A scheme was developed by the race-track people to run horses by night, and the project was ably seconded by an electric light company. A plant was placed, and the lights started. The project was a failure, for the horses objected to the lights. The plant was discontinued, and several hours after the fire was out under the boiler, a lineman, who was taking down the wire, was nearly prostrated by a shock from the line, while standing on the ground. There was something over a mile of No. 6 copper wire in the line, one end of which was hanging in the air, while the other was grounded through the lineman. There was no appreciable electrical disturbance in the elements; but there was in the wire an induced charge.

A well-known electrician here, while in a suburban street car, during a thunder storm, saw a house at a distance of half a mile or so away, across the prairie, struck by lightning, and distinctly felt the prickling sensation of escaping electricity—evidently an induced effect. This is, perhaps, not exactly patent to the matter of lightning arresters, but possibly bears indirectly on the subject.

Up to within a very few years, a considerable amount of ingenuity was expended in the endeavor to make use of electro-magnetism in constructing lightning arresters. But the time required to energize the core was sufficient to permit the mischievous current to do its work before the circuit was opened. It always seemed to me like a case of electioneering after the polls were closed. There are other and good devices, notably those of Professor Thomson, with which you are all familiar.

Mr. Geo. M. Mayer:—While I cannot really discuss Mr. Wurts' able paper on lightning arresters such as used in connection with light and power stations, yet I feel that I can contribute something from my experience with lightning arresters on telegraph and telephone lines. Judging from this experience I am inclined to believe that to protect a station and its apparatus from lightning, the only proper way is to place the lightning arresters outside of such station, and in the way the author proposes or has done already, and from all indications successfully.

Mr. Wurts has mentioned that a lightning discharge prefers to travel over a smooth surface, as, for instance, a polished marble plate in preference to an air-gap. The Government Telegraph Works, of Würtemberg, Germany, with which I was connected, adopted practically the plan of placing lightning arresters at regular intervals along the telephone lines in cities. Usually the wires were carried over the housetops, the insulators being placed on iron frame-work. By grounding this iron frame, any lightning discharge from the wires was carried at the next frame to ground. The "ground" consisted of a galvanized iron plate, laid where possible in existing wells, or else in moist ground, connection being made with the frame-work by a stranded copper wire cable of about $\frac{3}{8}$-inch diameter. This evidently agrees with Mr. Wurts' experience with marble plate arresters such as he described, inasmuch as the porcelain insulators used are acting as such, a surface giving the lightning discharge an easy path from line wire to frame and ground, yet preventing the telephonic currents from following the same course, therefore acting just like an air-gap. In addition to these peculiar lightning arresters, another air-gap arrester was placed in the building where the wires entered. The air-gap, however, was filled up with paper, which lightning discharges frequently pierced. However, comparatively little trouble was experienced, from which I draw the conclusion that such grounding of iron insulator-frames is as efficient on telephone lines in preventing destruction of apparatus, as air-gap arresters are on power or lighting circuits. It happened, however, that with this air-gap arrester where the paper was pierced, connection was also established between the line and the ground, showing that at the point of rupture the metal melted, forming a point which touched the ground plate. The same trouble was experienced in the railway telegraph offices where the lightning arresters consist of comparatively large plates, about 7 inches by 5 inches, which were separated by an air space of about $\frac{1}{32}$-inch, the surfaces of the plates being V-shape grooved, the grooves, however, running at right angles, therefore presenting a great number of points, similar to saw-tooth arresters. Although some apparatus used in connection with railroad telegraphy, such as bell signals on crossings, and at stations, have individual air-gap arresters, generally consisting of two pointed screws, yet better protection of the stations and station apparatus could be obtained by placing more arresters on the line.

Perhaps it would be a good plan on lines where many wires are carried on a row of poles to make these poles of iron, if the ground is fairly moist, thereby establishing a ground connection for lightning discharges at every insulator.

Of course the leakage of current would be larger, and the first cost of construction would also be greater, but it may be a feasible plan and cheaper in the end.

Allow me to add to these remarks a few words on the construction of an arrester made by Siemens and Halske, Berlin, for telephone lines, which consisted of a brass screw, being connected to ground, and in the threads of which was placed a silk-covered wire, which formed part of the line. A lightning discharge usually pierced the silk insulation while the telephone currents could not follow the same path.

As a good deal of damage to electrical apparatus of all kinds is continuously being done by electrical storms, it shows that our present methods of protecting lines and apparatus can be improved upon, and certainly have of late been improved, and experiments such as were carried out recently by Mr. Wurts, and so ably presented in his paper will tend to further improve and perfect our lightning arrester systems.

Upon motion of Mr. R. H. Pierce, a cordial vote of thanks was tendered Mr. Wurts for his kindness in making a special trip to Chicago in order to present his interesting lecture with its elaborate experiments before the meeting of the western members of the INSTITUTE.

Upon motion of Mr. Caldwell, the thanks of those present were tendered to Professor Stine and his assistants, to whom those present were under great obligations, for their kindness in placing at the disposal of the members the room in which the meeting was held, and providing all the necessary apparatus for carrying out the experiments shown by the lecturer.

After a number of announcements had been made by the local honorary secretary, relative to the result of the annual election at the Philadelphia meeting, the action of the INSTITUTE in assuming the expenses of the Chicago meeting, the distribution of the printed papers, and application blanks for new members, the meeting adjourned.

[COMMUNICATED AFTER ADJOURNMENT BY MR. CARL K. MACFADDEN.]

The paper of Mr. A. J. Wurts on Discriminating Lightning Arresters has been of unusual interest to me, especially in reference to the action of inductive resistance in producing discharges at certain given points on a line.

An experience of mine several years ago for which I have heard several explanations from various experts may be of interest.

A railroad company having a large terminal station in Chicago obtained current for lighting the same from an office building near by, which contained a plant consisting of two Edison dynamos connected to a three-wire system. The dynamo bearings were lubricated by means of a running stream of water from the city water system, thus grounding the dynamo frames permanently. The office building was so located, that there was but about 80 feet of each of the three-wire feeders exposed to the weather; those being between the office building and the metal train shed of the terminal station, and placed about 20 feet above the ground.

The ohmic resistance to ground of the wiring under the train shed was not 20,000 ohms, but the insulation on the dynamo conductor was always high (over 10 megohms) although the actual *body* of insulation was, of course, quite thin.

To facilitate lamp testing, etc., the three main wires, as they left the office building, and these three connecting wires were connected to an old fashioned three-point telegraph lightning arrester, using a number of thicknesses of heavy paraffined paper as insulation between the ground plate and the three connection plates to which the wires were attached. The lightning arrester had been put up simply to furnish a ready means of testing for grounds, but the first lightning storm punctured the paper under two of the three plates. Owing to the fact that several very high buildings were situated in the immediate neighborhood it seemed at first an impossibility for lightning to have punctured the paper.

The resistance to ground through the lamp wiring was low, and the thickness of armature insulation was so small, that either path seemed preferable to that through the lightning arrester to ground. Nevertheless, the paper in the arrester was punctured a number of times during succeeding lightning storms, and the whole affair had assumed a mysterious aspect, and lightning arresters were considered a necessity in the dynamo room from that time on, and to this day they have never had a discharge pass through them. The reason, in the light of later developments, will be apparent.

The high potential disruptive currents on the low pressure conductors, were without doubt due to the inductive effect of neighboring lightning discharges through the air.

And the dynamos were protected quite thoroughly by the large number of bends and crooks in the wiring between the exposed conductors and the dynamo (23 right angle turns and a spiral of several turns in each wire.)

The discharges to the poorly insulated wiring were also protected probably by several right angle bends in the wiring as it entered the train shed.

The discharges obtained at the improvised lightning arrester did not pass through any of the bends mentioned, but had a direct path to ground through the paper which it punctured.

A paper presented at the Eleventh General Meeting of the American Institute of Electrical Engineers, Philadelphia, May 16th, 1894. President Houston in the Chair.

UNIPOLAR DYNAMOS FOR ELECTRIC LIGHT AND POWER.

BY PROF. F. B. CROCKER AND C. H. PARMLY.

INTRODUCTION.

The object of the present paper is to call attention to the fact that unipolar dynamos and motors are much more practical and generally applicable than is ordinarily supposed to be the case. The term unipolar dynamo is here used in its ordinary sense to designate a machine in which electric currents are generated by the *continuous cutting* of lines of force. These machines are identical in principle with the original disk machine of Faraday. The term unipolar is by no means satisfactory, but is almost universally used in connection with machines in which the magnetism in the armature is not reversed by the rotation of the latter. In this broad sense, however, it is also used to include machines of the Mordey and other types, in which the lines of force always pass through the armature in the same direction; but in these machines there are separate pole-pieces and the lines of force vary in the armature just the same as in the ordinary bipolar and multipolar types. Moreover, machines of this kind usually have their armatures wound with many turns of wire, they require a commutator in order to generate direct currents, and are radically different in principle from the continuous cutting machines which are the subject of this paper.[1] The term "non-

1. The term unipolar is also applied to the Ball dynamo, but this also acts like an ordinary bipolar machine, except that the magnetic circuit is completed through the air and it is no sense a "continuous cutting" machine.

polar" suggested by Forbes is preferable to unipolar, but it is rather meaningless and is liable to be understood to mean any kind of iron-clad machine. Unipolar dynamos are also called *disk* or *tube* machines since the armature is usually made in one or the other of these forms. Neither of these terms, however, covers the other, and they are liable to be confused with other types of machine similar in form but different in principle. The writers of this paper suggest *continuous pole* dynamo as a good name for this type of generator.

Historical Notes.

The first machine of the unipolar type was Barlow's Wheel described by him in 1823, and consisting of a star-shaped disk with long points revolving between the poles of a magnet. A current was passed through the particular portions of the disk between the poles of the magnet, and the disk was thereby caused to rotate. This apparatus was, therefore, the first unipolar motor. Faraday's disk machine, constructed and described by him in 1831, was the prototype of the dynamo and was also a unipolar machine.[2] He also made an apparatus working upon the same principle but consisting of a copper cylinder suspended over, and revolving around the pole of a bar magnet. This was the first form of tube dynamo.[3] Faraday even at that early time appreciated the difficulty of obtaining a sufficiently high E. M. F. from generators of this type, and attempted to attain it by having several disks placed side by side and revolving in opposite directions. Similar ideas have been re-invented time after time, and even at present it is a favorite field for invention For many years after Faraday's discoveries the unipolar machine was almost neglected, progress being in the direction of machines having many turns of wire in the armature to obtain a high E. M. F. In 1878 Siemens constructed a unipolar machine with a tube armature of considerable size for actual commercial use in electrometallurgy. Delafield showed at the Philadelphia Electrical Exhibition of 1884, a unipolar dynamo of the tube type which generated a current of few volts and a large number of amperes. Forbes has also constructed and described unipolar machines, in which the armature is in the form of a cylinder of iron revolving within a field magnet which completely surrounds it. The

2. *Experimental Researches*, vol. i., art. 85.
3. *Loc. Cit.*, art. 219.

mechanical and magnetic design of this machine is very ingenious, and from the practical standpoint it was a considerable improvement upon anything which preceded it. The highest E. M. F. obtained from these machines, appears to have been about 6 volts, and nothing further has been heard concerning them for several years. The complete neglect into which this type of dynamo has now fallen, is best proved by the fact that no example of them was shown at the Chicago Exposition. In fact it might be said that their present importance is actually *negative*, since the only attention which has been given to them lately consists of a few articles on "Unipolar dynamos which do not work," that have appeared in the electrical journals during the past few months.

General Principles.

The action of unipolar dynamos, is based upon the fact that a conductor moving in a magnetic field so as to cut lines of force will have an E. M. F. set up in it whether the field be uniform in intensity or variable. The error is very commonly made of supposing a *variation* in the number or density of the lines of force is necessary in order to produce a current by magneto-electric induction. As a matter of fact, however, an E. M. F. must always be produced if any lines of force whatever are cut. It may happen, and in the case of a *coil* of wire it usually does happen, that lines of force are cut in one direction by one portion of the conductor, and in the opposite direction by the other portion, in which case one effect neutralizes the other. The simplest example of this is a closed metallic ring moving perpendicularly to the lines of force in a uniform field. In this case one-half of the ring generates an E M. F. in one direction and the other half in the opposite direction, so that no *current* is produced; but the full difference of potential corresponding to the number of lines cut per second, will nevertheless exist between the two sides of the ring.

In the unipolar dynamo or motor, a variation in the lines of force is not only unnecessary but is positively objectionable, since it would cause serious losses from eddy currents and hysteresis, whereas if the field is perfectly uniform these losses are practically avoided.

Any break or weakening of the intensity of the field due to "blow-holes" in the casting, for example, would allow the armature current to flow back at that point, and would act as a sort of short-circuit on the rest of the armature. For example, in Fig. 1, let A A represent a disk armature revolving upon a shaft S. The field is of uniform intensity and an equal E. M. F. is generated at each radius, as indicated by the arrows, except that the portion B of the armature is in a weaker field. In this case the current will flow back through the portion B, as indicated by the arrow, and the armature will be like a short-circuited one of the ordinary kind. This back current will flow and produce heating and loss of energy, even though the external circuit be open.

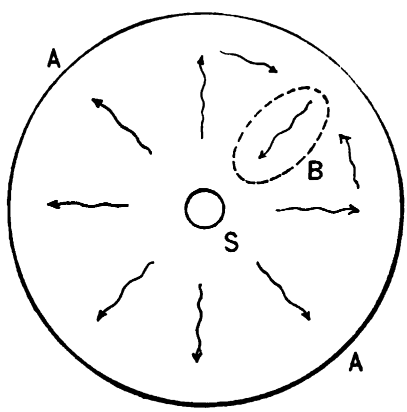


FIG. 1.

It is also a common error in regard to unipolar machines, to suppose that the armature cannot be made of iron. As a matter of fact, iron or steel is usually better than copper because it is a much better conductor of magnetism, and the reluctance of the magnetic circuit can be reduced far below what is possible with a copper armature, the only air-gaps being the small mechanical clearances required for free rotation. A steel or wrought-iron armature is much stronger than one of copper, not only on account of its greater strength for the same thickness, but also because it can be made much thicker without causing any appreciable increase in the reluctance of the magnetic circuit, whereas a copper armature would have to be made very thin. The greater specific resistance of iron is not objectionable, since an armature composed of it, would have a current capacity far greater than is required, and the thickness can be increased to make up for the higher specific resistance.

Methods of Multiplying the E. M. F.

The plan suggested by Faraday, was to have two or more disks side by side revolving in opposite directions, the edges being connected together by mercury or other contacts so that the current flows outward from the center to the periphery in one disk, inward in the next disk, and so on in series. In this way it is possible to multiply the voltage as many times as there are disks, but there are obvious mechanical difficulties in rotating these disks in opposite directions, and the numerous electrical contacts would be decidedly objectionable. Hundreds of modifications of this idea have been suggested; for example, Siemens in the tube machine already referred to, multiplied the E. M. F. by splitting the tube longitudinally and placing rings on the end of the tube, so that the current was carried first through one section of the tube, then through another, and so on. In this way the circuit may be threaded through the magnetic field a number of times, each time a certain E. M. F. being added. Another plan to accomplish the same result would be to have a number of thin tubes placed concentrically one within the other, but separated by a layer of insulating material, and connected so that the current passes first through one tube, is then carried around outside of the machine and is passed through another tube, and so on through all of them in series.

There is no theoretical limit to increasing the voltage by some such method, but practically any of these constructions would have very serious, if not fatal, mechanical and electrical difficulties. It would seem to be far preferable to construct the machine of sufficient size and run it at sufficient velocity to get the required voltage without complicating the construction. There is no objection, however, to connecting two unipolar machines in series, both being driven, for example, by one engine, which may be belted or direct coupled, and there is no serious complication involved in operating four or even more machines in series. The first cost and attendance required would probably be much less with four unipolar machines than with one direct current machine of the ordinary type and of equivalent capacity.

One very convenient way to multiply the voltage of a dynamo would be to charge a number of storage batteries by means of a low voltage machine of, say, 10 or 20 volts. When charged, these batteries could be connected in series to give any desired E. M. F., 115 or 230 volts for example, and the actual working current would be supplied by them. The charging could be done at hours when the current was not required, or two batteries could be used alternately to give an uninterrupted supply of current. In this way two advantages could be realized; first, the cheapness and simplicity of the unipolar dynamo, and, second, the uniformity of load with which the engine and dynamo could be run.

Practical Design.

It is not difficult to get up complicated puzzles in connection with peculiar forms of unipolar machines, but in the simple and probably the only practical forms, we have a single straight conductor which cuts all the lines of the magnetic field once per revolution. In the case of the disk machine we can consider any radius of the disk as constituting the conductor, and in the case of the tube machine it is any element of the cylinder.

The theory of the magnetic circuit is now well understood, and modern practice in the design of dynamos and motors fully appreciates the advantages which result from making the ratio between the length of the magnetic circuit and its cross-section a minimum. In a unipolar dynamo, this reduction of the reluctance of the circuit can be carried farther than in any other class of machines, because the creation of an electromotive force by the continuous cutting of the lines of force, enables a form of magnetic circuit to be employed, which in the highest degree combines the advantages of minimum length with maximum cross-section. This type of magnetic circuit is the circular ring; for if we surround a circular coil D D of wire carrying a current, with two circular rings of semi-circular cross-section, as shown in Fig. 2, we shall form a closed magnetic circuit of maximum induction for minimum excitation. Cut this circuit along any radius of the circular cross-section, and a magnetic field is obtained in which, if an inductor be rotated about A X as an axis, there will result a continuous cutting of the lines of force, which only requires that appropriate collectors be provided to form a closed electrical circuit, in order to procure a current of electricity. The magnetic circuit is indicated by dotted circles with arrowheads.

There are, however, but two directions in which the magnetic circuit can be cut, so as to form a field suitable for practical use. One is along the radius C D, in which case the inductor will rotate in a plane perpendicular to the axis, and the other is along the radius E D, in which case the inductor will move in the convex surface of a cylinder whose axis is the axis of rotation. In the first case the armature of the resulting dynamo will be a disk, and in the second case a cylinder. In both, the way to collect the current would be to provide one set of collectors at the edge of the armature, and the other set at the axis, thus utilizing the shaft as a portion of the electrical circuit. The rel-

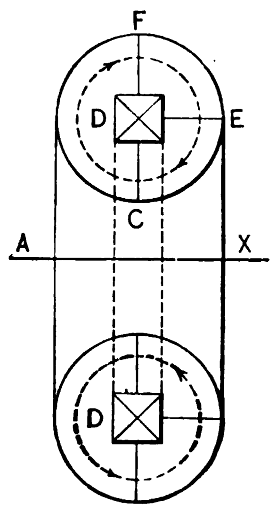


Fig. 2.

ative advantages and disadvantages of the disk and cylinder forms, depend upon circumstances in each particular instance.

In many cases the single magnetic circuit, shown in Fig. 2, is not the best for practical use, because it would be necessary to make the diameter of the ring larger than it would be if two rings were placed side by side, as shown in Fig. 3 and Fig. 7, since in this type of dynamo we must depend wholly upon the total magnetic induction and the speed of rotation, to produce the electromotive force, as it does not appear to be practicable to greatly augment the voltage by multiplying the number of inductors.

Another consideration of importance is the volume of the ring. It is evident that any required area of pole surface can be provided, by keeping the mean diameter of the ring constant, and varying the radius of the circular cross-section between certain limits, or by keeping this radius constant and varying the mean diameter of the whole ring. It is also evident that for any particular required area of pole surface, any decrease in the mean diameter of the ring requires a corresponding increase in the radius of the circular cross-section and *vice versa*. In Fig. 4 let the circle whose radius is r and the distance of whose center from the axis of X is b, revolve about that axis generating a volume of revolution. This volume forms the field magnet, and contains nearly all the weight and a large part of the cost of the

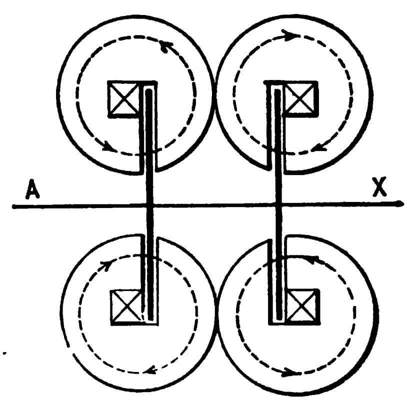


FIG. 3.

machine. This volume, which is an annulus, may be determined by the centrobaric method, according to which a volume of revolution is equal to the generating area multiplied by the circumference described by its center of gravity. Hence the volume

$$V = 2\,\pi^2\, r^2\, b. \qquad (1)$$

The factors r and b must have certain values in order to produce the required E. M. F., the latter being usually the definite object for which a dynamo is designed. The values of r and b are found as follows:

Let E = the required electromotive force expressed in volts.

Let n = the number of revolutions per minute.

Let B = the number of magnetic lines per square inch.

Let A = the number of square inches of pole surface.

Let r = the radius of the circular cross-section of the ring expressed in inches.

Then because the two rings are wound so that the electromotive force due to the field of one is added to that due to the other, each ring must be designed to provide enough lines to generate only $\frac{E}{2}$ volts. This requires that $\frac{E}{2} \times 10^8$ lines must be cut per second, or $\frac{E}{2} \times 10^8 \div \frac{n}{60}$ per revolution. Hence the number of square inches of pole service needed is

$$A = \frac{30\ E \times 10^8}{n\ B}. \qquad (2)$$

As shown in Fig. 3 the core of the ring will be utilized for the exciting coil, and for simplicity of construction we shall make its

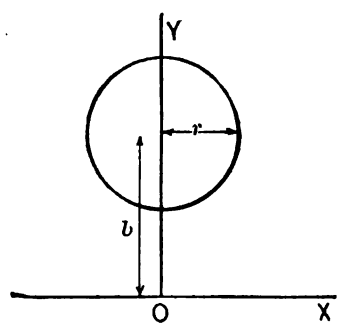


FIG. 4.

cross-section square. It is desirable to keep the size of this square as small as possible, because any increase in the length of a side increases the diameter of the circular cross-section an equal amount, and, therefore, enormously increases the volume. But this square cannot in practice be made indefinitely small, because too great a concentration of the lines immediately surrounding the coil must be avoided, and space enough provided to give ample excitation. One-half of the radius appears to be a length suitable for the side of this square. Referring to Fig. 5 we shall then have an area of pole surface.

$$A = 2\,\pi\, b\, \tfrac{3}{4}\, r. \qquad (3)$$

But we have seen in equation (2) that we need an area equal to

$\frac{30\ E \times 10^8}{n\ B}$, hence equating this with the above value of A we have

$$2\ \pi\ b\ \tfrac{3}{4}\ r = \frac{30\ E \times 10^8}{n\ B}.$$

The ratio between b and r which seems to best fulfil the conditions of good design is $b = 2.5\ r$. Making this substitution above and designing so that $2\ b$ equals the outer diameter D of the armature, we have

$$D = 10^5 \sqrt{\frac{2\ E}{\pi\ n\ B}}, \qquad (4)$$

which shows that the dimensions of the machine are directly proportional to the square root of the required electromotive force, and inversely proportional to the square roots of the speed and of the magnetic induction. It is to be further observed that since for machines varying widely in their outputs B is nearly constant, the most important ratio in equation (4) is $\frac{E}{n}$. In the design of a machine for any special voltage, however, E is fixed, and, therefore, the only arbitrary quantity is n; so that the whole problem is to determine the values of D and n which give the best design.

Let $v =$ the peripheral speed in feet per second, then we have

$$v = \frac{\pi\ n\ D}{720},$$

whence

$$D\ n = \frac{720\ v}{\pi}. \qquad (5)$$

Substituting in this equation the value of D given by equation (4) we have

$$n = 0.00000825\ \frac{v^2\ B}{E} \qquad (6)$$

and

$$D = \frac{10^9\ E}{36\ v\ B}, \qquad (7)$$

from which the values of D and n can be immediately determined for any prescribed values of E, B, and v.

The maximum velocity in feet per second, which the rim of a fly-wheel may with safety attain, is given by the equation[1]

$$v = 3\ \sqrt{t}$$

1. Thurston. *Manual of the Steam Engine*, Part ii., Art. 3, p. 422.

where t is the safe stress of the metal in pounds per square inch. If the inductor be of wrought iron or steel for which $t = 10{,}000$, we shall have $v = 300$; and if of cast iron for which $t = 5000$, we shall have $v = 210$ nearly. The much higher permeability of cast steel over cast iron, and its comparatively small additional cost make it the metal most economical to use, and we shall, therefore, assume that the inductor is either of wrought iron or of cast or forged steel. Hence making $v = 300$ in equation (5), we find that the maximum value which the product $D\,n$ can attain is $D\,n = 68{,}750$. This then is a condition which must not be exceeded in order that the design shall be mechanically safe. Making $v = 300$ in equations (6) and (7) we have

$$n = .7425\,\frac{B}{E} \qquad (8)$$

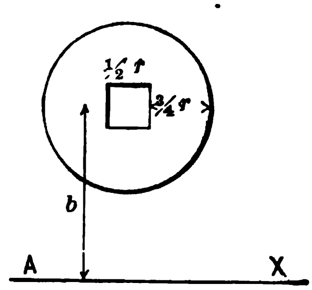


FIG. 5.

and

$$D = \frac{10^7\,E}{108\,B}. \qquad (9)$$

If the armature were made of forged steel it would be allowable to assume t to be as high as 20,000 since the armature is entirely enclosed and could do no harm even if it burst. In this case $v = 423$.

We have now obtained data for finding the number of ampere turns required to produce the necessary excitation. As a first approximation, and to enable us to determine the general conditions, the cross-section of the ring was assumed circular. In practice, however, this form of section must be modified, for a reference to Fig. 2 shows that in order to give the same area of cross-section perpendicular to the direction of the lines of force at D C and D F as at D E we must cause the circular cross-section to bulge out on the side toward the axis and contract on the side away from the axis.

On account of the diminution of cross-section due to the air-gap and to the holes for the brushes, it is found necessary, however, to preserve for the outer portion of the ring a semi-circular cross-section, and it ultimately assumes the form shown in Fig. 6.

The mean length of the magnetic circuit is, therefore, the length of the semi-circle *a b c* plus the length of the curve *a d c*. Assuming the total air-gap to be $.06 \sqrt{D}$ we have the data for determining the ampere-turns required.

The amount of current which the dynamo can generate is determined by the carrying capacity of the armature and brushes.

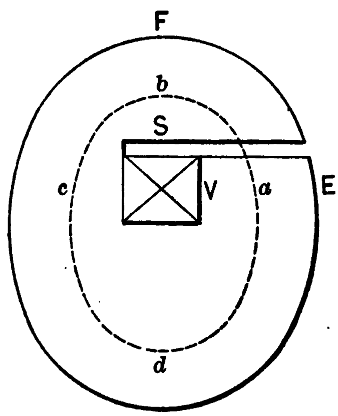


FIG. 6.

Since $.2 \sqrt{D}$ may be assumed to be the thickness of the inductor, we have for the current-carrying cross-section of the armature

$$.2 \pi D^{\frac{3}{2}}$$

square inches. Since iron or steel would carry at least 200 amperes per square inch the current capacity of the armature is

$$C = 125.7 \; D^{\frac{3}{2}} \qquad (10)$$

which at E volts gives the capacity of the machine in watts

$$W = 125.7 \; E \, D^{\frac{3}{2}} \qquad (11)$$

To show the application of these formulæ to two widely different classes of machines, let it be required:

First. To design a belt connected dynamo to generate 10 volts at 1,200 revolutions per minute for electro-metallurgical purposes. Making field magnets and armature all of cast steel, and taking the induction B at 90,000 lines per square inch we have from equation (4) $D = 24.28$ = diameter armature in inches.

Second. To design a direct connected dynamo to generate 130 volts at 200 revolutions per minute for light and power purposes. As before we have

$$D = 214.4.$$

These figures show the immense possibilities of the unipolar dynamo, possibilities so immense indeed that the practical diffi-

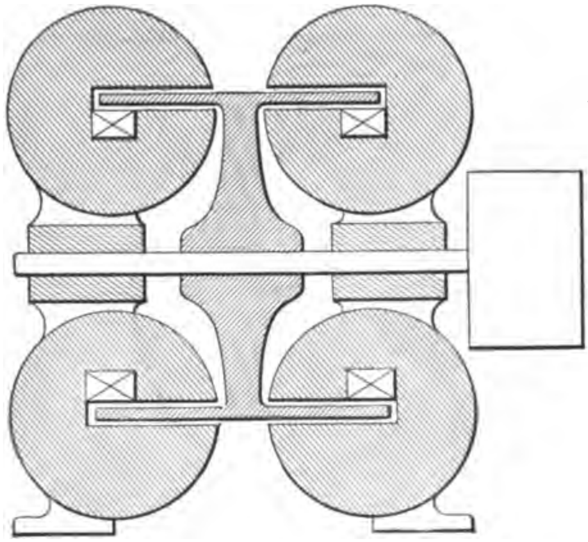


FIG. 7.

culty is not in the design of the dynamo, but in the design of the steam engine. Two machines like the last, coupled at the ends of the shaft of a 100,000 H. P. engine would supply more current than is at present consumed in any two cities in the world. Nor is the excessive current a disadvantage, for while we would point out the tremendous capacity of one of these dynamos, there is no necessity for running them at their maximum output. Even if we diminished the current to one-hundredth its ultimate value, and only use a 1.000 H. P. engine to drive the dynamo, the loss due to the resistance of the armature will still be only four or five per cent.

A more economical way of supplying current for light and power, however, would be to design unipolar dynamos of less current capacity and less voltage, and then join in series on the same shaft as many as are needed to produce the required electromotive force. For example, we may generate 130 volts by joining in series on the same belt-driven shaft two dynamos each giving 65 volts at 800 revolutions per minute.

The dimensions will be as follows:

$$D = 75.81.$$

Running this combination at one-tenth of its ultimate capacity by means of a 1,250 H. P. engine, we should still have a very high efficiency.

Methods of Taking Off the Current.—The best means of making electrical connection with the revolving armature is a very important but somewhat difficult matter. A great many devices have been tried or suggested. The principal of these are brushes of copper gauze or carbon, and mercury contacts applied to the edges of the disk or tube. Belts or straps made of flexible sheet copper or copper wire cable have also been used. One application of this latter device consists in connecting together two armatures by such a belt whereby the E. M. F. of the two machines are added together, and, if necessary, the mechanical power for driving one or both machines can be transmitted by this same belt. There would not seem to be any great difficulty in applying brushes to a unipolar dynamo; in fact for a given current it would seem to be a much easier problem than with a direct current machine having a commutator, since the former has a continuous and smooth bearing surface for the brushes instead of the somewhat uneven surface of copper and mica which exists on a commutator. It is a fact that the speed may be very high, but with the perfectly smooth surface and by the use of brushes with light pressure, and composed wholly or partly of graphite or some friction metal which might even be lubricated, it would appear to be possible to take off a current sufficient for almost any electric light or power use. If the very high speed makes the friction too great to permit of the use of brushes, the edge of the tube or disk may be arranged to run in a mercury trough, thereby making a good electrical contact with very little friction. In this case, however, provision should be made to prevent the mercury, from being thrown off by centrifugal force, as by the use of a guard ring extending all around the edge of the tube or disk. Another device would consist of ball bearings applied to the shaft or to the periphery of the disk or cylinder. These might perform their ordinary mechanical function and also serve to take off the current.

Method of Driving.—There are various mechanical arrangements available for connecting a unipolar dynamo to the source of power. The simplest would be to connect the dynamo to the engine by means of ordinary leather belting, either with or without the use of a countershaft. In this way a high speed can easily be obtained if desired, and is perfectly permissible since the solid disk or tube of steel which constitutes the armature is capable of a much higher speed of rotation than the ordinary built up form of armature now used. It would probably be safe to have a solid steel armature run at two to four times the speed of the ordinary armature having the same diameter.

If the dynamo be directly coupled to the engine, the speed of the latter and the diameter of the armature may be most economically proportioned to give any desired voltage by the application of the previous equations.

An arrangement which seems to possess great advantages would consist of a steam turbine running at 10,000 or 20,000 revolutions per minute, which is the ordinary speed of such machines, directly coupled to a unipolar dynamo. These two machines seem to be admirably suited to each other, the very high speed of the turbine compensating for the fact that there is but one inductor. This combination entirely avoids the necessity for reducing the speed of the engine by gearing which is done in the case of the Laval turbine, or the risk which is involved in running a wire-wound armature and sectional commutator at a speed of 10,000 or more revolutions per minute, which is the plan adopted with the Parsons turbine.

Advantages of Unipolar Dynamos and Motors.—The greatest advantage of the unipolar machine is its extreme simplicity. Its armature consists of nothing but a solid cylinder or disk of steel, or other suitable metal firmly mounted upon a shaft. We have only to compare this construction with that of the ordinary armature consisting of hundreds of pieces of sheet iron bolted or otherwise held together to form the armature core, and wound with a great many turns of wire or bars of copper which have to be thoroughly insulated from each other, and which are not very securely held in place. In addition to this we have the commutator, consisting of fifty or more sections of copper separated by strips of mica and held together by nuts, etc. The electrical connections between the armature and commutator also add to the complication. In short, it would be difficult to find any two pieces of machinery in which the contrast between simplicity and complication is greater. The construction of the field magnet and the rest of the machine is also very simple. The elimination of the commutator, although already mentioned in connection with simplicity, is nevertheless an essential feature of this type of machine, and is a great advantage. The almost infinitesimal armature resistance of these machines is decidedly advantageous, not only in increasing efficiency and decreasing heating, but also because it causes the machine to regulate more closely either as a dynamo or as a motor. According to all accepted theories there would be no hysteresis in these machines, because both the armature and field are always magnetized in exactly the same direction and to exactly the same intensity.[1] For similar reasons there would be no foucault currents, since the E. M. F. generated in any element of the armature would be exactly equal to that generated in any other element, and there could be no tendency to produce eddying currents. This perfect uniformity of the magnetic field is secured by the construction which should be exactly symmetrical, the air-gap being precisely the same at all points.

The question of armature reaction is somewhat doubtful since some authorities state that it is quite considerable, but in the opinion of the authors it is very small and certainly no greater than in other types of machine. The armature consists of only a single turn, consequently the maximum magnetizing effect of the armature in ampere-turns is numerically equal to its current capacity, and since the ampere-turns on the field would be made considerably greater than this, the armature reaction cannot be great. It is interesting to consider how armature reaction can occur in such a machine. The probability is that it has the effect of curving and slightly lengthening the lines of force so that they do not pass perpendicularly from one pole surface to the other in the air-gap, and have a spiral path in the iron since the field current tends to produce lines of force in planes passing through the axis, and the armature current acts at right angles,

1. There might be some molecular friction due to cutting the lines of force, but this would probably be slight.

producing an inclined resultant. There can, of course, be no change of distribution of magnetism as a result of armature reaction, which is the really objectionable effect that it produces in the present types of machines, and in the unipolar machines there are no back ampere-turns, and no magnetic leakage.

In conclusion we may say that unipolar machines are practically indestructible, since they are so simple and so strong that they are not likely to be damaged mechanically, and it is almost impossible to conceive of one being burnt out or otherwise injured electrically, since the engine would be stalled by the enormous current before the armature could be fused by it. A machine possessing all these important advantages certainly deserves a prominent place in electrical engineering, whereas it now has practically no existence whatever.

DISCUSSION.

MR. CARL HERING:—Prof. Crocker has worked out the shape of the field theoretically, but in practice I think it could be changed to advantage by making the polar surfaces at air-gap a little larger than he suggested, because by far the largest part of the reluctance of the magnetic field lies in the air-gap, even if an iron armature is used.

Prof. Crocker speaks of the reaction of the armature as not interfering appreciably with the field; in this I think he is no doubt correct, as the reaction of the armature will simply shift the lines of force and perhaps make the magnetic circuits longer, but it will not make the field ununiform. But it is different with the currents from the brushes to the outside of the machine; unless these currents are led out perfectly evenly through a disk, they will produce an irregularity in the field which will cause foucault currents in the armature, as Prof. Crocker stated in the beginning of his paper. I think one of the difficulties in the working of such machines lies in this irregular shifting of the fields due to the currents in the leading-out lines. About ten years ago I suggested that this might be partially overcome by leading the wires out in the form of a spiral in the plane of the exciting coil, making it a sort of continuation of the field coil, but I hardly think that it would be practicable.

Prof. Crocker has not made any allowance in his theoretical design for the open space which must be left near the coil space for the brushes. The brushes for such a machine must be quite large and numerous, and there must be quite a large space left for them next to the coil space; this in turn will change the proportioning of the field.

Another difficulty lies in the fact that those brushes must necessarily be in the inside of the field where they are not easily accessible. The machine must, therefore, be designed so that it can easily be opened at this part, so as to get at those brushes without requiring too much skilled labor and too many special tools; this will affect the simplicity of the machine somewhat. Regarding the term unipolar, I think myself that it is a very unfortunate one, but there is one way of looking at it which perhaps partially justifies its use. We can imagine unipolar as applying to the currents in the armature, in which case it means a current which is always in one direction as distinguished from one which is alternately in two opposite directions, as in the ordinary dynamos. This may serve at least as an excuse for the word, even if it does not justify its use. Regarding the maximum speed that Prof. Crocker has calculated, I would like to ask whether that is with, or without a factor of safety. If it is without a factor of safety, it seems to me that the dimensions calculated in those examples would have to be altered quite materially, because in a construction of that sort, the factor of safety is an exceedingly important quantity, even if it is taken as low as three, which is rather low for engineering structures of that kind, it will make quite a difference in the size of the machine.

PROF. CROCKER:—I will answer Mr. Hering's remarks in order. In regard to the air-gap I have with me a blue print of a design which shows the increase of the pole area as indicated in the sketch. A very small addition of material would greatly increase the pole surface. The weight of the machine would not be materially changed, and all the formulas and relations would not be substantially modified. Of course, in starting out, we must have a ground work which can be varied slightly, in order to fulfil practical requirements. As the clearance is very small and the magnetic circuit short, I expect to have sufficient magneto-motive force to carry the lines of force across that air-gap even at a high density. In regard to the holes for the brushes, and the disturb-

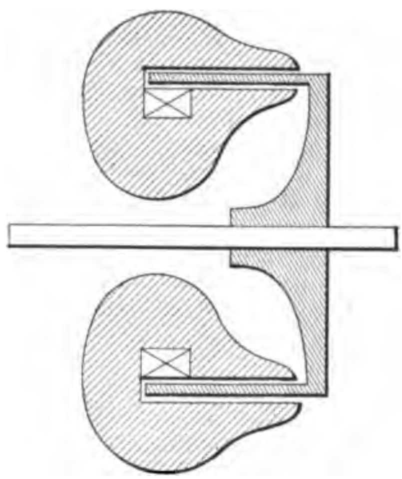


FIG. 8.—Cross-section of Field Magnet modified to give greater Pole Area.

ing effect which the current would have where it came out, that could be overcome by having it brought out at a number of points. If necessary, more space could be allowed for the brushes, which might consist of blocks of metal or carbon resting on the surface of the cylinder or disk, the current being brought out by strips of copper passing through holes in the machine. (See Fig. 8.)

This trouble could be overcome by having a copper band around the armature where the current is taken off, which would act as a sort of collector to bring the current to the brush contacts. Various schemes of that sort will suggest themselves. Of course the field magnet has got to be split in order to get the coil in. It could not be made solid, and it would be desirable to have it split up more or less in order to facilitate handling, etc.

In regard to the factor of safety: I stated in the paper that the *safe* allowable stress is assumed to be 10,000 pounds, that is derived from Thurston's work on the Steam Engine. Of course steel will have a breaking strain far in excess of 10,000 pounds, and even 20,000 pounds working stress allows a factor of safety of three or four, and considering that the armature is absolutely enclosed and could do no harm if it burst, I think that is sufficient. Of course, we could make this disk or cylinder of forged steel, if necessary. There is nothing to prevent it, though it would increase the cost somewhat. I think those are the principal points you mentioned. In regard to the name "unipolar," I think we must accept it, whether it is good or bad.

MR. STANLEY:—Some time ago I constructed a very simple machine which perhaps might be of interest to the INSTITUTE; reversing the whole design that Prof. Crocker has illustrated, keeping the conductor still, and moving the magnet, getting rid of the outside contact. The trouble that I found in making such a machine was, that I could not work with anything like such peripheral speed as Prof. Crocker suggests; so I took a copper band, the section of which was, say this (making a sketch), and constructed a machine very much like Mr. Mordey's alternator. The copper band projecting above and below the field was permanently connected to one terminal of the circuit. The magnetizing coil was placed in the cavity here as in the Mordey machine, and then a smooth surface brush, running at a low speed, kept contact with the interior of the copper band, and the current from the interior edge of the band was drawn off through the shaft of the machine. In this way I could increase the size of the machine very greatly and keep the peripheral contact speed down. Now this is perhaps very well, but what is the use of it all? Why do we want such a unipolar machine, when we can take a copper band, as Ferranti has done, and simply saw it up as Mr. Mordey has, and take both contacts outside, and so have no sliding contact whatever. In this way we get approximately the same output from the machine that Prof. Crocker points out, and have all the advantages of the unipolar design. I cannot see why we want to construct unipolar machines because they are unipolar. In the Mordey machine there are no sliding contacts. The type of the machine is only limited by the capacity to carry the currents generated, and there is very little armature reaction, and so it seems to me it would be going backwards to attempt to build a machine with a contact speed of 500 feet a second.

MR. KENNELLY:—Mr. Chairman, the paper is very interesting, because it resuscitates a type of machine that was supposed to be as dead as the mummies of Egypt. Some of the difficulties which are mentioned here, while they are no doubt considerable, may perhaps be still further reduced by proper methods of construction and design. Mr. Stanley has brought up an argument against resuscitating this machine, showing that so simple a departure from this particular type will give a high pressure alternating current generator, but that seems to me to introduce another question, the advantages of alternating versus continuous currents. Because if you were to make a continuous current dynamo of the same pressure, you would have to introduce the commutator and its disadvantages of cost, maintenance, etc. The paper deals with the continuous current generator and shows how the commutator can be avoided by utilizing a high peripheral surface speed. So the whole question at issue is, is it advantageous to do away with the commutator and replace it by an armature of very simple construction, of large diameter, perhaps with a high peripheral speed of brush contact. But there are one or two difficulties mentioned here, and which have been raised in discussion, that seem to me not so great as they look at first sight. For example, I do not think that any danger need to be anticipated from such a small trouble as a blow-hole. Those who have tried to produce eddy currents in a revolving sheet by deliberately making a very irregular field, heavily grooving it, or putting cavities in it, will realize how deep the cavities must be to produce serious disturbance of that character, and the reluctance of the air-gap is usually so great that the difference of density which is produced by any local variation on the surface is not large, and still less where the cavities, as in blow holes, are hidden from sight. The great necessity of uniformity of the field density is only in one direction. I would like to point that out on the board. (Illustrates.) If you have a disk which rotating in its own plane, and which is represented by that circle (referring to a sketch) then it does not matter how the flux density varies along any radius, it only matters how the flux density varies along any circle. If the flux density varies along any circle, then there will be a tendency for the eddy currents to produce themselves. But the flux densities may vary from the center to the periphery in the most arbitrary manner, provided that whatever variation takes place at this radius is symmetrically reproduced all around.

The form of field-magnet which is presented in this paper and which has struck Mr. Hering so forcibly, as being unnecessary, has probably, I presume, been introduced with the idea of constructing a gigantic machine, because the larger the machine is, the less is the fraction of the total reluctance formed by the air-gap, and the greater is the value of the iron in the immediate vicinity of the air-gap.

I presume therefore that all these designs are intended to be used on a very large scale in practice.

Mr. Steinmetz:—In this problem of unipolar machines we have to be very careful to guard against a mistake which is made quite frequently, especially in trying to reverse the action of a machine. In an electrical conductor an electromotive force is induced, if it moves through or relatively to a magnetic field, that is, either if the magnetic field varies in intensity, or if the magnetic field is constant and uniform, and the conductor moves through the field. Now take this unipolar machine in Fig. 3. There is a constant magnetic field, that is, a field which has at any point in space a constant intensity and constant direction, no matter whether you revolve or do not revolve the magnet; because if you abandon this idea of lines of force, which is nothing else but a physical hypothesis, and go back to the original meaning of the magnetic field, as the intensity and the direction of the magnetic displacement in the ether, you can find that in the machine, Fig. 3, even if you revolve the magnet, the field is con-

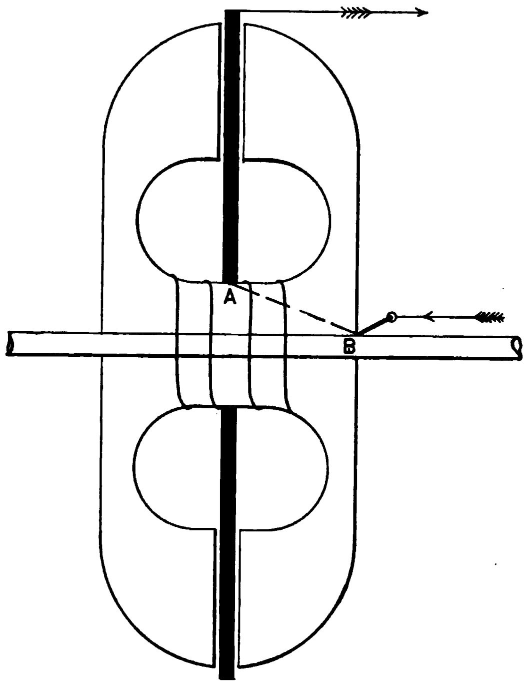


FIG. 9.

stant. Thus you can very easily get a machine which can never do anything. Take the machine that Mr. Stanley showed us Fig 9. Whether the magnet and the magnetizing coil stand still or whether they revolve, the intensity and direction of the magnetic field at any point is constant. Hence, there is no E. M. F. induced in the stationary armature conductor. Still you get a current from the machine. But where is the seat of the electromotive force? It is where the lines of force are cut. That is, inside of the revolving magnet, in the line A B.

The very large diameter of armatures necessary in a unipolar machine is no serious objection; because in direct-connected alternators we have running in this country armatures of 12 feet and 16 feet in diameter, though not with 200 revolutions by any means. But where I see a more serious objection is, that I fear the efficiency of the machine will be very seriously reduced by the brush friction, because you need an enormous brush to take off the current, and even if you use very little pressure of the brush, still you must consider that you have a speed of five to six miles per minute or over 300 miles per hour. These are tremendous speeds, about three times as high as the highest speed reached in revolving dynamo machinery, and about eight times as high as the highest brush speed on collectors or commutators. As to taking off the current by mercury contacts I have never tried it myself, but I remember some remarks of Professor Forbes about it. He said he had tried it and had been very unsuccessful. His statement was something like this: That he could not get any current, because before he could get any current the mercury had evaporated by friction.

I tried myself some years ago to build a unipolar machine to solve this problem of multiplying the electromotive force by

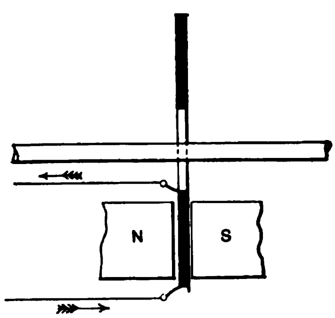


FIG. 10.

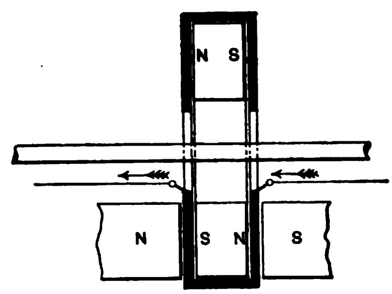


FIG. 11.

coiling the conductors, and I succeeded with it. It was very successful indeed, only when I was through it and saw what kind of machine I got, I was thoroughly disgusted and gave it up. I think I can show you the chain of reasoning. It is quite interesting in showing what a unipolar machine speculation can lead to. I started with Faraday's disk, Fig. 10. The magnet is N S. Then electromotive force is induced, say in the direction of the arrow. To multiply the E. M. F. I took a second disk.

Now, connecting the outsides of the disks with each other, in Fig. 11, I could return the E. M. F. of one disk by the other, and thereby bring the two brushes to the center, as shown in Fig. 11. By doing this, I have to revolve the part of the magnetic circuit between the disks, that is, make a kind of an armature, consisting of an iron ring between the two Faraday disks.

Evidently these disks have to be slotted to get rid of eddies. But if you slot them you are enabled to coil the conductor, as shown in Fig. 12, by offsetting the slots, so that the circuit leads from one segment to the next, and thus you have solved the problem of coiling the conductor in the unipolar machine, but if you look at what you have, in Fig. 12, you see it is nothing but the regular bipolar dynamo. Since that time I gave up trying to improve unipolar machines.

THE PRESIDENT:—I will ask Prof. Crocker to close the discussion.

PROF. CROCKER:—In regard to what Mr. Stanley has said, it seems to me that, as Mr. Kennelly stated, it is simply the question of the alternating versus direct current which is not the subject under discussion. But taking the case as Mr. Stanley stated, we should have there all the complications of armature windings. You would entirely lose that simple and solid armature construction. You would require insulation, etc.

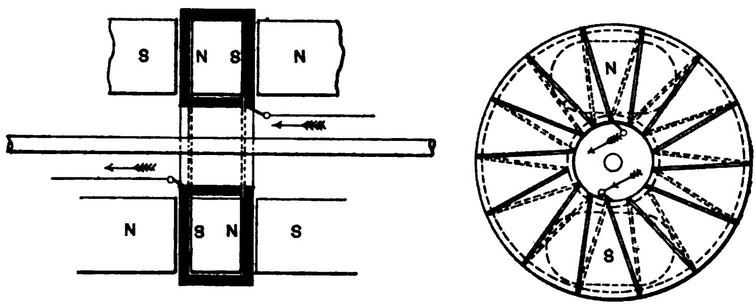


FIG. 12.

You complicate greatly the mechanical construction. Furthermore, the alternator certainly would not do for electro-metalurgical purposes. It would not do to-day, for instance, for a central station, where they are using direct current and want another dynamo to give a larger output. This paper relates simply to unipolar direct current machines, and I was not comparing them with alternators. I was comparing them to the ordinary type of direct current machines.

In regard to what Mr. Steinmetz has said, those ideas are exactly the things I wanted to avoid. My experience has shown me that when we depart from this simple disk or cylinder construction, we get into all these complications and finally arrive at a result which is very inferior to the ordinary Gramme or Siemens machine.

A paper presented at the eleventh General Meeting of the American Institute of Electrical Engineers, Philadelphia, May 16th, 1894. President Houston in the Chair.

ALTERNATING CURRENTS AND FUSES.

BY PROF. DUGALD C. JACKSON AND R. J. OCHSNER.

This paper is a report of an investigation made by Mr. Ochsner in the electrical engineering laboratory of the University of Wisconsin. The results are positive, and of sufficient practical value to justify consideration. They set at rest all questions regarding the direct effect of the alternating current upon the average commercial fuse wire. Practically speaking, there is no such action. Mr. Ochsner's report is as follows:

The following work was undertaken with a view to studying the disintegrating effect of alternating currents on fuse metals which has been reported, and, if possible, ascertaining the cause. As the work done at Cornell University[1] last year showed such a remarkable rise of resistance, and a lowering of the fusing points of the fuses placed on the alternating circuits, I decided to repeat the experiments, and if any change in the resistances should take place to prolong them.

1.—The Effect of Alternating Currents on the Resistance of Fuses.

Samples of fuse wire of five amperes rated capacity were obtained from five different manufacturers, and to these were added a 30-ampere fuse wire which we happened to have, and a copper, a german silver, and an iron wire. Pieces of wire from each sample, each of them varying from nine to ten feet in length, were wound upon cylindrical pieces of pine wood about a foot long and one and one-half inches in diameter. The table

1. "The Action of Continuous and Alternating Currents on Fuse Metals," by C. P. Matthews; TRANSACTIONS, vol. x, p. 251.

below gives the names of the firms from which the samples were obtained and their rated capacities.

	Rated Capacity.
Ansonia Electric Co. (Wirt fuse wire)	30
Shawmut Fuse Wire Co. (two samples)	5
The Independent Electric Co.	5
Peru Electric Manufacturing Co.	5
The E. S. Greeley & Co.	5
Taylor, Dee & Mack	5
Iron wire	
German silver wire	
Copper wire	

To prevent the turns of wire from crowding together on the cylinders, thus causing short-circuits; short, helical grooves were made with a file. To the end of each fuse, short pieces of heavy copper wire were soldered, and the joints were wiped with a damp cloth. These terminals were fastened to the wood by light staples, and the ends were amalgamated.

A simple rack was constructed so that all the fuses could be put in series, the copper terminals dipping into mercury cups.

The resistances were determined by means of the Anthony bridge in the laboratory of the University of Wisconsin, the readings being taken to the fourth decimal place. Connections were made by dipping the copper terminals of the fuses into mercury cups, and using short, heavy copper wires for leads. A dead-beat galvanometer was used, and in this way it was possible to do the work in about twenty minutes. As it was impossible to keep the laboratory at a constant temperature it was necessary to make temperature corrections, and, therefore, to determine the temperature coefficients of all the samples. It was, however, possible to keep the temperature within a few degrees of 20° centigrade, which I considered the standard temperature, and to which I reduced all my results.

Therefore it was thought sufficiently accurate to determine the coefficients in the following way, a small error in the coefficient introducing only a slight error for a correction of a few degrees. On one occasion the room was allowed to cool to the outdoor temperature and the resistances were measured, and the next day the room was heated as high as possible, and the resistances were again measured. A range of about 16 degrees centigrade was obtained. The average rise of resistances for one degree, and the coefficients, were then calculated.

The bridge has a temperature coil, from the resistance of which the temperature of the bridge coils is quickly found. The temperature coefficient of the bridge coils is only .00023, and, therefore, the corrections for them can be made very accurately. The rise of resistance for one degree centigrade and the coefficients, together with the data from which they were obtained are given in the following table, in which t is the higher temperature, t_1 the temperature of the bridge coils, and R the measured resistance corresponding. Similarly the letters marked with a prime correspond to the lower temperature.

No.	t	t_1	R	t'	t'_1	R'	Rise for 1° C.	Coeff.
1	23.2	22.8	.4794	7.26	10	.4505	.00190	.00400
2	"	"	.0858	"	10	.0813	.00304	.00357
3	"	"	1.3025	"	10	1.2354	.00444	.00344
4	"	"	.8273	"	9.9	.7816	.00301	.00368
5	"	"	.980.	"	9.9	.9259	.00357	.00368
6	"	"	.8683	"	9.9	.8209	.00313	.00364
7	"	"	.9701	"	9.9	.9107	.09390	.00406
8	"	"	.2184	"	9.8	.2001	.00119	.00535
9	"	"	.8309	"	9.8	.8302	.00020	.00024
10	"	"	.4792	"	9.8	.4503	.00190	.00401

The fuses were now put on the alternating lighting circuit which furnishes the light for the laboratory. Enough resistance was put in series to give a current of about three amperes. The pressure is 110 volts, and the frequency, approximately, 125. The lighting circuit is run continuously, except for about nine hours on Sundays; but to have a check on the time that the fuses were subjected to the current, a Thomson recording wattmeter was placed in circuit.

The resistances of the fuses were measured at fairly regular intervals, and the corrected results were plotted.

The following table is a sample of one measurement. The resistance of the temperature coil was considered to vary uniformly during the test.

No.	Temp.	Resis. Temp. Coil.	Resis. Leads.	Resistance.
1	20.30	267.2	.00322	.4785
2	20.30	....	.....	.0883
3	20.35	..	.. ...	1.2950
4	20.38	....	......	.8221
5	20.40	....	......	.9754
6	20.44	....	......	.8643
7	20.56	....	.....	.9647
8	20 60	....	......	.2190
9	20.60	267.4	.00320	.8317

Average = .00321.

While making the ninth measurement, I noticed that touching the fuses with the hands caused a decided increase in the resistance, and upon investigation it was found that a rise of

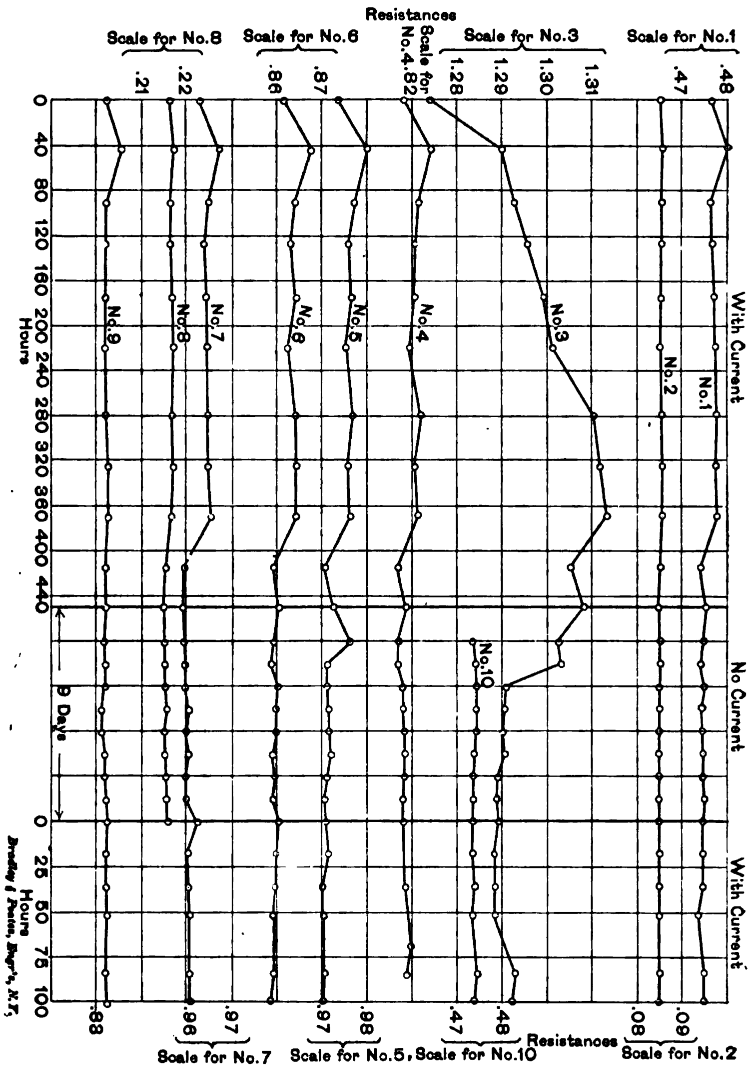


FIG. 1.—Changes in resistance of fuse wires, subjected to the action of alternating currents.

over one per cent. resulted from the heating of the fuses when holding them firmly with one hand. After that discovery they were handled with greater care, and a drop which is noticeable in the curves was the result. The heating effect of the bridge battery, although very small, was also found sufficient to introduce a slight error, but by depressing the keys only for an instant, the error was rendered negligible. A little later I dis-

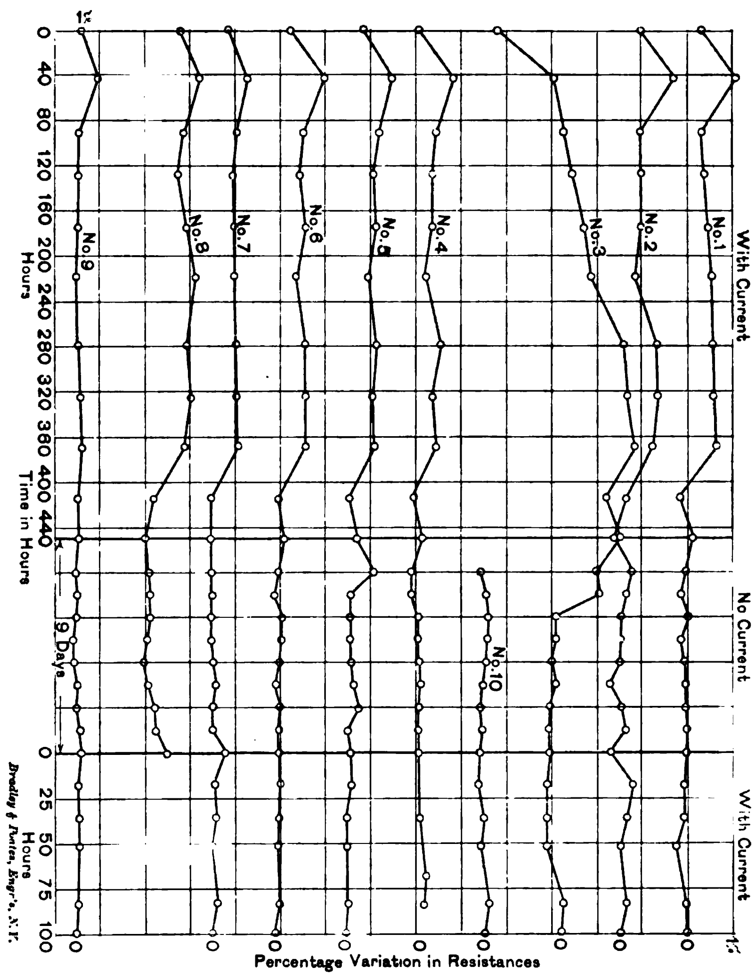


FIG. 2.—Percentage variations in the resistance of fuse wires subjected to the action of alternating current, vertical scale, 1 division equals 1 per cent.

covered that the indications of the thermometer which was hung near the fuses did not, by any means, represent the true temperature of the fuses. They had been heated by the alternating current to about 50° centigrade, and had not been given sufficient time to cool down to the temperature of the room, the pine wood retaining the heat for a considerable period. It was therefore decided not to pass any current through the fuse for some time, but to measure the resistance, as before, from day to day, being very careful in getting the temperatures accurately; but in the latter I was not entirely successful. It was found practically impossible to get the temperature of the fuses correct within a few tenths of a degree, because the temperature of the room would vary in spite of all care, and the fuses would not follow this variation so rapidly as the thermometer indicated it. Nevertheless, far better results were obtained than before.

The result of this part of the work is shown by the curves, Figs. 1 and 2, between the two heavy vertical lines.

After this, the fuses were again subjected to the alternating current for about 100 hours, measurements of resistance being made daily. Each day, as the current was turned off, they were cooled down rapidly by taking them into a cold room, and leaving them for some time, after which they were brought back to the working room and allowed to stand for about five or six hours before measuring the resistances. The results are shown by the last part of the curves. As fuse No. 3 seemed to show greater irregularity than the others, another piece of the same sample was added. It is the one numbered 10, and the curve is plotted directly under No. 3 for comparison.

From the fourteenth point on, the curves are all practically straight horizontal lines, the irregularities being about the same when no current passed, as when it did. Nearly all the points on the curves before the fourteenth were considerably higher. This shows plainly that the fuses had not cooled down to the temperature of the room when the resistance measurements were made, the variations depending on the length of time they were allowed to stand. Considering the average values of the last parts of the curves to represent the true resistances, the greatest variation was 1.8 per cent. in the case of No. 3, and even this is not too large to be explained by the above errors. In proof of this, I measured the resistance of one fuse immediately after turning off the current, and also 15, 20, 35 and 50 minutes later, and found the errors to be respectively 8 per cent., 6.2 per cent., 3 per cent, 2.3 per cent. and 1 per cent.

That the first point on each curve is nearly as low as the final values, is due to the fact that current had not been passing through the fuses before that measurement was made, and the fuses were, therefore, at the temperature of the surrounding air which was recovered by the thermometer. Another proof that the variations are due to temperature is shown by curve No. 9. This represents a wire which has a very small temperature coefficient, and accordingly, it varies the least of any. While the actual rise in curve No. 3 is much greater than in any of the others, the percentage rise is only slightly greater, the resistance of the fuse being the largest of all.

These experiments prove that there is no appreciable rise of resistance in fuses subjected to alternating currents, at least not within a period of 550 hours. More accurate results might undoubtedly have been obtained, had the fuses been mounted in such a manner as to avoid contact with any large masses of solid material, but the accuracy is amply sufficient to prove the case.

These results are diametrically opposed to those given in the report of similar work, published in the TRANSACTIONS, vol. x, p. 262 already referred to. While in the latter investigations, corrections were made for the standard resistance with which the fuses were compared, no such corrections were made for the fuse wires. The fuses were subjected to larger currents than in my tests, with consequent greater heating. The resistances were apparently measured without allowing sufficient time for the fuses to acquire the temperature of the room, and even if this was done, the variation in temperature of the room as given in the original report of the investigation (which Mr. C. P. Matthews, Instructor at Cornell University, kindly sent me) was sufficient to introduce serious errors. The resistances were measured by the fall of potential method. According to the drawing, showing the arrangement of the apparatus, no extra resistance is put in series with the battery to reduce the current strength. This current may have been sufficiently great, in the smaller sized fuses at least, to cause considerable heating. For these and other reasons relating to the accuracy of the tables, I think that this part of the work reported by Mr. Matthews is not to be relied upon, and, therefore, the conclusions based upon it are of no value.

2. The Effect of Alternating Currents on the Fusing Points.

Next the fusing points before and after passage of current were determined. As the continuous current from the dynamo which was available was too variable, I was obliged to use a few storage cells for fusing the wires.

A small, sensitive resistance which could be varied slowly was needed. A long german silver wire, stretched tightly, with a good spring clamp for a sliding contact, was found to serve admirably.

Six-inch lengths of the fuses were soldered to short, heavy copper wire terminals, the ends of which were amalgamated and dipped into mercury cups in making connections. The fuse, variable resistance, storage battery, and a Weston ammeter reading from 0 to 15 amperes, were placed in series. As the current approached the fusing point it was varied very slowly and gradually, giving it ample time to heat the fuse. Only the five ampere wires were tested. A number of pieces of each of these were tested; half of these had been subjected to the alternating current, and half had not. I shall call these the "old" and the "new" samples respectively. Pieces of the old and the new of each sample were tested alternately so as to eliminate the errors caused by a change in the temperature of the room. The results are given in the following tables:

Fusing Currents.

Nos. 3 and 10 New.	No. 3 Old.	No. 10 Old.
7.40	7.25	7.30
7.30	7.10	7.29
6.98	7.10	7.10
7.19	7.09	7.20
7.30	6.80	7.05
Average, 7.23	7.07	7.188
	Lowered 2.22 per cent.	Lowered .58 per cent.

No. 4.		No. 5.		No. 6.		No. 7.	
New	Old	New	Old	New	Old	New	Old
8.75	8.50	8 90	8.82	8.85	8.80	8.80	9.07
8.83	8.60	8.9	8.84	8.6[illegible]	8.69	8.96	9 06
8.80	8.60	8.20	9.12	8 87	8.88	8.90	9.02
8.70	8.70	9.20	8.88	8 78	8 98	8.89	9.05
8.76	8.65	8.95	8.88	8.75	8 98	8.90	9.10
8.70	8.70	9.10	8.97	8.60	8.80	9 06	8.85
8.62	8.64	9.06	8.70	8.75	8.74	9.10	8.95
8.71	8.64	9.08	8 80	8.50	8.70	8.78	9.10
8.50	8.64	8.93	8.79	8.96	8.95	8.66	8.80
8 88	8.65	9.09	8.83	8.77	8.90	8.90	9.12
Average 8.725	8.632	8.941	8.863	8.743	8 842	8.895	9.012
Lowered 1.07 per cent.		Lowered .87 per cent.		Rise of 1.13 per cent.		Rise of 1.32 per cent.	

Four of these samples show a slight lowering, while two show a slight rise in the fusing point, but the differences are of the same order as the differences between individual test pieces. While a test of only a comparatively small number of fuses in this way hardly establishes the correctness of the results, a slight change in the fusing points probably did take place, as is shown by the fair uniformity of the figures in each sample.

That such slight change as did occur was due to oxidation there is also little doubt, for all of the old fuses showed slight oxidation, and the effect is too small to be of practical moment.

As to the results in the earlier investigation already referred to [TRANS. Vol. x, p. 65.] on the fusing points, only the averages are given, and no mention is made of temperature; therefore it is impossible to properly judge of their accuracy. As far as can be determined from the original report, however, the tests do not seem to be any more reliable than the resistance tests.

Sir David Salomons in the discussion of a paper, by Mr. A. C. Cockburn, read before the Society of Telegraph Engineers and Electricians in 1886, stated that fuses alter in two ways with time. Most of the metals, especially alloys, according to Alexander Siemens, being subject to slight oxidation, which lowers the fusing point.

Dr. Salomons' own experience, he says, was rather the other way, the fusing point being raised with long use.

Mr. Cockburn had the same experience. Mr. Matthews also states that the formation of oxides in the case of tin, and the tin and lead alloys, may delay the fusion almost indefinitely, while the oxidation of copper and iron wire seems to favor its prompt fusion.

3. Conclusions.

In the light of the experiments herein described, and the statements made by Cockburn, Salomons, and Matthews, is it necessary to ascribe a slight change in the properties of the fuse to the disintegrating effect of the alternating current? I think not.

In practice, a fuse is seldom obliged to carry a current equal to its full rated capacity. In electric lighting, for instance, the lamps protected by a fuse are not likely to be all turned on at once, and therefore the fuse is seldom carrying more than a small part of its rated capacity. The experiments that I have made, therefore, represent the conditions of ordinary practice and prove that a fuse as used in practice is not directly affected by the alternating currents.

Fuses on alternating current circuits are sometimes found to blow without apparent reason, but something outside of the disintegrating effect of the current is the cause. Under some conditions, the result has come about through the mechanical shaking to which an alternating current fuse is sometimes subjected when not tightly clamped by the terminal screws.

Discussion.

Dr. Leonard Waldo:—Papers of this class would have a very much higher value if they were accompanied by an analysis of the metals that were used. The theory underlying such a paper is this:—that the fuse wire is made up of homogeneous material like a copper conductor. Nothing can be further from the truth. A fuse wire as usually made, consists of two or more metals which have very little affinity for each other, which have low melting points, and which under the repeated action of a heating-to-softness current simply shift the position of the relative metals. Any theories based on the resistance of the metal, must take into account the actual condition of the metal as the heating current is sent through it. I think the literature of fuse metals is in a very imperfect state, and if we are to have a theoretically perfect metal for fuse purposes it must embody the condition of not changing the physical condition of the metals of which it is composed in the act of heating, and it must therefore be a homogeneous metal, of the quality of pure tin, or iron or steel, or of aluminium-bronze, for instance. The valuable qualities of fuse metal must depend upon using the minimum amount of energy to affect the heating to a melting point, and it is actually finishing its existence as a continuous conductor at the point where the current becomes unsafe. I only wish to make the two points, that papers on fuse metals, should inform us of what the fuse metals were composed; and that the essential quality of a good fuse metal is that it should be homogeneous.

PROF. J. P. JACKSON:—I do not think the paper was intended to indicate what would be a good fuse metal. A few commercial samples of fuses were taken directly from the factory and careful tests made with as many corrections as possible. The tests were not as full as they should have been. Nevertheless they are quite accurate. and the results show a very even loss of resistance all through the period of the test of each fuse. They also show exceedingly small variation in the fusing pôints due to the effect of alternating current upon them. Of course if the fuse is not homogeneous—if it is made of cast iron or some such metal —the alternating current may possibly rack or crystallize it. In this case it will be affected. But on the commercial fuses under consideration—and I put stress on the word commercial, because the paper is given to show what the practical effect is on such fuses, without going into the chemical theory—on these commercial fuses the paper indicates that the effect of alternating currents is *not* injurious.

[COMMUNICATED BY SIR DAVID SALOMONS, OF LONDON.]

With regard to the remarks made in the paper on alternate currents and fuses, I will venture to give my experience with fuses since the time referred to, now many years ago. I still find that the fuse wire is apt to alter, and that in no two samples of wire is the alteration in the same way; which is chiefly due to impurities in the metals. I am a great advocate for tin wire, since it is cheap and very good in practice. If a fuse is to be of service the whole time during which the current is passing, the wire will be raised to a very high temperature; and from an exhaustive series of experiments made on various metals and alloys kept at high temperatures and periodically cooled, I find that it becomes more or less crystalline or granular after a time. In this respect, I observe that the experience of others agrees with my own; also that, when the wire is in the crystalline or granular state, a larger current may be passed before it gives way than when the fuse was new. One of the chief reasons why the alternate current appears to alter the fuse wire more than the direct current does, is I think, the fact the variations in the E. M. F. with the alternate current system are greater than with the direct. Consequently, the variations in the temperature of the fuse wire with the alternate current is greater than with the direct.

A paper presented at the Eleventh General Meeting of the American Institute of Electrical Engineers, Philadelphia, May 16th, 1894. President Houston in the Chair.

TEST OF A CLOSED-COIL ARC DYNAMO.

BY PROF. R. B. OWENS AND C. A. SKINNER.

That so much has been said and written concerning the design of constant potential machinery, effects of armature reactions, control of sparking, etc., and so little concerning the machinery used for arc lighting, seems rather remarkable considering the fact that by far the larger part of our outside lighting as well as much inside lighting is done by means of arc lamps in series, and I hope a discussion will follow in which more light will be thrown on the principles of design of arc machinery, for at present there seems to be much empiricism in the matter.

The immediate object of the present paper is to show something of the nature of the armature reactions which occur in arc light machines of the closed-coil type, maintaining constant current by automatically shifting the brushes to correspond with changes in load in the external circuit, and to point out certain alterations in design which it is believed may be adopted with advantage. Incidentally other points will be noted. Of course there are other well known methods of maintaining the current constant: notably by shunting the field magnets as in the Brush machine, or a combination of this with shifting the brushes as in the new Excelsior machine, or by varying the length of time per revolution during which the armature coils are in series and shunt to each other or short-circuited as in the T.-H. machine, but these will not be considered at present. Nor is it intended here to discuss the relative advantages of the several types, but only to give some results obtained in a test of a closed-coil machine regulating as above mentioned. Such machines are now taking a prominent part in the arc lighting industry, and their prominence merits for them more close study. Alternating current dynamos, as the Westinghouse arc light machine, have

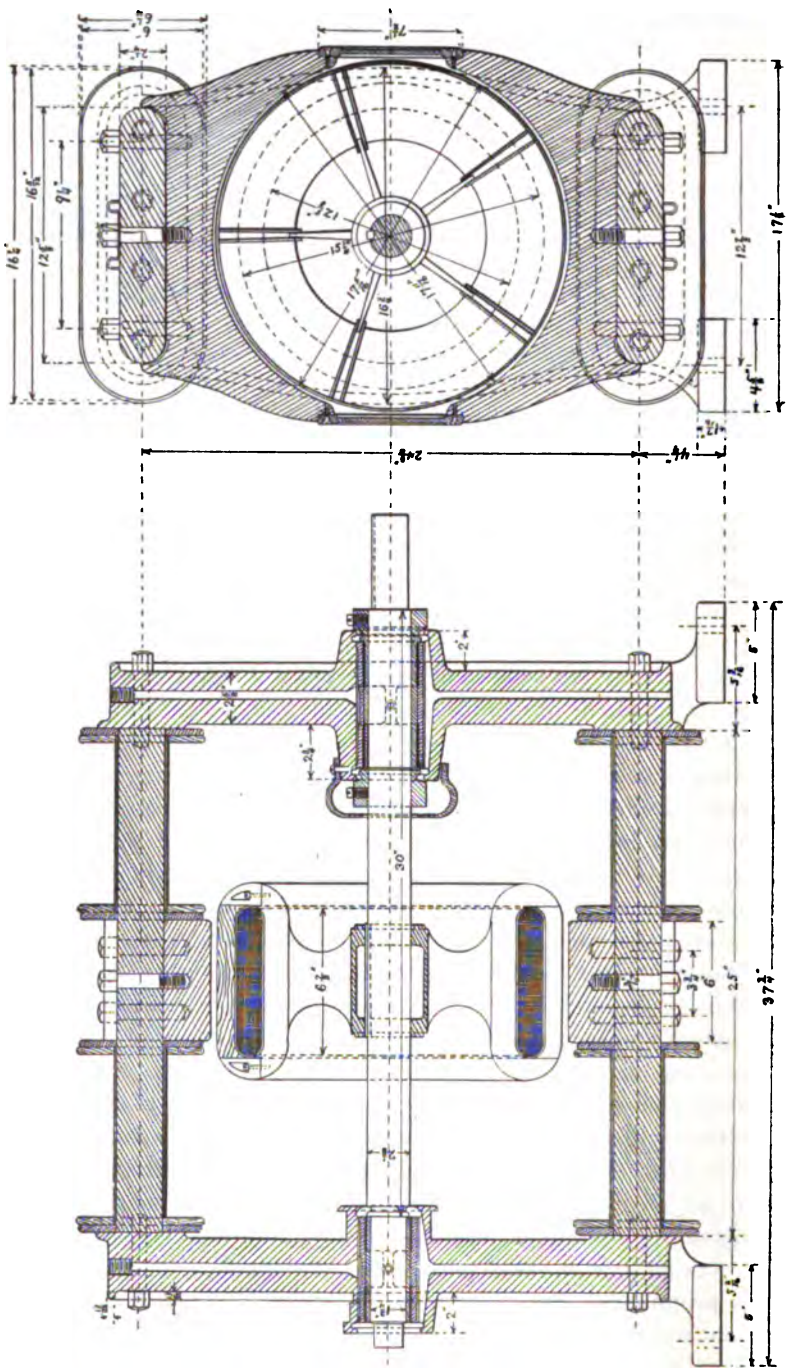


Fig. 1.

been devised which keep the square root of the mean square of the current remarkably constant through extremely wide variations in load, and without any external regulating mechanism whatever. They possess many advantages, but are not now widely used, for arc lamps seem as yet to work more satisfactorily on continuous current circuits. We believe it is also possible to build continuous current arc machines which by armature reactions alone can keep the current very nearly constant throughout a considerable range, but it would seem that their cost would be more, and their efficiency less, than in some of the types using an external regulating mechanism, although we have no exact comparative data.

The machine on which the following experiments were made is a No. 6, 25-light 2,000 C. P. Wood arc dynamo, a scale drawing of which is shown in Fig. 1.

From the drawing all dimensions as of magnet limbs, yoke, pole-pieces armature, etc., are at once seen. Its designer, Mr. James J. Wood, most courteously consenting to my giving the name of the machine tested, and its full data.

The winding data and other details as furnished by the makers are as follows:

The field magnet winding is composed of four coils of No. 10 B. and S. gauge copper wire, single cotton covered. The outside diameter when insulated is 0.114 in. Each coil contains 100 lbs. of wire in 15 layers of 74 turns each.

The insulation of the magnet cores, is 1-8 in. thick, and composed of one layer of enameled cloth, the enameled surface facing the iron, the remainder being composed of pressboard 0.025 in. thick. The magnet heads are wooden washers 5-16 in. thick, carefully dried and shellaced.

The armature core is composed of No. 10 B. and S. gauge annealed charcoal iron wire. This is wound on a former which is removable, and is composed of 15 layers, 6¾ in. wide. These wires are held together by interposed strips of linen tape. The core is then insulated with a layer of asbestos paper, 3 thicknesses of pressboard 0.15 in. thick, one layer of asbestos paper again, and then over this one more layer of pressboard 0.15 in. thick, making a smooth surface for the copper wires to be wound on. The insulations near the spider arms are built up of the same material and in the same manner as the armature core, until they attain a thickness of ¼ in. The armature is wound with 100 sections of No. 14 B. and S. gauge double covered cotton wire. Each section is composed of 93 feet, or 57 turns per section, making a total of 115 pounds of wire.

The regulator magnet is wound with No. 11 single cotton covered B. and S. gauge wire, in the manner shown on the dynamo. The insulation of the cores being 5-32 in. thick, and

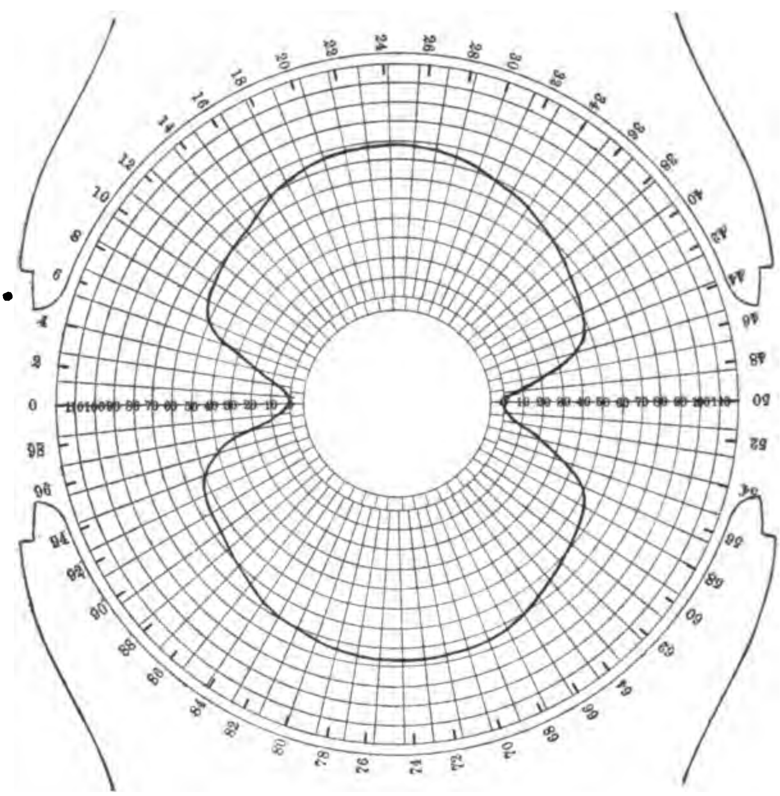


FIG. 2.—Showing distribution of E. M. F. with 10 amperes in field and no current in armature.

that of the magnet heads 3-16 in. Speed of dynamo, 1000 rev. per minute.

CONDITIONS OF TEST.

The dynamo was securely bolted to a firm foundation of masonry, and driven through a 7 in. belt by a small 8 in. x 10 in. high-speed automatic Atlas engine making 250 revolutions per minute. Steam was supplied at as nearly constant pressure as possible. The automatic regulator of the dynamo was removed, together with one pair of opposite collecting brushes, and the remaining pair reduced one-half in width parallel to the commutator bars, to allow of easier manipulation of the exploring brushes, their angular width being of course adjusted for each load to prevent sparking.

TABLE I.

DISTRIBUTION OF E. M. F. AROUND THE COMMUTATOR, WITH FIELDS SEPARATELY EXCITED; NO CURRENT IN ARMATURE. BY THE TWO-BRUSH METHOD.

Current in Fields—8 Amps.				Current in Fields—10 Amps.				Current in Fields—12 Amps.			
Scale Reading.	Volts.	Scale Reading.	Volts.	Scale Reading.	Volts.	Scale Reading.	Volts.	Scale Reading.	Volts.	Scale Reading.	Volts.
0	0.8	50	1 0	0	0.4	50	0.7	0	1.0	50	1.1
2	10.5	52	11.7	2	9.0	52	11.0	2	10.0	52	11.9
4	31.6	54	34.6	4	31.0	54	35.0	4	31.3	54	35.0
6	51 0	56	53.0	6	51.0	56	54.0	6	51.6	56	53.6
8	55.3	58	57.0	8	55.5	58	57.4	8	56 0	58	57.0
10	57.1	60	59.0	10	58.0	60	59.9	10	58.9	60	60 0
12	59.9	62	61.5	12	61.6	62	63.5	12	63.7	62	64.0
14	62.3	64	64.1	14	65.3	64	67.5	14	68.0	64	69.0
16	64.9	66	66.3	16	68.9	66	71.0	16	73.0	66	73.0
18	67.1	68	69.0	18	72.6	68	74.0	18	77.1	68	77.9
20	69.9	70	71.0	20	75.8	70	77.0	20	81 5	70	81.0
22	71.2	72	72.0	22	78.5	72	78.5	22	83.8	72	82.2
24	70.8	74	72.0	24	77.5	74	78.9	24	83.8	74	82.7
26	70.8	76	71.8	26	77.4	76	78.1	26	83.8	76	82.3
28	69.9	78	70.3	28	76.0	78	77.0	28	81.0	78	81.0
30	67.1	80	68.5	30	73.8	80	75.0	30	79.0	80	78.0
32	65.0	82	65.9	32	70.0	82	71.3	32	75.1	82	74.0
34	62.5	84	63.0	34	66 3	84	67.0	34	70.1	84	69.0
36	69.0	86	59.4	36	62.5	86	63.0	36	65.0	86	64.0
38	57.5	88	56.6	38	58.8	88	58.8	38	60.0	88	59.0
40	55.2	90	54.1	40	55.8	90	55.0	40	56.0	90	55.0
42	54.0	92	52.5	42	54.0	92	52.7	42	54.1	92	53.0
44	45.9	94	46.3	44	45.5	94	47.0	44	47.0	94	47.5
46	24.0	96	24.0	46	24.0	96	23.0	46	24.1	96	24.1
48	6.0	98	5.7	48	5.2	98	6.0	48	6.4	98	6.0

With 8, 10 and 12 amperes the neutral points remained at the positions .6 and 50.6. Check readings taken after each column of observations showed a variation of less than 2 per cent. in all cases.

It was at first attempted to use a number of arc lamps as load for the dynamo, but though carefully adjusted and using cored carbons, the variations of potential and current due to their feeding was greater than could be admitted, so the lamps were discarded for two water rheostats 4 feet × 1 foot × 1 foot, with carbon electrodes. These latter on the whole were found to work quite satisfactorily, their resistance hot was considerably less than when cold, but it changed so gradually that no trouble was experienced in correcting for it by adjusting the electrodes.

The distribution of the induction entering the armature at different loads was obtained by taking the E. M. F. at various points on the commutator between two small pilot brushes moved around and in contact with it.

The two brush method, though somewhat more difficult to

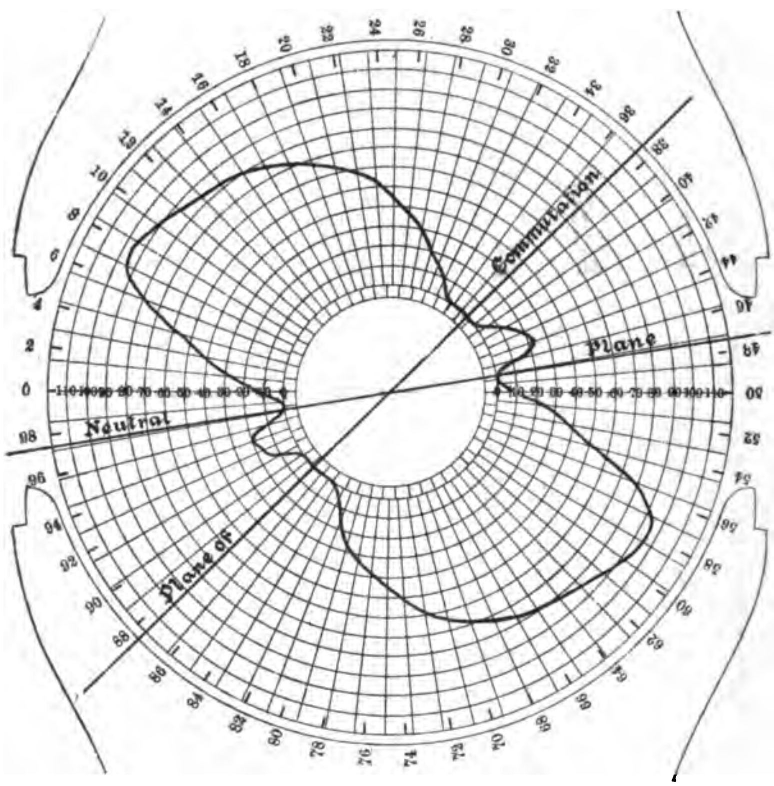


FIG. 3.—E. M. F. with 10 ampere current in field and armature, 25-light position.

work than the single brush method, has the advantage over the latter of giving the quantities sought directly, instead of as a difference between quantities which are large as compared with those desired. In some cases the integral readings of the brush method have been plotted, but curves so obtained do not indicate so clearly what is mainly sought, namely, the distribution of lines of force entering the armature. True, one curve can be approximately obtained from the other, if electrostatic instruments are used, but not with electromagnetic instruments, for they will give only a mean E. M. F. depending on the relative width of a commutator bar and insulation and distance apart of

TABLE II.

DISTRIBUTION OF E. M. F. AROUND THE COMMUTATOR, AS SHOWN BY THE TWO-BRUSH METHOD.

Position of the toe of collecting brushes, 38.8 and 88.8.
Number of segments covered by each collecting brush, 7.

Current—8 Amperes.				Current—10 Amperes.				Current—12 Amperes.			
Scale Reading.	Volts.	Scale Reading.	Volts.	Scale Reading.	Volts.	Scale Reading.	Volts.	Scale Reading.	Volts.	Scale Reading.	Volts.
0	+ 19.7	50	− 20.0	0	+ 22.5	50	− 22.0	0	+ 24.0	50	− 23.3
2	+ 33.8	52	− 34.9	2	+ 38.0	52	− 38.9	2	+ 39.9	52	− 40.5
4	+ 60.6	54	− 62.0	4	+ 66.0	54	− 70.5	4	+ 72.0	54	− 73.0
6	+ 85.0	56	− 86.2	6	+ 92.0	56	− 93.8	6	+ 100.0	56	− 101.5
8	+ 86.4	58	− 86.3	8	+ 92.9	58	− 94.0	8	+ 101.4	58	− 100.9
10	+ 82.0	60	− 82.4	10	+ 88.5	60	− 89.1	10	+ 96.0	60	− 95.7
12	+ 78.1	62	− 78.7	12	+ 84.0	62	− 85.2	12	+ 91.0	62	− 91.6
14	+ 74.7	64	− 75.0	14	+ 80.0	64	− 81.4	14	+ 87.0	64	− 87.2
16	+ 70.0	66	− 70.1	16	+ 76.0	66	− 77.4	16	+ 81.6	66	− 82.2
18	+ 65.6	68	− 65.4	18	+ 70.2	68	− 72.0	18	+ 75.9	68	− 76.5
20	+ 60.7	70	− 60.5	20	+ 64.3	70	− 66.6	20	+ 69.2	70	− 70.9
22	+ 54.1	72	− 54.0	22	+ 57.1	72	− 58.7	22	+ 59.6	72	− 61.2
24	+ 46.7	74	− 46.2	24	+ 46.7	74	− 49.3	24	+ 48.0	74	− 50.0
26	+ 38.0	76	− 38.0	26	+ 36.8	76	− 38.9	26	+ 37.6	76	− 37.6
28	+ 29.9	78	− 58.7	28	+ 24.5	78	− 27.8	28	+ 20.2	78	− 24.2
30	+ 20.3	80	− 18.8	30	+ 15.2	80	− 16.0	30	− 6.2	80	− 10.2
32	+ 9.9	82	− 9.1	32	+ 1.5	82	− 3.3	32	− 10.1	82	+ 4.7
34	0.0	84	− 0.7	34	0.0	84	− 0.8	34	0.0	84	+ 4.8
36	0.0	86	0.0	36	0.0	86	0.0	36	0.0	86	0.0
38	+ 0.3	88	0.0	38	+ 1.0	88	0.0	38	+ 0.4	88	0.0
40	+ 5.1	90	− 3.7	40	+ 8.8	90	− 4.0	40	+ 8.8	90	− 6.6
42	+ 14.4	92	− 12.4	42	+ 19.6	92	− 16.1	42	+ 21.2	92	− 18.0
44	+ 16.4	94	− 15.7	44	+ 21.5	94	− 19.2	44	+ 24.4	94	− 23.0
46	+ 3.0	96	− 3.2	46	+ 5.4	96	− 4.0	46	+ 4.3	96	− 6.9
48	− 9.0	98	+ 7.3	48	− 9.1	98	+ 9.1	48	− 9.8	98	+ 10.0

Check readings after each column of observations showed a variation of less than 2 per cent. in all cases.

With 8 amperes the voltage between the collecting brushes was 1160; between neutral points, 1225; the neutral points being at 47.6 and 97.6.

With 10 amperes the voltage between brushes was 1190; between neutral points, 1235; the position of the neutral points being 47.6 and 97.6.

With 12 amperes the voltage between brushes was 1236; between neutral points, 1319; the neutral points being at 47.7 and 97.7.

the pilot brushes. The results obtained however with a voltmeter of the Weston type, are proportional and for the present purpose are equally valuable.

The exploring brushes as finally used were pieces of steel watch springs firmly held in small fibre holders. These, in turn were rigidly secured by brass studs to a graduated sliding ring, moving within another stationary ring, attached to and insulated from the dynamo frame and carefully centered with the commutator.

Two brushes were used in each holder to better ensure good contact. Copper, brass and phosphor-bronze exploring brushes were tried at first, but found not as satisfactory as steel. It was also

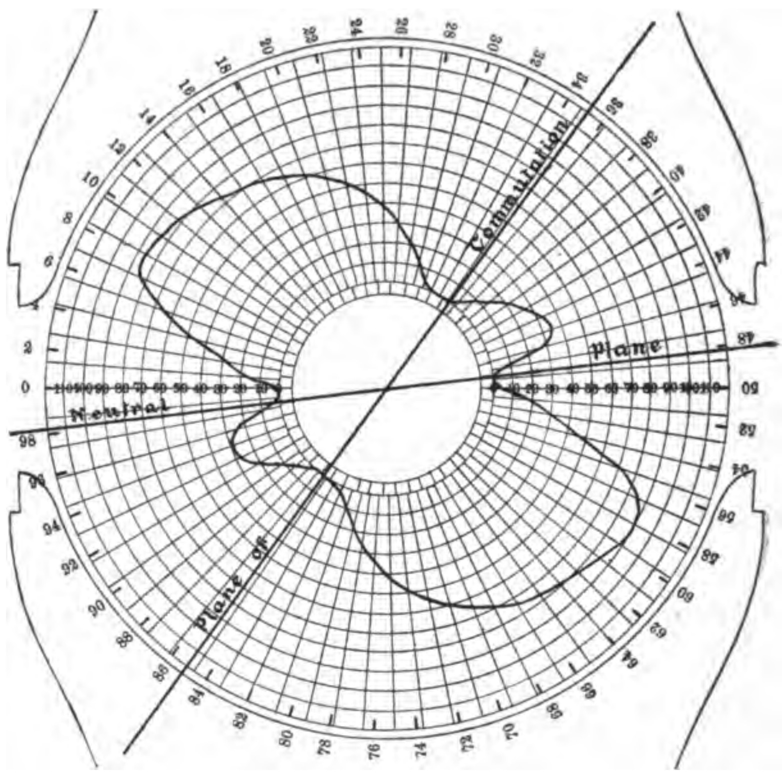


FIG. 4.—E. M. F. with 10 amperes in field and armature, 20-light position.

found very necessary at all times to keep the brushes and commutator as clean as possible.

The sliding ring was marked off into one hundred divisions, the number of commutator segments, and the exploring brushes made to cover just two commutator bars or one fiftieth of the circumference, but the ring might have been divided into degrees if desired. The curves shown in figures 2, 3, 4, 5, 6, and 7 are plotted so that the results may be read either in degrees or in divisions of the graduated ring.

The inner circle represents the commutator of one hundred segments, and the 100 divisions of the ring are marked on the outer circle.

TABLE III.

DISTRIBUTION OF E. M. F. AROUND THE COMMUTATOR, AS SHOWN BY THE TWO-BRUSH METHOD.

Position of the toe of collecting brushes, 31.6 and 81.6.
Number of segments covered by collecting brushes, 6.7.

Current—8 Amperes.				Current—10 Amperes.				Current—12 Amperes.			
Scale Reading.	Volts.	Scale Reading.	Volts.	Scale Reading.	Volts.	Scale Reading.	Volts.	Scale Reading.	Volts.	Scale Reading.	Volts.
0	+ 16.9	50	− 16.1	0	+ 17.4	50	− 17.2	0	+ 16.3	50	− 16.7
2	+ 32.0	52	− 33.0	2	+ 33.4	52	− 34.6	2	+ 33.5	52	− 35.2
4	+ 58.7	54	− 62.0	4	+ 61.7	54	− 66.0	4	+ 64.1	54	− 67.5
6	+ 82.0	56	− 83.5	6	+ 87.0	56	− 89.5	6	+ 90.0	56	− 92.5
8	+ 83.2	58	− 84.0	8	+ 87.8	58	− 89.7	8	+ 90.1	58	− 91.7
10	+ 78.4	60	− 79.5	10	+ 82.5	60	− 84.9	10	+ 84.5	60	− 86.1
12	+ 74.1	62	− 75.5	12	+ 78.0	62	− 80.0	12	+ 79.3	62	− 81.5
14	+ 70.1	64	− 71.5	14	+ 73.5	64	− 75.9	14	+ 74.6	64	− 76.7
16	+ 65.9	66	− 67.6	16	+ 68.9	66	− 71.6	16	+ 69.3	66	− 71.5
18	+ 60.8	68	− 62.6	18	+ 63.0	68	− 65.5	18	+ 62.7	68	− 65.3
20	+ 55.1	70	− 57.0	20	+ 56.3	70	− 58.9	20	+ 55.2	70	− 58.0
22	+ 48.5	72	− 50.6	22	+ 48.7	72	− 51.2	22	+ 46.1	72	− 49.2
24	+ 39.9	74	− 43.2	24	+ 37.9	74	− 40.8	24	+ 33.0	74	− 37.6
26	+ 31.0	76	− 34.6	26	+ 26.5	76	− 29.9	26	+ 19.5	76	− 24.0
28	+ 21.0	78	− 25.8	28	+ 13.0	78	− 18.5	28	+ 4.7	78	− 11.2
30	+ 12.0	80	− 16.2	30	+ 1.9	80	− 5.1	30	− 8.9	80	+ 1.5
32	0.0	82	0.0	32	0 0	82	0.0	32	0.0	82	+ 1.0
34	0.0	84	0.0	34	0.0	84	0.0	34	0.0	84	0.0
36	+ 5.4	86	− 1 0	36	+ 5.7	86	− .3	36	+ 5.3	86	− 1.5
38	+ 14.2	88	− 9.0	38	+ 15.5	88	− 9.0	38	+ 16.5	88	− 10.6
40	+ 21.8	90	− 17.0	40	+ 24.0	90	− 19.0	40	+ 26.0	90	− 21.5
42	+ 29.0	92	− 24.5	42	+ 32.4	92	− 27.8	42	+ 35.5	92	− 31.0
44	+ 28.8	94	− 26.9	44	+ 33.4	94	− 30.4	44	+ 35.5	94	− 33.0
46	+ 11.7	96	− 9 8	46	+ 13.1	96	− 11.1	46	+ 15.1	96	− 12.6
48	− 4.7	98	+ 4.8	48	− 4.0	98	+ 4.9	48	− 4.0	98	+ 3.5

Check readings taken after the observations in each column showed a variation of less than 1 per cent.

With current of 8 amperes the voltage between brushes was 970; between neutral points, 1182; the neutral points being at the positions 48.1 and 98.1.

With 10 amperes the voltage between brushes was 976; between neutral points, 1210; the neutral points being at positions 48.2 and 98.2.

With 12 amperes the voltage between brushes was 950; between neutral points, 1250; the neutral points being at positions 48.3 and 98.3.

With 8 and 10 amperes the sparking at brushes was very slight, but with 12 amperes it increased to destructive sparking.

The radial lines are 5 degrees apart, and the radial distance between concentric circles represents 10 volts. If the results had been plotted on a developed diagram, then the areas of the curves would have represented total or integral electromotive forces, but the circular diagram has the advantage of appealing more quickly to the eye, and though the total E. M. F. is not exactly represented by the area it is proportional to the number of the small approximate rectangles enclosed. In obtaining efficiency measurements, the power delivered to the dynamo was obtained by indicating the engine. The cards were taken with a Tabor

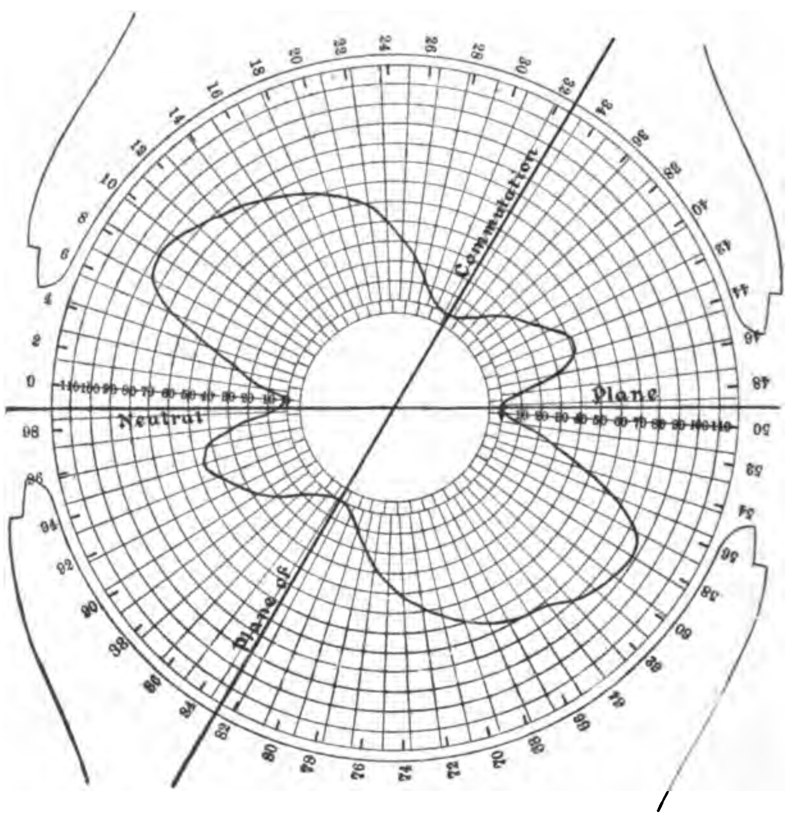


FIG. 5.—E. M. F. with 10 amperes in field and armature, 15-light position.

indicator, and worked up with a Coffin averaging instrument; the speed was obtained by a speed counter, and stop watch. The electrical instruments used were a Weston 0.15 amperemeter for current measurements. A Weston 0.15 and 0.150 voltmeter for potentials between pilot brushes, and a 0.150 and 0.1500 Weston voltmeter for total electromotive forces. All instruments were previously calibrated.

Results Obtained.

Fifteen sets of readings, fifty readings per set, were taken around the commutator with currents in armature and field, of

TABLE IV.

Distribution of E. M. F. around Commutator, as shown by the Two-Brush Method.

Position of the toe of collecting brushes, 30.1 and 80.1.
Number of segments covered by each collecting brush, 4.7.

Current—8 Amperes.				Current—10 Amperes.				Current—12 Amperes.			
Scale Reading.	Volts.	Scale Reading.	Volts.	Scale Reading.	Volts.	Scale Reading.	Volts.	Scale Reading.	Volts.	Scale Reading.	Volts.
0	+ 13.2	50	— 13.7	0	+ 14.0	50	— 14.5	0	+ 12.2	50	— 13.8
2	+ 28.8	52	— 30.0	2	+ 30.7	52	— 32.3	2	+ 29 5	52	— 32.6
4	+ 55.5	54	— 58.9	4	+ 60.3	54	— 64.2	4	+ 60 6	54	— 65.0
6	+ 79.4	56	— 82.0	6	+ 86.5	56	— 88.0	6	+ 88.0	56	— 90.0
8	+ 80 4	58	— 81.8	8	+ 87.0	58	— 87.7	8	+ 88.0	58	— 89.0
10	+ 76.0	60	— 76.3	10	+ 81.7	60	— 82.0	10	+ 82.1	60	— 82.7
12	+ 71.0	62	— 71.7	12	+ 76.7	62	— 76.9	12	+ 76.8	62	— 77.3
14	+ 67.0	64	— 67.0	14	+ 72.0	64	— 72 0	14	+ 72.2	64	— 72.0
16	+ 62.5	66	— 62.0	16	+ 67.0	66	— 66.7	16	+ 67.0	66	— 66.2
18	+ 57.0	68	— 55.9	18	+ 60.6	68	— 60.0	18	+ 59.9	68	— 59.1
20	+ 51.0	70	— 49.2	20	+ 53.3	70	— 52.6	20	+ 51.9	70	— 51.0
22	+ 44.3	72	— 42.1	22	+ 45.0	72	— 43.3	22	+ 41.9	72	— 40.1
24	+ 35.4	74	— 32.3	24	+ 33.1	74	— 32.2	24	+ 27 9	74	— 27.8
26	+ 25.3	76	— 22.2	26	+ 20 4	76	— 19.7	26	+ 13.0	76	— 13.8
28	+ 13.2	78	— 11.2	28	+ 7.0	78	— 6.7	28	— .3	78	+ 2.2
30	+ 3.0	80	— .8	30	— .8	80	0.0	30	— 5.5	80	+ 3.7
32	0.0	82	0.0	32	0.0	82	0.0	32	0.0	82	0.0
34	+ 3.0	84	— 5.6	34	+ 3.8	84	— 6 0	34	+ 4.1	84	— 6.8
36	+ 12.4	86	— 14.1	36	+ 14.8	86	— 16.0	36	+ 16.1	86	— 17.9
38	+ 20 8	88	— 21.9	38	+ 24 0	88	— 25.1	38	+ 26.6	88	— 27.0
40	+ 28.1	90	— 29.0	40	+ 32.5	90	— 33.0	40	+ 35.9	90	— 35.7
42	+ 35.1	92	— 35 9	42	+ 41.0	92	— 41.0	42	+ 45 2	92	— 44.0
44	+ 35.	94	— 36.9	44	+ 40.8	94	— 42.7	44	+ 45.1	94	— 45.7
45	+ 16.9	96	— 16.9	46	+ 19.2	96	— 20.0	46	+ 22.0	96	— 22.0
48	— 1.9	98	+ 1.3	48	— 1.2	98	— .9	48	— .2	98	0.0

After making the observations in each column, check readings were taken at numerous positions, and in all cases the variation was less than 1 per cent.

With current of 8 amperes the voltage between the collecting brushes was 785.

With the same current the voltage between the neutral points was 1150; the neutral plane passing through the 48.2 and 98.2 positions on the scale.

With 10 amperes the voltage between collecting brushes was 784; the voltage between neutral points was 1183; and the position of the neutral points was 48.9 and 98.9.

With 12 amperes the voltage between the collecting brushes was 740; the voltage between neutral points was 1200; and the position of the neutral points, 49 and 99.

With 8 amperes there was slight sparking at the brushes; with 10 amperes there was none; and with 12 amperes there was considerable sparking.

The volts between neutral points varied somewhat, while the volts between brushes remained constant throughout.

8, 10 and 12 amperes, at positions of the collecting brushes approximately corresponding to loads of 5, 10, 15, 20 and 25 lights. Three sets of readings were also taken with currents in the field of 8, 10 and 12 amperes and no current in the armature. The results obtained are given in tables, and for a current of ten amperes are plotted in corresponding plates.

The reactions of the armature are so clearly shown by the curves that comment hardly seems necessary. Briefly, we see that the total induction in armature, varies very slightly with lead, and the displacement of the neutral plane decreases with

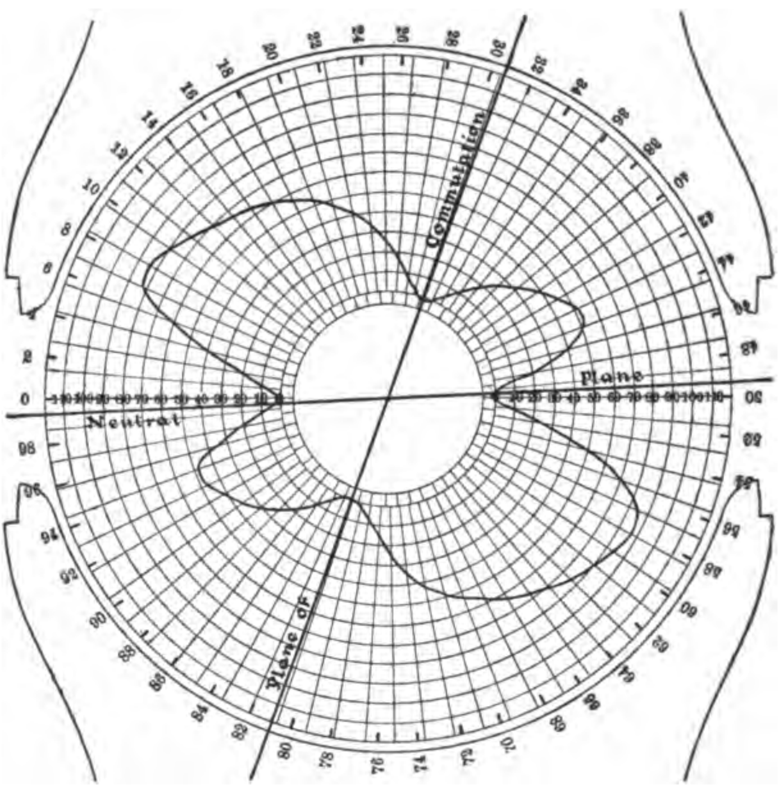


FIG. 6.—E. M. F. with 10 amperes in field and armature, 10-light position.

lead angle, but varies less than 10 degrees from no load to the maximum load used.

All electromotive forces on one side of the neutral plane are of the same sign but differ in sign from those on the opposite side.

On a developed diagram the electromotive forces between collecting brushes would be proportional to the difference between the whole area of curve on one side of the line of commutation and the area of the curve included in the angle of lead, the lead angle being defined as the angular advance of the collecting

TABLE V.

DISTRIBUTION OF E. M. F. AROUND THE COMMUTATOR, AS SHOWN BY THE TWO-BRUSH METHOD.

Position of the toe of the collecting brushes, 28.7 and 78.7.
Number of segments covered by the collecting brushes, 4.

Current—8 Amperes.				Current—10 Amperes.				Current—12 Amperes.			
Scale Reading.	Volts.	Scale Reading.	Volts.	Scale Reading.	Volts.	Scale Reading.	Volts.	Scale Reading.	Volts.	Scale Reading.	Volts.
0	+ 8.0	50	— 10.0	0	+ 9.0	50	— 10.8	0	+ 5.1	50	— 10.0
2	+ 24.5	52	— 26.9	2	+ 26.8	52	— 28.8	2	+ 24.7	52	— 28.5
4	+ 52.5	54	— 57.0	4	+ 57.5	54	— 61.0	4	+ 57.0	54	— 62.0
6	+ 76.0	56	— 79.0	6	+ 84.0	56	— 85.0	6	+ 84.0	56	— 87.0
8	+ 78.0	58	— 79.0	8	+ 84.5	58	— 84.2	8	+ 83.8	58	— 86.0
10	+ 72.0	60	— 73.8	10	+ 78.4	60	— 78.3	10	+ 77.0	60	— 79.1
12	+ 67.2	62	— 68.9	12	+ 73.0	62	— 73.0	12	+ 71.0	62	— 72.4
14	+ 63.0	64	— 64.1	14	+ 68.1	64	— 67.9	14	+ 65.1	64	— 68.0
16	+ 58.3	66	— 58.8	16	+ 63.0	66	— 62.0	16	+ 54.4	66	— 62.0
18	+ 52.6	68	— 52.8	13	+ 56.0	68	— 55.2	18	+ 51.2	68	— 54.1
20	+ 46.2	70	— 45.9	20	+ 48.7	70	— 47.4	20	+ 43.1	70	— 44.8
22	+ 38.4	72	— 37.4	22	+ 39 0	72	— 38.8	22	+ 32.4	72	— 33.8
24	+ 29.8	74	— 27.0	24	+ 27.0	74	— 24.9	24	+ 16.0	74	— 3.6
26	+ 19.7	76	— 15.9	26	+ 14.3	76	— 12.9	26	0.0	76	— 3.9
28	+ 5.2	78	— 3.0	28	+ .8	78	0.0	28	— 5.2	78	+ 13.1
30	0.0	80	0.0	30	— .6	80	0.0	30	0.0	80	+ 8.8
32	+ 3.8	82	— .7	32	+ 5.0	82	— 3 0	32	+ 2.1	82	— 1.0
34	+ 13.0	84	— 9.2	34	+ 15.9	84	— 13.7	34	+ 13.3	84	— 13.0
36	+ 21.4	86	— 18.0	36	+ 25.4	86	— 23.3	36	+ 25.0	86	— 23.7
38	+ 29.0	88	— 26.0	38	+ 34.0	88	— 31.8	38	+ 34.3	88	— 32.9
40	+ 35.8	90	— 33.2	40	+ 42.8	90	— 39.9	40	+ 43.1	90	— 41.9
42	+ 42.8	92	— 39.9	42	+ 50.7	92	— 47.9	42	+ 52.1	92	— 50.7
44	+ 41.3	94	— 40.6	44	+ 49.2	94	— 48.9	44	+ 51.1	94	— 42.0
46	+ 20.3	96	— 19.0	46	+ 25.4	96	— 25.3	46	+ 25.5	96	— 26.4
48	+ .8	98	— .7	48	+ 2.1	98	— 2.1	48	+ 1.9	98	— 2.5

Check readings taken after the observations in each column showed a variation of less than 1 per cent. in all cases.

With current of 8 amperes the voltage between brushes was 654; between neutral points, 1140; the neutral points being at the positions 49.2 and 99.2.

With current of 10 amperes the voltage between brushes was 622; between neutral points, 1170; the neutral points being at the positions 49.3 and 99.3.

With 12 amperes the voltage between brushes was 552; between neutral points, 1185; the neutral points being at 49.4 and 99.4.

With 8 amperes the sparking at the brushes was very slight; with 10 amperes, a minimum; and with 12 amperes it was considerable.

brushes from the neutral plane. An inspection of the curve of total E. M. F. with angle of lead given in Fig. 6, shows that the E. M. F. between neutral points remains nearly constant as lead varies, which it is believed would not be the case if contracted polar faces were used instead of the extended ones as shown, for then the shifting of the field could not be so easily effected. Of course a machine of the design tested, with extended pole tips, requires an automatic widening of the brush with increased load or independent control of sparking, but it would seem that the waste field at all loads would be less, and its weight efficiency

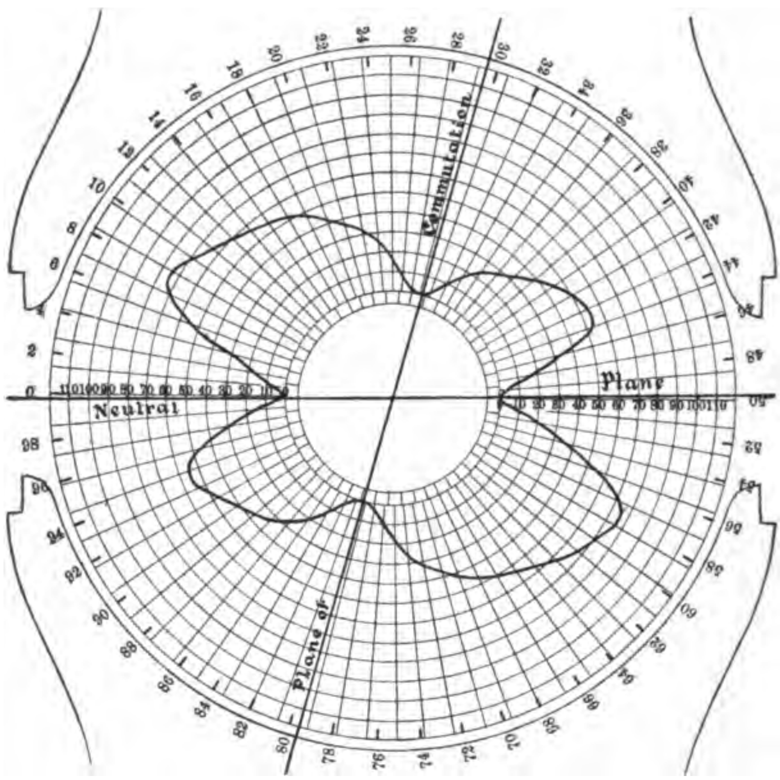


FIG. 7.—E. M. F. with 10 amperes in field and armature, 5-light position.

greater or cost for a given output less, than if its pole tips were cut away. Cutting away the pole tips or at least not extending them, would have the advantage of allowing the use of collecting brushes of constant width within a certain range, but whether this advantage more than compensates for the decreased range of its output is open to question. Further experimental evidence is however needed in this regard.

Regarding the relative amounts of iron in field and armature, we see no reason for the present practice of using so little iron in the armature, as compared with the field. The necessity of a

TABLE VI.

DISTRIBUTION OF E. M. F. AROUND THE COMMUTATOR, AS SHOWN BY THE TWO-BRUSH METHOD.

Position of the toe of collecting brushes, 27.9 and 77.9.
Number of segments covered by each collecting brush, 3.

Current—8 Amperes.				Current—10 Amperes.				Current—12 Amperes.			
Scale Reading.	Volts.	Scale Reading.	Volts.	Scale Reading.	Volts.	Scale Reading.	Volts.	Scale Reading.	Volts.	Scale Reading.	Volts.
0	+ 5.5	50	− 5.3	0	+ 5.5	50	− 12.2	0	+ 5.9	50	− 5.6
2	+ 20.5	52	− 21.5	2	+ 21.5	52	− 24.0	2	+ 20.2	52	− 23.0
4	+ 41.5	54	− 49.	4	+ 49.0	54	− 54.5	4	+ 49.3	54	− 54.5
6	+ 68.	56	− 69.	6	+ 72.0	56	− 75.7	6	+ 71.1	56	− 76.4
8	+ 67.9	58	− 67.9	8	+ 71.6	58	− 74.3	8	+ 72.9	58	− 74.1
10	+ 62.2	60	− 62.5	10	+ 65.8	60	− 68.0	10	+ 66.4	60	− 67.4
12	+ 57.7	62	− 57.5	12	+ 60.3	62	− 62.0	12	+ 60 1	62	− 61.0
14	+ 53.5	64	− 52.8	14	+ 55.5	64	− 56.4	14	+ 55 0	64	− 55.2
16	+ 48.7	66	− 47.	16	+ 51.0	66	− 51.0	16	+ 50.0	66	− 49.0
18	+ 43.	68	− 40.3	18	+ 44.5	68	− 43.4	18	+ 42.0	68	− 41.0
20	+ 36.3	70	− 33.4	20	+ 37.7	70	− 35.0	20	+ 35.1	70	− 31.3
22	+ 28 2	72	− 26.	22	+ 29.5	72	− 26.0	22	+ 26.5	72	− 21.0
24	+ 20.	74	− 16.1	24	+ 18.1	74	− 13.9	24	+ 12.5	74	− 7.0
26	+ 9.5	76	− 5.4	26	+ 6.0	76	− 1.0	26	0.0	76	+ 6.5
28	0.	78	− .2	28	0.0	78	0.0	28	0.0	78	+ 1.0
30	+ 2.1	80	− 4.0	30	+ 5.1	80	− 5.0	30	+ 3.5	80	− 6.1
32	+ 12.2	82	− 12.4	32	+ 16.3	82	− 14.7	32	+ 15.2	82	− 17.8
34	+ 20.5	84	− 20.6	34	+ 25.1	84	− 24.0	34	+ 25.1	84	− 27.1
36	+ 27.8	86	− 27.9	36	+ 32.1	86	− 32.0	36	+ 34.0	86	− 35.4
38	+ 34.7	88	− 34.4	38	+ 41.0	88	− 39.0	38	+ 42.0	88	− 42.6
40	+ 40.9	90	− 40.2	40	+ 47.8	90	− 46.0	40	+ 49.3	90	− 50.0
42	+ 47.	92	− 45.8	42	+ 54.9	92	− 52.7	42	+ 57.0	92	− 57.0
44	+ 45.	94	− 45.5	44	+ 53.0	94	− 52.2	44	+ 54.5	94	− 57.0
46	+ 23.	96	− 23.7	46	+ 28.9	96	− 28.0	46	+ 30.0	96	− 30.8
48	+ 3.2	98	− 3.1	48	+ 6.1	98	− 5.5	48	+ 5.5	98	− 6.0

After making the observations in each column several check readings were taken, and in all cases the variation was within 2 per cent., while in most of them it was not more than 1 per cent.

With current of 8 amperes the voltage between the collecting brushes was 390; the voltage between the neutral points, 1110; the neutral plane passing through the positions 49.7 and 99.7 on the scale.

With 10 amperes the voltage between the collecting brushes was 379; the voltage between neutral points was 1160, the neutral plane passing through 49.9 and 99.9

With 12 amperes the voltage between the collecting brushes was 285; the voltage between neutral points, 1170; the neutral plane passing through 50.1 and .1.

With 8 amperes there was slight sparking; with 10 amperes there was none; and with 12 amperes there was considerable sparking.

very high field induction or a thoroughly stiff field is apparent; but why when the lines are once generated by the field it is not sought to collect and utilize them all by means of an armature, with a generous amount of iron is not so easily seen. The number of commutator segments would of course have to be increased to prevent sparking, but the regulating qualities of the machine

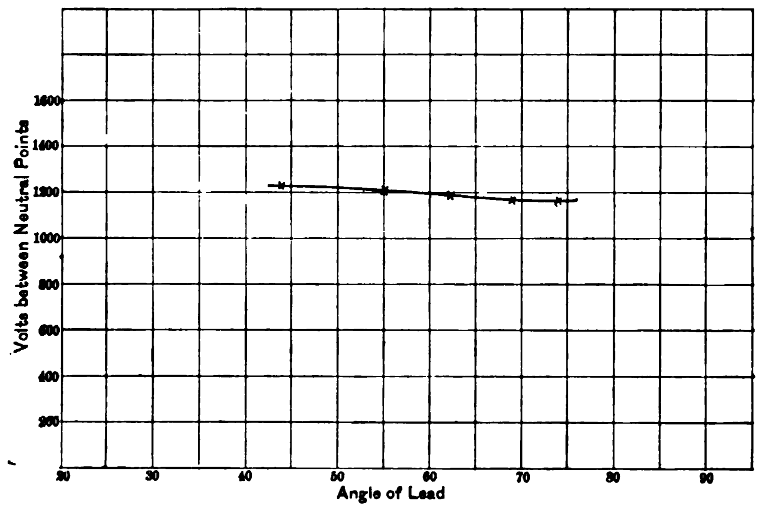


Fig. 8

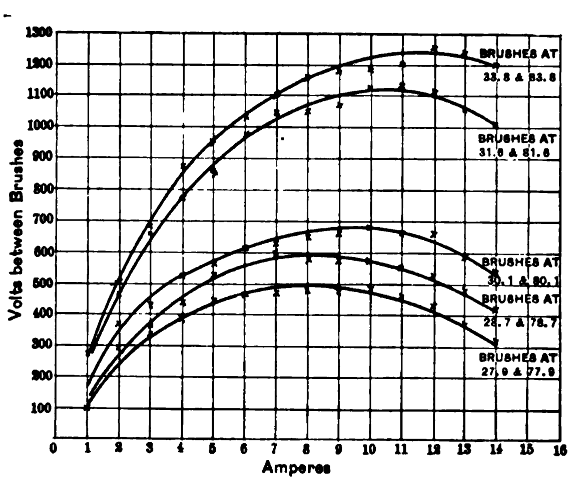


Fig. 9.—Characteristics.

would, it is believed, not be impaired. The result would be a larger output and greater efficiency.

Regarding the ratio of ampere-turns on field and armature such ratio will depend largely on the shape of the pole-pieces and desired width of brush, but is in all cases much less than in

TABLE VII.

CHANGE OF VOLTAGE BETWEEN NEUTRAL POINTS WITH ANGLE OF LEAD.

Angle of Lead.	Volts.
74°	1160
69°	1170
62°	1183
55°	1210
44°	1235

TABLE VIII.

SHOWING EFFECT PRODUCED BY SHUNTING THE FIELDS, WITH 10 AMPERES IN ARMATURE.

Position of Brushes.	Current in Fields.	Sparking.
28.7 and 78.7	10.	None.
" " "	9.	Too much for good service.
" " "	8.7	Excessive.
30.1 " 80.1	10.	None.
" " "	9.1	Too much for good service.
" " "	8 6	Excessive.
33.8 " 83.8	10.	None.
" " "	8.7	Too much for good service.
" " "	8.1	Excessive.

TABLE IX.

VARIATION OF E. M. F. AT BRUSHES WITH DIFFERENT CURRENTS, AND WITH DIFFERENT POSITIONS OF BRUSHES.

Currents.	Position of Brushes 27.9 and 77.9.	Position of Brushes 28.7 and 78.7.	Position of Brushes 30.1 and 80.1.	Position of Brushes 31.6 and 81.6.	Position of Brushes 33.8 and 83.8.
	Volts.	Volts.	Volts.	Volts.	Volts.
1	100	70	100	245	195
2	275	205	350	460	500
3	340	365	420	652	679
4	390	445	520	775	779
5	440	517	570	840	952
6	465	600	610	960	1028
7	480	600	630	1030	1100
8	490	590	655	1038	1150
9	480	582	670	1060	1155
10	480	573	678	1110	1177
11	450	550	665	1125	1196
12	420	521	650	1098	1250
13	365	474	590	1055	1230
14	310	417	530	1010	1100

constant potential machines. That in the present instance with the widths of brushes given, the ratio could not be widely departed from without violent sparking, is shown in table No. 8.

The characteristic curves shown in Fig. 9, and readings given in table No. 9, were taken at the 5, 10, 15, 20 and 25 light positions of the brushes, and show that with this machine, regulation is almost entirely effected by shifting the brushes, as the curves droop too slowly to assist to any material extent. Of

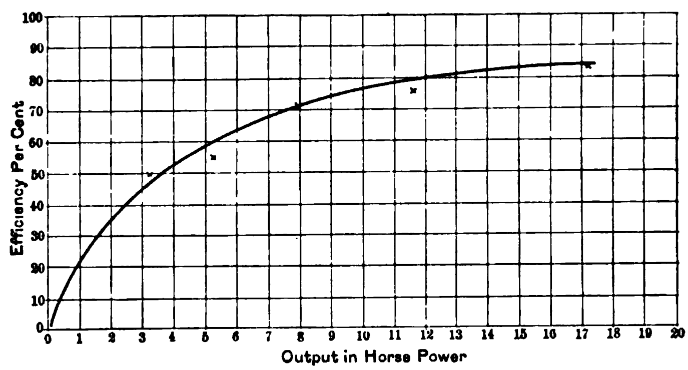


FIG. 10.

TABLE X.

EFFICENCY TEST.

Input. H. P.	Output. H. P.	Efficiency. Per cent.
6.86	3.3	49.6
9.7	5.3	54.6
12.7	9.0	71.0
15.3	11.6	75.8
20.5	17.2	83.9

MISCELLANEOUS.

Constant loss in fields with 10 amperes............... 1575 watts.
Radiating surface of fields........................ 1245 sq. in.
Radiating surface of fields per watt loss.............. .8 sq. in.

course they might have been made to droop much more rapidly if the collecting brushes had been moved into sparkless positions for each value of the current, but a curve so obtained is not at all a characteristic. The efficiency curve is given on Fig. 10, and readings in table No. 10. From this curve and from what we have seen of the machine it is evident that it would never pay to run machines of this type underloaded. If a number are used in one station, the connections of the external circuits at the switchboard should be so manipulated as to keep the machines actually in use always loaded to as near their full capacity as possible.

Our thanks are due to Mr. H. J. Podlesak, who greatly assisted in taking the observations and working up the results.

DISCUSSION.

MR. C. N. BLACK:—I think that the remark of Mr. Owens, in regard to the iron in the armature is very true. Arc machines as built heretofore have always had enormous field cores and very small armatures. We cannot get a constant current machine by any such arrangement. It is practically impossible. On the other hand if we build an armature with an ample cross-section and work it only just around the bend of the curve, we can build a machine that will be practically automatic, without outside regulation, and which will give a constant current with a very slight variation. That is the principle on which the latest 125-light Brush machine is built. The iron in the armature is worked at only about 90,000 to 95,000 lines per square inch at full load, and of course at lower loads the ampere-turns on the armature, cuts that induction down proportionally, so that at very low loads we have a very low induction, and we get a characteristic curve that without any regulator does not vary more than two and a half to three amperes, from 6,000 volts to zero. The machine works on the downward slope of the curve, part of which is absolutely vertical; for instance, from 4,500 volts down to 100 volts, there is not one-tenth of an ampere variation in the current.

MR. STEINMETZ:—I am of the opposite opinion and at the same time of the same opinion as the last speaker. I am of the opinion that the density of the iron should be very high, but I am inclined to think that the last speaker is so accustomed to the arc light dynamo densities of over 100,000 that he considers 95,000 as low density. This is for constant potential machines very high density, though with regard to some old arc light machines it is low density.

With regard to the problem of building arc light dynamos, it has been said that the worse a machine is, the better suitable it is as an arc light dynamo. But it is not so. The fact is, that the problems to be considered in the design of an arc light dynamo are so essentially different from those of a constant potential dynamo, that a bad design of a constant potential may be a very good design for an arc light dynamo, and in general, a good design of a constant potential machine is a bad design of an arc light machine. But it is of just as much theoretical importance to investigate and understand the problem of arc light dynamo design, as it is with the constant potential dynamo design.

What we want in an arc light machine, is a machine which will give constant current automatically if possible, without any regulating mechanism at all; or at least so that the regulating mechanism is only an additional control to take up secondary actions, but not to do the whole of the regulation—otherwise the machine must be a failure.

You can do this in two different ways. The ordinary shunt machine has the tendency to regulate for constant current, because if you have a shunt machine with an ideal characteristic, that is, a machine whose induced electromotive force is proportional to the ampere-turns in the field, and if the resistance and the armature reaction are negligible, then this machine will at any voltage just give the ampere-turns required to produce this voltage—that means, it will not give definite voltage, but any voltage desired by the conditions of the external circuit.

In the machine as it actually is, the excitation of the dynamo must be proportional to the terminal voltage, plus a constant quantity, that quantity which produces the voltage consumed in the armature. Thus a shunt machine with a constant separate excitation, will fulfil the condition of giving a terminal voltage proportional to the external resistance, that is, will act as a constant current dynamo for all voltages below saturation, that is, below the bend of the magnetic characteristic.

This machine will be a constant current dynamo, but not an arc light dynamo, because it will not regulate quickly enough. If the load is changed suddenly, it is a long time before the magnetism changes to the altered conditions of load and excitation, and thus either a sudden rush, or a sudden decrease of current takes place. For instance, you cut out half the lights. Then the current will suddenly double its values, and then gradually go down to normal. Such a machine will do very well for a series incandescent dynamo, but for arc light circuits you want machines which, when the load is suddenly varied, do not allow the current to go above or below a certain value.

The machine must be one whose armature will regulate automatically for constant current, that is, where a small change of the armature current, and thereby the armature M. M. F. will essentially change the field; if need be, destroy it. The effective field, I mean; for even when short-circuiting the machine, the field may not disappear entirely, but may be turned around in such a direction as to become ineffective with regard to the terminal voltage.

Thus, you require a machine of very large armature reaction, that is, a machine whose armature M. M. F. is of the same magnitude as the field M. M. F., and very large compared with the resultant M. M. F. required to produce the magnetism. In such a machine a small variation of the armature current, that is, the armature M. M. F., will vary the resultant M. M. F. and thereby the magnetism and the E. M. F. very greatly. For instance, let the ampere-turns of the field = 12,000 at full load, the component of armature ampere-turns in the direction of the field = 10,000, and the resultant or magnetizing ampere-turns = 2,000. When suddenly short-circuiting the machine, the current will rise only so far as to raise the armature ampere-turns to equality with the field ampere-turns, that is, from 10,000 to 12,000, or by 20 per cent., and thereby blow out the field.

This is the principle of a successful arc light dynamo, a dynamo regulating for constant current by its armature reaction. Such a machine will without any outside regulation as shifting of brushes, etc., keep the current practically constant under all loads, whether a continuous current dynamo or alternating.

It will, however, require an enormous M. M. F. on field and armature to get very close regulation. Here the external regulation comes in.

You can get along with very much less M. M. F., just enough not to get too large a fluctuation of current by a very sudden change of load before the regulator can act. Thus the arc light regulator is merely for the purpose of making the inherent regulation of the machine still closer. This is done by short-circuiting or shunting a part of the field, or by changing the E. M. F. of the armature by shifting the brushes, etc. So you have two ways of regulating the arc light dynamo, that is, of making a machine which automatically tends to give approximately constant current, to yield absolutely constant current.

The shifting of the brushes introduces a second difficulty. You must arrange the machine not to spark, that is, to commutate at any position where the brushes may stand during the change of load. That means, the field strength at the position where the brushes stand must always be such, that during the time of the reversal of the current in the short-circuited coil, the E. M. F. of self-induction which is induced by the reversal, is just counteracted and destroyed by the E. M. F. induced by the field magnetism in the short-circuited coil, so that the field has just reversed the current during the time from the beginning of the short-circuit, by the gap entering the brush until the time where the short-circuit is opened by the gap passing and leaving the brush. This requires a low density in the gap. This is secured by large extending pole-pieces, which besides are necessary again to get the large armature M. M. F. and low resultant M. M. F. for automatic regulation. It is theoretically possible to build machines so that the density of the field will be uniform wherever the brushes may stand, and that consequently in a closed circuit armature you can shift the brushes around the gap without changing the width of the brushes. Where this is not the case, but the resultant density at the position of brush varies with the shifting of the brush, sparkless commutation can be gotten by varying the frequency of commutation, that is, the width of the brush, or, in other words, using two brushes in parallel and shifting the one against the other. Similar considerations hold in the case of regulation by shunting the dynamo field, etc.

Another point of importance in arc light dynamos is the amount of iron. Theoretically the amount of iron in the armature is of no importance except that there must be enough iron to carry the flux. But if you have very much iron in the magnetic circuit of a constant current machine of, say, 10,000 ampere-turns on the armature and on the field, then if, suddenly, the armature current is broken, the effective or magnetizing ampere-turns rise from 12,000 — 10,000 = 2,000, to 12,000, the armature M.M.F. being withdrawn, and these ampere-turns being enormous compared with the ampere-turns that are necessary to send the normal flux through the armature, the density goes up to saturation and the E. M. F. jumps up, as in Mr. Stanley's arc light alternator—where it was very prettily taken care of by a spark gap short-circuiting the machine at abnormal rise of voltage.

This rise of E. M. F. is partially guarded against by running the armature iron at high saturation, so that even the enormously increased M. M. F. in the moment of opening the circuit cannot raise the density, and thereby the voltage seriously.

That is the reason why it has been found desirable to restrict the use of iron in the armature of arc light dynamos as far as possible.

MR. BLACK:—I would like to ask Mr. Steinmetz how he would get anywhere near as constant a current from a machine in which the iron in the armature is worked at 110,000 lines as he could get from a machine whose armature is worked just at the bend of the magnetic curve, for the reason that any counter M. M. F. set up in the armature at that point would decrease the flux in armature exactly in proportion to any increase in the current, so that the current would have to remain almost absolutely constant. Further, I cannot understand how he could get a very great increase of flux from the field magnets, without at the same time getting a counter M. M. F. in the armature (in the series machine) and the objection that the voltage would go up to double or treble what the machine was designed for, strikes me would not hold good, as any increase of current in the field, would be accompanied by a like increase in the armature, and if the machine was a constant current series machine the voltage would drop immediately, and not go up.

MR. STEINMETZ:—With regard to the second point, if the line is suddenly interrupted, in a separately excited machine, as an alternator; this means destruction, if the voltage is not kept within limits by magnetic saturation, or some lightning arrester or spark gap is used. In a series machine this danger is greatly reduced, since the field excitation disappears at open circuit. However, in suddenly opening the circuit, the total flux in the field may remain approximately the same as before, but while, due to the counter M. M. F. of the armature, this flux before passed largely around the armature as stray field, now with the withdrawal of the counter M. M. F. of the armature, the total flux passes through the armature, and the E. M. F. jumps up.

All this depends on the length, shape, etc. etc. of the magneto-circuit and other conditions, and we will therefore choose that density which is high enough to limit the E. M. F., and at the same time low enough not to use too many ampere-turns at normal running.

Mr. Black:—I think, as to the first question, that the voltage of the machine would be taken care of under any circumstances. The machine would simply flash at the commutator, which would immediately reduce the voltage. I cannot conceive of any combination of circumstances, by which we could keep the current in the field at its normal strength, and have no current in the armature, except in a separately excited machine.

Mr. Steinmetz:—But it is not necessary to go higher. You can avoid the flashing by not allowing the E. M. F. to rise very much higher.

Mr. Black:—It seems to me, if you have enough turns in the armature to get a back or a counter M. M. F. of any value, and if those ampere-turns are reduced to zero, and the field is kept the same, that the voltage of the machine will rise enormously. Now, in the Brush 60-light machine (3,000 volts) in which the iron is worked to a very high degree of saturation, if we separately excite the field at the normal current of 9.6 amperes, and take no current from the armature, the voltage of the machine will be practically doubled. In other words, you will get between 5,000 and 6,000 volts, notwithstanding the fact that the armature iron is worked at a very high point of saturation under normal conditions.

Mr. Steinmetz:—Yes. But in practice you will never get so high a voltage, because if you break the current, the current in the field disappears also, so you are quite within the limits of safety. The proportional increase of voltage is less with a small amount of iron than with a large amount of iron.

The President:—If there is no further discussion, I will call for the next paper, on the "Relative Advantages of Toothed and Smooth Core Armatures," by Mr. Alton D. Adams, of Worcester, Massachusetts.

In the absence of the author, Professor Anthony read the following paper:

A paper presented at the Eleventh General Meeting of the American Institute of Electrical Engineers, Philadelphia, May 16th, 1894, President Houston in the Chair.

RELATIVE ADVANTAGES OF TOOTHED AND SMOOTH CORE ARMATURES.

BY ALTON D. ADAMS.

The merits of different methods of construction in dynamo electric machinery, as in other lines, must evidently be decided by their comparative costs, all else being equal.

Although questions concerning the relative merits of toothed and smooth core armatures have long been discussed, very little seems to have been written to show whether actual saving in cost may be effected by one construction over the other, when employed to produce the same results. The practice of dynamo builders in this country, and abroad, embodies both types, and the history of the art records many changes from each to the other. In view of the above, the inquiry, whether in the light of present facts any saving can be effected by the use of toothed core armatures, seems of interest.

The limits of this paper do not permit consideration of this question in connection with all classes of electrical machinery, and its bearing on direct current constant pressure machines only, will be taken up.

The principal disadvantages of toothed, compared with smooth core armatures, are greater first cost, large change of lead, and excessive sparking, when used with too short air-gaps, and the production of heat in pole-pieces; their advantages are, that inductors are positively driven, large solid inductors protected from eddy currents, and that a reduction may be made in the length and consequent magnetic resistance of the air-gap. Change of lead may be fixed within any desired limits, and sparking abated by such proportions of air-gap and teeth, as give them sufficient magnetic resistance.

Heat in pole-pieces may be reduced by their lamination, by the use of very narrow teeth and slots, by forms of teeth that present a nearly continuous surface of iron to the pole-pieces, and still more, by the use of openings in core disks which do not cut through their outside surface, or a continuous magnetic sheath outside the teeth. For any given form of tooth, the heating of pole-pieces is less, the longer the air-gap.

The mechanical strength of armature teeth, as usually employed, is far in excess of that required to hold inductors in position, even under conditions of short-circuit, and driving-pins inserted in the core, at proper intervals, are much cheaper and take up less valuable room on the armature circumference.

Either teeth or substantial driving-pins are, of course, preferable mechanically to the slender bits of hard fibre which have been much used, and frequently give way under the heavy strains to which large generators are subject.

When large wires or copper rods are used as inductors, their protection from eddy currents is an important matter, but proper stranding of inductors reduces the eddy loss in them, when used on smooth cores, to a very small amount, and has the further important advantage that inductors may be bent into the proper shape at armature ends, and the joints necessary when rods are used, avoided. The chief possible advantage, then, to be gained by the use of toothed armatures, is through a reduction in the length of the air-gap, and the consequent reduction in the ampere-turns required on field magnet, weight of copper, or energy in winding, and the length and weight of iron core. To make this advantage available, it must be practical to use air-gaps shorter than are required for insulation, winding and clearance.

As is well understood, the armature winding of a dynamo or motor, in operation, has a magnetizing action which is measured in ampere-turns for a bipolar machine, by one quarter the product of all the inductors of the armature, into the total armature current. The ampere-turns on the armature evidently tend to set up a flow of magnetism, having a complete circuit through the armature core, twice across each air-gap and through the iron of pole-pieces.

About half the ampere-turns furnished by the inductors under pole-pieces, evidently act against the field ampere turns in each air-gap at the polar tips, and the ratio between the armature and field ampere-turns at this point, necessary to give sparkless reversal there, must determine whether the required magnetic resistance be greater or less than that of an air-gap long enough for insulation, winding, and clearance with a smooth-core armature.

As an armature coil in an operating dynamo or motor passes under the brush, the current flowing in it must stop, and one in the opposite direction be set up; and if this action is to be accomplished without sparking, a sufficient electromotive force must be provided in the coil while in direct contact with the brush. In the ordinary dynamo or motor, magnetism forced across the path of the coil, by the field ampere-turns expended in air-gap, must provide this reversing electromotive force.

The data of a number of smooth core armature machines of different make, show ratios of field to armature ampere-turns in air-gap, of from about one and one-half to one, to two and one-half to one, and the writer's experience is that a ratio of two to one will give sparkless operation at full load, with brushes set just outside pole corners.

It is a matter of common experience that the ratio between field and armature ampere-turns in the air-gap may be so reduced, even in machines with smooth core armatures, as to require excessive change of lead to secure even approximate freedom from sparking. If it be desired therefore to build machines having an expenditure of field ampere-turns in the air-gap not much greater than those of the armature, we need not resort to toothed cores.

Take, for example, the case of a 260-ampere dynamo, with 120 inductors on its armature in one layer; an air-gap induction of 25,000 lines per square inch, and 80 per cent. of inductors under the pole-pieces.

An air-gap of .45 inch between the armature core and each pole-piece will be sufficient for insulation, winding and clearance, and the field armature turns expended in each air-gap will therefore be 3523, while the armature ampere-turns, active under each pole tip, will be 3,120. A considerable change of lead and sparking can be readily predicted for this machine.

In some types of small machines, the room required by insulation, winding and clearance, makes the air-gap longer than necessary for sparkless operation, and in such machines the utility of teeth seems to depend on their cost, compared with the saving to be effected by their use.

As the ampere-turns, furnished by the inductors under any pole-piece grow less, in a machine of given capacity, when the number of poles is increased, very short air-gaps may be used, if the number of poles is sufficiently large.

As, however, an increase in the number of poles usually makes a machine of given capacity more expensive, the question at once comes up, to what extent the number of poles may be increased without a greater expenditure than the saving to be effected.

In large multipolar machines of four to six poles, such as are commonly used, the length of air-gap required for sparkless operation, is considerable, and those who have watched the development of these machines with toothed core armatures during the last four or five years, have seen the air-gaps gradually widen until machines of this character are not hard to find, in which the copper inductors between the teeth could be taken out, wound outside the teeth, and still leave room enough for good clearance. Additional mechanical security, of course, furnishes a considerable argument for the use of teeth in very large slow speed machines.

A number of devices have been suggested from time to time, to enable toothed core armatures to be used with short air-gaps, and the consequent saving in iron and copper effected. No machines with these devices, however, have yet stood the test of time and competition with those of ordinary type, and they have yet to prove their ability to produce results, as at present attained at a less cost.

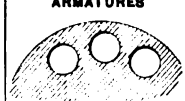
The seeming opportunity to save material by the use of toothed armatures is very attractive, and we cannot but hope it may some day be practical; in the light of present knowledge, however, there seems little to be gained by their use in medium and large bipolar machines.

Worcester, Mass, May 9, 1894.

Discussion.

Mr. A. E. Wiener:—After enumerating the advantages and disadvantages of toothed armatures, the author comes to the conclusion that the chief advantage of toothed armatures over smooth ones is the reduction of the length of the air-gap, and the consequent decrease of the exciting power required. But he left out one very important disadvantage, namely the leakage through the armature core, caused by a portion of the lines entering the teeth, and passing through the armature without cutting the conductors. From a large number of machines I have found that, if otherwise well designed, the disadvantage of this core-leakage just about cancels the advantage of the air-gap reduction; for in order to allow for the waste armature-field due to this core-leakage, the exciting power has to be increased in about the same degree, as the lessening of the magnetic resistance of the gap would otherwise decrease it. The chief advantage claimed by the author, consequently, is thus only an imaginary one, and, indeed, there are no such striking advantages in either of the two kinds of armatures as to make any one of them superior in all cases over the other. On the contrary, it is really a matter of choice, with reference to the special application of the machine to be designed, whether to employ a smooth or a toothed armature. In the case of a slow speed motor, for instance, as used in single-reduction, and in gearless street car motors, where great torque is wanted, a toothed armature core will offer better advantages, and its somewhat higher cost of manufacture has to be overlooked. When, on the other hand, the machine is to be used as a generator for very low potential (electroplating dynamos, etc.), a smooth armature core is preferable.

TABLE I.

RATIO OF WIDTH OF SLOTS TO THEIR PITCH ON OUTER CIRCUMFERENCE	FACTOR OF ARMATURE LEAKAGE		
	TOOTHED ARMATURES		PERFORATED ARMATURES
	STRAIGHT TEETH	PROJECTING TEETH	
			
0.4	1.40 to 1.35	1.55 to 1.50	———
.45	1.35 " 1.30	1.50 " 1.45	———
.5	1.30 " 1.25	1.45 " 1.40	1.50 to 1.45
.55	1.25 " 1.20	1.40 " 1.35	1.45 " 1.40
.6	1.20 " 1.15	1.35 " 1.30	1.40 " 1.35
.65	———	———	1.35 " 1.30
.7	———	———	1.30 " 1.25

Core Leakage in Toothed and Perforated Armatures.

From quite a number of machines I have averaged the following table of armature leakage in toothed, and perforated armatures, to appear in a series of articles entitled "Practical Notes on Dynamo Calculation," and commenced in this week's issue of the *Electrical World*.[1]

The amount of the core leakage depends upon the ratio of the width of the slots to the width of the teeth, and, if the armature is otherwise properly designed, will vary within the limits shown in Table I.

On account of this core leakage, the field-densities obtained in toothed and perforated armatures are considerably smaller than in smooth core machines, as can be noted from the following table which gives the practical field densities of various dynamos, derived from the data and tests of about two hundred of the best modern dynamos:

From Tables I and II follows the practical truth of my above

TABLE II.

Field Densities, in Lines of Force per square Centimeter

Capacity in Kilo Watts	Bipolar Dynamos						Multipolar Dynamos						Capacity in Kilo Watts
	Smooth Armature Core		Toothed Armature Core				Smooth Armature Core		Toothed Armature Core				
			Straight Teeth		Projecting Teeth				Straight Teeth		Projecting Teeth		
	Cast Iron Polepieces	Wr't Iron or Steel Polepieces	Cast Iron Polepieces	Wr't Iron or Steel Polepieces	Cast Iron Polepieces	Wr't Iron or Steel Polepieces	Cast Iron Polepieces	Wr't Iron or Steel Polepieces	Cast Iron Polepieces	Wr't Iron or Steel Polepieces	Cast Iron Polepieces	Wr't Iron or Steel Polepieces	
.1	1550	2300	1250	1850	—	—	2150	3100	1850	2800	—	—	.1
.25	1850	2800	1550	2300	—	—	2500	3700	2150	3350	—	—	.25
.5	2150	3100	1850	2800	—	—	2800	4200	2500	3700	—	—	.5
1	2300	3400	2000	2950	1250	1850	2950	4350	2650	3850	1550	2300	1
2.5	2500	3700	2150	3100	1400	2150	3100	4500	2800	4000	1700	2500	2.5
5	2650	3850	2300	3400	1550	2300	3250	4700	2950	4350	1850	2800	5
7.5	2800	4000	2500	3700	1700	2500	3400	5000	3100	4700	2000	3100	7.5
10	2950	4350	2650	3850	1850	2800	3700	5400	3250	5000	2150	3300	10
25	3100	4700	2800	4200	2000	3100	4000	5900	3400	5400	2300	3500	25
50	3400	5100	3100	4700	2150	3400	4350	6400	3550	5900	2500	3700	50
100	3700	5600	3400	5100	2500	3700	4700	6800	3850	6200	2650	3850	100
200	4200	6200	3700	5600	2800	4200	5000	7300	4200	6500	2800	4000	200
300	4700	7000	4200	6200	3100	4700	5400	7750	4500	6800	2950	4350	300
500	—	—	—	—	—	—	5900	8200	4800	7200	3100	4700	500
1000	—	—	—	—	—	—	6400	8700	5100	7500	3400	5000	1000
2000	—	—	—	—	—	—	7000	9300	5400	7800	3700	5400	2000

statement, viz., that about the same exciting power, or same number of ampere-turns will be required in both types of armatures; for, also, the reduction of the gap resistance, if the radial depth of the armature core is properly dimensioned, depends upon the ratio of the width of the slots to the width of the teeth.

MR. STEINMETZ:—I was somewhat astonished when hearing this paper, in-so-far as the conclusions drawn therein. They are to a large part just opposite to what I concluded by theoretical reasoning, and found proved by practical experience.

What I intend to say, however, refers only to larger machines, machines of some hundreds of kilowatts. With regard to the

1. See *Electrical World*, xxiii, p. 675. (May 19, 1894.)

superiority of the toothed armature in these machines, I may first point to the fact that neither in this country nor in any other country is any large power generator running which will commutate sparkless, from no load to full load and overload, without shifting of the brushes, as it is required for instance in railway generators, where, due to the constant and sudden fluctuations of load, a shifting of the brushes is impracticable. On the other hand, hundreds of thousands of horse power of machines with toothed armatures, which will fulfil this condition of sparkless commutation without shifting of the brushes, are in daily use in this country alone. The reason for this superiority is, that the distortion of the field in a properly designed machine with toothed core is very much less than in the machine with smooth core. You can indeed by shifting the brushes get sparkless commutation in smooth core machines also, and you have in smooth core machines the decided advantage of lower self-induction.

But if you have to commutate without shifting the brushes, smooth core machines are out of competition.

The advantage of the toothed armature is not so much the lesser ampere-turns consumed in the gap. In larger machines you cannot do anything like what is proposed here, to use .45 of an inch as total clearance from iron to iron. This would give a clearance of about $\frac{1}{16}$ of an inch from the binding wires of the armature to the iron of the field poles, which may be all right for a small machine, but not for a large power generator. In such a machine, with toothed core, the clearance between armature and field is determined by mechanical reasons only, and is from $\frac{1}{4}$ inch to $\frac{1}{8}$ inch from the head of the armature teeth to the field poles.

The foremost feature of the modern toothed core power generator is the lesser distortion of the field, as the following diagram will show.

Let, in Fig. 1, be represented the M. M. F. diagram of a smooth core machine. The field pole P and the armature C with the armature conductors are shown in development in drawn lines. The M. M. F. exerted upon the air-gap by the field spools is represented by the dotted line F. The M. M. F. exerted upon the air-gap by the current in the armature conductors (armature reaction) is represented by the dotted line A, which is positive at the trailing, negative at the leading pole-horn, in a generator. Thus the total or resultant M. M. F. acting upon the air-gap is represented by the dotted line T, and proportional thereto, in a smooth core machine, is the magnetic density in the air-gap. As seen, the magnetic gap density is very much less at the leading pole-horn, where it is needed for sparkless commutation, than at the trailing horn, where it is not needed. At the same time the field has shifted considerably.

Now consider the toothed core machine, in Fig. 2. If the total effective gap, that is the space between pole-face and *foot* of slots, the same M. M. F., F, is exerted by the field spools—that is, the tooth density is appropriately high—the total or resultant M. M. F., T, is again the same as with the smooth core in Fig. 1. But here the magnetic density B is not proportional to the M. M. F. T, but B varies very much less than F, due to the effect of magnetic saturation, and thus the gap density is very much more uniform, as shown by dotted line B in Fig. 2, and the field has shifted very little. Thus the machine in Fig. 2 will commutate sparkless from no load to full load, without shifting the brushes, while machine, Fig. 1, will spark furiously if the brushes are not shifted with varying load.

On the other hand, due to the small self-induction of the armature conductors, the smooth core machine is very desirable for lighting machines where very large currents have to be commutated at low voltage between the commutator bars, and the load does not fluctuate suddenly by hundreds or thousands of amperes.

But for railway generators, where you have to use larger voltage between commutator bars, and where you have to leave

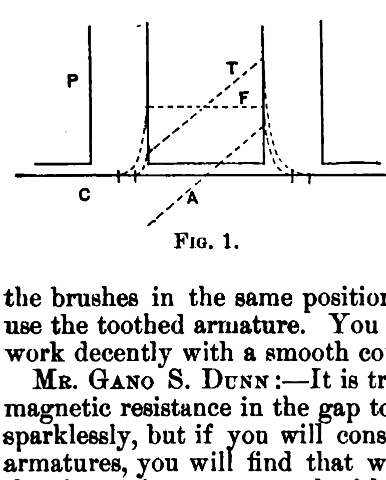


FIG. 1.

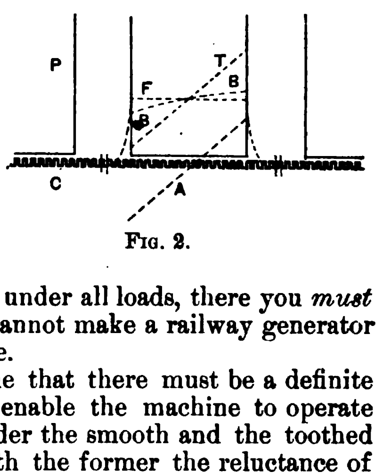


FIG. 2.

the brushes in the same position under all loads, there you *must* use the toothed armature. You cannot make a railway generator work decently with a smooth core.

MR. GANO S. DUNN:—It is true that there must be a definite magnetic resistance in the gap to enable the machine to operate sparklessly, but if you will consider the smooth and the toothed armatures, you will find that with the former the reluctance of the air-gap is constant, and with the latter it increases as a result of armature reaction, with load. This is due to the variable reluctance of the iron which constitutes the teeth, and which at saturation approaches air, giving a low resistance and a high resistance air-gap for light and full loads respectively. To put it a little clearer, the effect is similar to the case of a smooth armature run with an air-gap which as the load increases, lengthens, and thereby preserves sparkless commutation. For the above reasons a toothed armature air-gap can have a lower average reluctance than that of a smooth armature. The reluctance of the latter is constant and always at its maximum value.

In small machines however, the above considerations do not apply, because sparklessness need not be dependent upon a fringe at the pole tip to reverse the current in the coil. Machines may be lighter and neater which depend on other means for sparkless commutation. If for instance we cause the resistance of the carbon brushes to make the current reversal, then we can greatly diminish the reluctance of our air-gap and use a proportionately smaller field current and weight of copper, and secure important advantages in efficiency and lightness.

Another point which at present is not of so much importance, is that with dynamotors or direct current transformers, there is practically no armature reaction or tendency to spark, and consequently no necessity for reluctance in the air-gap. The toothed armature allows us to take advantage of these conditions in a manner impossible with an armature which of necessity demands a high reluctance air-gap.

With regard to mechanical advantages, the toothed armature with wires embedded and held firmly in slots, is so superior to the smooth armature wound with wire held on by various devices, that even were the toothed armature more expensive, which I am not ready to admit, it would be preferable.

MR. WM. STANLEY:—I would like to ask the last speaker if he does not consider that the extra self-induction due to the armature teeth, especially in the larger machines, is the cause of the sparking that we see on the street railway generators, and that if it were possible to arrange the wires on the circumference of the machine—I do not say that it is mechanical to do so—but if it were possible to do it, if the machine would not be more sparkless? Does not the self-induction of the buried wire in the iron core increase the sparking from one commutator bar to another?

MR. DUNN:—There is no question that burying the wire increases the self-induction, and if the openings at the top of the slot are too narrow, or if as in the case of the Wenstrom armature, there is no opening, this is a very serious defect, but the openings can be so proportioned that the increase of self-induction while a disadvantage, is not serious.

MR. STANLEY:—Is it not the principal disadvantage?

MR. DUNN:—Yes, I should consider that it was. The paper alleges that the toothed armature requires more material; that this is true, generally speaking, I do not agree. I think the self-induction is the greatest disadvantage.

MR. STEINMETZ:—I do not think that the representation of the toothed core by a gap, which widens with the increase of a load quite meets the point. What I wanted to show by the diagram is, that the distribution of the magnetic density at the gap at full load is not proportional to the distribution of the resultant M. M. F., but is very much more uniform, due to the effect of saturation, and therefore the field does not shift seriously, and the brushes can be left in the same position at all loads, in the fringe of the reversed field, which is required to overcome the self-induction of the current in the coil which is being commutated. For if you have to reverse hundreds of amperes, backed up by a large E. M. F. of self-induction, you have to have forced commutation.

It is to get rid of the shifting of the field, due to the armature reaction, which really constitutes the advantage of the toothed armature, which enables us to get a very much higher efficiency with the toothed armature than with the smooth core.

MR. C. N. BLACK:—There is another point I would like to make and that is, it seems to me practically impossible to build dynamos of large units, such as we are building now for railway work, and use a smooth armature, on account of the great losses we would get from eddy currents set up in the conductors themselves. The size of the conductors is such, that if they are made solid, that loss could not be neglected and would be a source of a good deal of heat, at the same time decreasing the efficiency of the machine quite materially. If we laminate the conductors it makes a very difficult construction, in fact, almost impracticable. There is one other point that I do not think has been mentioned, and that is in series motors, where we use a toothed armature, we get a much more constant speed. At light loads, the teeth being worked at a low point of magnetization, offer but little reluctance to the flux through the armature, while at heavy loads the teeth become saturated, and in consequence add a considerable reluctance to the magnetic circuit, thereby cutting down the induction from what it would be if increased in proportion to the increased number of ampere-turns on the field, consequently we get a motor that is much nearer self-regulating than one with the smooth core armature.

MR. A. E. KENNELLY:—The point has not been brought out, I think, that if you have a toothed armature, you can much more readily ventilate the armature from within, whereby you can carry off the heat from the surface far more efficiently than if the armature is entirely covered by wire.

MR. OBERLIN SMITH:—I would like to hear an expression of opinion as to the relative cost of a toothless ring and that toothed ring.

MR. STEINMETZ:—I think the question of ventilation is not such a drawback against the smooth core machine, because the modern design of lighting machines, which are smooth cores for the commutation of very large currents, are ventilated also by ventilating spaces between the armature conductors and even the twisting of the conductors is not so difficult, but is done in smooth core armatures. I have some machines being built which will have these twisted conductors, and which will exclude eddy currents almost perfectly—smooth core machines for low voltage, of 400 kilowatts. With regard to the relative cost of both types of machines, it is difficult to state it, because they are different types of machines for different purposes, and thus cannot be compared properly.

Smooth armatures are built for lighting where the toothed armature does not offer any advantages, where you have very low speed, large conductors and low voltage from bar to bar, while the ironclad is perfect as a high voltage power generator. Thus you have never built two machines, one smooth core and one ironclad, for the same purpose and of the same dimensions. In general I am of the opinion that the toothed armature is the cheaper one.

[The meeting then adjourned for the day.]

In the evening the Annual Dinner took place at the Hotel Metropole, at which 76 members and guests were present.

A paper presented at the Eleventh General Meeting of the American Institute of Electrical Engineers, Philadelphia, May 17th, 1894. President Houston in the Chair.

STANDARDIZING ELECTRICAL MEASURING INSTRUMENTS.

(*a*) By the Potentiometer Method.

(*b*) An Improved Direct Reading Potentiometer.

BY ELMER G. WILLYOUNG.

During the early days of applied electricity, yet easily within the memory of even the youngest among us, so rapid has been its development, electrical devices in general were, as a matter of course, but very poor affairs as compared with those of the present. One machine was a dynamo if it would but produce current no matter how fluctuating, nor at what cost of internal energy consumption; another a motor if one of its parts would but rotate when supplied with electrical energy regardless of the rate of this rotation or whether constant or not under conditions of variable load. Having such uncertain quantities to deal with, electrical measuring instruments were, similarly, but few in number and of the crudest possible construction—not measuring instruments at all in any true sense of the word, but merely indicators, and very poor ones at that. An instrument was supposed to be a non-essential, a decoration for fancy dynamo rooms, and perfectly equivalented by an incandescent lamp for all practical purposes. Indeed, this idea has not yet entirely disappeared, and we may still occasionally find plants running with nothing more than the pilot lamp to go by.

In general, however, all this is now changed. Business competition and the popular demand for electricity has caused rapid and steady improvement all along the line until now we have much time and study devoted to the problem of how to raise machine efficiencies a fraction of one per cent. or so, how to build armatures so as to secure more perfect ventilation and consequent reduction of internal heating, how to produce as good results with a little less iron, a little less wire, a little less wear, etc. Losses in joints, switches, etc., hitherto so small as to have been neglected must be guarded against and, if present, reduced to the smallest possible amount. The whole problem of manufacture and installation has become one of the gaining of petty victories.

All this has reacted upon the instrument maker. In order to measure these small effects, very perfect and accurate instruments are necessary, and great improvements have been made in this direction. The commercial measuring instrument of to-day is, indeed, an entirely new appearance, and demands exactly the same application of mechanical principles and scientific methods as is demanded in the construction of bridges and steam engines.

The necessity of a complete outfit of proper measuring instruments, voltmeters, ammeters, ground detectors, etc.,—as a factor of economical operation in all kinds of lighting and power systems, is now almost universally conceded, and we find such instruments all over the country in vast numbers. In the stations we have the station instruments; for general all round testing work, measuring drops, joint and switch resistances etc., the portable instrument is useful.

As as at present made, all of these various instruments have in them dangerous elements of change. If the instrument has a permanent magnet, then the strength of its magnet is liable to be seriously affected in time by the perpetual jars and vibrations to which it is subjected; proximity to strong fields, especially if variable, will tend to produce similar results. Any instrument containing springs is also likely to vary in its indications in time through slow changes of elasticity in the springs be they ever so carefully gauged. A hot wire instrument may change on account of the change in radiating quality of the surface of the working wire produced by its slow oxidation; molecular changes in the wire are also liable to go on when continually heated, since our experience with standard resistances shows us that changes of this kind do go on very slowly even with wires kept at normal temperatures. And so of any type of commercial instrument which may be mentioned—none of them can be absolutely trusted not to change.

This being so, we must, therefore, frequently examine these various instruments and determine if such changes have taken place, and, if so, what has been their magnitude. As a rule this is done at present not by the owner of the instrument, but by the instrument maker, the instrument being taken down and sent back to the maker from time to time for recalibration, or, if it is not, it should be. This, in itself, is expensive—express charges must be paid—time must be spent in correspondence—often, if not always, a charge is made for the work of restandardizing. Not only this, but there is no surety whatever that these restandardized instruments may not again "get out" while on their way back to the owner, as the sharp, "high frequency" jars of transit are known to be very hard upon magnets and fine mechanism.

Consideration of the above, long ago convinced me that this restandardizing should be done by the stations themselves in all cases except when quite small, and that some arrangement of apparatus should be devised which would make the utilization of some absolute standard method inexpensive and convenient, and possible to any one of ordinary intelligence. After a good deal of thought and personal experience, I became convinced that the only method which could be made to at all satisfy these requirements was the potentiometer method. Before speaking specifically of the improved apparatus which I have devised for the commercial utilization of this method I wish to discuss briefly the potentiometer method in general and to outline what I consider its advantages over all other methods.

THE POTENTIOMETER METHOD. Broadly, the potentiometer method consists in opposing some known *proportion* of the drop of an unknown E. M. F. through a given resistance to a definitely *known* E. M. F., the proportion being so chosen that no current is produced by the latter known source. This condition being established, an equation involving the unknown E. M. F. as the only unknown quantity immediately obtains. The method in general is fairly well known, being variously called the standard cell method, the Poggendorf or Rayleigh-Poggendorf compensation method, the Rayleigh method, etc. It is not by any means so widely known, however, as it deserves to be.

Diagrammatically the method is represented in Fig. 1. Here A B is a wire of high resistance and any desired length stretched between two fixed points. B_a is the battery whose unknown E. M. F. we desire to know, supposed higher than that of S. C. which is a known standard cell. In series with S. C. is a galvanometer, GA. BA and S. C. are so placed as to oppose one another so that when the circuits are closed a position, S, of the slider upon the wire may be found such that the fall of E. M. F. of BA between S and B

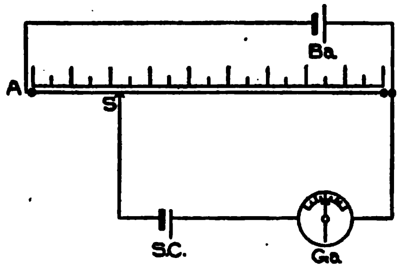


FIG. 1.

will exactly equal the E. M. F. of S. C. There will then be no deflection of GA, and we will have

$$\frac{E_{Ba}}{E_{S.C.}} = \frac{A\ B}{S\ B} \text{ or } E_{Ba.} = E_{S.C.} \frac{A\ B}{S\ B}. (1),$$

an equation involving only a known E. M. F., and the ratio of two lengths. The resistance A B should, of course, be chosen so large that the internal instance of BA is negligible as compared with it. In practice this form of the method is commonly known as the Rayleigh or Rayleigh-Poggendorf compensation method; and instead of a straight wire, A B, this is equivalented by two resistance boxes of about 5,000 ohms each. This arrangement is shown in Fig. 2. Plugs are removed in R and R′ so as to have in all a *total* of, say, 5,000 ohms out. This total is maintained

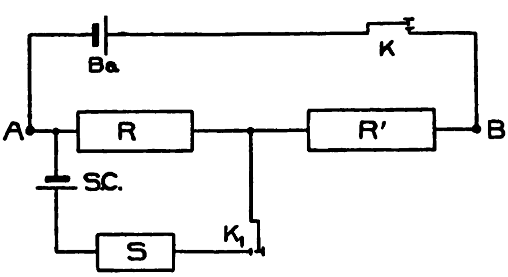


FIG. 2.

constant, whatever plugs are taken out of one box, being inserted in another. When a balance is secured, the resistance in the two boxes must be counted up and then equation (1) applied.[1]

1. The galvanometer, in both Figs. 2 and 4, has inadvertently been omitted; it should, in both instances, be placed in the "derived" circuit similarly to fig. 1.

This arrangemenent of the method is very suitable for making comparisons of standard cells, B_a being merely any form of constant battery. A balance having been secured for S. C. it is then removed and another standard cell, S_1. C_1., substituted; a new balance is then gotten. By properly choosing B_a and making the total of R + R′ 10,000 ohms, we may cause each ohm difference in balance between different cells, to signify a difference of 0.01 per cent. If a properly sensitive galvanometer be used, a variation of one ohm from balance is very easily seen. This arrangement is also very accurate, and easily used in determining the temperature coefficient of a cell, the cell being surrounded by a water bath, and balance being obtained for different temperatures.

A modification of this method was proposed some years ago by Dr. Fleming, and is shown in Fig. 3.[1] It is, as is evident, exactly the same as Fig. 1, save that a resistance, R, is inserted in

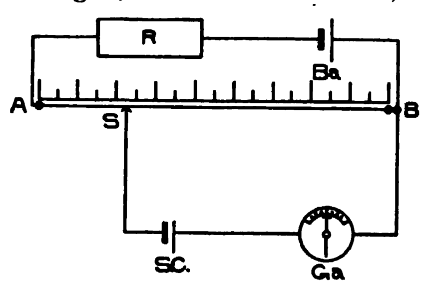


FIG. 3.

the main circuit. Here B_a, instead of being the E. M. F. to be measured, is merely any fairly constant source, such as *e. g.*, a cell of storage battery. The wire, A B, is stretched along a scale uniformly divided into, say, 150 parts, 100 of which are then made to correspond to a fall of B_a of one volt by adjusting S to a position corresponding to balance, if this assumption were true, and then *making* it true by an adjustment of the variable resistance, R. If A B is so great that the current produced by B_a is small compared with its normal discharge rate, then the difference of 1.5 volts between A and B may be assumed constant for a considerable period, especially as the circuits should be closed only long enough for balance. Any *inconstancy* may be immediately detected by again placing S at the balance point. Balance should

1. See "Short Lectures to Electrical Artisans," by J. A. Fleming; also "Electrical Measuring Instruments," by Jas. Swinburne–Proceedings. Institution of Civil Engineers, Vol. C.X., Session 1891–92, Part IV.

persist without further adjustment of R. To measure any unknown E. M. F., now, S. C. is simply removed, and the unknown source substituted for it. If this source is less than 1.5 volts then a position of S will be found, at which there will be balance, and the number of parts between S and B will give the E. M. F. directly by pointing off two places of decimals. Should the E. M. F. be higher than 1.5 volts, then it may be placed in series with a high resistance, and the E. M. F. over a portion only of this resistance measured. The two place reading of S multiplied by

$$\frac{\text{whole of high resistance}}{\text{portion of high resistance in derived circuit}}$$

will again be the unknown E. M. F desired.

Not only may we thus measure E. M. F. but, by arranging as in Fig. 4, we may also measure *current* by this method if we know the resistance of shunt S. Similarly, we may measure resistances by comparing the fall of E. M. F. around a known resistance with that

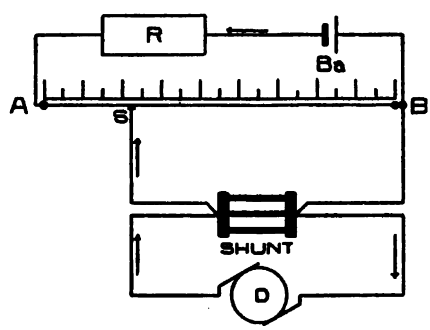


FIG. 4.

around the unknown resistance placed in series with it, a constant current being maintained through both.

The advantages of the potentiometer are:

(1.) It is a zero method; a calibrated galvanometer is, therefore, not necessary.

(2.) Accuracy depends only upon a standard cell and a standard resistance, and results obtained by the best authorities over a number of years, show that both of these may be relied upon within extremely small limits of error, with proper treatment, for a practically indefinite time.

(3.) It requires but simple apparatus, always obtainable, and ordinary care. It can be used without inconvenience in regions of great mechanical instability and of intense and variable magnetic fields.

OTHER METHODS OF STANDARDIZING.

The advantages of the potentiometer method can, perhaps, be better appreciated by briefly recapitulating the other possible methods. These are but few in number. We have:

THE DIRECT INSTRUMENTAL METHOD.

Here the values must be obtained by direct measurement with instruments which do not contain elements of change. About the only instruments satisfying this condition are the tangent galvanometer in some form or other, and the Thomson balance.

THE TANGENT GALVANOMETER.

Disadvantages:—(a) The instrument must be very accurately constructed and is hence costly.

(b) To secure accuracy, the instrument must have a suspended system which is liable to continual break-down, and is difficult of repair except by an expert.

(c) Accuracy depends upon the constancy of H, the horizontal magnetic component; this depends upon local conditions and varies with change in position of neighboring magnetic materials and with the temperature of fixed masses of such metals.

(d) Indications are greatly affected by variable currents or magnetic fields in the vicinity.

(e) Mechanical stability must be had; hence such an instrument is difficult to use in stations or localities where mechanical vibration is large.

(f) The instrument must be very carefully adjusted in the first instance—this requires considerable time and skill. It must, therefore, be *permanently* set up in combination with the observing telescope or lamp and scale, in a room or part of a room which must not be used for any other purpose in order that the outfit may not be disturbed.

THE THOMSON BALANCE.

This instrument is much superior to the tangent galvanometer, requiring much less skill in use and practically no adjustment. It is not affected by being moved about from place to place if carefully handled, and is not appreciably varied by magnetic changes of small amount.

Disadvantages:—(a) It is affected by fluctuating currents or fields of large value if very near.

(b) A large number of instruments is required to cover very much of a range of measurement, any one instrument only measuring within limits of 1 to 100 as *e. g.*, from 1 to 100 centi-amperes, 1 to 100 amperes, etc.

THE VOLTAMETER METHOD.

This depends upon the maintenance of a steady current for at least 20 minutes or a half hour through an electrolytic solution, usually silver or copper, and an accurate weighing of the amount of decomposition thus produced.

Disadvantages:—(a) It is slow, but one value being obtainable in this time.

(b) It requires considerable skill in making accurate weighings with a delicate balance; also in adjusting the current values used to the area of the electrodes, since otherwise the amount of decomposition is irregular and unreliable.

THE VIENNA SHUNT METHOD.

This is an indirect method in which the current is obtained by measuring the E. M. F. produced at the terminals of a suitable shunt by its steady flow. The shunt resistance must, of course, be known.

Disadvantages:—(a) The E. M. F.'s to be thus measured are usually quite small and must be measured by a very sensitive galvanometer. This galvanometer must itself be calibrated, a work requiring considerable time and skill. This calibration, once obtained, cannot be depended upon from day to day owing to the fact that the slightest change in level in the instrument, irregular loss of magnetism in the needle, if a Thomson galvanometer, or of torsional rigidity in the suspension, if a D'Arsonval galvanometer, will alter it. In a Thomson galvanometer the indications are greatly thrown out by even very small changes in the local magnetic field such as may be produced by keys or a knife upon the person, or active circuits even when some considerable distance away.

(b) Great mechanical stability is usually required in order that observations of the reflected beam of light may be possible.

(c) Thermal E. M. F.'s are met with which are tedious to eliminate or allow for.

(*b*) AN IMPROVED DIRECT READING POTENTIOMETER

This instrument, invented by the writer and illustrated in Fig. 5, is capable of being used for measurements of voltage from 0 up to 1,500 volts, and of current from 0 up to any required upper limit, with a maximum error of not over $\frac{1}{10}$ per cent. It is based

FIG. 5.

upon the form of potentiometer originally suggested by Dr. Fleming, which is shown in diagram in Fig. 3. Some good suggestions were also obtained from the improved form of Mr. Crompton.[1]

1. See *Electrician.* (Lon.) May 12, 1893, p. 32.

A diagram of the arrangement of circuits in my improved form of apparatus is shown in Fig. 6. The wire A B of Fig. 3 is here equivalented by three sets of coils marked respectively Q, M, and S. The regulating resistance (R of Fig. 3) is made up of two parts, one resistance, R_Q, consisting of coils, and the other, R_S, being made up of a bare wire of resistance equal to one of the coils of R_Q. This wire is laid back and forth upon about 350° of an ebonite cylinder and has a fixed brush, P, so arranged with reference to it that P short-circuits more or less all of the V's simultaneously, thus giving a large range of variation for a very small angle of rotation. The quick and rough regulation is effected by R_Q, while the finer adjustment is obtained by R_S.[1] The two series of coils, M and S, have at their centers two switches which are in series with the galvanometer and unknown E. M. F.; moving these switches over the coils is exactly the same as shifting the points S and B of the derived circuit in Fig. 3. M is the medium movement, each coil being $\frac{1}{150}$ of the entire resistance between A and B, while S is the very slow movement, each of the nine coils in S being equal to $\frac{1}{10}$ of the resistance of one of the coils of M. In order to get a rate of separation of the two derived circuit terminals by steps of 10 times the value of a coil of M, a third series of resistances, Q, is provided. This is a rather peculiarly arranged affair, its function being to take out resistance from between the derived circuit terminals and place it outside, or *vice versa*, the total resistance between A and B remaining always constant. The construction of this arrangement is shown in Fig. 8; here merely the diagram of circuits can be depicted. It is evident that this change of resistance from one part of the circuit to the other accomplishes exactly the same result as is obtained by M and S, viz., the separation or bringing together of the two derived circuit terminals. We have, therefore, the entire resistance, A B, divided into 1,500 parts, and the derived circuit terminals separable along this resistance by steps of 100 parts, using Q; ten parts, using M; and single parts, using S. This means an ability to set to 1 part in 1,500 or to $\frac{1}{15}$ per cent.

In order to make the measurement of any unknown potential convenient and quick, a suitable arrangement of switches is provided. In the lower part of the diagram, R is a high resistance

1. Instead of this form of regulator I have thought of substituting some form of carbon or graphite resistance since all that is needed is invariability for a certain period. E. G. W.

connected between two points F and G. To F is permanently joined one derived circuit terminal. The other derived terminal passes, by way of a key, K′, through a galvanometer and thence to a switch, W, which plays over a number of contact points connected as shown. The two points, S. C.–1 and S. C.–2, are joined each to a terminal of a Carhart one volt cell, the other terminals of which are joined in common to G.[1] The remaining points are connected to points along F G, dividing the whole resistance in the ratio $\frac{1}{1000}$, $\frac{1}{100}$, $\frac{1}{10}$ and $\frac{1}{1}$. In this way, with W on one or the other of the standard cell contacts, we may by proper setting of Q, M and S, and adjustment of the regulators R_S and R_Q get the E. M. F. between A and B accurately 1.5 volts; we may, also, compare the two cells with one another, and thus detect any possible variations in either, as the probability that both would change alike, in the event of any change taking place, is extremely small. By now setting W on R, any unknown E. M. F. joined to F and G and less than 1.5 volts immediately becomes measurable. Should the E. M. F. be greater than 1.5 volts and less than 15 volts, W is set on R × 10 instead of on R; greater than 15 and less than 150 volts on R × 100; greater than 150 and less than 1,500 volts on R × 1,000. When measuring, current leads are brought to F and G from the shunt terminals and the E. M. F. measured like any other unknown E. M. F. In order to avoid the risk of accidentally getting too large a current through either of the standard cells, a high resistance, H R, of about 10,000 ohms is permanently placed in the galvanometer circuit. A shunt is also placed around the galvanometer which may be thrown in or not, as desired, by means of the little switch placed for the purpose.

Some of the details of construction employed in this instrument are, it is believed, sufficiently interesting to be worthy of description. Fig. 7 shows a novel way of economizing space and material in the construction of the coils and contact points suggested by Mr. H. L. Sayen. The contacts instead of being brass rod or portions of rod, as usual, are tubes having a metal end. Into these tubes the coil, wound on a slim spool as usual is slipped, one end of the wire being soldered to the open end of the tube. Holes just large enough to receive these tubes are then drilled into the rubber top of the instrument and the tubes slipped up from below with closed ends uppermost until stopped by a flange which has previously been soldered around the tube at about $\frac{3}{4}$ inch

1. "A One-Volt Standard Cell," by Henry S. Carhart. *Am. Jour. of Science*, vol. xlvi., July, 1893.

from its end. A rubber plate, P, about $\frac{1}{8}$ inch thick and also drilled through with holes corresponding to the tubes, is slipped over them from below, and screwed fast to the under portion of the top plate. All the tubes are thus firmly clamped in place by means of their flanges. The other ends of each coil are then soldered to the next tube, etc.

Fig. 8 shows the construction of the resistance exchanging arrangement, Q. There are two concentric circles of segments as pictured, these segments being provided with spring pieces. The coils are joined between diagonal segments. Pivoted at their center is a switch formed of a circular plate of hard rubber, carrying at its periphery a number of angled brass wedges or knives

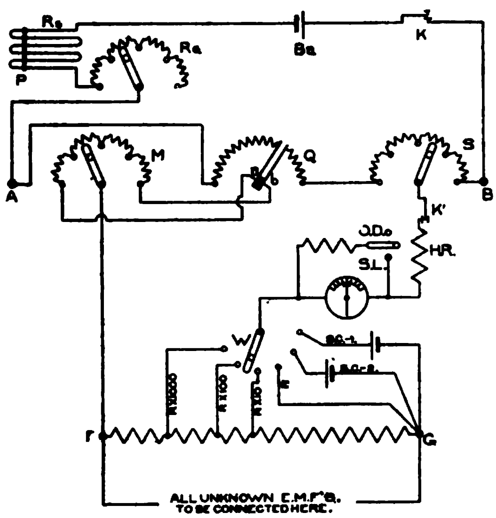


Fig. 6.

corresponding to the segments of the coils. Instead of *one* of these wedges is a *pair* of contact pieces corresponding to a and b, of Fig. 6, and joined to the rest of the circuit as shown in that figure; each piece of this pair makes contact with but *one* segment. It is obvious, therefore, that as the switch rotates, the series is always broken between a and b, while the continuity of all the rest of the series is maintained by the wedges which pass between the segments and keep them metallically joined.

In order to get at the standard cells in case either or both of them should show any signs of giving out, all that is necessary is to remove the four screws in the raised block seen in the upper left-hand corner of Fig. 5. This will expose the binding posts to which the cell terminals are joined, the cells themselves being contained in two brass tubes passing down through the top. The binding screws are loosened and the cell or cells drawn out, when they can be shipped back to the maker for exchange.

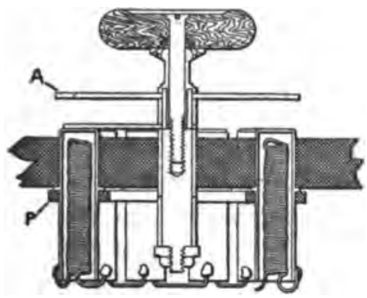


FIG. 7.

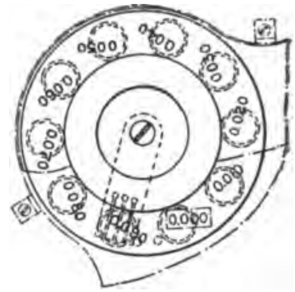


FIG. 9.

This form of potentiometer is practically direct reading, the required values of E. M. F., being numerically set down upon the face of the instrument when balance is attained. The manner in which this is effected is very readily seen from inspection of Fig. 9, which shows a composite view of one of the switch devices M or S. Fixed to the switch handle so as to rotate with it and above the contact and contactor itself is a disk, A of Fig. 7, of hard rubber; this has engraved upon its upper surface a series of numbers cor-

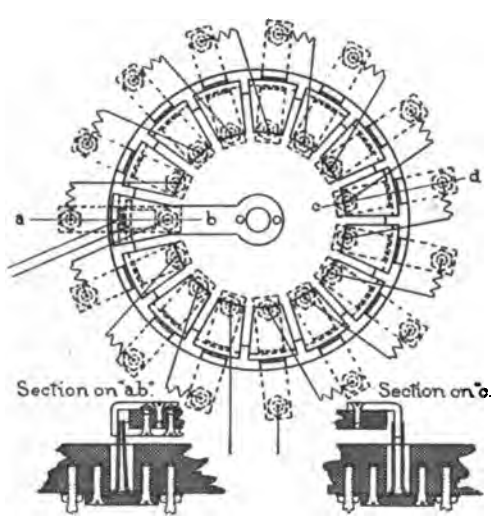


FIG. 8.

responding to the coils of the series, and differing in value by an amount equal to the value of the resistance of each coil in terms of the whole resistance between A and B. Thus the disk of Q having fourteen resistances, each $\frac{1}{14}$ of the whole resistance, is engraved with the series of numbers 0.100, 0.200, 0.300, etc., up through 1.400.; S, on the other hand, is engraved 0.001, 0.002, 0.003, etc., through 0.009.; M in the same way has its disk, which is engraved 0.010, 0.020, 0.030, etc. In the instrument these three switch devices are grouped together as shown in the general view and in plan in Fig. 10. A raised metal box, in shape something like a three-leaf clover, covers their operative portions. In each box there is cut a rectangular hole just large enough to expose a single one of the numbers engraved upon the rubber disk below. These numbers are so placed upon the disk that the one visible, indicates the number of parts of the whole resistance, which by virtue of the position of *its* switch are placed between the points of the derived circuit. The sum of the three numbers displayed is, therefore, the total number of parts of the whole placed between the derived circuit terminals by virtue of the position of *all* the switches, the number of tenths being taken from one, of hundredths from another, and of thousandths from the third.

In practice the operation of the instrument is exceedingly convenient and speedy. It is first turned to $\frac{S.\,C.}{1}$ and Q, M and S turned until they read 1.000, since the Carhart one-volt cell gives exactly one volt E. M. F. The galvanometer switch being turned to "Galv. Shunted," K and K′ should be depressed for an instant. (In the instrument a *single* ivory button projecting up through the top operates a double contact below). If the index swings to "R High" the two regulators, R_s and R_q, should be turned "Up," as shown by the arrow engraved upon them; if to "R Low," then the regulators should go down. The key being thus alternately depressed and the regulators altered, when balance is finally approached the shunt may be removed from the galvanometer by throwing the switch to "Galv. Direct," which will make the adjustment more sensitive, and the process continued until balance is perfectly attained. This having been done, we know that the points A and B differ in E. M. F. by exactly 1.5 volts. To assure ourselves of this we may turn W to $\frac{S.\,C.}{2}$ and again depress the key; this should also give a balance. To measure, now, any unknown E. M. F., it is joined to the terminals F and G; the switch W should be turned to R, R $\times$ 10, R $\times$ 100 or R $\times$ 1,000, according as we believe the E. M. F. to be between 0—1.5, 1.5—15, 15—150, or 150—1,500 volts. If we have no idea what the E. M. F. is, as is very rarely the case, then we may try the points successively. Shunt the galvanometer and depress the key; if needle swings to "R High" turn Q, M and S "Down," as shown by the engraved arrow until the needle swings to "R Low." If the needle cannot be reversed with W on this point, try the next one, and so on until finally, if the E. M. F. does not exceed 1,500 volts, reversal can be accomplished; then remove the shunt from the galvanometer and balance as accurately as possible. This done, the reading of the

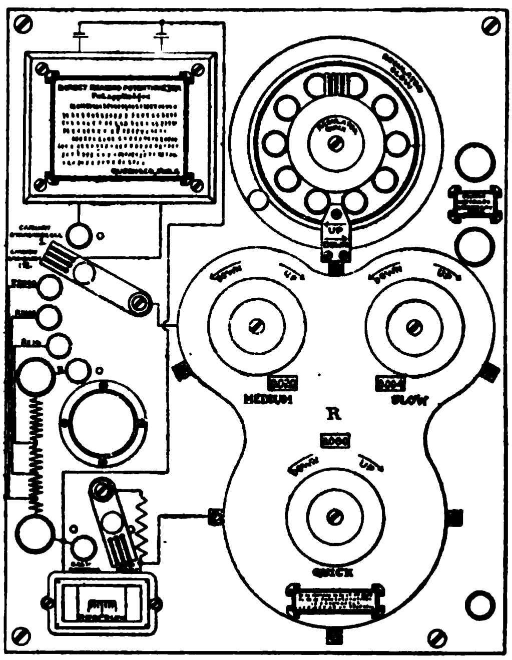


FIG. 10.

dials, R, multiplied by 1, 10, 100 or 1,000, according to which of the points W is on will be the desired E. M. F. If current is being measured, proceed in the same way, only multiplying the E. M. F. thus obtained by the resistance of the shunt in order to get the current itself. The accuracy of the work may be checked at any time by throwing W back on one of the standard cells, and Q, M and S back to 1.000; perfect balance will usually persist, provided the main battery, B_3, has any charge worth speaking of.

In order to guard against any possible change in the E. M. F. between A and B, during a series of measurements, due to change in the battery, B_a, the latter should be of good size, say with a discharge rate of four or five amperes at any rate. The resistance between A and B is 120 ohms, being always constant; this means a current of about $\frac{13}{1000}$ amperes through A B, *while the key is closed*; as the key is only closed for an instant at a time, this drain cannot affect even to the most infinitesimal extent the E. M. F. between A and B.

It may seem as if the large number of rubbing contacts necessitated by this device, 28 in all, was a feature liable to introduce considerable error into the result. This is, however, not the case, for, assuming the largest possible variation in each contact resistance to be 0.001 ohms, (the writer has never found it larger than this in switches of ordinarily decent mechanical construction) we have, as the maximum total variation in Q, 0.028 ohms. The total resistance of A B being 120 ohms, this is seen to be less than 4 parts in 12,000 or but a little over 0.02 per cent.

In connection with this, it is interesting to note that in adjusting the resistances of the coils Q, M, and S; the maximum allowable error of adjustment is a constant percentage of the *total* resistance A B, rather than of the individual coils. Hence if the 8 ohm coils of Q are adjusted to $\frac{1}{25}$ per cent., comparatively easy for coils of so high value, the 0.8 ohm coils of M need only be adjusted to $\frac{10}{25}=\frac{4}{10}$ per cent. and the 0.08 ohm coils of S to 4 per cent. to secure the same accuracy as regards the total of A B, *i.e.*, as regards *result*. The low resistances, therefore, are no more difficult of adjustment than those of the highest value, if indeed they are not easier, so that the resistances as a whole are easily brought within the required limit of not over $\frac{1}{15}$ per cent. error in result.

The value of this instrument as a convenient and quick way of obtaining absolutely reliable determinations of current and E. M. F. is, we believe, very great. There are scores of engineers, laboratories and stations who constantly find it necessary to secure a standardization of their ammeters or voltmeters. To make a voltameter determination takes a great deal of time, even if but one value is to be gotten, and an amount of skill and painstaking, not always immediately available. The absolutely steady current, suitable solution, chemical balance, etc., are also adjuncts not always at hand. To keep any form of standard instrument about for purposes of comparison is usually not feasible. If the instrument has springs, the springs may change. If permanent magnets they are almost bound to change. Other forms of apparatus more free from variable elements, as *e. g.*, tangent galvanometers, are easily affected by strong fields and mechanical disturbances and require either to be always kept set up absolutely undisturbed, or else to have considerable time spent upon them each time before using, in order to adjust them into good condition. In the potentiometer there are practically no variable elements, at least within limits practically infinitesimal in practical measurements. Dependence is placed solely and entirely upon standard resistances and standard cells, both of which have been thoroughly investigated by many workers, and the variations of which are well understood. The apparatus is simple and compact, and its mode of use can be successfully learned by a schoolboy in a few moments.[1] Being a zero method it is absolutely unaffected by neighboring currents or magnetic fields and the galvanometer being a dead beat and jewel suspended D'Arsonval, may be used under the most severe conditions of mechanical shock and vibration. On board ship it is believed absolutely the *only* apparatus which could possibly be used.

1. The dimensions over all are 11 in. by 14 in. by 5 in. deep; weight less than 10 lbs.

DISCUSSION.

MR. R. O. HEINRICH:—Assuming that Mr. Willyoung's introductory remarks in regard to the unsatisfactory constancy of commercial voltmeters and ammeters be true, it should be borne in mind that in the best types of commercial measuring instruments this condition of affairs is not due so much to the principles and methods employed in the construction of such instruments, as to the very exacting conditions under which they are used. We may safely say that with proper care, instruments employing electromagnetic or permanent magnetic fields are at least as constant as standard cells, and in considering therefore the advisability of adopting one or the other method for the purpose of comparing and standardizing commercial instruments, convenience and simplicity become the most important factors. I think there is no question that a direct reading instrument is by far the most preferable, under "direct reading," being understood that the instrument gives direct indications which are read on a scale, without any further manipulation than connecting the instrument in circuit.

The potentiometer method in the hands of a skilled person is undoubtedly a very valuable one, being a zero method and entirely independent of surrounding magnetic fields. It is, however, not a direct reading method in the above sense of the word, and it becomes very tedious and even unreliable, if comparisons with fluctuating currents have to be made. This, I believe, is one very potent reason why all attempts of introducing the potentiometer method in the form of a commercial measuring instrument have lacked success.

The "dangerous elements of changes" attributed by Mr. Willyoung to all other methods, are in my opinion not at all obviated by the use of a standard cell, at least such cells as are now to be had in the open market.

Mr. Willyoung mentions in his paper that his apparatus is capable of being used for measurements of voltage from 0 up to 1,500 volts, and of currents from 0 up to any required upper limit with a *maximum error of not over one-tenth of one per cent.* Mr. Willyoung is to be highly congratulated if his statements are borne out by actual facts. I have found very great difficulty in obtaining an absolute accuracy of one-tenth of one per cent. working with the best appliances under the best conditions in a laboratory. But assuming that the general construction of the apparatus, the introduction of a complicity of sliding contacts for comparatively low resistances, the adjustment of such resistances and finally the sensibility of the galvanometer used in the apparatus will allow such remarkable accuracy after the instrument has left the factory, it remains to be proven whether the standard cell employed, and in general any standard cell can be relied upon to such a degree of accuracy for any reasonable length of time.

On the strength of information collected from various sources and on that of my own experience I am very loath to accept Mr. Willyoung's statement in the affirmative, at least not without a good deal of reserve.

Dr. Kahle, of the Imperial Physico-Technical Institute of Berlin, in a most exhaustive treatise on the Clark cell (*Zeitschrift für Instrumenten Kunde*, vol. xii, p. 117 and (vol. xiii, pp. 191 and 293) mentions (vol. xiii, p. 303) that six Clark cells, made according to the instructions of the Board of Trade, were sent to him from Cambridge, England. Of these six cells, two were spoiled in transit. The remaining four were tested at intervals for a period of nine months together with five cells made in Berlin according to the above mentioned instructions. The results of these tests are given in Table I. For the convenience of comparison I give the differences of these cells from the normal E. M. F. in percentages. Dr. Kahle gives the differences in hundred-thousandths of a volt.

TABLE I.

Date of Test.	Set up in England and sent to Berlin.				Set up in Berlin.				
	E_1.	E_2.	E_3.	E_4.	I.	II.	III.	IV.	V.
Sept. 14, 1892...	− 0.035	− 0.037	+ 0.007	− 0.011	+ 0.016	+ 0.017	− 0.036	− 0.039	+ 0.010
" 15, " ..	− 0.034	− 0.020	+ 0.011	− 0.009	+ 0.016	+ 0.015	− 0.038	− 0.041	+ 0.012
Oct. 31, " ..	− 0.004	+ 0.013	+ 0.014	+ 0.010	+ 0.002	− 0.009	− 0.029	− 0.004	− 0.014
Nov. 1, " ..	− 0.004	+ 0.017	No test.	+ 0.010	+ 0.002	− 0.009	− 0.029	− 0.004	− 0.016
" 10, " ..	− 0.178	0.000	"	− 0.116	+ 0.004	− 0.070	− 0.091	− 0.110	− 0.103
" 12 " ..	− 0.142	− 0.098	"	− 0.070	+ 0.008	− 0.050	− 0.050	− 0.070	− 0.067
April 19, 1893...	− 0.184	− 0.370	+ 0.004	− 0.104	0.000	− 0.092	− 0.020	− 0.362	No test.
June 14, " ..	− 0.131	− 0.035	+ 0.005	− 0.067	+ 0.002	− 0.060	− 0.022	− 0.191	"
" 17, " ..	− 0.252	− 0.030	+ 0.007	− 0.055	No test.	− 0.064	− 0.025	− 0.167	"

It will be seen from this table that of the four English cells only one, and of the five cells prepared in Berlin only three would answer Mr. Willyoung's requirements.

Dr. Kahle ascribes the desultory and sometimes very considerable variations to the general construction of these cells, since the variations were shown when the cells had never been disturbed and had been kept at almost constant temperature during the entire period of the test.

In the same article he says:

"The E. M. F. of the Feussner cell (a Clark cell of somewhat "large dimensions with mercurous sulphate and mercury elec- "trode retained in a porous cup) and especially of the English "cells, even at constant temperature, depends entirely upon the "conditions of temperature to which the cell was exposed dur- "ing the previous day. This uncertainty of E. M. F. surpasses in "the Feussner cell 0.001 volt (0.07 per cent.) and may amount "to 0.003 volts (0.21 per cent.) to 0.005 volts (0.35 per cent.) in "the English cells. * * * * On account of the large and "indefinite changes with change of temperature, of the E. M. F. "of the Clark cells, it has been tried to replace it with others "which are claimed to behave more favorably in this respect. "Prof. Carhart has reduced the temperature coefficient of the "Clark cell by using a zinc sulphate solution which is concen- "trated at 0°C. The advantage gained by this is, however, an- "nulled by the disadvantage that such cells according to the "experience collected here, cannot be reproduced with such "exactness as the cells with zinc sulphate crystals in excess. It "is very difficult to make a solution which is saturated at exactly "0°C. If it is saturated for a temperature deviating only a few "tenths of a degree from zero, a deviation of the E. M. F. "amounting to several ten thousandths of a volt from the normal "will be the consequence."

However this may be for the reproduction of these cells by any person skilled in the arts, my experience has been that Professor Carhart knows how to reproduce them with admirable agreement. Their portability seems also to be very satisfactory.

Of the 19 Carhart cells which have passed under my observation I have a definite knowledge of the condition when received, of only eight. Seven of these showed a very good agreement amongst themselves in a test made about two weeks after they were received. Cell 332 was tested two days after receipt, and showed an E. M. F. 0.075 per cent. above the normal of 1.440 volts. Tested two months later its E. M. F. had dropped 0.117 per cent. below its original value.

In point of constancy the results have been less satisfactory, the changes being most likely due to leakage.

It is quite evident that in Professor Carhart's modified Clark cell, the slightest leakage must be detrimental since this would at once alter the concentration of the zinc sulphate solution. Although there seems to be a marked improvement in the cells received lately from Professor Carhart, it will be seen from Table III. that in five of the ten cells a continual dropping of

TABLE II.

	1	2	3	4	5
Carhart-Clark.	Set up.	Received.	Test, August, 1893. Compared with No. 284.	Difference in per cent. 284 Standard.	Difference in per cent. 210 Standard.
No. 202........	Sept. 25, 1891	Feb. 10, 1892	0.9976	— 0.24	— 0.09
" 203........	" 26, "	" " "	0.9981	— 0.19	— 0.04
" 204..	" " "	" " "	0.9983	— 0.17	— 0 02
" 205..... ..	" " "	" " "	0.9979	— 0.21	— 0.06
" 207..... ..	" " "	" " "	0 9986	— 0.14	+ 0 01
" 209........	" " "	" " "	0.9987	— 0.13	+ 0.02
" 210..	" " "	" " "	0.9985	— 0.15	0.000
" 275.... ...	Nov. 19, 1892	August, 1893	0.9998	— 0 02	
" 284..	" 24, "	" "	1.0000		

TABLE III.

Date of Test.	311	312	313	314	315	316	317	275	284	332	Temp. at test.	Average of Cells 311 to 317.	Average E.M.F.
Nov. 7, 1893	1.0000	1.0001	1.0001	1.0001	1.0002	1.0000	1.0001	No test	No test	No test	14.9	1.00006*	
Dec. 7, "	"	1.0000	0.9999	0.9998	0.9997	0.9997	0.9998	0.9992	0.9997	"	18.75	0.99999	1.4402 I.V.
Dec. 14, "	"	1.0000	1.0000	0.9999	0.9997	0.9997	0.9997	0.9993	0.9997	"	19.00	0.99999	1.4402
Dec. 18, "	"	1.0000	0.9999	0.9999	0.9996	0.9996	0.9999	0.9992	0.9996	"	18.80	9.99989	1.4402
Dec. 20, "	"	1.0000	0.9999	0.9999	0.9996	0.9996	0.9998	0.9992	0.9997	"	19.60	0.99988	1.4402
Jan. 16, 1894	"	0.9999	0.9999	0.9999	0.9996	0.9995	0.9999	No tcst	No test	"	20.50	0.99987	1.4401
Jan. 24, "	"	1.0000	0.9999	0.9999	0.9994	0.9993	0.9997	"	"	"	19.80	0.99982	1.4401—
Mar. 21, "	"	1.0000	0.9999	0.,999	0.9994	0.9990	0.9997	"	"	1.0005	18.30	0.99979	1.4400
May 7, "	"	0.9999	0.9999	0.9999	0.9993	0.9987	0.9998	"	"	1.0005	19.85	0.99975	1.4400—
May 9, "	"	1.0000	0.9999	0.9999	0.9993	0.9987	0.9998	"	"	1.0005	19.60	0.99976	1.4400
May 25, "	"	1.00000	0.99984	0.99990	0.99927	0.99862	0.99973	0.99919	0.99953	0.99933	19.51	0.999736	1.4439+
May 10, "	"	0.99997	0.99987	0.99987	0.99929	0.99869	0.99972	No test	0.99953	0.99933	19.85	0.999741	1.4439+
June 5, "	"	0.99995	0.99999	0.99990	0.99927	0.99857	0.99972	0.99918	0.99951	0.99933	20.40	0.999740	1.4439+

The comparisons were made taking 311 as standard. The E. M. F. of 311 was determined by five silver voltmeter tests and gave an average of 1.44031 inter. volts. Prof. Carhart's figures for the same cell 1.4402, satisfactory proof that this cell remained very constant. The figures in the last column show that the average E. M. F. of the cells 311 to 317 differs not more than 0.014 per cent. from the normal 1.4402 intern. volts whilst cells 315 and 316 have reached very nearly the $\frac{1}{10}$ per cent. limit. Another proof that a large number of cells should be used to obtain accordant results. It will be interesting to investigate further how the cells showing tendency to change will behave during the hot summer weather, since this seems to me to be critical to the seals of most cells.

* The first horizontal line is Prof. Carhart's figures.

the E. M. F. has taken place. These cells were kept continually in an oil bath, covered completely by oil, and were subjected to only very small variations of temperature. Cells 275 and 284 had been kept in open air in the laboratory from August '93 to November '93. Cells 147, 150, 202 to 210 were also kept in open air in the laboratory and had to undergo changes of temperature during the year between 15° and 30° centigrade; the same would be the case for cells used in an apparatus as proposed by Mr. Willyoung.

Remarks to Table II.—Cells 202 to 275 were compared with 284 as standard. Assuming that at that time 284 was very nearly correct, since it was just received, and still shows in Table IV a fair agreement with the normal E. M. F., the figures in column 4 would give the percentage difference from the normal E. M. F. Cells 202 to 210 were returned on account of these changes, therefore no further test could be made. The figures in column 5 are rather interesting, as they show that a number of cells of

TABLE IV.

Carhart-Clark.	Set up.	Received.	Test, May 25, 1894. E. M. F. at 15° C.	Correct Value at 15° C.	Difference in per cent.
Cell No.					
147	Mar. 22, 1890	1891	1.430	1.441	— 0.75
150	" 29, "	"	1.431	1.441	— 0.69
275	Nov. 19, 1892	August, 1893	1.438	1.440	— 0.14
284	" 24, "	" "	1.439	1.440	— 0.08
311	" 7, 1893	November, 1893	1.4404	1.4402	+ 0.014
312	" " "	" "	1.4404	1 4403	+ 0.007
313	" " "	" "	1.4402	1.4403	— 0.007
314	" " "	" "	1.4402	1.4403	— 0.007
315	" " "	" "	1.4390	1.4404	— 0.10
316	" " "	" "	1.4384	1.4401	— 0.12
317	" " "	" "	1.4400	1.4403	— 0.02
322	Dec. 15, 1894	March, 1894	1.439	1 440	— 0.08

TABLE V.

Clark Cells.	Set up.	Received.	Test, May 25, 1894, at 15° C.	Correct E. M. F. 15° C.	Difference in per cent.	Remarks.
Wirt-Clark, 252 A..	?	April, '91.	1.422	1.434	— 0.85	Signs of leakage.
" 252 B..	?	" "	1.415	"	— 1.35	
" 265 A .	?	" "	1.371	"	— 4.4	Signs of leakage.
" 265 B..	?	" "	1.390	"	— 3.6	" "
Weston-Clark, 1....	May, 1891	Prepared in Weston Laboratory.	0.986	"	— 24.2	Apparently in good condition.
" 2....	" "		1.432	"	— 0.14	Glass cracked in sealing, leaked.
" 3....	" "		1.433	"	— 0.07	Apparently in good condition.
" 4....	" "		1.432	"	— 0.14	" "
" 5....	" "		1.430	"	— 0.28	" "
" 6....	" "		1.432	"	— 0.14	" "
" 7....	" "		1.381	"	— 3.7	Cracked in sealing, leaked.
Feussner-Clark, 457	Begin. '93	July, 1893	1.371	"	— 4.4	Leakage, spoiled in transit.

the same batch may compare very well and still differ considerably from the normal E. M. F. *A very good proof against the assumption that two standard cells, kept under the same conditions, are correct if they agree with each other.*

Cells 147 and 150 (Table IV.) are now almost dried up, the seal being completely covered by effloresced zinc sulphate; they had shown signs of leakage already in the early part of 1892. Cells 202 to 210 showed whitish spots between the seals and the glass. A yellowish white coat on the seal was explained by Prof. Carhart, as silicate of sodium acted upon by the air.

Table V gives the results of tests of Clark cells of various origin. With very few exceptions the cells have changed considerably more than one-tenth of one per cent.

The Wirt-Clark cells were contained in two Wirt voltmeters, *i. e.*, direct reading potentiometers. Instrument 265 when revived gave the following results on April 14th, 1891:

Standard 100 volts.... ....100.2	265 A.	+0.2	per cent.
100.85	265 B.	+0.85	"
10 volts.............. 10.85	265 A.	+8.5	"
10.70	265 B.	+7.0	"

On May 25th, 1894:

Standard 100 volts...............77.0	265 A.	—23.0	per cent.
79.0	265 B.	—21.0	"
10 volts 9.73	265 A.	— 2.7	"
9.53	265 B.	— 4.7	"

In Table V the cells are shown to differ from the normal — 4.5 and — 3.6 per cent. at May 25th. It is evident that the sliding contacts and changes in the resistances have in the course of time introduced a much larger error than the cells. The instruments were not used, but had been standing in the laboratory for three years.

Weston-Clark cell 1 is a remarkable case, in-so-far as no sign of leakage could be detected, and that the cell was apparently in faultless condition. There is then a record of 31 standard cells, of which 21 have given out at this date. It would be difficult to say what the useful life of these cells has been, since no record was kept at sufficiently close intervals.

My experience with the Calomel cell has been too limited to allow of forming a definite opinion. With two Calomel cells the following results were obtained by silver voltameter determination.

Cell 303.	April 3d....................1.00001	intern.	volts	at	20° C.
	April 6th...................1.00009	"	"	"	
	April 16th..................1.00002	"	"	"	
	May 10th....................1.00010	"	"	"	

One test made May 8th gave the E. M. F. of the same cell as 0.99922 intern. volts at 20° C. Previous to this test the cell was used very continually during one hour, a grounded circuit giving trouble in adjusting. Although the circuit contained resistances, considerably more than 100,000 ohms in series with the cell, it is thought that the continual use caused the drop in E. M. F., the more so as a comparison with Carhart-Clark cell 311, made shortly after the experiment, indicated also a lower E. M. F. than usual. A silver voltameter test made two days later gave a result agreeing well with previous determinations.

These determinations would make the E. M. F. of this particular cell 0.99961 intern. volts at 15° C.

Carhart Calomel cell 5 loaned through Professor Carhart's courtesy for comparison with cell 303 showed by comparison 1.00018 intern. volts at 15°C, a difference of 0.06 per cent. between the two cells. These two cells seem to have stood transportation well and make a very satisfactory showing. How they will behave under more exacting conditions with an occasional abuse is a question yet to be determined.

It is evident, however, that the standard cell which Mr. Willyoung proposes to use, has to be a very much superior article to anything which we now have, in order to fulfil the requirements of remaining for a reasonable time within an accuracy of one-tenth of one per cent. Moreover, allowing for occasional abuse of the apparatus and taking it into consideration that a multiplicity of sliding contacts is used in connection with comparatively low resistances, I should consider an accuracy of 0.2 per cent. quite remarkable.

On the other hand I know from experience and from carefully kept records extending over five years, that there is no difficulty whatever in maintaining an accuracy of 0.2 per cent. with direct reading instruments as defined previously, and employing electromagnetic or permanent magnetic fields, and affording the same flexibility in range as the potentiometer described.

The use of the potentiometer method for commercial measuring instruments dates back quite a few years. The Wirt and Howell voltmeters were constructed on this principle. Dr. Feussner describes a potentiometer in 1890 in *Zeitschrift für Instrumenten Kunde* and points out, how the instrument may be made direct reading in substantially the same way as Mr. Willyoung describes. Mr. Crompton's potentiometer is referred to in Mr. Willyoung's paper. It is rather significant that, in spite of these various attempts, the actual use of such instruments is very limited indeed.

For the commercial calibration of a large number of instruments the method is altogether too slow. For occasional comparisons of instruments in daily use a direct reading instrument of known standard qualities seems preferable, the accuracy attainable with such instruments being amply sufficient for all practical purposes.

For laboratory use, where time is usually a minor consideration, the potentiometer would be commendable; but when it comes to accuracies as high as one-tenth of one per cent., I should always take the silver voltameter and a standard resistance as a last resort for a check on the standard cell.

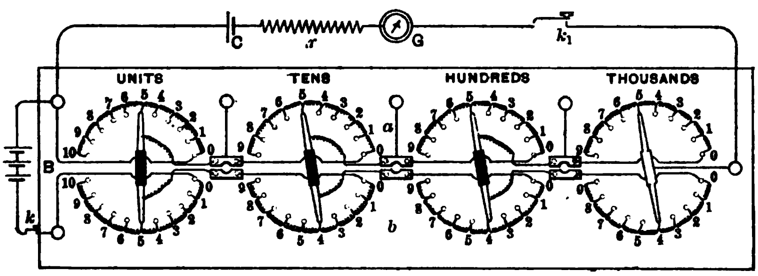


FIG. 1.—Rheostat for Rayleigh's Compensation Method.

In regard to one detail of construction I would say that one idea used also by Mr. Willyoung occured to me some time ago. Mr. Weston designed a potentiometer which was to shorten the tedious adjustment of resistances in Lord Rayleigh's method. Fig. 1 shows its general arrangement. A resistance of 10,000

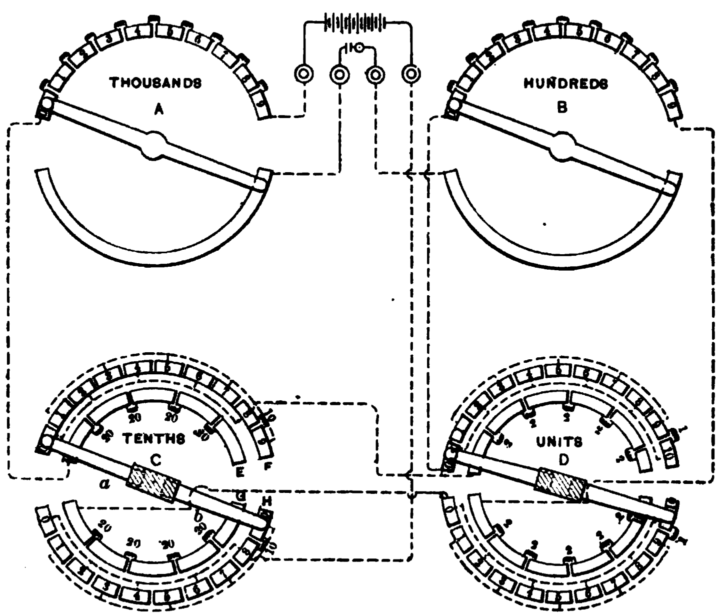


FIG. 2.

ohms is kept in circuit continuously with an auxiliary battery. It will easily be seen from the diagram that by moving the cranks of any of the four dials, resistance cut in on the circuit containing the standard cell c (on the *a* side) is cut out on the opposite side (*b* side). This potentiometer necessitates, however, the adjustment and use of 80 resistance coils. In the modification shown in Fig. 2, the number of resistance coils is reduced to 40 although the same number of steps from one to 10,000 is obtained. The identity with Mr. Willyoung's idea will be found in the arrangement of dials c and D. These two dials are identical in their arrangement, taking dial c for a description; the two halves *a* and *b* of the crank carrying the sliding contacts serve to make a short-circuit between the segments of E, F and G, H. The upper part of these two dials forms always up to the position of the crank, counting from left to

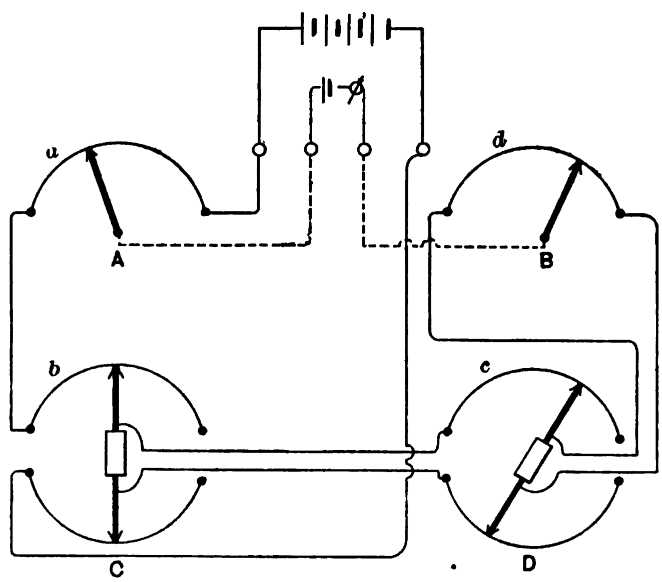


FIG. 3.

right, a part of the circuit containing the standard cell, whilst the lower part is up to the position of the crank in series with the auxiliary battery. Fig. 3 will show this more clearly.

$a + b + c + d$ is then the resistance of the circuit containing the standard cell, whilst the total resistance is always kept constant at 10,000 ohms.

Such an arrangement may of course be made direct reading in the sense taken by Mr. Willyoung, by proper adjustment of the current through the total resistance of 10,000 ohms; it has the advantage of very much higher resistances which I believe essential for accuracy whenever sliding contacts are used.

In summing up my remarks I would say that I do not wish to appear opposed to the use of standard cells, or to that of the potentiometer method. I consider both in the hands of an experienced and careful man extremely valuable and useful. I do not believe, however, that the use of the potentiometer method avoids "dangerous elements of change" any more than any other instruments of standard qualities.

I should consider it an injustice to the user of an apparatus to make him believe that he can measure to a certain percentage of accuracy, when the chances are so very great that he does nothing of the kind.

I am not a believer in "universal portable instruments," with which anything and everything can be done; usually they are crowded into much too small space.

My doubts about sliding contacts in connection with low resistances have been expressed before.

From a superficial inspection of Mr. Willyoung's apparatus I should expect difficulty and trouble in regard to proper insulation, especially if higher voltages are to be measured. If the galvanometer used is of the same sensibility as the one contained in Queen and Company's portable testing sets, I should consider its sensibility insufficient for the attainment of an accuracy of one-tenth of one per cent., as claimed.

A paper presented at the Eleventh General Meeting of the American Institute of Electrical Engineers, Philadelphia, May 17th, 1894. President Houston in the Chair.

AN OPTICAL PHASE INDICATOR AND SYNCHRONIZER.

BY PROF. GEORGE S. MOLER AND DR. FREDERICK BEDELL.

In starting a synchronous alternating current motor, it is usual to bring the motor up to speed by some external means, and to switch it into connection with the generator when the motor and generator are running synchronously but are in opposite phase. Various devices have been employed to indicate synchronism, and to show when the motor is in opposite phase to the generator, one of the simplest of these devices consisting of an incandescent lamp used as a pilot lamp. The lamp is connected directly in the circuit supplying the motor so that all the current through the motor armature passes through it. Before the motor is started the lamp glows steadily. As the motor attains considerable speed, the lamp suddenly flashes up and dies out alternately according to whether the electromotive force generated by the motor, and the electromotive force from the alternator are in the same or in opposite phases. Beats are thus produced which occur at longer intervals as the motor approaches synchronism with the alternator. When the intervals are long enough to be quite marked, the motor is connected directly to the generator circuit by cutting the lamp out at a moment when it is dark, indicating that the machines are in opposite phase. At the same time the external power, which has driven the motor to synchronism, is removed. Instead of one lamp, several lamps or a lamp together with dead resistance may be used where required.

This device is simple and efficient. It does not, however, indicate the moment when exact synchronism is reached, nor does it show whether the motor is running at a greater or less speed than that corresponding to the generator. It does not show the exact phase difference between the motor and generator, and does not indicate the phase relations after the motor has been connected to the alternator and is being driven by it.

The following instrument has been devised by the writers to give definite information in regard to the relative speeds and phase-positions of the motor and generator in laboratory investigations. It shows:

(1.) When the machines are synchronous:

(2.) Which machine is running the faster when they are not synchronous.

(3.) The angle by which the motor lags behind the generator.

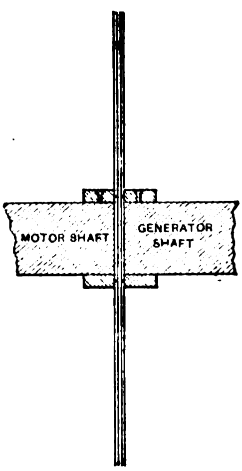


FIG. 1.

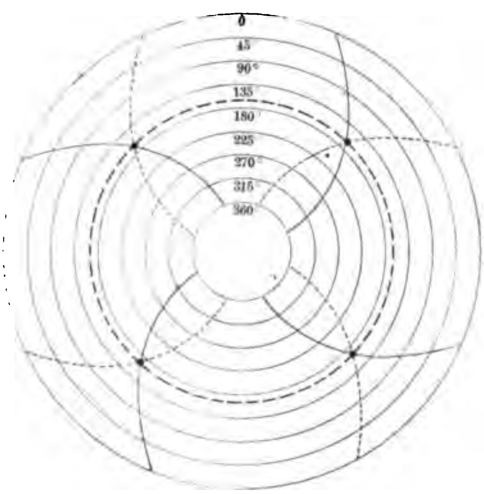


FIG. 3.

We will first describe the simplest form of the phase-indicator. The motor and generator are placed together, with shafts in line and abutting, but not quite touching. The two machines must have the same number of poles so that a revolution of the armature of each, represents the same number of alternations. The abutting ends of the shafts carry two disks, one connected rigidly with the motor armature, the other similarly connected with the armature of the generator, as shown in Fig. 1. In these disks are curved slits, one slit for each pair of poles of the machines. These slits are shown in Fig. 2 for an eight-pole machine. The two disks are in every way similar; the one being the reverse of the other. The two disks are practically superimposed and together form one disk with four holes where the slits of one disk cross over the slits of the other. Evidently the distances of these four holes from the center depends upon the relative positions of the two armatures; they move in and out as the armatures shift their relative positions. From the symmetrical arrangement of the slits, if one armature is stationary and the other is moved past two pole-pieces or through 90° of arc (corresponding to a complete period of alternation or 360° of phase) the intersection of the slits will be the same distance from the center as before. The curvature of the slits is such, that the distance to or from the center that the intersections of the two sets of slits move, is proportional to the change in relative position of the two armatures.

When the two armatures are running at the same speed in the

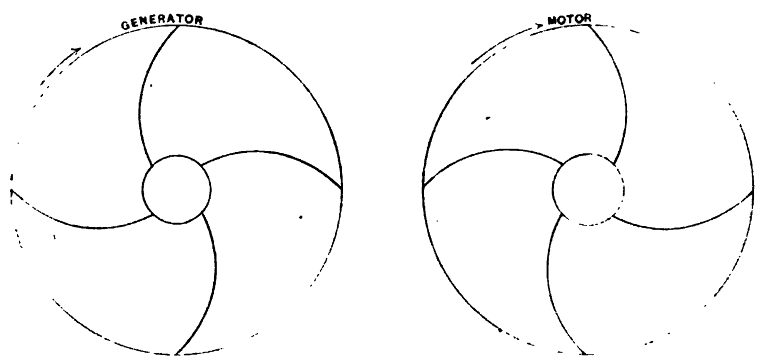


FIG. 2.

same direction, and there is a source of light on one side of the disks, the intersection of the slits, as seen from the other side, appears as a continuous ring of light. A slight difference of speed causes this ring of light to move outward or inward, according to which disk is revolving the faster. The more rapidly the ring moves in or out, the greater the difference in speed of the two disks. If the ring is moving out, a new ring starts at the center when one ring reaches the edges, and these rings keep following one another outward. If the difference in speed is the other way, the successive rings move inward.

In Fig. 3, the heavy dotted line represents the ring of light for a particular position of the two disks.

The position of the ring of light indicates the relative position of the two armatures. The disks may be secured to the shafts so that when the armatures are in the same positions with reference to the pole-pieces (*i. e.*, the machines are in the same phase) the ring of light will be at the inner or outer ends of the slits. The concentric rings in Fig. 3 represent the phase differences corresponding to positions of the ring of light in this case.

For convenience in operation, the arrangement of the apparatus which has thus far proved satisfactory, has been as follows: On one side of the pair of disks is placed an incandescent lamp enclosed in a box. One side of the box is close to the disk and has a slit in it about half an inch wide extending from the shaft to the circumference of the disk. This slit is covered with a piece of oiled paper so as to give a diffused light upon the disks. A complete ring of light is no longer seen from the other side, but only a small portion corresponding to the width of the half-inch slit. A stationary scale is fixed so as to extend from the shaft to the edge of the disks on the opposite side from the lamps, so that the distance of the changing line of light from the center, may be read so as to give the phase difference of the two machines by direct reading. To enable one to see the scale and line of light most conveniently, a mirror is arranged at forty-five degrees with the disks, so that the line of sight is at right angles to the shaft.

The disks may be arranged in the manner just described upon the abutting ends of the motor and generator shafts, only in case the two machines have the same number of poles. Where such is not the case, one or both of the disks can be driven by gears which will give the proper relative speeds to the two disks.

In operation, the instrument has proved quite satisfactory, giving exact and definite information concerning the changes in the armature lag of the motor. The fluctuations in this lag are usually quite marked, and the conditions which cause them can be readily investigated by this apparatus. For instance, this fluctuation is small with proper field excitation; as the field current of the motor is diminished, this fluctuation increases, the line of light moving rapidly back and forth through a greater and greater distance, which finally becomes so great as the excitation is weakened that it goes a distance beyond which it cannot recover; *i. e.*, the motor gets out of step and stops. It would be possible to make a more detailed investigation of these fluctuations by means of a revolving mirror, and they may be photographed and made of permanent record by means of a moving plate.

The apparatus can be applied to other lines of work involving an investigation of phase differences and synchronism, and may be modified to meet the requirements of the problem in hand; but it is peculiarly suited for use in synchronous motor work for which it was designed. The scope of the present paper admits only of a general description of the apparatus here given.

A paper presented at the Eleventh Annual Meeting of the American Institute of Electrical Engineers, Philadelphia, May 17th, 1894. President Houston in the Chair.

A RELIABLE METHOD OF RECORDING VARIABLE CURRENT CURVES.

BY. DR. ALBERT C. CREHORE.

INTRODUCTION.

A practical problem that has in more recent years presented itself to the electrician and physicist alike is: "How shall we measure the exact current which flows in a conductor at any instant of time, and record all the irregular changes to which it is subject?" Probably every one who has thought of such matters at all has considered this problem in some of the phases which it presents. The importance of the question, since the introduction and extensive use of the alternating current, has emphasized the fact that we need a "reliable method" of measuring the instantaneous values of a variable current, which is not a "method by points," but "a method which continuously records the current."

Under "a method by points" is included any method in which the current is obtained from readings (usually of an electrostatic voltmeter) due to the charge of a condenser which may be connected in at any point of time. The essential characteristic of the method is that the current is *supposed* to repeat itself exactly during successive periods, or more generally when the conditions are exactly repeated. There can be no doubt that the current *does repeat* itself under exactly similar conditions, but can we be *sure* that those conditions *are exactly repeated?* By this method a number of points are found, the time occupied being at least several minutes, and the collection of points properly arranged is a representation of the current during as short a time as the one-hundredth of a second, perhaps. Yet this method has proved to be a very useful and practical one, and has given us information concerning the currents and potentials of generators and transformers which is of paramount importance. Yet all will agree that this "method by points" is too limited in its application, and does not show us any sudden temporary change taking place in a current which does not *repeat* itself. Such, for instance, as a sudden "make," or "break," or "change" in an alternating current would not be easily shown by this method. The second method, previously designated "*a method which continuously records the current*," is the one to which this paper more particularly refers. Under this head are included all methods which attempt to record the current by causing it, either directly or indirectly, to move a material "something" so that its displacement is some single valued function of the current. As an example of this method may be mentioned the well-known experiments of Frölich in which a telephone is used, upon the disk of which is mounted a mirror that permits a beam of light to be reflected from it. Any vibration of the disk gives an angular motion to the ray of light, and this motion is in turn recorded upon a moving photographic plate. Other examples might be mentioned in illustration of this method, for instance, a wire which is deflected in a magnetic field, or stream of mercury so influenced; but it will be noticed that in all of these cases an appreciable amount of *ponderable* matter is required to be moved backward and forward during each reversal of the current. When the current reverses hundreds of times per second, the unavoidable difficulty is introduced that the forced oscillations of this ponderable matter, no matter how small in amount, become so superimposed upon those of the current which it is desired to measure that they are inseparably mixed together; and the record does not show the true current, but the resultant vibrations of the instrument. That this is the case with the method of the telephone above referred to, has been established beyond a doubt it seems, by experiments conducted at Cornell University by Mr. Henry Floy. The current furnished to the telephone was carefully measured by the "method by points," and care was taken to see that the current as measured by points was the same as that used in the telephone. The vibrations of the telephone did not even approximately agree with the current as measured by well-established methods.

Bearing these points in mind, and remembering the high frequency of some of the oscillations which it is desired to record, may we not with some degree of certainty predict that any of these methods requiring the rapid motion of ponderable matter will be open to precisely the same objections which are noticed in the case of the telephone? Without answering this question, probably all will agree that the difficulty may *certainly* be avoided by using as a vibrator, instead of this so-called "*ponderable matter*," a vibrator that has *no* weight. It is to this question of finding a form of vibrator *without weight* that I invite your attention.

The Weightless Vibrator.

The idea of the weightless vibrator is perhaps already suggested in the beam of light. But how shall we cause a beam of light to have a change in direction simply by means of a current flowing in a circuit without the intervention of some moving material? A way of influencing a beam of light directly by an electric current (or more properly by its magnetic field) is that discovered long ago by Faraday. It is by means of the discovery of the rotation of the plane of polarization by an electric current that I propose a method of obtaining a weightless vibrator. The explanation will be made clearer by reference to the diagram of apparatus (Fig. 1.) A beam of light is passed through a polarizer (Nicol prism), so that the vibrations of the beam take place in only one plane upon emergency. If it is then passed directly through an analyzer (Nicol prism) the latter may be set at such an angle as to prevent all light from passing through it, and thus produce darkness beyond the analyzer. Faraday's discovery was, that if a beam of polarized light is passed through some *substance* in the direction of the lines of magnetization within that substance, there is a rotation of the plane of polarization in a direction which is the same as the direction of the current required to produce such a magnetic field. The direction of rotation is unaltered, therefore, whether the light beam advances in the same or the opposite direction to the magnetization, so that a beam reflected back and forth through the substance several times, has its rotation increased by equal amounts each time. If the direction of the ray of light be at right angles to the lines of magnetization, there is no rotation produced. The amount of this rotation has been carefully investigated by Verdet, who announced laws by which it may be expressed. They are summed up in the following statement: "The rotation of the plane of polarization for monochromatic "light is in any given substance proportional to the difference in "magnetic potential between the points of entrance and emer-"gence of the ray"; that is, it is equal to a constant times this "difference of potential, and is expressed by the formula

$$\theta = v\ V, \tag{1}$$

where θ = angle of rotation, V = difference in magnetic potential, and v for a given wave-length is constant in any one sub-

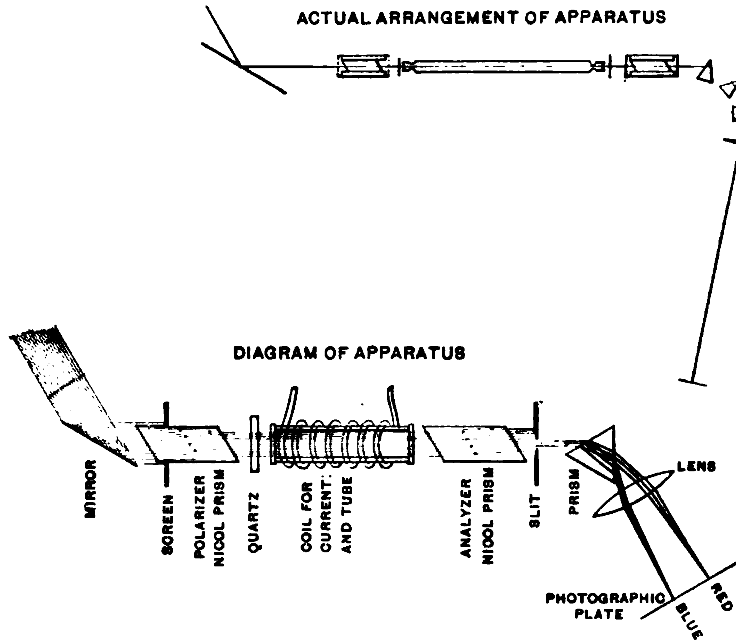


FIG. 1.

stance. This constant is known as Verdet's constant. If now the light is passed through the polarizer and then through a tube containing the substance used, around which is wound a coil of wire, and thence through the analyzer, an observer would find complete darkness upon looking through the analyzer, when set in the crossed position. But if without moving the analyzer a current is sent through the coil on the tube, light appears to the observer. This is because the plane of polarization has been rotated by the current, and practically the prisms are no longer crossed. Now let the analyzer be rotated while the current is still flowing, and the observer will see a series of beautiful colors through the analyzer, a different one for each position of it; but as long as the current flows, he cannot produce darkness again by any amount of rotation of the analyzer.

The effect suggests what is known to be a fact, that the different wave-lengths composing white light are rotated by the current in different amounts, so that when the analyzer is turned to the angle corresponding to the yellow light, say, only the yellow light is prevented from passing through the analyzer. All the other rays, being rotated by different amounts, pass through the analyzer, and there being mixed together they give rise to the series of beautiful complex colors above mentioned. A different color is seen for each position of the analyzer, because in each position a different color is subtracted from white light, and the observer sees what is left, or merely the complementary color.

The law which tells the amount of rotation given to the different colors is pretty accurately known; and theory in this case is in close accord with the observed facts. The equation which closely expresses the amount of the dispersion for the different wave-lengths may be written:—

$$v = \frac{c\, n^3}{\lambda^2}\left(1 - \frac{\lambda}{n}\frac{d\,n}{d\,\lambda}\right), \tag{2}$$

where v is the so-called Verdet's constant, λ the wave-length, and n the index of refraction of the medium: c is a constant for any one medium, which is, however, for different media, inversely proportional to the permeability of the medium. This is a formula at which Maxwell arrived from his theory of molecular vortices, and we shall see how closely it is in accord with observation. We see by this formula that Verdet's constant depends not only upon the wave-length, but upon the index of refraction corresponding to that particular wave-length, and also upon the rate of change of the index with respect to the wave-length. If this rate of change of n with respect to λ is small, as would be the case with a substance where the dispersion is small, and the index of refraction regarded as approximately constant, then it is seen that the formula reduces to an extremely simple form, viz:—

$$v = \frac{c_1}{\lambda^2} \tag{3}$$

Here Verdet's constant is inversely proportional to the square of the wave-length. Using this approximate form for the present, we see from Verdet's law, equation (1), that

$$\theta = v\ V = c_1 \frac{V}{\lambda^2}. \qquad (3)$$

But the difference of magnetic potential, V, is $\frac{4\pi S i}{10}$, where $S i$ is ampere-turns, and thus we have

$$\theta = 4\pi c_1 S i / 10 \lambda^2 = c_2 i / \lambda^2, \qquad (5)$$

where

$$c_2 = 4\pi c_1 S/10. \qquad (6)$$

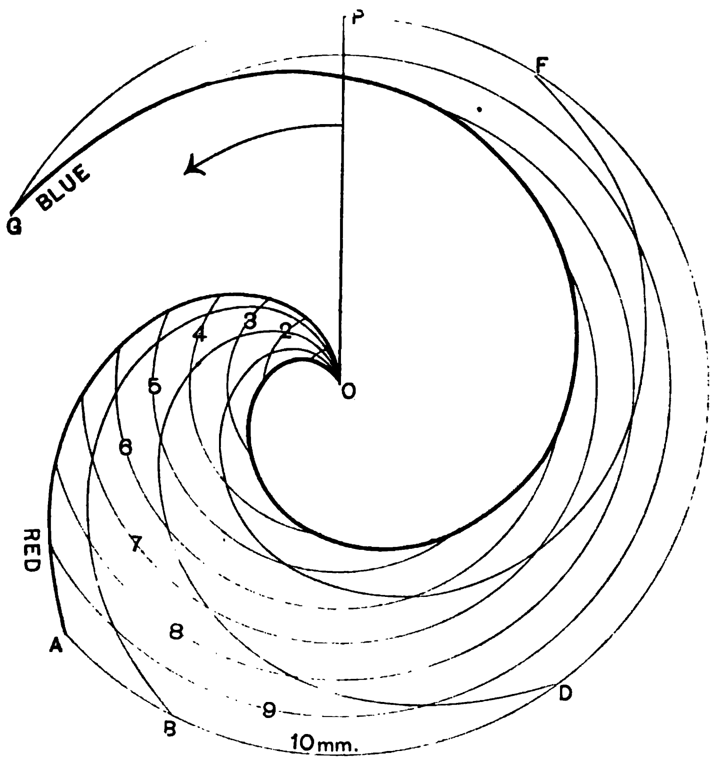


Fig. 2.

A reference to Fig. 2 will show this relation between angle of rotation, wave-length, and current. Several spirals are shown, corresponding to the several lines of the spectrum, known as A, B, D, F and G. The radii of the circles which intersect these spirals are proportional to the current flowing in the circuit, while the angle, which the radius, drawn to any point of intersection, makes with O P, represents the rotation for that particular wave-length and current. The spiral $O\ A$ is in the extreme red, and $O\ G$ in the violet of the spectrum, and the diagram thus indicates that the red rays are not rotated so much as the blue. The direction of rotation in the diagram is as indicated by the arrow. Now, returning to the observer looking through the analyzer, if he could resolve the light there seen into the pure colors of the spectrum, what he should expect would be, with no current, a complete spectrum, since *all* rays are rotated by the current. But let him rotate the analyzer, and he finds that first one color and then another disappears, and a *dark band* is seen to move across the spectrum as he rotates the analyzer. Again, let him rotate the analyzer to a certain angle and leave it there while he varies the current. He should expect that the band would move, but would vanish entirely with zero current, and thus prevent observation for small currents.

Fortunately we have substances which naturally rotate a beam of polarized light, for by means of this aid we may obviate the difficulty that the band vanishes with no current. For instance, a parallel plate cut from a crystal of quartz perpendicular to the optic axis has this property of rotating the plane of polarization. Quartz is selected for the material used because of its great transparency and high specific rotary power. The law of the rotation is similar to that already mentioned for the rotation by the current. The approximate law is, that the rotation is inversely as the square of the wave-length, which may be expressed:—

$$\varphi = c_3\, e \;/\; \lambda_2, \qquad (7)$$

where φ is the angle of rotation for the wave-length λ, c_3 is a constant, and e is the thickness of the plate. The thickness of the quartz plate is seen to correspond to the current in equation (5). Fig. 2 represents the actual rotation for different thicknesses of quartz, each circle corresponding to a plate one millimetre thick. The equivalent of a quartz plate one millimetre thick is represented approximately by 35,700 ampere-turns wound upon a tube containing carbon bisulphide. This latter is the substance used, being selected on account of its high transparency and specific rotation.

If a quartz plate be placed between polarizer and analyzer, the effect is the same as if the current circulated around the tube of carbon bisulphide, and we may, by rotating the analyzer, move the dark band completely across the spectrum by means of the quartz plate without any current. But suppose we set the analyzer so that the dark band remains in the center of the spectrum, and then pass a current through the coil. We observe a motion of this dark band back and forth through the spectrum as the current is repeatedly reversed. For any given current its position is always the same, so that its motion may be calibrated by passing known currents through the coil. Have we not in this, found a weightless vibrator that is sure to move in unison with the currents, if the term is allowed?

Before passing on to the more practical side of the question, it may be asked, will this band move back and forth so that its displacement is approximately proportional to the current? The answer to this question lies so near at hand that your attention is invited to it for a moment. The rotation of the plane by quartz is approximately represented by the formula:—

$$\varphi = c_3\, e / \lambda^2$$

The rotation by the current is represented by

$$\theta = c_2\, i / \lambda^2.$$

If both these rotations take place together, the resultant is merely the sum of the components, and

$$\chi = \varphi + \theta = (c_3\, e + c_2\, i)\, \frac{1}{\lambda^2}. \tag{8}$$

The position of the dark band depends upon the position of the analyzer. Let the analyzer be set at some convenient angle, a, with the position of complete darkness, and let it remain there. Then the wave-length or color where the dark band occurs for the quartz plate (being called λ_0) is given by (7) above, and we have

$$a = c_3\, \frac{e}{\lambda_0^2} \tag{9}$$

The wave-length corresponding to this constant angle, a, when both quartz and current are used, is given by (8), and we have

$$a = (c_3\, e + c_2\, i)\, \frac{1}{\lambda^2}, \tag{10}$$

in which λ is that wave-length corresponding to a certain current, i, and therefore i and λ are co-ordinate variables. Equating the values of a, we have

$$\frac{c_3\, e}{\lambda_0^2} = \frac{c_3\, e + c_2\, i}{\lambda^2} \tag{11}$$

This may be written

$$\lambda^2 = \frac{c_2 \lambda_0^2}{c_3 e} i + \lambda_0^2, \qquad (12)$$

and in this form the relation between wave-length and current is seen to be represented by a parabola. In Fig. 3 are represented two sets of parabolas obtained from equation (12) by assuming that λ_0 and e take in succession different values. When $e = 2$ mm. the set of parabolas marked I is obtained, and where $e = 6$ mm. set II is obtained. By giving λ different values we merely vary the parameter of the parabola without changing the origin. It will be remembered that λ_0 represents that wave-length corres-

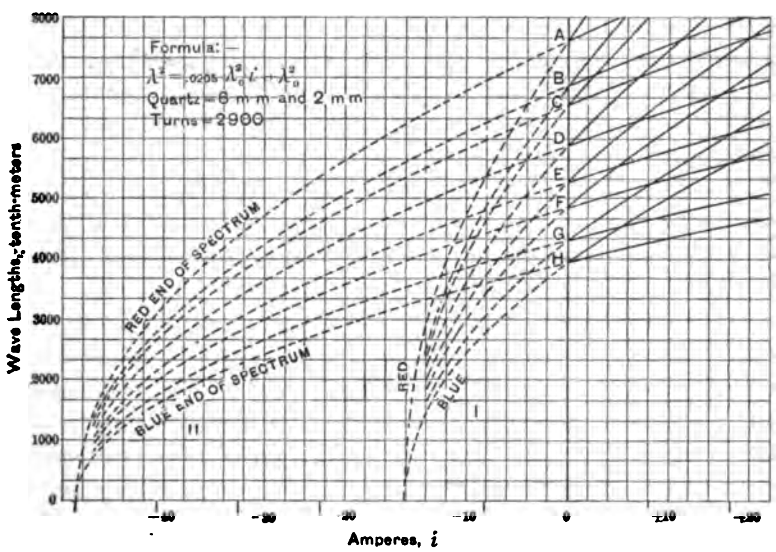


FIG. 3.

ponding to the positions of the dark band for no current. It is therefore the value of λ when i is equal to zero, as appears from the equation independently. The axis of λ is the vertical line to the right of the figure, upon which the letters A, B, etc., are written. These letters show the positions of the various Fraunhofer lines, and one parabola is drawn for each line. Each parabola then corresponds to one setting of the analyzer and the dark band is found at these lines of the spectrum for zero current. The upper parabola is at the red end, and the lower at the blue end, of the spectrum. The axis of current is the base line of the diagram, and currents to the left of the vertical line are called negative, while those to the right are called positive. The axes of all the parabolas coincide with each other and with that of the current, and for a given quartz plate they all intersect this axis at the same point, so that taking different settings of the polarizer is equivalent to changing the parameter only of the parabola.

The interpretation of these results may be put as follows: If we have a spectrum in which the wave-lengths are proportional to the distances along the spectrum (which is the case with Professor Rowland's arrangement of a concave grating), then the displacement of the dark band to one side or the other, due to the current, will be exactly according to the shape of these parabolas near the zero point; that is, near the vertical line lettered A, B, etc. Since it appears that each parabola at such a great distance from the origin is nearly a straight line, the displacement in such a spectrum will be nearly proportional to the current.

A noticeable feature, easily revealed by the graphical construction, is that in the red end of the spectrum, where the inclination of the parabola to the i axis is the greatest, the motion of the band will be the greatest for a given current.

Of course it is understood that this construction has to do with the relation between the wave length and the current, and not between the displacement and current, unless the wave-length and displacement are proportional. It does not apply, for instance, to the displacement in the spectrum of most prisms. In the prisms used, the red rays were so crowded together that the motion as observed was nearly the same in the red as in the blue. The width of the band, however, is for this reason narrower in the red than in the blue—a consideration of considerable practical importance.

Description of Some of the Apparatus.

The tube upon which the coil carrying the current was wound, was a glass tube 1.4 cm. internal, and 1.8 cm. external, diameter, and 70.15 cm. long. The tube was filled with carbon bisulphide, which was confined in the tube by means of two plane parallel plates of glass, each 1.3 cm. thick, fitted tightly upon the ground ends of the tube. Upon this tube was wound six layers of No. 18 double cotton copper magnet wire, occupying a length on the tube of 61.5 cm. The wire was wound so that 100 turns occupied 12.7 cm. Thus the total number of turns, 2,900, is very large considering the size of wire.

The light used was sunlight reflected from the mirror of the heliostat.

The Nicol prisms are two fine specimens which were obtained by Dartmouth College at a time when larger specimens could be obtained than may now easily be found. The slit does not need to be very narrow. A width of a quarter to a half-millimetre will do better than a narrower one, because more light is admitted to the photographic plate, and in passing through so many different substances even sunlight is rendered comparatively feeble by the time it strikes the photographic plate.

A further description of the apparatus is hardly deemed to be necessary, inasmuch as no claim is made to having obtained more than the most crude of first results, which may be the results obtained by apparatus arranged in a comparatively poor manner for the end sought. Yet the results obtained seem to be so promising for the future, that the subject is presented to you at this early date in the experiment, in the hope that it may soon receive an impetus from other experimenters who have better facilities than those at my disposal, and thus become a fruitful source of extending our knowledge of instantaneous current flow in conductors.

The objections which most naturally suggest themselves against this method of taking current curves are perhaps the following. The photographic plate must move so quickly that the time of exposure of any one part of the plate is extremely short. To meet this demand the most sensitive plates that can be made should be used. The width of the band with any given plate depends largely upon the time of exposure. Then, too, a plate is to be desired that will photograph toward the red end of the spectrum as well as in the blue. The band does not possess very sharp outlines, but gradually shades off from dark to light.

These objections do not have so much weight, however, in cases where the general direction of the variation of the current is what is wanted, more than any exact measurement of its amount, and in the majority of cases this is really what is required, yet it cannot be said that in these preliminary experiments the band used was nearly as sharp as may be obtained.

Another objection of a different nature that seems difficult to avoid is the fact that the coil, which is wound upon the tube, must necessarily possess a small amount of self-induction. It may be said, however, that even though we are prohibited from measuring certain currents on account of this self-induction, we are always sure that we are measuring the exact current which is flowing through the coil.

A More Exact Expression of the Relation between the Wave-Length and Verdet's Constant.

The approximate relation between the wave-length and Verdet's constant used above was that Verdet's constant varied inversely as the square of the wave-length. It is considered of sufficient interest to inquire just how nearly this is an approximate formula.

By reference to equation (2) it is evident that if we only knew the relation between the index of refraction and the wave-length, we might obtain the relation between the wave-length and Verdet's constant in terms of these two quantities alone and constants.

Such a relation is afforded by Briot's formula, which is a modification and improvement upon the well-known formula of Cauchy. This is

$$1/n^2 = k\lambda^2 + A + B/\lambda^2 + C/\lambda^4 + \dots, \qquad (14)$$

where n is the index of refraction corresponding to the wave-length λ, and k, A, B, etc., are constants for the given substance. Assuming that all terms beyond B/λ^2 are negligible, we may differentiate with respect to λ and obtain the equation

$$\frac{\lambda}{n}\frac{dn}{d\lambda} = n^2\left(\frac{B}{\lambda^2} - k\lambda^2\right). \qquad (15)$$

Upon eliminating B/λ^2 between (14) and (15), we obtain

$$\frac{\lambda}{n}\frac{dn}{d\lambda} = 1 - 2kn^2\lambda^2 - An^2. \qquad (16)$$

Substituting in (2) the expression thus obtained, we have

$$v = cn^5(2k + A/\lambda^2). \qquad (17)$$

But by (14)

$$n^5 = (k\lambda^2 + A + B/\lambda^2)^{-\frac{5}{2}}. \qquad (18)$$

Hence

$$v = c(2k + A/\lambda^2)(k\lambda^2 + A + B/\lambda^2)^{-\frac{5}{2}}. \qquad (19)$$

This formula represents to a high degree of accuracy the observed values of Verdet's constant for carbon bisulphide. It probably would for any other substances, but carbon bisulphide is the only one to which it has been applied by me. The constants k, A, and B may be found by means of equation (14) and the observed values of the refractive index for known wave-lengths. The values used are those observed by Messrs. Gladstone and Dale. (See Glazebrook's Physical Optics, page 243.)

Line of spectrum.	Index of refraction for carbon bisulphide.	Line of spectrum.	Index of refraction for carbon bisulphide.
A 7621	1.6142	E 5270	1.6465
B 6870	1.6207	F 4861	1.6584
C 6563	1.6240	G 4308	1.6836
D 5893	1.6333	H 3969	1.7090

These values give curve I, in Fig. 4. The lines A, F, and

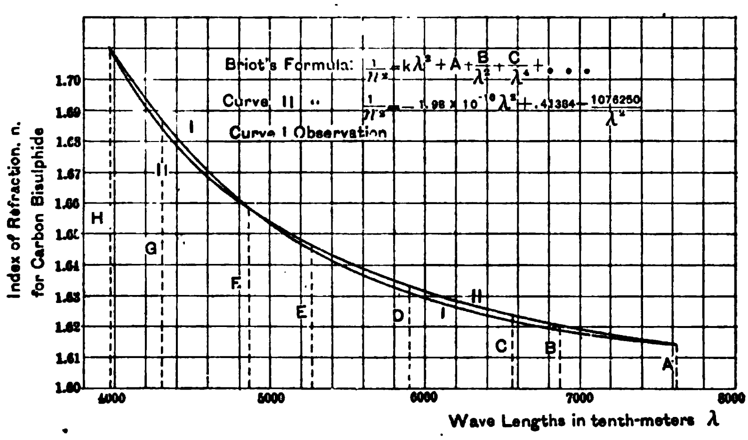


FIG. 4.

H were selected and three simultaneous equations formed from (14), so that the resulting curve II should pass through these three observed points.

The values

$$k = -1.98 \times 10^{-10}, \qquad (20)$$

$$A = .41384, \qquad (21)$$

$$B = 1076250, \qquad (22)$$

were obtained by the determinant solution of these three simultaneous equations. The resulting black curve may not appear to coincide very closely with the red, but it must be remembered that the origin of coördinates is a long distance below the paper, and the apparent differences are but a very small fraction of the whole. The unit is the tenth metre for wave-lengths.

Having these constants, they may now be substituted in (19),

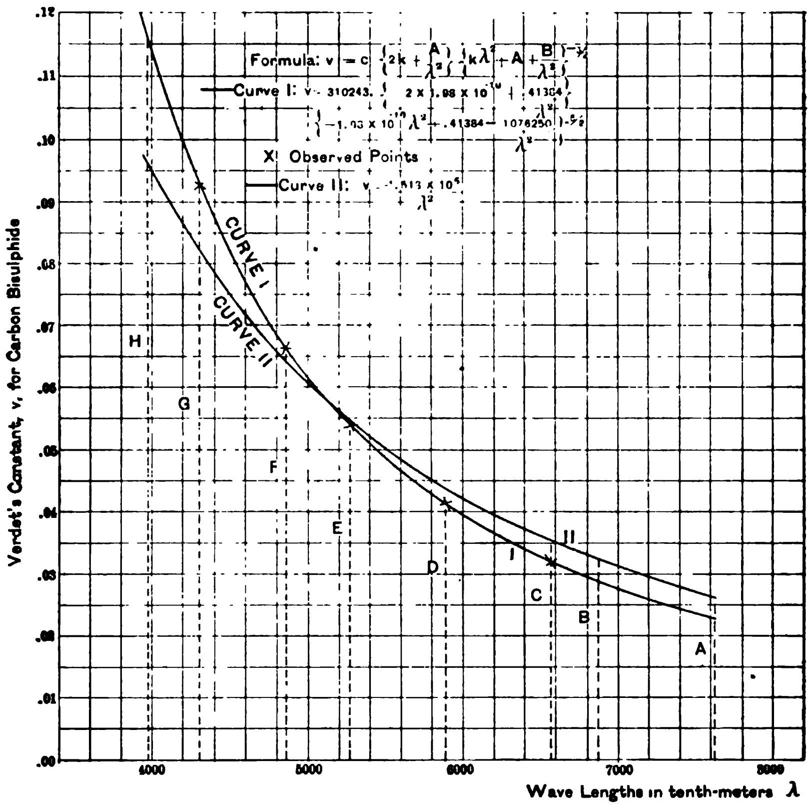


FIG. 5.

and c determined from the observed values of Verdet's constant. These observed values are:

Line of Spectrum. (λ)	Verdet's Constant for Carbon Bisulphide. (v)
C 6563	ᴦ.0319'
D 5893	0.0415'
E 5270	0.0537'
F 4861	0 0667'
G 4308	0.0920'

The constant thus determined, where a minute is the unit of angle, gives

$$c = 310243. \tag{23}$$

Using these constants for equation (19), we obtain curve I, in Fig. 5. The points marked × are observed values, and the calculated curve practically passes through them all.

Curve II in this diagram represents the approximate law that Verdet's constant is inversely as the square of the wave-length, and the degree of the approximation may be observed.

DISCUSSION.

MR. STEINMETZ:—I have listened to this important paper with a very great interest. As you remember, some years ago Dr. Fröhlich, of Germany, proposed a method of taking indicator cards of alternating currents. His method, however, had the serious defect of all former methods to employ moving parts. Now here, in this method, we see a beam of light, which has no inertia whatever, trace the picture of the alternating current, so that we can expect here to get a true picture of the alternating current wave or any other electric current, not by a series of complicated and tedious instantaneous readings, but with almost the same ease as a steam engine indicator card. This, I think, is a very important step in advance. For practical use, it is of great importance to get the values—the amplitude of oscillation of the black band in the spectrum, proportional to the intensity of the current, so that the photograph need not be reduced. The use of the glass prism gives the amplitude, in the red, larger than in the violet part of spectrum. However, this is not unavoidable in this method, because by replacing the glass prism by a refraction grating, at the expense of a large amount of light, indeed, you can get proportionality between the current and the amplitude of the black band. So it is a question of the intensity of the picture against proportionality to the current.

It is especially interesting to note from the parabolic equation, that is the dependence of the current on the amplitude of oscillations of the black band, that that part of the parabola which is within the range of the spectrum, is very nearly straight, that is to say, the motion of the black band in the spectrum is almost proportional to the intensity of currents. All this goes to make the tracing of the instrument the direct picture of the wave, and not merely a picture which has to be reduced to get the exact value. Though not so important for the physical laboratory, this is very important for the practical use of such an instrument. I think, therefore, that this method is exceedingly valuable, and will perhaps, give us one of the most important instruments of electrical engineering when worked out in detail, of the same usefulness as the steam engine indicator.

PROF. ANTHONY:—I do not care to spend time, at this late hour, in the discussion of the paper. I merely want to say that this has been an extremely interesting paper to me, and I believe that the ingenuity displayed in bringing out the results here is something that we ought fully to recognize. I believe that this instrument will be a most important instrument in the investigation of alternating currents.

[The meeting then adjourned for the day.]

The afternoon was devoted to a trip down the river, a visit being made to Cramps' shipyard. Gloucester, N. J. was next visited and after an excursion on the electric railway, and a visit to the power-house, a "planked-shad" dinner was served, through the courtesy of the Engineers and Manufacturers of Philadelphia, and under the management of the Sub-Committee on Entertainment. The party returned to Philadelphia by a special steamer up the Schuylkill river.

A paper presented at the Eleventh General Meeting of the American Institute of Electrical Engineers, Philadelphia, May 18th, 1894, President Houston in the Chair.

RESONANCE ANALYSIS OF ALTERNATING AND POLYPHASE CURRENTS.

BY M. I. PUPIN, PH. D.

I. Introduction.

The presence of upper harmonics in an alternating current wave is a fact which deserves careful consideration, both on account of the purely scientific interest which is attached to it, and also on account of the technical bearing of electrical resonance upon the construction of conductors possessing appreciable distributed capacity.

That alternating current and electromotive force waves of a great variety of forms can be produced by properly designing the pole-pieces of the field magnet, and the iron core of the armature of an alternator, is a fact nearly as old as the discovery of electro-magnetic induction. Fully as old is also the knowledge that a great variety of alternating current and electromotive force waves can be obtained by means of the induction of an intermittent current.

A careful investigation of these waves was first made more than forty years ago by Lenz[1] and Koosen,[2] who employed alternators with iron in the armature. They plotted these waves from the instantaneous values of current and electromotive force obtained by means of the now well-known revolving sliding contact. Employing the same method of investigation Joubert[3] showed in 1880 that the electromotive force wave obtained from an eight pole Siemens alternator without iron in the armature is very nearly a pure sine wave. The method is now known as *Jou-*

1. *Pogg Ann.* 76 p. 494, 1849; —92 p. 123, 1854.
2. *P gg Ann.* 87 p. 386, 1852.
3. *Comptes Rendus*, Vol. xci, p. 161, 1880 ; Ann. de l'ecole super. 10 p. 131, 1881.

bert's method of sliding contact. In 1888, Dr. L. Duncan[1] showed how successfully this method can be employed in the study of alternating current waves produced by commercial machines in actual operation. The same method was considerably elaborated by Professor H. J. Ryan[2] in an investigation of the action of transformers. The name, "*indicator diagram*," has been applied to wave curves of current and electromotive force obtained by Joubert's method, and very properly, I think, because they do very clearly indicate the action of alternating current apparatus. The process of taking these indicator diagrams has been shortened very much by Dr. L. Duncan's four dynamometer method.

Our knowledge of the action of alternating current apparatus has been extended considerably by these indicator diagrams.

For instance, we are now much more certain of the limitations which must be imposed upon the simple harmonic wave theory of alternating currents than we were a few years ago, and it looks very much as if progress in this direction, even more than in any other, meant progress towards a complete theory of the working of alternating current apparatus. Hence the desirability of as large a number of workers in this particular region of electrical research as possible.

There is no doubt that a simpler method would increase this number: for though much must be said in favor of the sliding contact method of obtaining indicator diagrams, yet it must also be acknowledged that the method is a very laborious and uninteresting process of investigation. A great many attempts have been made to devise some optical or some automatic method, but with little success. There is another reason why a new method of studying alternating current waves seems desirable. It is this: The method of sliding contact is not sufficiently sensitive to detect small deviations from a true sine wave, and consequently it is not capable of following up the causes of these deviations, when the effects seem to be absent. For instance, the primary current of a transformer can differ very much from a true sine form when the secondary circuit is open, but when a large current is flowing through an approximately non-self-inductive secondary circuit, then the primary can be made to differ inappreciably from a true sine wave. *The question arises now,*

1. See article by Duncan, Hutchinson and Wilkes in the *Electrical World*, March, 1888.
2. TRANSACTIONS, vol. vii, p. 1, Jan., 1890.

what has become of these causes when the secondary carries a heavy load?

This question is of deep scientific interest; it is also of considerable technical importance. For, if these causes are present at all loads, and only hidden by the principal wave, then, considering that these hidden small causes can produce large effects when conditions favoring resonance arise, it is evident that they must be carefully watched and guarded against in the construction of long lines possessing distributed capacity. I do not think that indicator diagrams obtained by the method of sliding contact are capable of giving a definite answer to this important question.

The method of analyzing alternating current waves by electrical resonance which I employed in the following investigation was suggested by me a year ago[1]. It is the object of this paper to describe this method at some length, and to illustrate, by some of the more definite results so far obtained, the simplicity, sensitiveness and reliability of the method. I shall also point out that this method of resonance analysis works quite satisfactorily even in those cases alluded to above, where the sliding contact method would, in all probability, fail.

II. Description of the Method.

Consider the following arrangement of circuits:—A non-self-inductive resistance *ab* Fig. 1*a* is inserted in the circuit of an alternator A and the primary B of a transformer. In shunt with *ab* is a circuit, *a*, *c*, *d*, *b*, consisting of an inertia coil, *c*, of a large number of turns of copper wire of low resistance, about 10 ohms, but containing no iron, and a condenser, *d*, divided into subdivisions ranging from .001 M. F. up. In shunt with the condenser, *d*, is an electrostatic voltmeter, *e*. The self-induction of the coil, *c*, can be varied by throwing a larger or smaller number of its sections into the circuit. The resistance can be varied by a rheostat, *f*. Suppose now that the self-induction of *c* is kept constant, and that the capacity of the condenser is gradually increased from zero up. Whenever a capacity has been reached which, with the self-induction of the circuit *a*, *c*, *d*, *f* *b*, *a*, produces resonance with one of the harmonics in the main circuit, then the resonant rise of potential will produce a large deflection in the voltmeter. In this manner all

1. M. I. Pupin, "Electrical Oscillations of Low Frequency and their Resonants,' *American Journal of Science*, vol. xlv., p. 429, May, 1893.

the harmonics which are present in the current of the main circuit can be detected in the course of a few minutes. If the *resonator circuit, a, c, d, f. b.* is placed in shunt with the non-self-inductive circuit (this circuit is denoted in Fig. 1*a* by a line beaded with asterisks and running from one pole of the alternator to the other) consisting of a bank of incandescent lamps, then the harmonics of the impressed electromotive force can be detected in the same manner. The ratio of the amplitudes of these harmonics to that of the fundamental can also be determined by this method, if desirable, provided the conditions of

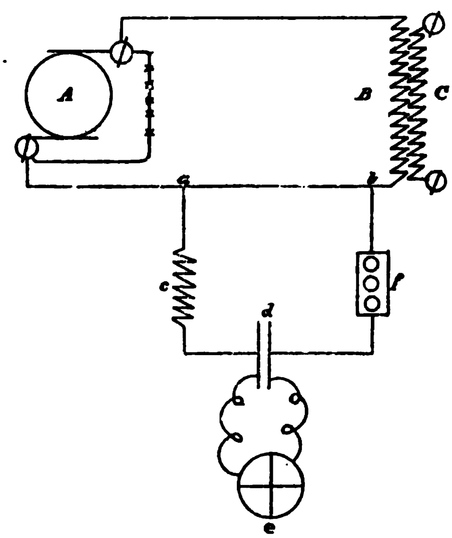


FIG. 1*a*.

the experiment are properly arranged. For let the current in the main circuit be

$$x = a_1 \sin pt + a_3 \sin 3\,pt + \ldots + a_\alpha \sin \alpha\, pt + \ldots$$

then the drop between a and b can be represented by

$$e = b_1 \sin pt + \ldots + b_\alpha \sin \alpha\, pt,$$

where $b_\alpha = a_\alpha\, r,$

and r = ohmic resistance between a and b.

Denoting now by

L the self-induction of the resonator $a\ c\ d\ f\ b\ a$.
R the resistance of the resonator $a\ c\ d\ f\ b\ a$.
C the capacity of the resonator $a\ c\ d\ f\ b\ a$,

then it can be easily shown[1] that the current in the resonator will be

$$y = \sum \frac{b_a}{\sqrt{a^2 p^2 \left(\frac{1}{a^2 p^2 C} - L\right)^2 + R^2}} \sin (a \text{ pt} - \varphi_a).$$

If, therefore, the capacity C is adjusted in such a way that—

$$\frac{1}{a^2 p^2 C} - L = 0,$$

then the circuit will be in resonance with the harmonic of frequency $a\,p$; and if L is sufficiently large and R sufficiently small (two conditions which are very easily fulfilled) then the current y is to within a small fraction of a per cent. given by

$$y = \frac{b_a}{R} \sin a \text{ pt}$$

The amplitude of the potential difference in the condenser which is measured by the voltmeter e is given by

$$P_a = \frac{a\,p\,L}{R} b_a.$$

In the same way we obtain for the fundamental frequency

$$P_1 = \frac{p\,L}{R} b_1.$$

Hence

$$\frac{P_a}{P_1} = a \frac{b_a}{b_1} = a \frac{a_a}{a_1}.$$

This gives the ratio of the amplitude a_a of the harmonic of frequency $a\,p$ to that of the fundamental. Let $a = 5$, then,

$$\tfrac{1}{5} \frac{P_5}{P_1} = \frac{b_5}{b_1} = \frac{a_5}{a_1}.$$

The voltmeter readings which give P_5 and P_1 magnify that ratio five times, in the case of the fifth harmonic, and it can be easily seen that a similar relation holds true for other harmonics. This is a very desirable feature of the method, considering that the amplitudes of the upper harmonics are generally small in comparison to the amplitude of the fundamental.

When quantitatively very accurate results are desired, then a low resistance, say one ohm, should be used for the section $a\ b$,

1. For further information see the authors's paper cited above.

and an electrometer capable of giving a large deflection for about ten volts.

The principal interest, however, in the study of the distortion of alternating current waves, is centered not so much in the exact ratio of the amplitudes of the harmonics to the amplitude of the fundamental wave, as it is in the causes producing these harmonics, and the conditions which modify the effects of these causes. Hence a quantitatively less accurate arrangement will do, provided that it is very sensitive, simple and easily manageable. Such an arrangement is given in Fig. 1*b*.

It differs from that given in Fig. 1*a* in the substitution of an air core transformer coil *a′ b′* for the non-self-inductive resistance *a b*. The secondary of this coil forms a part of the resonator

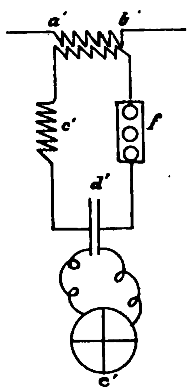


FIG. 1*b*.

circuit. For every harmonic of the inducing current we shall have a harmonic electromotive force of the same frequency in the resonant circuit. By varying the capacity in the resonator and watching the voltmeter needle, we can tell, by the deflection of the needle, whenever we have reached the capacity which, with the self-induction of the resonator brings this circuit into resonance with one of the harmonics. A reference to Fig. 2 will explain this more clearly.

In this figure the lower horizontal row of figures refers to the two-peaked curve, the upper row refers to the dotted flat-peaked curve. The vertical column denotes the voltmeter readings in volts. Consider now the two-peaked curve. It expresses the law of variation of the voltmeter readings when the capacity of the resonator circuit is varied from 0 to 2 microfarads, the self-induction being kept constant. The readings are recorded in Table I.

TABLE I.

Capacity in M. F.	Voltmeter readings in Volts.
.18	62
.181	68
.182	73.5
.183	79
.184	89
.185	96
.186	104
.187	110
.188	120
.189	126
.190	127
.191	125
.194	99
.198	71
.202	Very low
1.65	69
1.70	89
1.75	120
1.80	146
1.808	146
1.817	145
1.897	96
1.976	60

The voltmeter employed in all these experiments was a Sir William Thomson's multicellular voltmeter with a range from 60 to 240 volts. The curve was obtained from a 10 H. P. Fort Wayne eight-pole alternator with smooth core armature feeding a 5 K. W. Stanley transformer (closed magnetic circuit), the secondary being open. It is seen that resonance took place at .190 M. F. and 1.8 M. F. The capacity of the inertia coil c' Fig. 1*b*, and of the voltmeter as gathered from all experimental data was about .011 M.F., so that the real capacities at which resonance took place were .201 M. F. and 1.81 M. F., that is, in a ratio to each other as $1:3^2$. It will be seen, however, that a very accurate knowledge of capacity is not required in the following experiments.

The frequencies detected by the two-peaked curve, which I shall call the *resonance diagram*, were the fundamental and the first odd harmonic, that is, the harmonic of three times the frequency of the fundamental. The resonance diagram has, of course, as many peaks as there are frequencies in the inducing current.[1] The dotted curve (flat-peaked) in Fig 2 was plotted on an enlarged scale from the readings taken in detecting the first harmonic, represented by the sharp peak of the resonance diagram, and represents this peak spread out, so as to show how the various readings fit into a well defined and symmetrical curve such as required by theory. It also shows that a condenser of small sub-divisions must be employed in order to detect higher harmonics. But it should be observed that these higher harmonics can also be detected with coarser sub-divisions, provided the self-induction of the inertia coil is made small enough.

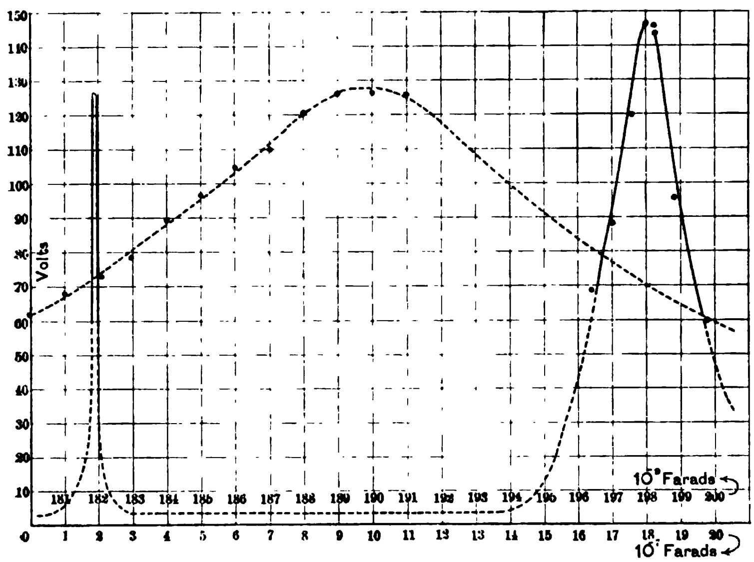


Fig. 2.

Description of Experiments.

The *resonance diagram* obtained by the method of Fig. 1 *b* gives the number of harmonics which are present in the inducing current. It does not give the exact value of the amplitudes of these harmonics. It would be somewhat premature to discuss the theory of the resonance diagram obtained by this arrange-

1. I have never detected an even harmonic in alternating current waves produced by ordinary commercial alternating current apparatus, and conclude, therefore, that such harmonics do not exist there. In machines of perfectly symmetrical construction, even harmonics should of course not appear, as observed some time ago by Prof. Ayrton. Alternators with slotted armatures give waves in which all the odd harmonics up to the harmonic of nine times the frequency of the fundamental can be easily detected. As a rule the first odd harmonic is the strongest.

ment, and to show how the ratio of the amplitudes of the harmonics to that of the fundamental frequency of the inducing current, that is the exact color of this current, could be calculated from the ratio of the height of the peaks in the resonance diagram. Suffice it, for the present, to mention only that the peaks of this diagram represent the amplitudes of the harmonics, magnified about proportionally to the square of the frequency. For instance, the resonance diagram in Fig. 2 tells us that the amplitude of the first odd harmonic in the inducing current is about one-ninth of the amplitude of the fundamental. *The determi-*

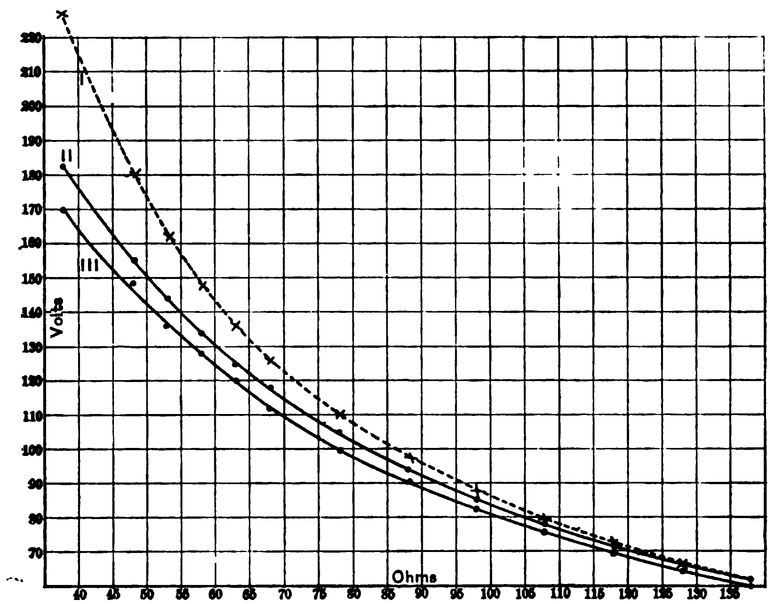


FIG. 3.

nation of the exact value of this ratio was not the object of the following experiments. Their aim was to detect the presence of harmonics, to trace their origin and to study their variation with the variation of the load, and of other elements of the circuit on which these harmonics seem to depend.

Preliminary Tests.—In order to form an estimate in how far the experimental data obtained by the arrangement of Fig. 1*b* agreed with the theory, the following tests were applied.

a. Study of the damping effect of the dielectric in the condenser.

Let L = self-induction of the resonator circuit.

Let R = resistance of the resonator circuit.

Let P = amplitude of the difference of potential in the condenser when point of resonance has been reached.

Let E = amplitude of impressed electromotive force in the resonance circuit:

then according to theory

$$P = \frac{p L}{R} E.$$

Hence if the R is varied, P will vary also, but in such a way that

$$P R = \text{constant}.$$

That is to say, if we vary the resistance of a resonant circuit, and tabulate the voltmeter deflection for every particular resistance, and then plot a curve taking the resistances for abscissæ, and the voltmeter readings for ordinates we should, according to theory, obtain an equilateral hyperbola. Curves II and III, Fig. 3, were obtained that way, the frequency employed was that of the 10 H. P. alternator, that is, 130 periods per second.

TABLE II.

Resistance in Ohms.	Voltmeter Reading with a Mica Condenser.	Voltmeter Reading with a Paraffin Condenser.	Theoretical Value of Voltmeter Reading.
38	183	170	225.6
48	155	148	178.9
53	144	137	161.8
58	134	128	147.8
63	125	120	136
68	118	113	126
78	105	101	110
88	94	91	97.4
98	85	83	87.5
108	78	76	79.4
118	72.5	70	72.6
128	67	65	65.5
138	62	60	60

The experimental data from which they were plotted are given in Table II. Curve II was plotted from voltmeter readings obtained with a mica condenser. Curve III represents the corresponding readings obtained with a paraffin condenser and given in the third column of Table II. Curve I represents the theoretical curve, which would have been obtained if the law of variation of the voltmeter readings with the resistance had been the same throughout as it was at low readings. On account of the damping effect due to dielectric viscosity in the condenser, a deviation from the above mentioned hyperbolic relation was, of course, expected, but it was quite a pleasant surprise to find a perfect regularity of these deviations. These curves indicate a rapid increase in the dielectric damping with the voltage, and also the superiority of mica to paraffin, especially at higher voltages. They also suggest that at low voltages and at frequencies over a hundred periods per second, this difference between the two substances is inappreciable. Similarly the damping effect of the magnetic viscosity of iron is small at low magnetizations, such, for instance, as would be produced by a telephonic current, and at frequencies which are well within the range of higher telephonic frequencies, say 750 periods per second. It is well to point out here that electrical resonance

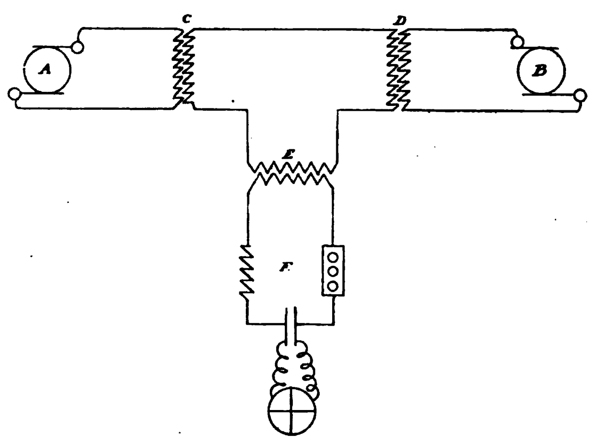


FIG. 4.

offers a very convenient method for studying the viscosity of iron and of dielectrics.

Similar curves and similar results were obtained with higher harmonics. This experimental test shows, therefore, that the relative values of the amplitudes of the harmonics to that of the fundamental frequencies are not seriously distorted by the dielectric damping of the condensers, especially when one operates with moderate voltages as was the case in the following experiments.

(*b*). *Second Test of the Resonator Indications.*—This test is represented graphically by diagram Fig. 4. Two transformers C and D had their secondaries connected in series. The primary of the air-core transformer E formed a part of their circuit. The secondary of this transformer was a part of the resonator F. The transformer C, a Stanley 5 K. W. (closed magnetic circuit) was fed by the 10 H. P. alternator mentioned above (130 P. P. S.), the transformer D, of induction coil type with a cylindrical core, of fine iron wire, was fed by a 1 H. P. alternator with slotted armature (278 P. P. S.). Both alternators were run simultaneously at full excitation. First, the primary circuit of the large alternator was broken so that the current in the circuit O D E was due to the action of the small machine alone. The resonator detected a resonant rise of 240 volts at capacity .407 M. F., and another of 150 volts at capacity .044 M. F. These were evidently the fundamental and the first odd harmonic. Then the circuit of the small machine was broken and that of the large machine closed, so that the current in the resonator was due to the action of the large machine alone. The resonator detected a resonant rise of 220 volts at capacity 1.78 M. F. This corresponded to the fundamental frequency (130 P. P. S.) of the large machines. Finally both circuits were closed, so that the current in the resonator was due to the simultaneous action of the two machines. *The same resonant rises of potential were detected by the resonator and at the same capacities as before, in perfect agreement with theory.*

This experiment afforded another opportunity of testing the theory which underlies this resonance method of studying the wave curves of current and electromotive force. It is this: If two or more electromotive forces of different frequencies are impressed upon the resonator circuit and their resonant rises of potential are determined for a given resistance in this circuit, then, according to theory, the ratio of these rises should remain the same for all other resistances within the limits within which the periodicity of the circuit is practically independent of the ohmic resistance. Accordingly, the resistance of the resonator, F., Fig. 4, was varied gradually from 100 to 250 ohms, and the resonant rises of potential produced by the fundamental frequencies of the two machines (130 and 278, P. P. S.) were carefully determined for each particular resistance. The ratio of these rises remained constant to within five per cent., but the deviations were now in one direction and now in the other, and they were undoubtedly due to the variation in the excitation, and the speed of the small machine, both of which depended on the potential of the electric mains of the College plant, which, of course, could not be kept very constant for so long an interval of time as is necessary for this experiment, which was about 15 minutes.

It is interesting to observe here that three air-core transformers like E, with three resonators like F, when placed in the circuit, C D E, and each resonator tuned to one of the frequencies of the circuit represent an exact analogy of Helmholtz's well-known arrangement of accoustical resonators which he employed in the analysis of vowel sounds. The variety of exceedingly instructive experiments which one can perform with such a multiplex resonance circuit is very large and most interesting. It has long formed my favorite subject for an extended series of experiments whose results, however, are beyond the limits of this paper. In all experiments of this kind the voltmeter, which is attached to each resonator, performs the same office as König's sensitive manometric flames in the well-known experiments on acoustical resonance, and, it should be noted here, that they are just as sensitive. In fact, in electrical resonance experiments one is continually impressed with the striking resemblance between resonance phenomena in electricity on one hand, and those in sound on the other. This resemblance is a trusty and suggestive guide.

(*c*). *Sympathetic Resonance.*—Now, an acoustical resonator tuned to a certain pitch will respond feebly, to be sure, but distinctly, to a simple sound of a frequency which is an exact submultiple of its own frequency. A similar phenomenon might exist in electrical resonance, though ordinary alternating current theory does not lead us to expect anything of the kind. But some experimental results, which will be given below, led me to suspect that a sort of *sympathetic resonance* might exist, that is to say, a simple harmonic current might perhaps be capable of producing by induction a resonant rise of potential in a resonator, which is tuned to a frequency which is an exact multiple of the frequency of the inducing current. If that were the case, then a resonant rise of potential in a resonator would not necessarily mean that an upper harmonic in the inducing current has been detected, and, therefore, the indications of an electric resonator might be misleading. To investigate this point the air-core transformer E with the resonator F, Fig. 4, was connected to the secondary of the 5 K. W. transformer, and the current gradually increased by varying gradually an electrolyte resistance. The feeding machine was the above-mentioned 10 H. P. alternator, with smooth core armature. Here I must disturb somewhat the logical sequence of this paper by stating that the secondary current, produced by this machine and transformer, when flowing through a non-self-inductive resistance in the external circuit or through a coil of small self-inductance possessing no iron core, showed under ordinary tests no distortion worth mentioning. But when the secondary current was increased, and with it the electromotive force induced in the resonator, the harmonics began to appear more and more distinctly. When the secondary current was 11.5 amperes and the electromotive force induced in the resonator was 133 volts, the resonant rise of potential was as follows:

For the harmonic of:

3	times the frequency	of the fundamental	the potential	rose	from	133 to 155 volts.	
5	"	"	"	"	"	133 to 139	"
7	"	"	"	"	"	faintly.	
9	"	"	"	"	"	not perceptibly.	

The resonant rise with the fundamental frequency would have been with the initial potential of 133 volts several thousand volts, so that the resonant rises obtained for the upper harmonics were extremely small in comparison. The conclusion is, therefore, that if such a thing as sympathetic resonance really exists, it is so feeble at small initial voltages in the resonator as to escape detection. It could, therefore, in no way modify the results recorded in this paper, since these were obtained almost invariably with resonant rises of potential which started from very small initial voltage.

These preliminary experimental tests demonstrate clearly that a resonator of the type given in Fig 1*b* is quite capable of detecting all the frequencies that may exist in an alternating current wave, that its indications are in good agreement with the theory as far as the fundamental frequency is concerned, and that it gives us a fairly approximate idea of the relative strength of the harmonics. Additional evidences proving the correctness of its indications will be found among the results of the following experiments.

IV. Location of the Origin of Upper Harmonics.

A. EXPERIMENTS WITH ALTERNATOR OF SMOOTH CORE ARMATURE.

1*st Series.*—The first set of experiments in this direction was performed with the 10 H. P. Fort Wayne 8-pole alternator with smooth core armature, and the Stanley 5 K. W. transformer (closed magnetic circuit). The secondary circuit was open and a

Cardew voltmeter indicated the secondary voltage. The current which excited the field of the alternator was gradually increased. The secondary voltage measured the strength of this excitation. The air-core transformer with the resonator was inserted into the primary circuit as indicated in Fig. 1*b*. The resonant rise of potential, recorded by the multicellular voltmeter e', was carefully determined at every excitation for the fundamental frequency and the first odd harmonic. Higher harmonics were present, but very faint. The results are given in Table IV. and plotted in Fig. 5. The initial voltage in the resonant circuit was small, just perceptible in the multicellular voltmeter.

TABLE IV.

Secondary Voltage.	Resonant Rise in Volts Due to the Fundamental.	Resonant Rise in Volts Due to the First Odd Harmonic.
43	122	58
48	130	65
53.5	136	72
56	138	73
62	146	80.5
66.75	152	86
75	160	94
83	170	104
88	175	110
97	185	117
104	195	128.5

The curves in Fig. 5 were plotted from this table by taking the readings of the first column for the abscissae and the corresponding readings of the second and third columns for ordinates. The upper curve corresponds to the fundamental and the lower curve to the harmonic. *The two curves are two straight lines parallel to each other*, which means that the fundamental and the harmonic increase at the same rate from nearly one third excitation to full excitation of the alternator. This result I did not expect, but its correctness was verified beyond all reasonable doubt.

The same series of experiments was extended to lower excitations of the alternator, but, since I had no low reading alternating current voltmeter, the excitation was measured by measuring the exciting field current. This current was 10 amperes at full excitation and the series of experiments extended down to 1.5 amperes, hence to nearly one-seventh of the full excitation. To bring the readings of the resonant rises of potential within the scale of the multicellular voltmeter at low excitations the number of turns in the air-core transformer was suitably increased. Within all these limits of excitation, both the fundamental and the harmonic increased at the same rate and proportionally to the excitation. This curious relation I mistrusted at first and suspected the existence of something like sympathetic resonance mentioned above. But all experimental evidence is in favor of its correctness.

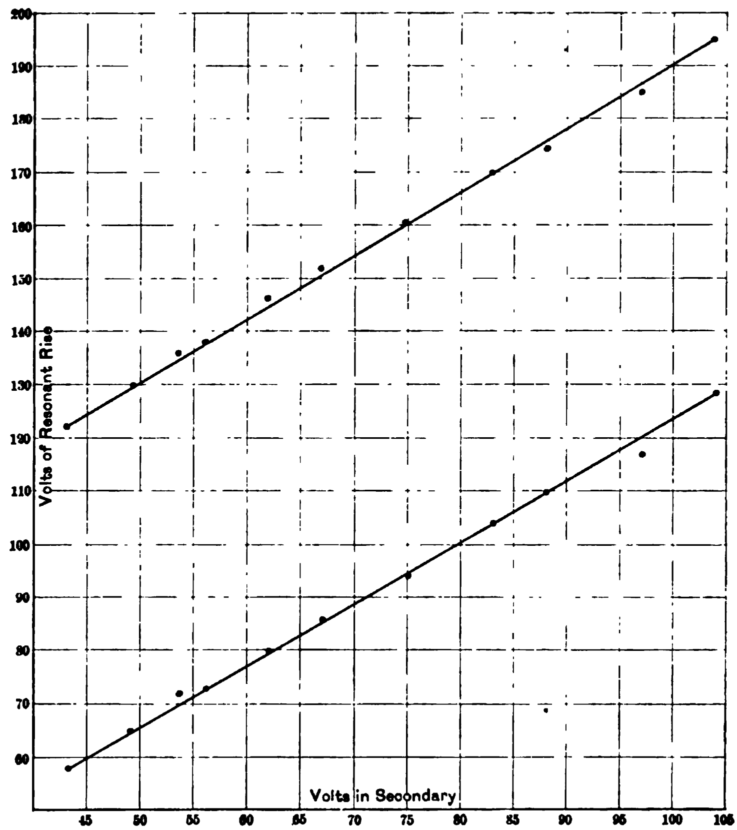


FIG. 5.

2*d. Series.*—To determine whether the presence of the harmonic was due to the action of the transformer or to that of the alternator, the transformer was disconnected, and two series of incandescent lamps, connected in parallel, were substituted in its place. Each series consisted of 13 lamps, each about 24 C. P. The resonator, with its air-core transformer, remained in circuit as before. First one series of lamps was placed in circuit. The rise due to the fundamental was stronger than in the preceding experiments, but that due to the harmonic was exceedingly faint. When both series of lamps were thrown in, the harmonic appeared a trifle stonger, but still very weak. Hence the inference that the harmonic was due almost exclusively to the action of the transformer.

It should be observed here that the alternator armature, though well laminated, runs fairly hot in a short time, hence it must be the seat of a decidedly strong hysteretic process. On the other hand, the transformer does not heat nearly as much as the alternator armature, and yet its action produces the harmonic. This certainly seems to speak strongly against the view that harmonics are due to hysteresis. Other evidences against this view will be given below.

3d Series.—A series of experiments with open magnetic circuit transformers of induction coil type in place of the lamps, showed the harmonic much stronger than the lamps did, but weaker than the experiments with the transformer of closed magnetic circuit. Although I have not succeeded in obtaining accurate numerical comparisons between the two types of transformers in this respect, one thing is certain, and that is, closed magnetic circuit transformers distort under similar conditions the primary current considerably more than transformers with open magnetic circuits; on the other hand, in the first case the distortion is confined almost entirely to the primary circuit, when the secondary is closed by a non-self-inductive resistance, whereas in the second case it is felt in the secondary circuit also, though considerably less than in the primary.

The general conclusion of this group of experiments may be summed up as follows:

I. A ferric self-inductance in circuit with an alternator which gives a simple harmonic electromotive force, distorts the current by introducing higher odd harmonics, principally the harmonic of three times the frequency of the fundamental.

II. This harmonic (and in all probability all other harmonics) increase at the same rate as the fundamental when the excitation increases, the rate of increase being up to 4000 C. G. S. lines of force per sq. cm. proportional to the intensity of magnetic induction in the core of the ferric inductance.

III. When this ferric induction is a transformer, then the distortion appears, but not seriously, in the induced secondary electromotive force; if the transformer has an open magnetic circuit, it does not appear there to any extent worth considering if the magnetic circuit is a closed one.

IV. A practically simple harmonic electromotive force is produced by alternators with smooth core armatures, even if the machine is worked at considerable degrees of magnetization of the armature core.

B. EXPERIMENTS WITH ALTERNATOR OF SLOTTED CORE ARMATURE TYPE.

The machine employed in these experiments was the 1 H. P. alternator mentioned above. It is a 16-pole machine; its armature is a Crocker-Wheeler 1 H. P. motor armature wound for 500 volts. It gives at full excitation, and the speed at which I usually ran it in these experiments about 1,500 volts.[1] The transformer connected with it was of induction coil type with a cylindrical iron core made up of very carefully insulated thin iron wire. The same series of experiments were performed as under group A. The first series in this group gave exactly the same results as the corresponding series in group A. The excitation varied from one-seventh of the full to the full excitation; the amplitude of the fundamental and the first odd harmonic[2] varied at the same rate during the whole interval, so that a parallel pair of straight lines like those in Fig. 5 could be plotted in this case also. The second series resulted in the conclusion that the harmonic was very strong and due in a very large measure to the action of the armature, and not to that of the transformer as in the other case, although the transformer, also, contributed a distinct but small measure to the strength of the harmonic. The third series showed that the harmonic appears in the secondary of an open magnetic circuit transformer, although considerably weaker, but does not appear there to any appreciable extent when the magnetic circuit is a closed one.

To the four conclusions given at the end of the series of experi-

1. A more complete description of this machine and the transformer will be found in *Amer. Jour. of Science*, June, 1893, p. 510, etc. Owing to the accident which somewhat impaired the insulation of the armature the machine was run last year at low excitation, and hence low voltage, although the speed was then considerably higher.

2. The second odd harmonic, that is the harmonic whose frequency is five times that of the fundamental, was there but weak.

ments under group A we may, therefore, add the following additional conclusions :—

V. An alternator with slotted core armature produces a complex harmonic electromotive force in which the upper harmonic of three times the frequency of the fundamental is generally by far the strongest.

VI. The amplitudes of the fundamental and the harmonic increase at the same rate with the increase of excitation ; this rate is proportional to the excitation, that is to say proportional to the magnetization of the armature.

VII. A ferric inductance in circuit with a slotted iron core armature introduces no new harmonics. It seems to strengthen those already existing in the electromotive force, that is odd harmonics, especially the first odd harmonic.

The same conclusions will evidently hold true for alternators of ordinary types, that is alternators whose armature is made up of coils wound on iron cores which are bolted to a cylindrical iron drum common to all of them.

V. Effect of the Load Upon the Harmonics.

It is a well-known fact that the distortion of the primary current disappears gradually with the increase of the secondary load, that is when the external part of the secondary circuit is a non-self-inductive resistance. The question arises now, what becomes of the harmonics which produce the distortion in the primary when the secondary current increases? The following experiments seem to answer this question definitely.

The arrangement of circuits was that given in Fig. 1 *b*. The secondary circuit of the large 5 K. W. transformer contained an electrolyte resistance and the secondary current was measured by means of a Siemens electro-dynamometer. For every particular value of the secondary current the resonant rises of potential due to the harmonic and the fundamental were carefully determined by means of the multicellular voltmeter. Table V. contains the observations relating to the harmonic of three times the frequency of the fundamental; Table VI. relates to the fundamental (130 P. P. S.). The apparatus employed was the large alternator and the 5 K. W. transformer.

Table VI. requires explanation. When the secondary current was over 3.6 amperes the resonant rise of the fundamental was too high for the voltmeter employed, and also too risky for the

TABLE V.

Secondary Current in Amperes.	Resonant Rise of the Harmonic in Volts.
0	65
3.6	65
4.8	66
6.9	68
8.5	70
11.5	76
15.7	85
20	97
28	120
40	162.5
56	202

TABLE VI.

Secondary Current in Amperes.	Resonant Rise of the Fundamental in Volts (observed).	Auxiliary Resistance in the Resonator in Ohms.	Resonant Rise of the Fundamental in Volts (calculated).
0	80	0	80
3.6	240	0	240
5.0	122	50	503
6.7	150	50	616
9.0	200	50	825
17.3	200	100	1,450
27.0	185	210	2,613
44.0	155	410	4,127
56.0	160	510	5,100

condenser. An auxiliary resistance had to be introduced into the resonator to bring the resonant rise down to the limits of the voltmeter. These auxiliary resistances are given in the third column. The readings that would have been obtained without these auxiliary resistances were then calculated, roughly, as follows:—According to theory which was verified by experiments described in the beginning of this paper, the resonant rise multiplied by the resistance of the resonator gives a constant product for all resistances as long as the period of the resonator is practically independent of these resistances. The resistance of the resonator coils was 16 ohms. Hence if, for instance, x denote the rise which would have been obtained without auxiliary resistance in the resonator when the secondary current was 5 amperes, then since with an auxiliary resistance of 50 ohms the resonant rise was 122 volts we have with a rough approximation.

$$x = \frac{122 \times 66}{16} = 503 \text{ volts.}$$

In this manner the figures of the fourth column were obtained. They are only very rough approximations, but still they give a very fair idea of the ratio of the fundamental to the upper harmonic at various loads. Curves I and II, Fig. 6 were plotted from these data. The secondary amperes were taken for the abscissæ and the corresponding resonant rises in volts for the ordinates. Curve III represents curve II plotted on a different scale for the

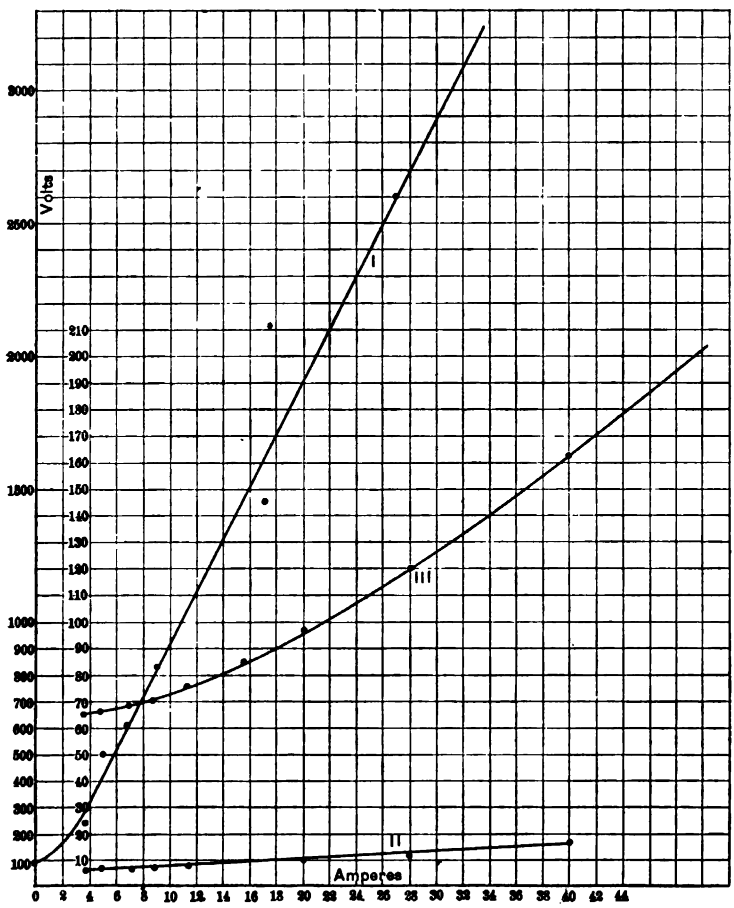


FIG. 6.

volts of the resonant rise of potential. These are given in the right hand vertical column of the diagram. This curve gives a better picture of the gradual *apparent* increase of the harmonic. An inspection of I and II shows clearly how much more rapidly the fundamental increases than the harmonic. *In reality the increase* *is even more rapid;* for according to Table V it appears as if the strength of the harmonic increased with the secondary current, only much less rapidly than the fundamental. For instance, at open secondary the voltmeter indicated 65 volts for the resonant rise of the fundamental; and at 56 amperes in the secondary this rise was indicated by 202 volts. *But it must be noted that in the first case the voltmeter needle went from practically zero at no resonance, to 62 when resonance was reached; whereas in the second case it went from 135 volts at no resonance to 202 volts when resonance was reached, so that the real resonant rise was practically the same in both cases.* Similarly for all other loads in the secondary. It follows, therefore, that if the harmonic increased at all with the increase of the load, this increase was much smaller than appears at first sight from the data of Table V. *The more important conclusion, however, which follows from this experiment, and which I wish to point out more particularly, is that the harmonic which manifests itself in the distortion of the primary current when there is no load in the secondary is present at all loads, if not stronger, then certainly with about the same strength. At full load this harmonic could not possibly be detected by Joubert's method of sliding contact; it is so exceedingly small in comparison to the fundamental.*

This persistence of harmonics at all loads even when completely hidden by the fundamental wave holds true also when their origin can be traced to the action of the armature of the generator as in the case of the machine with slotted iron core armature. In all cases their strength seems to depend upon the mean intensity of magnetization of the magnetic circuits to which they owe their origin and upon nothing else.

Another somewhat more difficult but very instructive way of proving the persistence of the harmonics is represented in Fig. 7. In circuit with the primary of the large machine and transformer described above are two equal air-core transformers, *a b* and *a′ b′*. By means of a double switch either one of the two can be made a part of the resonating circuit, *c*, *d*, *f*. A number of condensers, D, in series, are connected across primary circuit as indicated. The two air-core transformers, *a b*, and *a′ b′*, will be equivalent when the resonator voltmeter, *e*, gives the same indications, no matter which one of the two transformers be connected to the resonator. This balanced arrangement having been obtained, the balance will be disturbed as soon as the condenser, D, is plugged in, and it will be disturbed in a great variety of ways, according to the capacity plugged in. But when the transformer, B, is of closed magnetic circuit type, then the resonator indications remain practically the same as long as the resonator is switched on the air-core transformer a' b', no matter what capacity is plugged in the condenser, D. When the resonator is switched on the air-core transformer, a b, then its indications will be different for every particular capacity in D.

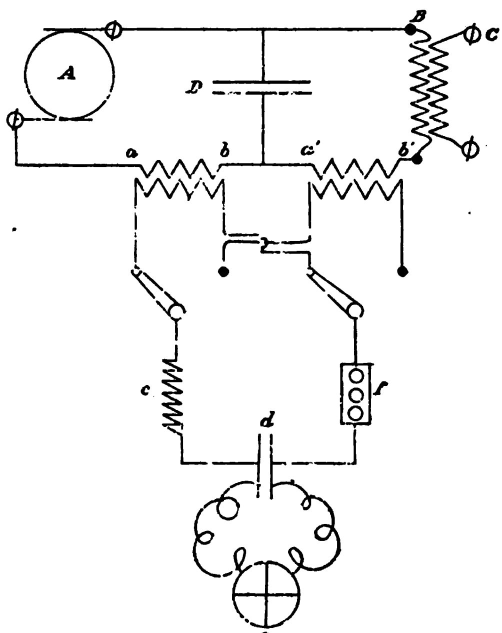


FIG. 7.

In fact the circuit A, a, b, D, A, can be treated as an entirely separate circuit from the circuit A, a', b', B, A.

This statement needs practically no modification in order to cover that case also in which the self-inductance of the primary of B is diminished by putting a non-self-inductive load on the secondary. This utter disagreement between theory and experiment deserves a closer discussion, but since its connection with the subject of this paper is only an indirect one I prefer to reserve it for some other time. That which has a direct bearing upon the present discussion is the method which the above mentioned relation offers for observing the variation of the harmonics with the load, without the disturbing inductive effect of the large primary current. It is this: Connect the air-core transformer *a b* (and with it the resonator,) in series with the condenser. Add to this series an auxiliary coil *c* (no iron core). By the combination, thus obtained, bridge the primary circuit, so that in place of the simple condenser bridge D given in Fig. 7 there will be a bridge consisting of condenser D, the air-core transformer *a b* and the auxiliary coil *c*. The office of the auxiliary coil will be explained presently. The secondary o being open, tune the circuit consisting of the alternator armature, the primary conductors up to the bridge, and the bridge, to any one of the harmonics. The tuning is done by means of varying the capacity of the condenser and the self-inductance of the auxiliary coil. Then close the secondary circuit by means of an electrolyte resistance and vary the secondary current. It will be found that the harmonic diminishes only slightly with the increase of the secondary load. As an example I give the following: The circuit just mentioned was tuned to the harmonic of five times the frequency of the fundamental, that is 650 P. P. S. At no load the resonator indicated a rise of 108 volts, at full load the rise was 94 volts. But this drop, small as it was, might have been caused by armature reaction.

Whatever the ultimate meaning of the appearance and the persistance of the odd harmonics in an alternating current wave may be, I am not quite prepared to state with any high degree of confidence. *One thing is certain, and that is that they are present at all loads with almost undiminished strength. Their presence is hidden by the fundamental wave at heavy loads, but when conditions favoring resonance with any one of them arise they will certainly come out and do all the mischief they can to the insulation.* The self-induction of a motor or that of a closed magnetic circuit transformer has practically no bearing upon the conditions of their resonance. These conditions depend in such circuits solely upon the self-induction of the alternator on the one hand, and the self-induction and static capacity of the line on the other. According to the experiments just described, the resonant current is confined entirely to the alternator and the line, the dielectric forming a part of its circuit. These observations will be modified in the case of transformers with open magnetic circuits and their equivalents, that is, closed magnetic circuits possessing considerable magnetic leakage, especially when the conditions of the line favor resonance with the fundamental frequency, this frequency being low; such magnetic circuits possess much less magnetic sluggishness and can influence considerably the conditions of resonance with a low frequency.

VI.—Distortion of the Secondary Current.

It was pointed out that the superposition of harmonics upon the fundamental wave was confined to the primary circuit when the secondary is closed by a non-self-inductive resistance, that is, if the transformer is of closed magnetic circuit type. With an open magnetic circuit transformer, the deviation of the primary current wave from the simple harmonic form, due to action of the generator or the transformer or both, is felt more or less in the secondary current also. If, however, the secondary is closed by a ferric self-inductance, then odd harmonics will appear in this circuit also in both types of transformers. In fact, the secondary circuit should now, as far as the harmonics are concerned, be considered as a separate circuit, in which the secondary coil of the transformer and the ferric inductance in the secondary circuit play the same part as the armature of the alternator and the transformer play in the primary circuit.

The series of experiments which related to the origin and growth of harmonics in the secondary circuit was exactly the same as the one described above, by means of which the so-called distortion of the primary current was studied. The results were the same. The presence of harmonics is due to the action of the ferric inductance; their strength increases proportionally to the intensity of magnetization of the iron in the ferric inductance. They seem to be entirely independent of hysteresis, that is, if by hysteresis the process be understood by means of which most of the heat is generated in a very finely laminated, well insulated and well annealed iron core, when such a core is subjected to rapid reversals of magnetism. I shall describe briefly an experiment bearing upon this point. The secondary circuit of the five K. W. transformer was closed by an electrolyte resistance, and a short cylindrical coil having about 120 turns coarse copper wire. A short cylindrical core made up of very fine (No. 26 B. and S.), and well annealed iron wire could be inserted into this coil. The core was 40 cm. high and 5 cm. in diameter. The wires were fairly well insulated from each other. A layer of fine copper wire surrounding this coil formed part of the resonator circuit. First, the secondary current was passed through the coil before the iron core was inserted. The resonator could detect no harmonic worth mentioning even when the current was increased almost to full load. But as soon as the iron core was introduced the odd harmonics appeared, especially the third harmonic; its strength increased proportionally to the current. Placing now another similar iron core on the top of the first, and adjusting it in such a way that it allowed a small rocking motion the two cores could be set into violent vibration by the inductive action of the current which gave a very loud note corresponding in pitch to the frequency of the alternator. The vibration could be stopped by pressing the top core against the lower core and against the table. The vibration produced no appreciable difference in the strength of the harmonic; if anything it seemed to make it stronger. This experiment seems to me to render the theory, which ascribes the origin of harmonics to the hysteretic action of iron, completely untenable.

I do not think that the proper time has arrived yet for the formulation of a physical theory which will give a complete account of the peculiar behavior of iron, by means of which it superposes odd harmonics upon the wave of a simple harmonic current. The view which irresistibly suggests itself to my mind is simply this: Upper harmonics will be generated whenever more or less abrupt changes of the magnetic state in any part of the magnetic field through which an alternating current flows occur. A slotted core armature or an armature made up of coils with iron cores distributed over a drum common to all of them will introduce such abrupt changes. An alternating current motor, especially when it is not of a smooth core armature type, will also cause abrupt changes of magnetism and hence cause strong deviations of the feeding current from the simple harmonic form. But if this view be correct, then every complete cycle of magnetization to which iron is subjected when under the inductive action of a simple harmonic current must be accompanied by some abrupt changes in magnetism, and that, too, whether the mean magnetic intensity of the cycle be large or small. A great many things may be suggested which could account for such cyclic abrupt changes. One thing is certain and that is, that hysteresis, as commonly understood, will not account for them; for these peculiar abrupt cyclic changes, if they really exist and are the cause of harmonics, are not affected by mechanical vibrations by which, as is well known, all hysteretic effects are influenced very much. *But* *whatever the real theory underlying these upper harmonics may be, the bare fact which the engineers have to face is: There is no cure against harmonics as long as the circuits contain iron. Hence construct your lines in such a way that conditions favoring resonance with the frequency of the fundamental or with one of its odd upper harmonics will seldom occur, and whenever they do occur the resonant rise of potential should not be capable of producing any damage. Avoid slotted armatures and armatures with projecting pole-pieces and keep the magnetization down as much as possible.*

VII. Fluctuations of the Rotary Field.

Before closing this paper I will describe briefly the application of the resonance method of analysis to the study of the intensity fluctuations of a rotary magnetic field. The investigation was carried out by two students of the Electrical Department of Columbia College, at my suggestion, and will be published in the near future. The method, briefly stated, is this: A suitable number of turns of wire are subjected to the induction of a rotary magnetic field. These turns form part of a resonator. Whatever fluctuations there be in intensity of the rotary field they will be periodic, their period bearing a perfectly definite ratio to the periodicity of the current which produces the rotary field. For instance, in a three-phase combination of alternating currents, the intensity of the rotary field will, according to theory, show six maxima and six minima during each complete revolution, the maxima differing from the minima by about 14 per cent. A circuit, subjected to the inductive action of such a field should have a periodic electromotive force induced in it whose frequency will be either three or six times the frequency of the fundamental, according to the shape of the curve of fluctuations. Similarly in a rotary magnetic field produced by a two-phase combination of alternating currents. If such electromotive forces were induced the resonator would detect them, and from the resonant rise of potential the extent of the fluctuations producing these electromotive forces could be estimated.

No electromotive forces of this type were detected in either a tri-phase or a two-phase combination. Hence the inference: *Rotary magnetic fields produced by reasonably well constructed machines are not accompanied by fluctuations in their intensity.*

In conclusion I wish to thank my friend, Professor H. A.

Storrs of the University of Vermont, who during his whole stay at my laboratory this year was always anxious to aid me in my work in a most ready and disinterested way. His most efficient assistance was of the greatest value to me.

DISCUSSION.

PROF. ANTHONY:—On account of the lateness of the hour, I will confine myself to just one point, although I would like very much to have something to say about this most beautiful method, and these results that Dr. Pupin has brought forth. I want to say one word about the rotary field. I never could see any reason why the rotary field should be distorted or irregular. It seems to me that all these statements that there was an irregularity in the rotary field were based upon the idea that the two quarter-phase vibrations were compounded in a straight line, just as though we had a vibration following along there, and another following along in the same way. But here we have two vibrations at right angles to each other. Here is a vibration in the horizontal plane and another in the vertical plane, and exactly as in the two tuning forks we ought to get a perfect circle, not a complicated vibration, but the compound vibration ought to continually change its angle and remain constant.

THE PRESIDENT:—Of course the members are aware of the fact that the TRANSACTIONS are open to them for the publication of any proper criticism to be made of any of the papers.

MR. STEINMETZ:—I was very much interested in Prof. Pupin's valuable paper on these higher harmonics. Having had some occasion to investigate these higher harmonics I may quote some of my experience. I think the cause of this discrepancy between the different kinds of higher harmonics, those produced by the generator and those produced by the transformer, is the fact that the ones are higher harmonics of the electromotive force, the others higher harmonics of the current. Thus the higher harmonics of the generator wave are higher harmonics of the electromotive force, and follow the laws of electromotive forces, while the others do not follow these laws, not being electromotive forces. This explains quite a number and probably most of the differences between the one and the other.

The wave of the alternating current generator may be nearly a sine wave, may be distorted from sine shape due to the shape of the magnetic field in the alternator and so on. This distorted wave of electromotive force can be dissolved into a fundamental sine wave and a number of higher harmonics of frequencies, triple, quintuple, and so on. These different components will follow the same law. We have in the circuit, resistance, inductance and capacity, and according to the conditions of the circuit these various harmonics will be affected.

Take a non-inductive circuit; the current will be proportional to the E. M. F. and in the wave of the current all the various harmonics of the E. M. F. will show up in the same proportion. The harmonics of the generator are transformed by the transformer, whose magnetism depends upon the impressed E. M. F., and closely follows its variations. Thus these higher harmonics of E. M. F. reappear in the secondary.

Take the case of an inductive resistance, that is, of an induction motor or some apparatus like this: In an inductive resistance a current will flow which is less than corresponds to the true ohmic resistance, due to the counter E. M. F. of self-induction. The reactance of self-induction, however, is proportional to the frequency, consequently three times as large for the triple, and five times as large for the quintuple harmonic. Thus the wave of current produced by the multiple harmonic of the generator no longer shows these higher harmonics in the same proportion. It shows them in a decreased manner—a decrease due to the effect of self-induction. Thus, if you have a motor in the secondary of the transformer, the motor circuit will not show the harmonic any more, or at least not in the same strengths as the generator on a non-inductive resistance.

It is different with a condenser in the circuit. In a condenser the current is proportional to the E. M. F. and the frequency; consequently it would be higher for these higher harmonics.

In other words with a condenser in the circuit of a generator, whose E. M. F. wave contains higher harmonics, the current wave will show these harmonics accentuated, while in an inductive circuit they will be more or less suppressed in the current wave, and pass unaffected in a non-inductive circuit. You see that you can get all kinds of phenomena, according to the relative proportion of non-inductive resistance, inductance and capacity. But still you always get the same thing; different E. M. F. waves acting upon the same circuit, and the current produced by these waves independently superimposed upon each other to the resultant current.

It is quite different with the second class of harmonics, the higher harmonics of current.

If on a transformer, a sine wave of E. M. F. is impressed, the magnetism must be a sine wave also, if the impedance is negligible, because the counter E. M. F. being equal to the impressed E. M. F. but opposite, and equal to the change of magnetism, requires that the magnetism be a sine wave. If the E. M. F. is a sine wave, the current can *not* be a sine wave, but the current *must* be distorted due to the effect of hysteresis, that is, due to the lack of proportionality between current and E. M. F., and this distortion of the current wave means the production of higher harmonics, and the amplitude of the higher harmonics with regard to the fundamental depends upon the difference; the lack of proportionality between current or M. M. F., and magnetism. Thus, in an open circuit transformer, where the reluctance is nearly all air, you find the amplitude of these higher harmonics of current very much less in proportion to the fundamental than in a closed circuit transformer; or rather in a closed circuit transformer the fundamental is less, while the higher harmonic is the same as in the open circuit transformer (other things being equal), and thus the higher harmonics will more and more disappear in their relation to the fundamental by an increase in the fundamental due to open magnetic circuit, or due to the increase of load on the secondary. Such cases are shown in my paper on "Hysteresis, Part III.," which I propose to read presently. You will see that this harmonic is merely produced by the necessity of the current to maintain a sine wave of magnetism; this is a secondary phenomenon, and has no "stability," that is, no existence independent of the conditions producing it.

It is obvious that these latter higher harmonics can not be brought to resonance, like the higher harmonics of the generator E. M. F., since they are no E. M. F.'s at all, and only E. M. F.'s can be brought to resonance.

If the generator does not give a sine wave, but a wave which contains higher harmonics, and if it feeds a transformer, which, due to the discrepancy between magnetizing current and magnetism, gives higher harmonics also, you get a circuit containing higher harmonics of E. M. F., and higher harmonics of current, which according to the circumstances may either aid or oppose each other.

The experiment showing that the hysteresis does not cause any distortion in the alternator armature proves nothing, because it is quite possible that there is no hysteresis in the armature of the generator. There is a waste of energy by molecular magnetic friction; but molecular magnetic friction is *not* identical with hysteresis as I have shown in my second paper on hysteresis.[1] Magnetic hysteresis is the disproportionality between magnetomotive force and magnetism, as the name signifies, and this disproportionality is produced if upon the alternating magnetic circuit no external energy is expended, since energy is consumed in the circuit by the molecular magnetic friction, which energy must be supplied from somewhere, and in the absence of any other source of energy is supplied by the magnetomotive force in the form of magnetic hysteresis. But this energy may be supplied by mechanical energy, and this is the case in the smooth core machine, and thus we may not find any hysteresis, any disproportionality between magnetic field strength and magnetomotive force.

In a transformer, the iron core is indeed set in vibration by the alternating magnetism, but this vibration cannot supply the energy consumed by molecular friction, and thus can not eliminate hysteresis, because this vibration is produced by the alternating magnetomotive force, and has the effect of intensifying the loss of energy—increasing the area of the hysteretic loop by the loss of energy in mechanical vibration, thus intensifying the higher harmonics, since now energy is expended not only by molecular magnetic friction, but also by mechanical vibration.

1. TRANSACTIONS, vol. ix, pp. 712-716.

Proof of this increase of hysteresis by mechanical vibration due to the alternation of the magnetism I have also shown in my second paper on hysteresis.[1]

With regard to the ratio of the amplitude of the higher harmonics to the fundamental, I have in the paper which I propose to read, a number of such higher harmonics, construed from theoretical reasons, from the cycle of hysteresis, but I do not find a strict proportionality. It is proportional within a very limited range only, and I believe the exact proportionality which Prof. Pupin found, is due to this very limited range of magnetization used in his experiments. So far as I understand, he used densities up to $B = 4{,}000$ only. But the proportion changes very much when you go higher. I had a chance to go very high —to go to $B > 20{,}000$. But I found that at these densities, the higher harmonics are so enormous, that occasionally they obscure the fundamental altogether. You get this effect, if you take a transformer built for 125 cycles and 50 volts, and run this transformer at 25 cycles and 100 volts, whereby the effective magnetization will go up beyond $B = 25{,}000$. Then the exciting current gets enormous and gets utterly distorted, and you can just detect that there is a fundamental left, but the most of the current is higher harmonics.

With regard to this much discussed phenomenon of resonance, let us see what it really means, and whether it is a separate phenomenon at all, or merely a special case of a well-known phenomenon.

You know that if you insert in an alternating circuit a self-induction, the self-induction will consume an E. M. F. lagging behind the impressed E. M. F. by 90°, or a quarter period. The resistance will consume an E. M. F. in opposition to the impressed E. M. F.; and a condenser inserted in the circuit will take a current leading 90°.

Now consider what takes place if you have a circuit closed by a condenser, that is a circuit whose current leads the E. M. F. by 90° or a quarter period, and have in series with that condenser, self-induction.

By self-induction, a counter E. M. F. is induced which lags 90° behind the current. But the current is 90° ahead of the impressed E. M. F. Thus, the counter E. M. F. of self-induction is in phase with the impressed E. M. F. and adds itself thereto. Hence, the effective E. M. F. is increased by the self-induction being brought into phase with the impressed E. M. F., and if this increase is excessive, we call it "resonance," but it is nothing different from the action of the choking coil, which produces a counter E. M. F., 90° behind the current and thereby, according to the phase relation between current and impressed E. M. F., either reduces the E. M. F., if the current is lagging, or increases the E. M. F. if the current is leading.

1. TRANSACTIONS, vol. ix, p. 715.

Thus, if you take a condenser with an impressed E. M. F. of say 100 volts, and suppose the condenser takes 10 amperes. Now insert a self-induction which with 10 amperes current will produce a counter E. M. F. of 100 volts; this counter E. M. F. of 100 volts is now due to the lead of the current, in phase with the impressed 100 volts, adds itself thereto and thus gives 200 volts at the condenser terminals, with 100 volts impressed at the generator. These 200 volts will double the current and you get 20 amperes through. These 20 amperes will induce a counter E M. F. of self-induction of 200 volts. Then you get 400 volts at the condenser terminals. That means 40 amperes and 400 volts at the self-induction. So you see that the self-induction in series will raise the condenser E. M. F. infinitely, that is, until the increase of current causes the resistance to consume the total impressed E. M. F.

You see it is nothing but our well-known old friend self-induction introducing an E. M. F. lagging 90° behind the current and thereby affecting the impressed E. M. F. according to the phase relation of the current.

MR. KENNELLY:—While this paper appears to give us valuable information concerning an important method of analysis of alternating electromotive forces and currents, it also generally attacks Fleming's theory of distortion of the current wave in alternating current circuits, and I beg to submit that in spite of all that has been said in this paper, and without impugning in any way any of the observations recorded there, the evidence is not in any way conclusive that the existing theory is wrong. I do not mean to say that the existing theory is right, because I hope it is not. I hope it is so far incomplete that there is much more to learn. But it is one thing to say a theory is right, and another to say it is wrong, and I do not think that the evidence here is at all conclusive that the theory is wrong, partly for reasons brought up by Mr. Steinmetz, and partly for the reason that the beautiful method employed by Dr. Pupin is necessarily and fundamentally weak in one very important particular, namely, that while it brings out the harmonic in any periodic current of electromotive force, it does not bring out, and cannot bring out, the phase relations of those harmonics, and that consequently you can have a large harmonic in an alternating current electromotive force or current strength, and the shifting of its phase will produce an enormous difference in the shape of the wave, whether it is in step with the fundamental or whether it is not in step, and as you shift it along, the consequences which will be produced on the shape, and on the results of that shape, may be so great as to totally defeat the conclusions that you would base on your observations.

DR. PUPIN:—I think that I have distinctly referred to an experiment which met the objection of Mr. Kennelly, and that is that the phase relations had nothing to do with the amplitude of the harmonics, as determined in my investigations. No more than it makes any difference in the analysis of acoustic vibrations by means of Helmholtz's resonators, whether the phase relations between the various harmonic components of a complex sound be this or that. As mentioned in my paper, we really care very little for the exact shape of the alternating current wave. What we want is the relative value of the amplitude of the harmonics, and we do not even care whether these harmonics be in phase with the fundamental or not, provided that phase directions will not affect our results, and experiment has shown that they do not.

I have not attacked Prof. Fleming's theory of the generation of harmonics by hysteresis. I brought facts which seem to me to be difficult to reconcile with the hysteresis theory of harmonics. In fact, I do not really see on what foundation rests Prof. Fleming's theory of the generation of harmonics by hysteresis. Nor do I see that there is a sufficiently strong foundation for any other theory. Every investigator of course, has some temporary physical theory of the phenomena which he investigates; but he generally keeps it to himself as a suggestive guide. If he chooses to offer it to others it is always with a view of inducing them to test its validity. Such a theory seems to me to be Prof. Fleming's theory of the hysteretic distortion of alternating currents, and I fail to see any objection to a reasonable criticism of it. Mr. Kennelly's remarks led me to think that in his opinion Prof. Fleming's theory is beyond criticism, or that, at any rate, my criticism of it is not a reasonable one. I do not think that Mr. Kennelly's discussion of my paper can justify any one of these two views on his part.

Mr. Steinmetz states that my experiments with vibrating and otherwise mechanically disturbed cores, seem only to throw some doubt on the hysteresis theory, but that in reality they do not affect it in the least. Hysteresis is affected by mechanical vibrations, according to experiments of Prof. Ewing and Prof. Ryan. Hence if hysteretical processes are affected by mechanical vibrations, and harmonics are due to a hysteretic action, then there ought to be some connection between them. That is, the harmonics being generated ought to be diminished or affected in some way by mechanical vibrations; but they are not. To get out of the way of this difficulty, Mr. Steinmetz simply states that it is not that hysteresis which is affected by mechanical vibrations that causes a distortion of an alternating current wave, but an entirely different kind of hysteresis, namely *molecular friction!* A very ingenious way of arguing! But making new hypotheses in order to get out of the way of old difficulties seems to me to help matters very little. I prefer to face the difficulties and try to clear them away by experimental evidences rather than by skilful manipulation of neat hypotheses.

I am very sorry that I did not understand the rest of Mr. Steinmetz' discussion. It seemed to me that what he wished to say was to explain things that I thought were perfectly plain to everybody. We all know that self-induction will offer a larger impedance to higher than to lower harmonics There is nothing new in that. So far as his explanation of electrical resonance is concerned, it may be very good according to his own taste. It certainly does not suit mine. I do not think that anybody has looked upon electrical resonance as something which is outside the well-known laws and well-known phenomena. Electrical resonance is an ordinary every day affair. At any rate I always tried to represent it as such, and I am sorry that Mr. Steinmetz has not succeeded in catching my meaning. I am quite sure that I wrote a very popular paper on the subject over a year ago, and am rather surprised to find Mr. Steinmetz so anxious about the possibility of having people look upon electrical resonance as something mysterious.

MR. STEINMETZ :—I think Mr. Kennelly is perfectly right, because what interests us is, how high will the E. M. F. go, and so it is of importance to know whether the different component waves will reach their maximum at the same time, or whether they will overlap each other, so that when one is high, the other is low, and that the sum total will not be increased or will even be reduced.

DR. PUPIN :—I beg your pardon, I wish to correct your statement right here. If we have a certain number of frequencies, and the line is in resonance with one of them, it cannot at the same time be in resonance with another. It is only in that case that we could have an overlapping of these different amplitudes.

MR. STEINMETZ :—I do not think you understood me. If you have a fundamental of say 10,000 volts, and superimpose a triple harmonic of 10,000 volts, you will either get 20,000 volts, or you may get almost nothing if they go against each other I mean the overlapping of the one higher harmonic, which is in resonance, with the fundamental, which is already quite high.

With regard to the difference between molecular friction and hysteresis, and the proof that these are entirely different phenomena, which have directly nothing to do with each other, I may refer to my second paper on hysteresis, where I think I brought conclusive proof of my statement, and to my present paper, where I fully discuss the matter.

THE PRESIDENT :—If there is no other discussion we will proceed to the next paper. I wish, however, to give Mr. Binney the floor. I believe he has a resolution to offer.

MR. HAROLD BINNEY :—Mr. Chairman, this is the resolution proposed :

"*Resolved*, that a special committee be appointed to prepare "appropriate letters of thanks."

[The resolution vas adopted, and the President appointed Mr. Harold Binney, Dr. M. I. Pupin and Dr. Louis Duncan, members of the Committee.]

THE PRESIDENT:—A committee was appointed to consider some rules governing installations. I will ask the Chairman, Mr. W. J. Hammer, for a report.

MR. HAMMER:—Copies of the report which were sent by the Secretary of the National Electric Light Association have been distributed and doubtless you have all received them. In behalf of the Committee I present the following report:

PHILADELPHIA, PA., May 18, 1894.

MR. PRESIDENT AND GENTLEMEN:—

Your Committee to whom was referred the question of the advisability of indorsing the Standard Rules for Electric Construction and Operation of the National Electric Light Association, beg to report that the scope and character of these Rules preclude the possibility of the Committee's giving the matter sufficient attention in the limited time at their disposal, and at this time recommend such action on the part of the Institute.

We however wish to express our hearty appreciation of the importance and value of such work as that carried out by the National Electric Light Association and the advisability of securing the adoption of one code of Rules which shall be acceptable to all electrical insurance and other interests.

Approved,

(Signed) WILLIAM J. HAMMER, *Chairman.*
N. W. PERRY.
A. E. KENNELLY.
CHAS. P. STEINMETZ.

I wish to say just one word, and that is, that I think this is all that the National Electric Light Association desired or expected as an expression from the INSTITUTE, and that in all probability before the next edition of this report is issued by the Association some joint action will be secured by the co-operation of the insurance or underwriters' interests and the AMERICAN INTITUTE OF ELECTRICAL ENGINEERS, in order to secure this much to be desired adoption of one single report which can be generally acceptable.

[On motion the report was accepted.]

In the absence of the author, Mr. Kennelly read the following paper by Dr. Louis Bell:

A paper presented at the eleventh General Meeting of the American Institute of Electrical Engineers, Philadelphia, May 18th, 1894. President Houston in the Chair.

SOME FACTS ABOUT POLYPHASE MOTORS.

BY LOUIS BELL, PH. D.

In connection with the long distance transmission of power, most engineers will, I think, agree with me in the position that alternating current motors of some kind, and of alternating currents preferably the polyphase variety, are almost a necessity. We have been forced to their use by the exigencies of long distance service, which compel either the total abandonment of continuous current or its use under very embarrassing conditions.

There has, however, been a tendency to look upon the polyphase motor as a somewhat undesirable resort to which we have been driven by long distance work. Such indeed was my own belief from *a priori* reasoning, and before I had obtained that practical knowledge of the subject which can only come from personal experience both with the design and application of any class of apparatus. By such experience, and by the commercial demands which have been coming in with steadily increasing frequency, I have been forced into taking the position that the polyphase motor is intrinsically preferable to the continuous current motor for a vast majority of all the uses to which such machinery may be applied.

At present, appearances indicate that not only will the polyphase motor displace direct current apparatus for most long distance transmission plants, but will prove a formidable competitor in all applications of motors to industrial purposes, and this, although the polyphase apparatus is the growth of only a few years, while the direct current motor is the outcome of more than a decade of experience.

We may then, in instituting a comparison between polyphase and direct current motors, consider the various properties which a good motor of any kind must necessarily have. Mechanically speaking, we would all agree that it should be simple in construction, strong, not liable to frequent or considerable repairs, convenient in form and not excessive in weight. As regards its properties, it should in general run cool, stand overloading without serious danger, run at a nearly constant speed, or be capable of considerable variation in speed if necessary. It should be capable for certain uses of sudden and violent exertion, and of easily changing its direction of rotation. In a purely electrical way, it should be simple, efficient both at high and low loads, and should not take excessive amounts of current either in starting or in running.

This represents a difficult list of conditions to fulfil with any one motor, but I believe they can be met better by polyphase than by direct current machines. A direct current motor, for instance, of a given construction and reasonable weight, cannot both run at a constant speed independent of load in a particular case, and at the same time be capable of running at a wide variety of other speeds. It usually will not stand considerable overloads without sparking, and at the same time give a fair efficiency at low loads; and so one might go on piling up difficulties. The polyphase motor, too, cannot meet all these conditions with equal success, but examining them one by one, you will find that on the whole we can obtain very excellent results.

Taking up *seriatim* the desirable properties which I have enumerated, and applying them to polyphase motors, we find as regards the first count, that their construction is singularly free from complexity. They consist in general of two concentric masses of lamination forming respectively the field magnet and armature. The armature is assembled on its shaft much as in direct current machinery, while the field laminations are held together by a clamping spider of very simple construction. The bearings are supported either on end spiders fitted to the field spiders by lathe work only, or on pillow blocks of the ordinary kind. Commutator and commutator connections there are none, nor should there be collecting rings except in rare instances. The armature need be exposed only to low voltages, and should preferably be wound with a comparatively small number of rather massive conductors, united at the ends either by a single plate or by very simple connectors, the latter form being preferable. The field winding is usually in a greater number of coils than is a direct current field winding, but each coil has a comparatively small number of convolutions, making the total winding by no means complicated.

In lieu of the starting rheostat of the direct current motor, we have a starting resistance which should preferably be placed within the armature, and consists of a few zig-zags of metal united at one end, and connected at the other to three or more contact pieces. A solid collar short-circuiting these contacts and a forked lever to move the collar completes the equipment, as I am accustomed to employ it. Its most noticeable feature is that the revolving armature, the most troublesome and delicate part of a direct current machine, is free from complication, and that it is almost as solid as if it were a solid mass of metal, and scarcely more liable to injury.

The very obvious simplicity of construction and mechanical strength, is strong evidence of unusual freedom from repairs, and as a result of experience I have found that the induction motor is singularly free from liability to accidents of every kind. I have never succeeded even by the severest kind of experimental work in burning out a field coil or doing any serious injury whatever to a motor, although I have kept some of them on static torque tests in rapid succession for hours at a time and have held them at rest and poured current through them until the leads burned off, the motor still remaining undamaged. If there is a test of extraordinary severity that I have not applied to induction motors, I have yet to learn of it. These properties are invaluable in commercial work inasmuch as they practically remove the danger of crippling the motor even under exceptionally unfavorable conditions.

As regards the convenience of the form of polyphase induction motors, I think an inspection of any of the types which have been brought out will render argument unnecessary.

The magnetic necessities of the case have led all makers of such apparatus in this country and elsewhere, to adopt a species of barrel shape as the general outline of the motor, modified only in the proportions of diameter to length, and in the adoption of one form or another of bearing. The tendency of this construction is to bring the center of gravity of the machine very low, thus ensuring unusual stability and freedom from general vibration. This form, too, enables one to place the motor in almost any position which is convenient in applying it, upside down, as a side bracket, and the like.

The largest installation of induction motors in the world just put in operation in Columbia, S. C., aggregating over 1300 H. P., is composed of inverted motors with their bases bolted to the ceiling timbers of the rooms.

As regards weight, the abolition of any sparking limit to the output, and the excellent magnetic materials used, might naturally be supposed to lead to motors of unusually light weight, and such is in fact the case. Sixty to seventy pounds per horse-power in motors of moderate size is a figure easily reached without any sacrifice of efficiency, and if occasion requires, these limits can be passed with great facility, 25 to 30 pounds of material per horse-power being quite attainable in large units while still retaining satisfactory general properties. I must say however, that for most uses I do not consider extreme lightness either necessary or desirable, although it is important to be able to secure it if necessary. So much for the mechanical character of induction motors.

Electrically speaking, the case is just as favorable. Unless forced to a very large output per pound of weight, an induction motor will run quite cool, at a heating limit in fact below that of most direct current machines of similar weight and output. This advantage is mainly due to the very substantial character of winding which can be conveniently employed, and to the fact that the winding is distributed so that the losses in the copper are not localized, while the laminated character of the structure facilitates thorough ventilation. This freedom from excessive heating indicates that the polyphase motor can stand considerable overloading without any serious results, and experience has shown this to be the case. The worst that can really happen is that the motor may fall out of synchronism when the load is sufficiently great, thereby blowing the fuses in the line.

As sparking is obviated in this type of machine it can readily be rated at such output as will give a proper limit of heating, and this output will in most cases allow from 30 to 60 per cent. of overloading before the machine will drop out of step. A wider range than this can be obtained if desirable, which it is generally not.

The limit of possible overloading fixes in a general way the possible static torque that can be obtained from a given machine, and this is apparently purely a matter of convenience in design, anything that can reasonably be required being quite attainable.

There is no special difficulty in arranging polyphase motors for a starting torque four or five times the running torque, although this would be unnecessary except for severe hoisting and tramway work. At running torque the starting current taken may readily be no greater than the running current. From this it will readily be seen that a properly planned polyphase motor is capable of very great and violent exertions in a case of necessity. It will even endure complete reversal under full load within 10 or 15 seconds on motors of ordinary sizes, this time being sufficient for the machine to pass from full load in one direction, to full load and speed in the other direction. This reversal is, as is well known, accomplished simply by reversing any two of the primary wires, the effect being to rotate the field in the opposite direction from the armature, thus causing an enormous rate of cutting lines of force and consequently immense effort causing the motor to stop and reverse.

Speed Variation in Polyphase Motors.

This subject has been for the most part in a rather hazy condition up to the present time. The induction motor has been generally known as non-synchronous, and such indeed it is. The name, however, has been frequently used in ignorance of the fact that an induction motor always tends towards synchronous running.

Under ordinary conditions the polyphase induction motor can be made to run at nearly constant speed independent of load, resembling in this respect a well-designed shunt motor. A variation from no load to full load of five to six per cent. in speed would represent ordinary good practice, either in a shunt motor or a polyphase one, this limit being exceeded only in small motors or types which may be regarded as special. It is by no means difficult, however, so to design a polyphase motor that the speed shall possess very remarkable uniformity. This condition has been valuable in the Columbia plant previously alluded to. In this case, tests of 17 motors showed a maximum variation in speed, from an output of 75 H. P. to friction load of the motor, of only 2.2 per cent., individual motors showing slighter variations down to 1½ per cent.

The task of these particular motors is driving a cotton mill, hence the necessity for uniform speed. And this uniformity in speed is not greatly affected by variations in voltage, which would be quite sufficient to cause considerable speed variation in a shunt wound motor, in fact the induction motor is remarkably insensitive to moderate variations in voltage, unless it is heavily loaded.

This uniformity in speed has frequently been urged as an objection to the induction motor, barring its employment in cases where speed variation is necessary. This point is not well taken.

The induction motor can not be made successfully to run at reduced speed by varying the primary voltage. Under these

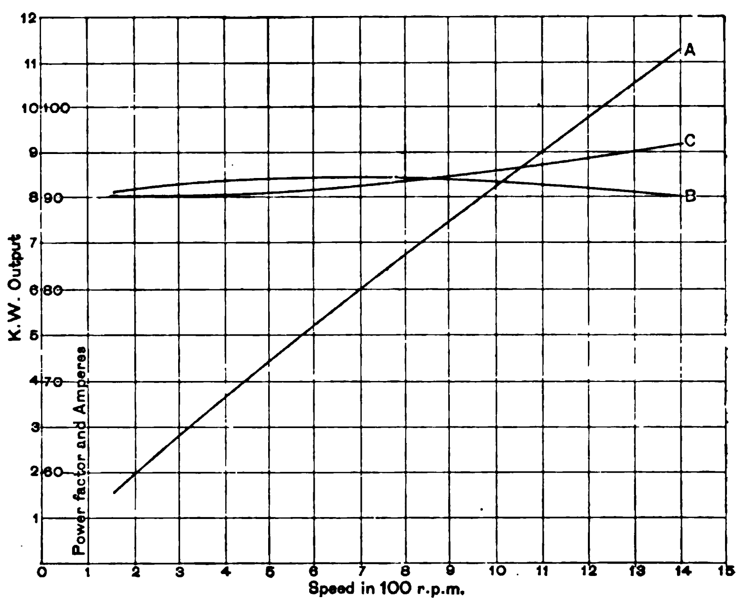


FIG. 1.

circumstances the output of the machine falls off somewhat more rapidly than the square of the voltage, so that only trifling speed variations are possible. It is a fact, not generally known, however, that the speed of a polyphase motor can be varied with the same facility and within the same wide range as is possible in the case of a series wound continuous current machine, such as a railway motor. This is accomplished in the induction motor by a rheostat in the secondary circuit, just as it is accomplished in the series motor by a rheostat in the main circuit. Thus equipped the two machines behave almost exactly alike. The speed at constant torque can be made to vary from full speed down to almost no speed, thus simulating the action of the series motor in the closest possible fashion. At any given speed an increase or decrease of the torque will decrease or increase the speed substantially alike in both classes of motors. In both too, the efficiency is initially similar and falls off in practically the same ratio. A non-inductive resistance is necessary in case of the polyphase motor, an inductive one throwing the armature current so far out of phase as to interfere with the proper action of the motor.

Fig. 1, gives an excellent idea of the behavior of a polyphase motor with resistance in the secondary circuit.

Curve A shows the speed and output of a certain motor under these circumstances. It was a four-pole machine operated at 50 cycles per second and the initial speed was reduced to 1400 by the resistance of the leads reaching across the room to the rheostat composed of loops of manganin strip which could be systematically varied. It will be seen that the word curve is almost a misnomer, the ratio between the speed and output at constant torque being almost a linear function, even when the speed fell to as low as 150 revolutions per minute. It was not carried lower than this only because of lack of adaptability in the rheostat. No series motor could show a more satisfactory result.

Curve B shows the power factor under these varying conditions. It is high at all loads and speeds, varying slightly with a maximum at about half speed.

Curve C shows the variation in current. This, as can be seen is almost constant, falling off slightly at the lower speeds, the voltage being uniform throughout the test.

Speed variation by this method is not as efficient as might be wished, but still compares favorably with that obtained in a series motor with rheostatic control. Some modified methods of control promising a somewhat better efficiency have been suggested, but it seems probable that in the net result we shall find that continuous current and polyphase motors are about on a par in this respect. It should be noted that in continuous current motors, speed variation by weakening the field is only practicable within a very limited range, and requires an abnormally heavy motor. I think that with a similar considerable change in design, the polyphase motor could be made to operate nearly as well by change in its field strength. An efficient speed variation through a very wide range is attainable in either class of machine only by extraordinary means, as an elaborate combination of direct current machines, or frequency changing devices in the polyphase machines. From what has been said it will be apparent that the polyphase motor is perfectly capable of a complete control of speed on the same terms generally obtained with continuous current motors.

COMPARISON OF A POLYPHASE AND CONTINUOUS MOTOR.

Fig. 2, gives a striking comparison between the properties of the two classes of machines under consideration.

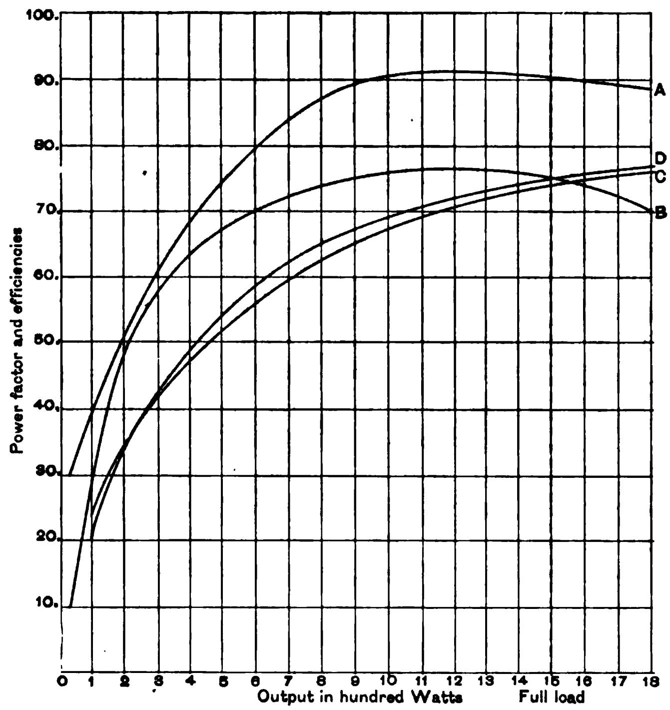


FIG. 2.

The polyphase motor selected for comparison is of two H. P. output representing the average small motor to be found in central station practice. This particular size weighs 218 lbs. complete, and runs at a speed of about 1400 revolutions per minute on 50 cycles loaded. It is relatively neither better nor worse than polyphase motors of other sizes, as may be seen by reference to the curves in my previous paper on this general topic. The power factor in this case rises quite sharply, reaching 86 per cent.

at half load, and through most of the working range of the motor remains at or near 90 per cent., nearly 91 per cent. as a maximum. The efficiency has its maximum a little under full load of the motor and reaches nearly 77 per cent., being 75 per cent. at full load. Both power factor and efficiency hold high values from half load up, and do not fall off seriously until some distance below half load.

Contrast with curves A and B belonging to the polyphase motor, curves C and D; the former is the efficiency curve of a two H. P. 500-volt motor of one of the well known American makes, and curve D is a similar curve for a two H. P. 110-volt motor of European manufacture. These are not selected curves but were the two completest available. Both these curves, C and D,

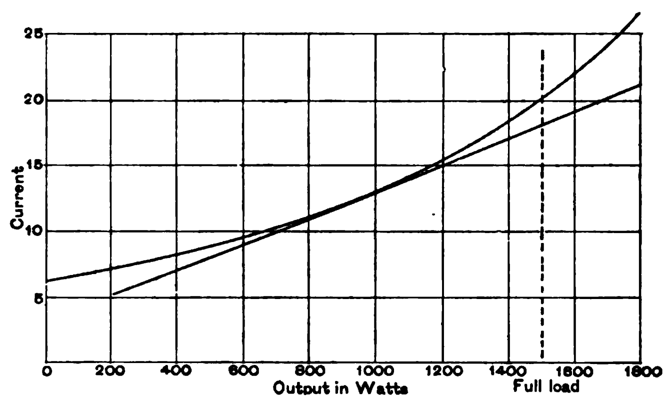


FIG. 3.

show remarkable similarity. Neither of the motors sparked seriously at full load, the load limit being set rather by the heating. Both curves rise slowly and attain their maximum values at some point beyond the available load of the machine. At full load the efficiency is substantially the same as that of the triphase motor. At low loads it is noticeably worse. I think C and D are fair average machines. In tests of a wide variety of motors, some higher and lower efficiencies would be found. Such too would be the case in testing a variety of polyphase motors. In fact the mate of the triphase motor shown, sent through the factory at the same time, showed about 1½ per cent. higher efficiency, but a complete test was not attainable as the machine had to be immediately shipped. A comparison of these curves will render it evident, that it is quite practicable to produce a polyphase motor having an efficiency fully equal to that of direct current motors of similar size, and I think the tendency will be towards better efficiency at moderate loads. It should be mentioned here that the two horse-power triphase motor was made of ordinary good armature iron, not selected, or specially treated in any way.

Finally let me call sharp attention to curve 3, which shows the effect of the power factor, which I regret to say has been talked about not wisely, but too well, in most of the discussions pertaining to polyphase apparatus. Two curves in Fig. 3, show the total current in the line in the two H. P. triphase motor and the 110-volt continuous current motor above mentioned. The current curve of the continuous current machine is nearly a straight line, that of the polyphase machine is almost tangent to it and slightly concave upwards. A noticeable fact displayed is, that throughout the ordinary working range of these two motors the currents were substantially equal, the existence of the power factor in the polyphase motor being only noticeable at very low loads and at overloads. This comparison should be a sufficient answer to the charges of excessive current that have so often been made against the polyphase machines. In a bad polyphase motor they would have foundation in fact. In a rather good one the net effect of the lagging current is trifling. It should further be noted that the current in this triphase motor at friction load is only about 30 per cent. of the current at full load, and of this small amount only 30 per cent. represents loss of energy. This is in marked contrast with the results obtained from a foreign triphase motor of similar size exhibited at the World's Fair, in which the no load current was nearly equal to that at full load, and the power factor at full load was barely 50 per cent.

In this brief discussion of some of the properties of modern polyphase motors, I have endeavored to show how nearly they fulfil the conditions which may be regarded as desirable in electric motors in general. That they do so as well, if not better, than the continuous current machines of similar capacity, I believe that I have satisfactorily shown.

The demand which certainly is arising for polyphase motors for general power purposes based on their intrinsic merits, indicates that the older type of machinery has found a dangerous rival, all questions of long distance transmission aside.

DISCUSSION.

MR. CARL HERING:—Dr. Bell speaks of "polyphase" motors. "Poly" is from the Greek, meaning "many," but I presume from one of the remarks in the paper that these were all three-phase motors. As the terms "two-phase" and "three-phase" are in use and are very definite, and as there are such things as motors in which more than three phases are used, I would suggest that it is preferable to use the self-explaining term "three-phase" for the values referred to in the paper, especially as there is then no doubt that they are not "two-phase." If polyphase can be used to refer to three phases it certainly could be used equally well for two phases, notwithstanding such sayings as "two are company and three are a crowd."

MR. STEINMETZ:—I think I can answer this question. These motors referred to were three-phase motors, but practically identical theoretical considerations apply to quarter-phase motors as well as any other polyphase motors.

I may add with regard to the induction motor referred to by Dr. Bell that if it interests the members, they can find the speed and torque characteristic of the motor in the discussion of the last paper of Dr. Bell on "Practical Properties of Polyphase Apparatus," read at the 83rd meeting of the INSTITUTE, January 17th, 1894.

THE PRESIDENT:—If there is no further discussion I will call for the next paper, "The Law of Hysteresis (Part III.) and the Calculation of Ferric Inductances," by Mr. Steinmetz.

A paper presented at the Eleventh General Meeting of the American Institute of Electrical Engineers, Philadelphia, May 18th, 1894, President Houston in the Chair.

ON THE LAW OF HYSTERESIS (PART III.), AND THE THEORY OF FERRIC INDUCTANCES.

BY CHARLES PROTEUS STEINMETZ.

CHAPTER I.—COEFFICIENT OF MOLECULAR MAGNETIC FRICTION.

In two former papers, of January 19 and September 27, 1892, I have shown that the loss of energy by magnetic hysteresis, due to molecular friction, can, with sufficient exactness, be expressed by the empirical formula—

$$H = \eta \, B^{1.6}$$

where H = loss of energy per cm³. and per cycle, in ergs,
B = amplitude of magnetic variation,
η = coefficient of molecular friction,
the loss of energy by eddy currents can be expressed by

$$h = \varepsilon \, N \, B^2,$$

where h = loss of energy per cm³. and per cycle, in ergs,
ε = coefficient of eddy currents.

Since then it has been shown by Mr. R. Arno, of Turin, that the loss of energy by static dielectric hysteresis, *i.e.*, the loss of energy in a dielectric in an electro-static field can be expressed by the same formula:

$$H = \delta \, F^{\mathcal{K}},$$

where H = loss of energy per cycle,
F = electro-static field intensity or intensity of dielectric stress in the material,
δ = coefficient of dielectric hysteresis.

Here the exponent $\mathcal{K}$ was found approximately to = 1.6 at the low electro-static field intensities used.

At the frequencies and electro-static field strengths met in condensers used in alternate current circuits, I found the loss of energy by dielectric hysteresis proportional to the square of the field strength.

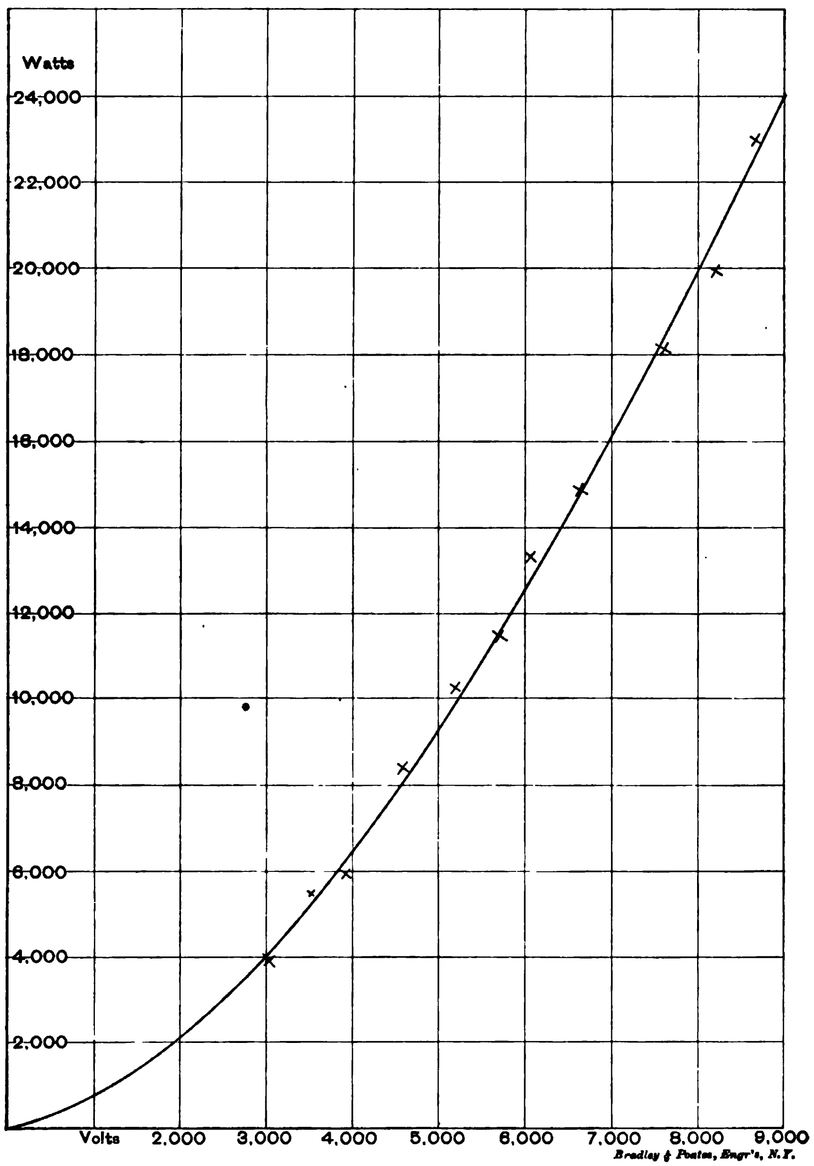


FIG. 1.

Other observations made afterwards agreed with this result.
With regard to magnetic hysteresis, essentially new discoveries

have not been made since, and the explanation of this exponent 1.6 is still unknown.

In the calculation of the core losses in dynamo electrical machinery and in transformers, the law of hysteresis has found its application, and so far as it is not obscured by the superposition of eddy currents has been fully confirmed by practical experience.

As an instance is shown in Fig. 1, the observed core loss of a high voltage 500 K. W. alternate current generator for power transmission. The curve is plotted with the core loss as abscissæ and the terminal volts as ordinates. The observed values are marked by crosses, while the curve of 1.6 power is shown by the drawn line.

The core loss is a very large and in alternators like the present machine, even the largest part of the total loss of energy in the machine.

With regard to the numerical values of the coefficient of hysteresis, the observations up to the time of my last paper cover the range,

$$\eta \times 10^3 =$$

Materials	From	To	Average.
Wrought iron ... / Sheet iron and sheet steel ...	2.00	5.48	3.0 to 3.3
Cast iron ...	11.3	16.2	13.0
Soft cast steel and mitis metal ...	3.18	• 12 0	6.0
Hard cast steel ...	...	27.9	
Welded steel ...	14.5	74.8	
Magnetite ...	20.4	23.5	
Nickel ...	12.2	38.5	
Cobalt ...	11.9		

While no new materials have been investigated in the meantime, for some, especially sheet iron and sheet steel, the range of observed value of η has been greatly extended, and, I am glad to state, mostly towards lower value of η, that is, better iron.

While at the time of my former paper, the value of hysteresis $\eta \times 10^3 = 2.0$, taken from Ewing's tests, was unequaled, and the best material I could secure, a very soft Norway iron, gave $\eta \times 10^3 = 2.275$, now quite frequently values considerably better than Ewing's soft iron wire are found, as the following table shows, which gives the lowest and the highest values of hysteretic loss observed in sheet iron and sheet steel, intended for electrical machinery.

The values are taken at random from the factory records of the General Electric Company.

Values of $\eta \times 10^3$.

Lowest.	Highest.
1.24	5.30
1.33	5.15
1.35	5.12
1.58	4.78
1.59	4.77
1.59	4.72
1.66	4.58
1.66	4.55
1.68	4.27
1.70	
1.71	
1.76	
1.80	
1.82	
1.88	
1.90	
1.93	
1.94	
1.94	

As seen, all the values of the first column refer to iron superior in its quality even to the sample of Ewing $\eta \times 10^3 = 2.0$, unequaled before.

The lowest value is $\eta \times 10^3 = 1.24$, that is, 38 per cent. better than Ewing's iron. A sample of this iron I have here. As you see, it is very soft material. Its chemical analysis does not show anything special. The chemical constitution of the next best sample $\eta \times 10^3 = 1.33$ is almost exactly the same as the constitution of samples $\eta \times 10^3 = 4.77$ and $\eta \times 10^3 = 3.22$, showing quite conclusively that the chemical constitution has no direct influence upon the hysteretic loss.

In consequence of this extension of η towards lower values, the total range of η yet known in iron and steel is from $\eta \times 10^3 = 1.24$ in best sheet iron to $\eta \times 10^3 = 74.8$ in glass-hard steel, and $\eta \times 10^3 = 81.8$ in manganese steel, giving a ratio of 1 to 66.

With regard to the exponent $\mathcal{H}$ in

$$H = \eta \, B^{\mathcal{H}},$$

which I found to be approximately $= 1.6$ over the whole range of magnetization, Ewing has investigated its variation, and found that it varies somewhat at different magnetizations, and that its variation corresponds to the shape of the magnetization curve, showing its three stages.[1]

1. J. A. Ewing, *Philosophical Transactions of the Royal Society*, London, June 15, 1893.

Tests of the variation of the hysteretic loss per cycle as function of the temperature have been published by Dr. W. Kunz[1], for temperatures from 20° and 800° Cent. They show that with rising temperature, the hysteretic loss decreases very greatly, and this decrease consists of two parts, one part, which disappears again with the decrease of temperature and is directly proportional to the increase of temperature, thus making the hysteretic loss a linear function of the temperature, and another part, which has become permanent, and seems to be due to a permanent change of the molecular structure produced by heating. This latter part is in soft iron, proportional to the temperature also, but irregular in steel.

CHAPTER II.—MOLECULAR FRICTION AND MAGNETIC HYSTERESIS.

In an alternating magnetic circuit in iron and other magnetic material, energy is converted into heat by molecular magnetic friction. The area of the hysteretic loop, with the M. M. F. as abscissæ and the magnetization as ordinates, represents the energy expended by the M. M. F. during the cyclic change of magnetization.

If energy is neither consumed nor applied outside of the magnetic circuit by any other source, the area of the hysteretic loop, *i. e.*, the energy consumed by hysteresis, measures and represents the energy wasted by molecular magnetic friction.

In general, however, the energy expended by the M. M. F.—the area of the hysteretic loop—needs not to be equal to the molecular friction. In the armature of the dynamo machine, it probably is not, but, while the hysteretic loop more or less collapses under the influence of mechanical vibration, the loss of energy by molecular friction remains the same, hence is no longer measured by the area of the hysteretic loop.

Thus a sharp distinction is to be drawn between the phenomenon of magnetic hysteresis, which represents the expenditure of energy by the M. M. F., and the molecular friction.

In stationary alternating current apparatus, as ferric inductances, hysteretic loss and molecular magnetic friction are generally identical.

In revolving machinery, the discrepancy between molecular friction and magnetic hysteresis may become very large, and the magnetic loop may even be overturned and represent, not expen-

1. *Elektrotechnische Zeitschrift*, April 5th, 1894.

diture, but production of electrical energy from mechanical energy; or inversely, the magnetic loop may represent not only the electrical energy converted into heat by molecular friction, but also electrical energy converted into mechanical motion.

Two such cases are shown in Figs. 2 and 3 and in Figs. 4 and 5. In these cases the magnetic reluctance and thus the inductance of the circuit was variable. That is, the magnetic circuit was opened and closed by the revolution of a shuttle-shaped armature.

The curve *s* represents the inductances of the magnetic circuit

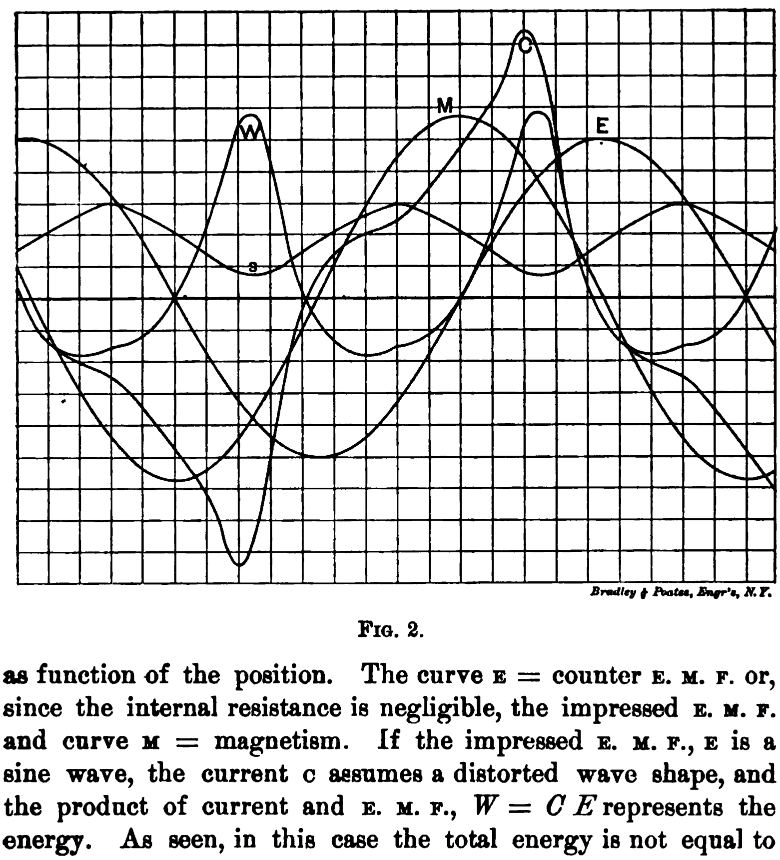


FIG. 2.

as function of the position. The curve E = counter E. M. F. or, since the internal resistance is negligible, the impressed E. M. F. and curve M = magnetism. If the impressed E. M. F., E is a sine wave, the current C assumes a distorted wave shape, and the product of current and E. M. F., $W = C E$ represents the energy. As seen, in this case the total energy is not equal to zero, *i. e.*, the E. M. F. or self-induction E not wattless as usually supposed, but represents production of electrical energy in the first, consumption in the second case. Thus, if the apparatus is driven by exterior power, it assumes the phase relation shown in

Fig. 2, and yields electrical energy as a self-exciting alternate current generator; if now the driving power is withdrawn it drops into the phase relation shown in Fig. 4, and then continues to rèvolve and to yield mechanical energy as a synchronous motor.

The magnetic cycles or H-B curves, or rather for convenience, the C-M curves, are shown in Figs. 3 and 5.

As seen in Fig. 5, the magnetic loop is greatly increased in area and represents not only the energy consumed by molecular magnetic friction, but also the energy converted into mechanical power, while the loop in Fig. 3 is overturned or negative, thus representing the electrical energy produced, minus loss by molecular friction.

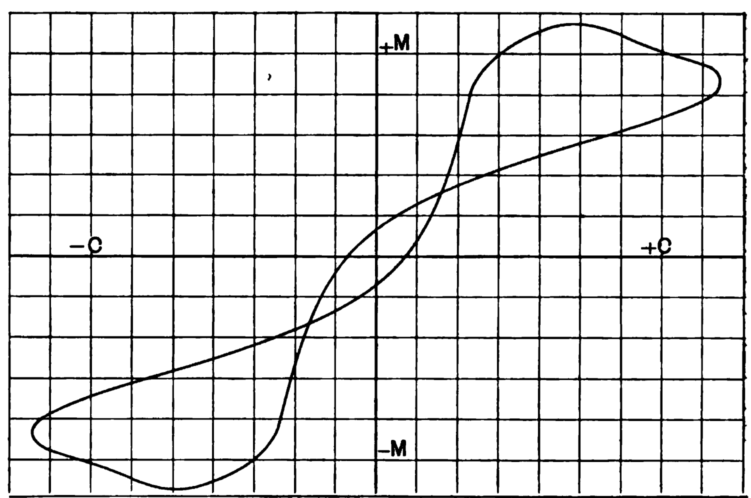


FIG. 3.

This is the same apparatus, of which two hysteretic loops were shown in my last paper, an indicator-alternator of the "humming bird" type.

Thus magnetic hysteresis is not identical with molecular magnetic friction, but is one of the phenomena caused by it.

CHAPTER III.—THEORY AND CALCULATION OF FERRIC INDUCTANCES.

In the discussion of inductive circuits, generally the assumption is made, that the circuit contains no iron. Such non-ferric inductances are, however, of little interest, since inductances are almost always ironclad or ferric inductances.

With our present knowledge of the alternating magnetic circuit, the ferric inductances can now be treated analytically with the same exactness and almost the same simplicity as non-ferric inductances.

Before entering into the discussion of ferric inductances, some terms will be introduced, which are of great value in simplifying the treatment.

Referring back to the continuous current circuit, it is known that, if in a continuous current circuit a number of resistances,

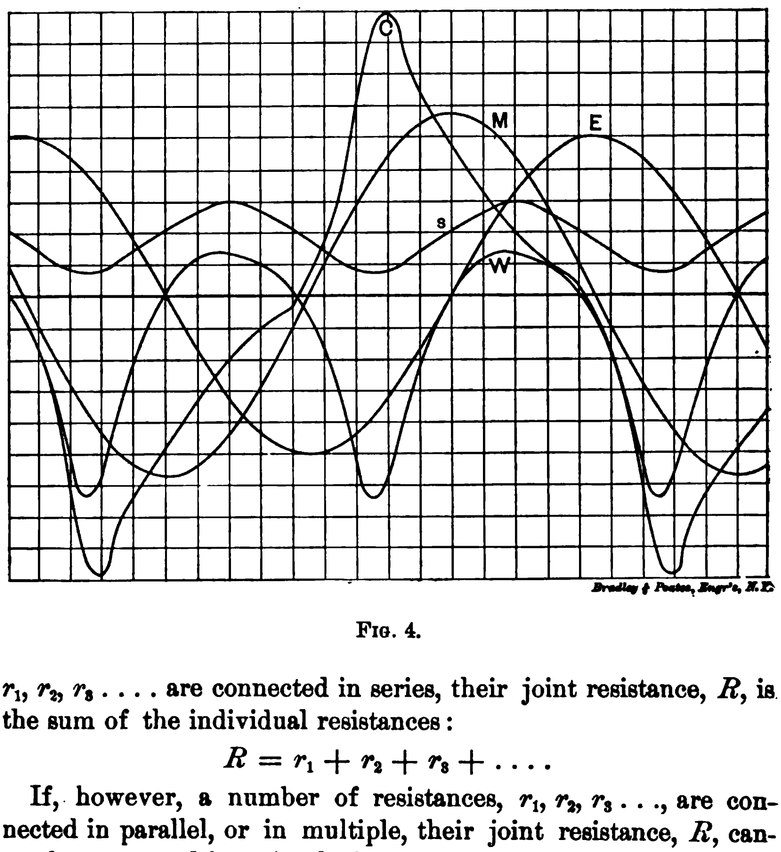


FIG. 4.

$r_1, r_2, r_3 \ldots .$ are connected in series, their joint resistance, R, is the sum of the individual resistances:

$$R = r_1 + r_2 + r_3 + \ldots .$$

If, however, a number of resistances, $r_1, r_2, r_3 \ldots$, are connected in parallel, or in multiple, their joint resistance, R, cannot be expressed in a simple form, but is:

$$R = \frac{1}{\frac{1}{r^1} + \frac{1}{r_2} + \frac{1}{r_3} + \ldots}$$

Hence, in the latter case, it is preferable, instead of the term "resistance," to introduce its reciprocal, or inverse value, the term "*conductance*" $\rho = \frac{1}{r}$. Then we get:

"If a number of conductances, $\rho_1, \rho_2, \rho_3 \ldots$, are connected in parallel, their joined conductance is the sum of the individual conductances:

$$P = \rho_1 + \rho_2 + \rho_3 + \ldots$$

When using the term conductance, the joined conductance of

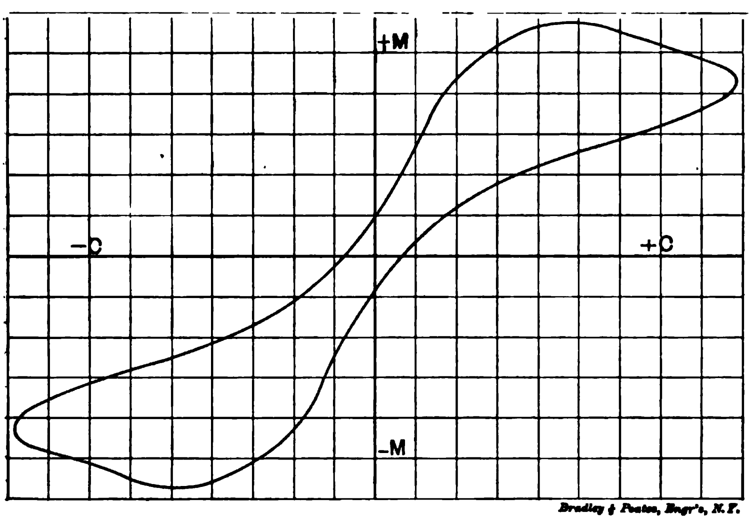


FIG. 5.

a number of series connected conductances, $\rho_1, \rho_2, \rho_3 \ldots$ becomes a complicated expression:

$$P = \frac{1}{\frac{1}{\rho_1} + \frac{1}{\rho_2} + \frac{1}{\rho_3} + \ldots}$$

Hence the use of the term "resistance" is preferable in the case of series connection, the use of the reciprocal term "conductance," in parallel connection, and we have thus:

"*The joined resistance of a number of series connected resistances is equal to the sum of the individual resistances, the joined conductance of a number of parallel connected conductances is equal to the sum of the individual conductances.*"

In alternating current circuits, in place of the term "resistance" we have the term "*impedance*," expressed in complex quantities by the symbol:

$$U = r - j\,s,$$

with its two components, the "*resistance*" r and the "*reactance*" s, in the formula of Ohm's law:

$$E = C\,U.$$ [1]

The resistance, r, gives the coefficient of the E. M. F. in phase with the current, or the energy component of E. M. F., $C\,r$; the reactance, s, gives the coefficient of the E. M. F. in quadrature with the current, or the wattless component of E. M. F., $C\,s$, both combined give the total E. M. F.

$$C\,u = C\,\sqrt{r^2 + s^2}.$$

This *reactance*, s, is positive as inductive reactance:

$$s = 2\,\pi\,N\,L,$$

or negative as capacity reactance:

$$s = -\frac{1}{2\,\pi\,N\,K},$$

where,

N = frequency,

L = coefficient of self-induction, in henrys,

K = capacity, in farads.

Since E. M. F.'s are combined by adding their complex expressions, we have:

"The joined impedance of a number of series connected impedances, is the sum of the individual impedances, when expressed in complex quantities."

In graphical representation, impedances have not to be added, but combined in their proper phase, by the law of parallelogram, like the E. M. F.'s consumed by them.

The term "impedance" becomes inconvenient, however, when dealing with parallel connected circuits, or, in other words, when several currents are produced by the same E. M. F., in cases where Ohm's law is expressed in the form:

$$C = \frac{E}{U}.$$

It is preferable then, to introduce the reciprocal of "impe-

1. "Complex Quantities and their use in Electrical Engineering," a paper read before Section A of the International Electrical Congress at Chicago, 1893.

dance," which may be called the "*admittance*" of the circuit:

$$Y = \frac{1}{U}.$$

As the reciprocal of the complex quantity

$$U = r - j\, s,$$

the admittance is a complex quantity also:

$$Y = \rho + j\, \sigma,$$

consisting of the component, ρ, which represents the coefficient of current in phase with the E. M. F., or energy current, $\rho\, E$, in the equation of Ohm's law:

$$C = Y E = (\rho + j\, \sigma)\, E,$$

and the component, σ, which represents the coefficient of current in quadrature with the E. M. F., or wattless component of current, $\sigma\, E$.

ρ may be called the "*conductance*," σ the "*susceptance*" of the circuit. Hence the conductance, ρ, is the energy component, the susceptance, σ, the wattless component of the admittance

$$Y = \rho + j\, \sigma,$$

and the numerical value of admittance is:

$$v = \sqrt{\rho^2 + \sigma^2};$$

the resistance, r, is the energy component, the reactance, s, the wattless component of the impedance

$$U = r - j\, s,$$

and the numerical value of impedance is

$$u = \sqrt{r^2 + s^2}.$$

As seen, the term "admittance" means dissolving the current into two components, in phase and in quadrature with the E. M. F., or the energy current and the wattless current; while the term "impedance" means dissolving the E. M. F. into two components, in phase and in quadrature with the current, or the energy E. M. F. and the wattless E. M. F.

It must be understood, however, that the "conductance" is not the reciprocal of the resistance, but depends upon the resistance as well as upon the reactance. Only when the reactance $s = 0$, or in continuous current circuits, is the conductance the reciprocal of resistance.

Again, only in circuits with zero resistance $r = 0$, is the susceptance the reciprocal of reactance; otherwise the susceptance depends upon reactance and upon resistance.

From the definition of the admittance:

$$Y = \rho + j\,\sigma$$

as the reciprocal of the impedance:

$$U = r - j\,s$$

we get

$$Y = \frac{1}{U},$$

or

$$\rho + j\,\sigma = \frac{1}{r - j\,s}$$

or, multiplying on the right side numerator and denominator by $(r + j\,s)$:

$$\rho + j\,\sigma = \frac{r + j\,s}{(r - j\,s)\,(r + j\,s)},$$

hence, since

$$(r - j\,s)\,(r + j\,s) = r^2 + s^2 = u^2:$$

$$\rho + j\,\sigma = \frac{r}{r^2 + s^2} + j\,\frac{s}{r^2 + s^2} = \frac{r}{u^2} + j\,\frac{s}{u^2},$$

or,

$$\rho = \frac{r}{r^2 + s^2} = \frac{r}{u^2}$$

$$\sigma = \frac{s}{r^2 + s^2} = \frac{s}{u^2},$$

and inversely:

$$r = \frac{\rho}{\rho^2 + \sigma^2} = \frac{\rho}{v^2}$$

$$s = \frac{\sigma}{\rho^2 + \sigma^2} = \frac{\sigma}{v^2}.$$

By these equations, from resistance and reactance, the conductance and susceptance can be calculated, and inversely.

Multiplying the equations for ρ and r, we get:

$$\rho\,r = \frac{r\,\rho}{u^2\,v^2},$$

hence,

$$u^2\,v^2 = (r^2 + s^2)\,(\rho^2 + \sigma^2) = 1,$$

and

$$u = \frac{1}{v} = \frac{1}{\sqrt{\rho^2 + \sigma^2}}$$

the absolute value of impedance,

$$v = \frac{1}{u} = \frac{1}{\sqrt{r^2 + s^2}}$$

the absolute value of admittance.

The sign of "admittance" is always opposite to that of "impedance," that means, if the current lags behind the E. M. F., the E. M. F. leads the current, and inversely, as obvious.

Thus we can express Ohm's law in the two forms:

$$E = C\,U.$$
$$C = E\,Y,$$

and have

"*The joined impedance of a number of series connected impedances is equal to the sum of the individual impedances; the joined admittance of a number of parallel connected admittances is equal to the sum of the individual admittances, if expressed in complex quantities; in diagrammatic representation, combination by the parallelogram law takes the place of addition of the complex quantities.*"

The resistance of an electric circuit is determined:

1. By direct comparison with a known resistance (Wheatstone bridge method, etc.). This method gives what may be called the true ohmic resistance of the circuit.

2. By the ratio:

$$\frac{\text{Volts consumed in circuit}}{\text{Amperes in circuit}}.$$

In an alternating current circuit, this method gives not the resistance, but the impedance

$$u = \sqrt{r^2 + s^2}$$

of the circuit.

3. By the ratio:

$$r = \frac{\text{Power consumed}}{(\text{current})^2} = \frac{(\text{E. M. F.})^2}{\text{Power consumed}},$$

where, however, the "power" and the "E. M. F." do not include the work done by the circuit, and the counter E. M. F.'s representing it, as for instance, the counter E. M. F. of a motor.

In alternating current circuits, this value of resistance is the energy coefficient of the E. M. F., and is:

$$r = \frac{\text{Energy component of E. M. F.}}{\text{Total current}}.$$

It is called the "*equivalent resistance*" of the circuit, and the energy coefficient of current:

$$\rho = \frac{\text{Energy component of current}}{\text{Total E. M. F.}},$$

is called the "*equivalent conductance*" of the circuit.

In the same way the value:

$$s = \frac{\text{Wattless component of E. M. F.}}{\text{Total current}}$$

is the "*equivalent reactance*," and

$$\sigma = \frac{\text{Wattless component of current}}{\text{Total E. M. F.}}$$

is the "*equivalent susceptance*" of the circuit.

While the true ohmic resistance represents the expenditure of energy as heat, inside of the electric conductor, by a current of uniform density, the "equivalent resistance" represents the total expenditure of energy.

Since in an alternating current circuit in general, energy is expended not only in the conductor, but also outside thereof, by hysteresis, secondary currents, etc., the equivalent resistance frequently differs from the true ohmic resistance, in such way as to represent a larger expenditure of energy.

In dealing with alternating current circuits, it is necessary, therefore, to substitute everywhere the values "equivalent resistance," "equivalent reactance," "equivalent conductance," "equivalent susceptance," to make the calculation applicable to general alternating current circuits, as ferric inductance, etc.

While the true ohmic resistance is a constant of the circuit, depending upon the temperature only, but not upon the E. M. F., etc., the "equivalent resistance" and "equivalent reactance" is in general not a constant, but depends upon the E. M. F., current, etc.

This dependence is the cause of most of the difficulties met in dealing analytically with alternating current circuits containing iron.

The foremost sources of energy loss in alternating current circuits, outside of the true ohmic resistance loss, are:

1. Molecular friction, as:
   (*a*) magnetic hysteresis;
   (*b*) dielectric hysteresis.

2. Primary electric currents, as:
   (*a*) leakage or escape of current through the insulation, brush discharge;
   (*b*) eddy-currents in the conductor, or unequal current distribution.
3. Secondary or induced currents, as:
   (*a*) eddy or Foucault currents in surrounding magnetic materials;
   (*b*) eddy or Foucault currents in surrounding conducting materials;
   (*c*) secondary currents of mutual inductance in neighboring circuits.
4. Induced electric charges, electro-static influence.

While all these losses can be included in the terms "equivalent resistance," etc., only the magnetic hysteresis and the eddy-currents in the iron will form the object of the present paper.

I.—*Magnetic Hysteresis.*

To examine this phenomenon, first a circuit of very high inductance, but negligible true ohmic resistance may be considered, that is, a circuit entirely surrounded by iron; for instance, the primary circuit of an alternating current transformer with open secondary circuit.

The wave of current produces in the iron an alternating magnetic flux, which induces in the electric circuit an E. M. F., the counter E. M. F. of self-induction. If the ohmic resistance is negligible, the counter E. M. F. equals the impressed E. M. F., hence, if the impressed E. M. F. is a sine-wave, the counter E. M. F., and therefore the magnetism which induces the counter E. M. F. must be sine-waves also. The alternating wave of current is not a sine-wave in this case, but is distorted by hysteresis. It is possible, however, to plot the current wave in this case from the hysteretic cycle of magnetization.

From the number of turns n of the electric circuit, the effective counter E. M. F. E, and the frequency N of the current, the maximum magnetic flux M is found by the formula:

$$E = \sqrt{2}\, \pi\, n\, N\, M\, 10^{-8};$$

hence:

$$M = \frac{E\, 10^{8}}{\sqrt{2}\, \pi\, n\, N}.$$

Maximum flux M and magnetic cross-section S give the maximum magnetic induction $B = \frac{M}{S}$.

If the magnetic induction varies periodically between $+B$ and $-B$, the M. M. F. varies between the corresponding values $+F$ and $-F$, and describes a looped curve, the cycle of hysteresis.

If the ordinates are given in lines of magnetic force, the abscissæ in tens of ampere-turns, the area of the loop equals the energy consumed by hysteresis, in ergs per cycle.

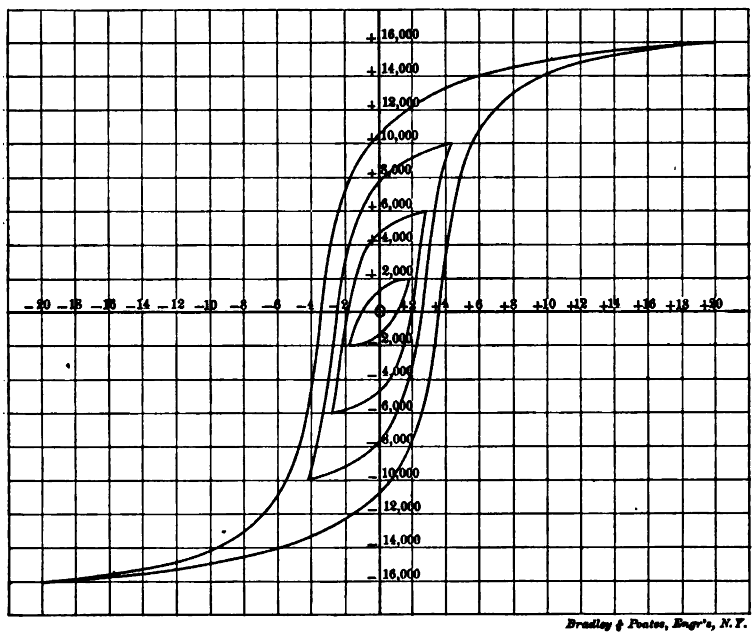


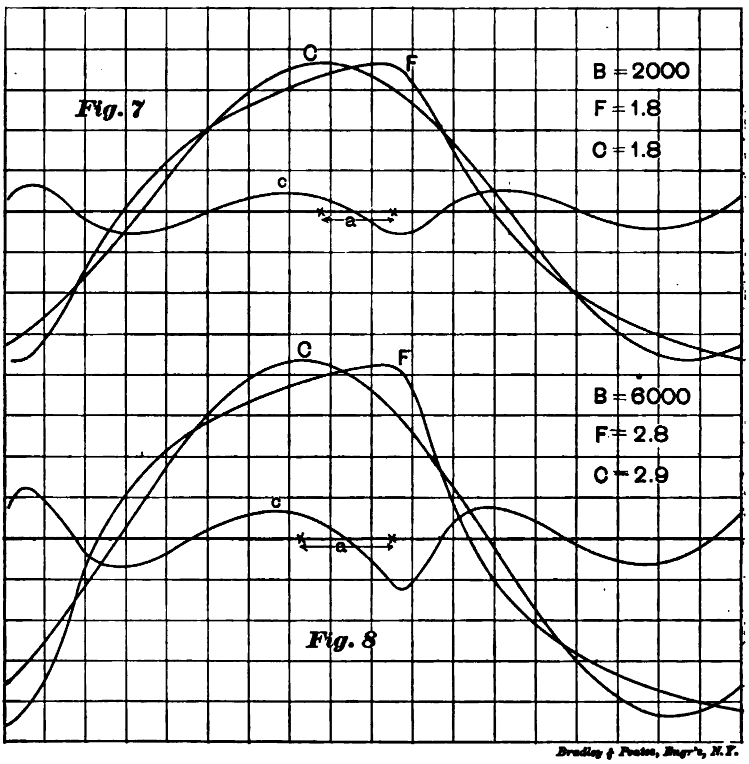
FIG. 6.

From the hysteretic loop is found the instantaneous value of M. M. F. corresponding to an instantaneous value of magnetic flux, that is of induced E. M. F., and from the M. M. F., F, in ampere-turns per unit length of magnetic circuit, the length l of the magnetic circuit, and the number of turns n of the electric circuit, are found the instantaneous values of current c corresponding to a M. M. F. F, that is a magnetic induction B and thus induced E. M. F. e, as:

$$c = \frac{F l}{n}.$$

In Fig. 6 four magnetic cycles are plotted, with the maximum values of magnetic inductions: B = 2,000, 6,000, 10,000 and 16,000, and the corresponding maximum M. M. F.'s: F = 1.8, 2.8, 4.3, 20.0. They show the well-known hysteretic loop, which becomes pointed when magnetic saturation is approached.

These magnetic cycles correspond to average good sheet iron or sheet steel of hysteretic coefficient: η = .0033, and are given

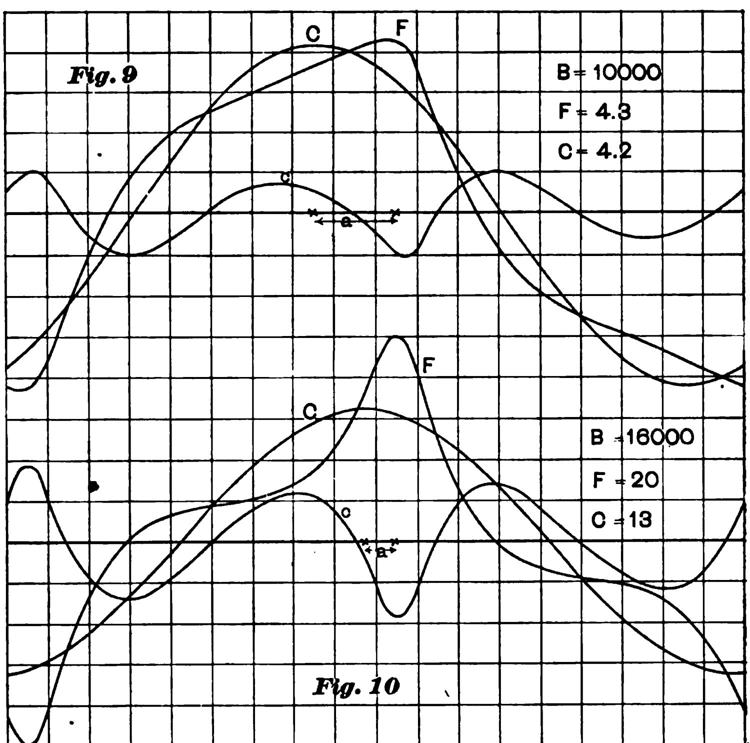


with ampere-turns per cm. as abscissæ, and kilolines of magnetic force as ordinates.

In Figs. 7, 8, 9 and 10 the magnetism, or rather the magnetic induction, as derived from the induced E. M. F., is assumed as sine-curve. For the different values of magnetic induction of this sine-curve, the corresponding values of M. M. F., hence of current, are taken from Fig. 6, and plotted, giving thus the exciting current required to produce the sine-wave of magnetism;

that is, the wave of current, which a sine-wave of impressed E. M. F. will send through the circuit.

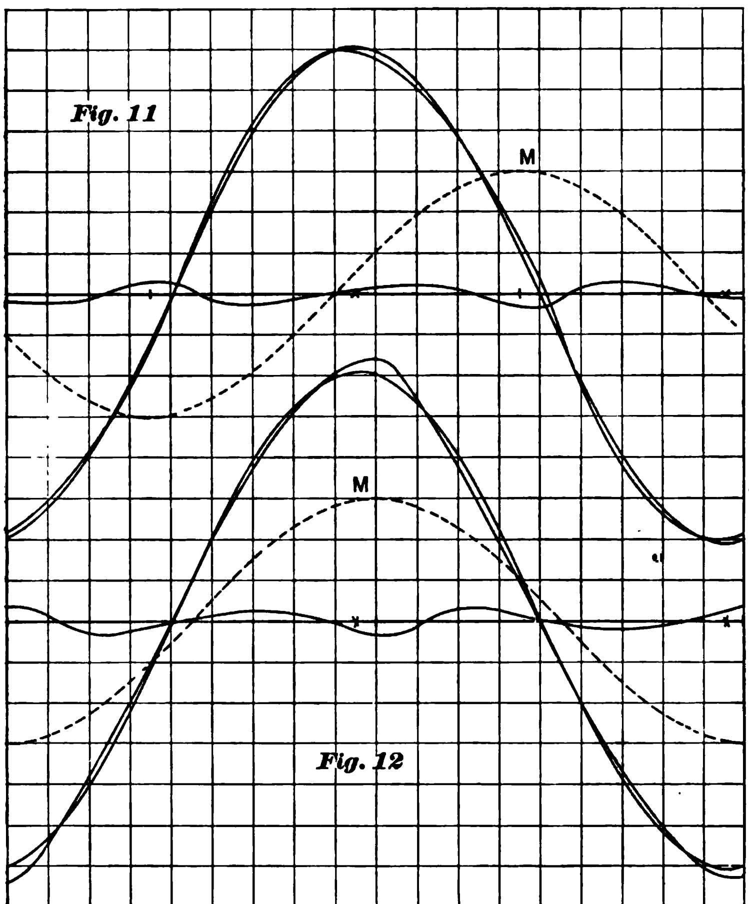
As seen from Figs. 4 to 10, these waves of alternating current *F* are not sine-waves, but are distorted by the superposition of higher harmonics, that is, are complex harmonic waves. They reach their maximum value at the same time with the maximum of magnetism, that is, 90° ahead of the maximum induced E. M. F.,



Bradley & Poates, Engr's, N.Y.

hence about 90° behind the maximum impressed E. M. F., but pass the zero line considerably ahead of the zero value of magnetism: 42, 52, 50 and 41 degrees respectively.

The general character of these current waves is, that the maximum point of the wave coincides in time with the maximum point of the sine-wave of magnetism, but the current wave is bulged out greatly at the rising; hollowed in at the decreasing side. With increasing magnetization, the maximum of the current wave becomes more pointed, as the curve of Fig. 9, for $B = 10{,}000$ shows, and at still higher saturation a peak is formed at the maximum point, as in the curve of Fig. 10, for $B = 16{,}000$. This is the case, when the curve of magnetization reaches within the range of magnetic saturation, since in the proximity of saturation



the current near the maximum point of magnetization has to rise abnormally, to cause a small increase of magnetization only.

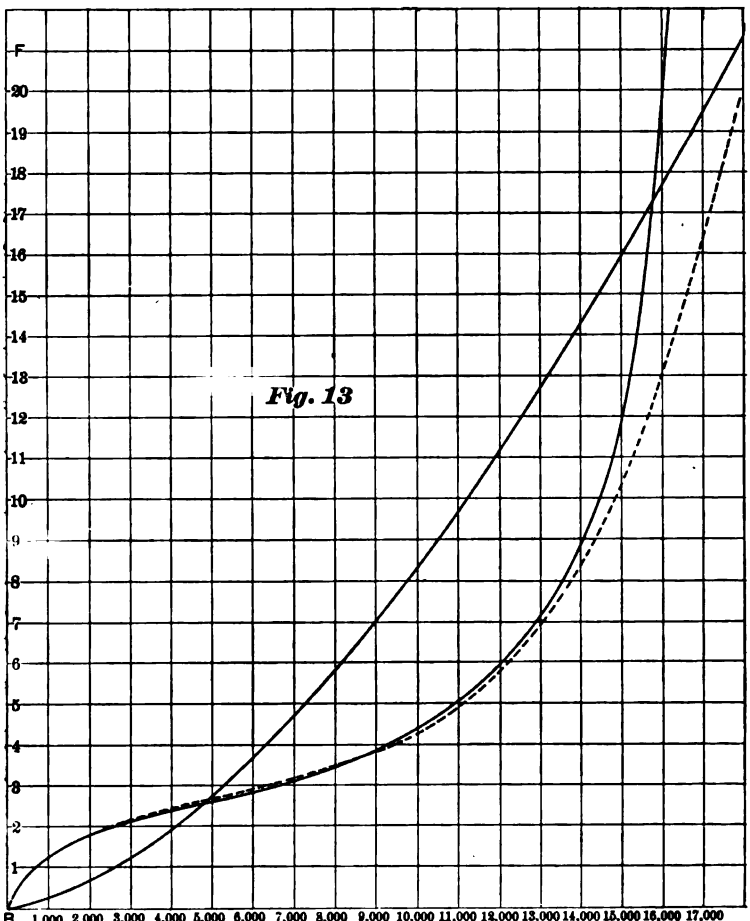
The distortion of the wave of magnetizing current is so large as shown here, only in an iron closed magnetic circuit expending energy by hysteresis only, as in the ironclad transformer at open secondary circuit. As soon as the circuit expends energy in any other way, as in resistance, or by mutual inductance, or if an air-gap is introduced in the magnetic circuit, the distortion of the current wave rapidly decreases and practically disappears, and the current becomes more sinusoidal. That is, while the distorting component remains the same, the sinusoidal component of current greatly increases, and obscures the distortion. For instance, in Figs. 11 and 12 two waves are shown, corresponding in magnetization to the curve of Fig. 8, as the worst distorted. The curve in Fig. 11 is the current wave of a transformer at $\frac{1}{10}$ load. At higher load the distortion is still correspondingly less. The curve of Fig. 12 is the exciting current of a magnetic circuit, containing an air-gap, whose length equals $\frac{1}{400}$ the length of the magnetic circuit. These two curves are drawn in $\frac{1}{3}$ the size of the curve in Fig. 8. As seen, both curves are practically sine-waves.

The distorted wave of current can be dissolved in two components: *a true sine-wave of equal effective intensity and equal power with the distorted wave*, called the "*equivalent sine-wave*," and a *wattless higher harmonic*, consisting chiefly of a term of triple frequency.

In Figs. 7 to 12 are shown, in drawn lines, the equivalent sine-waves, and the wattless complex higher harmonics, which together form the distorted current wave. The equivalent sine-wave of M. M. F., or of current, in Figs. 7 to 10, leads the magnetism by 34, 44, 38 and 15.5 degrees respectively. In Figs. 11 and 12 the equivalent sine-wave almost coincides with the distorted curve, and leads the magnetism by only 9°.

It is interesting to note, that even in the greatly distorted curves of Figs. 7 to 9 the maximum value of the equivalent sine-wave is nearly the same as the maximum value of the original distorted wave of M. M. F., as long as magnetic saturation is not approached, being 1.8, 2.9 and 4.2 respectively, against 1.8, 2.8 and 4.3 as maximum values of the distorted curve. Since by the definition the effective value of the equivalent sine-wave is the same as that of the distorted wave, this means, that the distorted wave of exciting current shares with the sine-wave the feature, that the maximum value and the effective value have the ratio: $\sqrt{2} \div 1$. Hence, below saturation, the maximum value of the distorted curve can be calculated from the effective value—which is given by the reading of an electro-dynamometer—by the same ratio as with a true sine-wave, and the magnetic characteristic can thus be determined by means of alternating currents, by the electro-dynamometer method, with sufficient exactness.

In Fig. 13 is shown the true magnetic characteristic of a sample of average good sheet iron, as found by the method of slow



reversals by the magnetometer, and for comparison in dotted lines the same characteristic, as determined by alternating currents, by the electro-dynamometer, with ampere-turns per cm. as ordinates, and magnetic inductions as abscissæ. As seen, the two curves practically coincide up to $B = 10{,}000 \sim 14{,}000$.

For higher saturations, the curves rapidly diverge, and the electro-dynamometer curve shows comparatively small M. M. F.'s producing apparently very high magnetizations.

The same Fig. 13 gives the curve of hysteretic loss, in ergs per $cm.^3$ and cycle, as ordinates, and magnetic inductions as abscissæ.

So far as current strength and energy consumption is concerned, the distorted wave can be replaced by the equivalent sine-wave, and the higher harmonics neglected.

All the measurements of alternating currents, with the only exception of instantaneous readings, yield the equivalent sine-wave only, but suppress the higher harmonic, since all measuring instruments give either the mean square of the current wave, or the mean product of instantaneous values of current and E. M. F., which are by definition the same in the equivalent sine-wave as in the distorted wave.

Hence, in all practical applications, it is permissible to neglect the higher harmonic altogether, and replace the distorted wave by its equivalent sine-wave, keeping in mind, however, the existence of a higher harmonic as a possible disturbing factor, which may become noticeable in those very infrequent cases, where the frequency of the higher harmonic is near the frequency of resonance of the circuit.

The equivalent sine-wave of exciting current leads the sine-wave of magnetism by an angle a, which is called the "*angle of hysteretic advance of phase.*" Hence the current lags behind the E. M. F. by $90° - a$, and the power is, therefore:

$$P = C\,E \cos (90° - a) = C\,E \sin a.$$

Thus the *exciting current* C consists of an energy component: $C \sin a$, which is called the "*hysteretic energy current,*" and a wattless component: $C \cos a$, which is called the "*magnetizing current.*" Or inversely, the E. M. F. consists of an energy component: $E \sin a$, the "*hysteretic energy* E. M. F.", and a wattless component: $E \cos a$, the "E. M. F. *of self-induction.*"

Denoting the absolute value of the impedance of the circuit $\frac{E}{C}$, by u—where u is determined by the magnetic characteristic of the iron, and the shape of the magnetic and electric circuit—the impedance is represented, in phase and intensity, by the symbolic expression:

$$U = r - j\,s = u \sin a - j\,u \cos a,$$

and the admittance by:

$$Y = \rho + j\,\sigma = \frac{1}{u}\sin\alpha + j\,\frac{1}{u}\cos\alpha = v\sin\alpha + j\,v\cos\alpha.$$

The quantities: u, r, s and v, ρ, σ are not constants, however, in this case, as in the circuit without iron, but depend upon the intensity of magnetization, B, that is, upon the E. M. F.

This dependence complicates the investigation of circuits containing iron.

In a circuit entirely enclosed by iron, α is quite considerable, from 30 to 50 degrees for values below saturation. Hence even with negligible true ohmic resistance *no great lag can be produced in ironclad alternating current circuits.*

As I have proved, the loss of energy by hysteresis due to molecular friction is with sufficient exactness proportional to the 1.6th power of magnetic induction, B. Hence, it can be expressed by the formula:

$$H = \eta\,B^{1.6},$$

where

H = loss of energy per cycle, in ergs or (C. G. S.) units ($= 10^{-7}$ Joules) per cm.^{3},

B = maximum magnetic induction, in lines of force per cm.^{2}, and,

η = the "coefficient of hysteresis."

At the frequency, N, in the volume, V, the loss of power is by this formula:

$$P = \eta\,N\,V\,B^{1.6}\,10^{-7} \text{ watts,}$$
$$= \eta\,N\,V\left(\frac{M}{S}\right)^{1.6} 10^{-7} \text{ watts,}$$

where S is the cross-section of the total magnetic flux, M.

The maximum magnetic flux, M, depends upon the counter E. M. F. of self-induction, E, by the equation:

$$E = \sqrt{2}\,\pi\,N\,n\,M\,10^{-8},$$

or,

$$M = \frac{E\,10^{8}}{\sqrt{2}\,\pi\,N\,n},$$

where n = number of turns of the electric circuit.

Substituting this in the value of the power, P, and cancelling, we get:

$$P = \eta\,\frac{E^{1.6}}{N^{.6}}\,\frac{V\,10^{5.8}}{2^{.8}\,\pi^{1.6}\,S^{1.6}\,n^{1.6}} = 58\,\eta\,\frac{E^{1.6}}{N^{1.6}}\,\frac{V\,10^{3}}{S^{1.6}\,n^{1.6}}$$

or

$$P = a\,\frac{E^{1.6}}{N^{.6}}, \text{ where: } a = \eta\,\frac{V\,10^{5.8}}{2^{.6}\,\pi^{1.6}\,S^{1.6}\,n^{1.6}} = 58\,\eta\,\frac{V\,10^{8}}{S^{1.6}\,n^{1.6}},$$

or, substituting

$$\eta = .0033:$$

$$a = 191.4\,\frac{V}{S^{1.6}\,n^{1.6}},$$

or, substituting

$$V = S\,L, \text{ where } L = \text{length of magnetic circuit:}$$

$$a = \eta\,\frac{L\,10^{5.8}}{2^{.6}\,\pi^{1.6}\,S^{.6}\,n^{1.6}} = \frac{58\,\eta\,L\,10^{8}}{S^{.6}\,n^{1.6}} = 191.4\,\frac{L}{S^{.6}\,n^{1.6}}$$

and

$$P = \frac{58\,\eta\,E^{1.6}\,L\,10^{8}}{N^{.6}\,S^{.6}\,n^{1.6}} = \frac{191.4\,E^{1.6}\,L}{N^{.6}\,S^{.6}\,n^{1.6}}.$$

As seen, the hysteretic loss is proportional to the 1.6th power of the E. M. F., inverse proportional to the 1.6th power of the number of turns, and inverse proportional to the .6th power of frequency, and of cross-section.

If ρ = equivalent conductance, the energy component of current is $C = E\,\rho$, and the energy consumed in conductance ρ is:

$$P = C\,E = E^2\,\rho.$$

Since, however,

$$P = a\,\frac{E^{1.6}}{N^{.6}},$$

it is:

$$a\,\frac{E^{1.6}}{N^{.6}}\,E^2\,\rho,$$

or,

$$\rho = \frac{a}{N^{.6}\,E^{.4}} = \frac{58\,\eta\,L\,10^{8}}{E^{.4}\,N^{.6}\,S^{.6}\,n^{1.6}} = 191.4\,\frac{L}{E^{.4}\,N^{.6}\,S^{.6}\,n^{1.6}}.$$

That is:

"*The equivalent conductance due to magnetic hysteresis, is proportional to the coefficient of hysteresis, η, and to the length of the magnetic circuit, L, and inverse proportional to the .4th power of the E. M. F., E, to the .6th power of the frequency, N, and of the cross-section of the magnetic circuit, S, and to the 1.6th power of the number of turns, n.*"

Hence, the equivalent hysteretic conductance increases with decreasing E. M. F., and decreases with increasing E. M. F.; it varies, however, much slower than the E. M. F., so that, if the hysteretic conductance represents only a part of the total energy consumption, it can within a limited range of variation, as for instance, in constant potential transformers, without serious error be assumed as constant.

If:

P = magnetic reluctance of a circuit,

F = maximum M. M. F.,

C = effective current, hence

$C\sqrt{2}$ = maximum current, it is the magnetic flux:

$$M = \frac{F}{P} = \frac{n\,C\,\sqrt{2}}{P}.$$

Substituting this in the equation of the counter E. M. F. of self-induction:

$$E = \sqrt{2}\,\pi\,N\,n\,M\,10^{-8},$$

it is:

$$E = \frac{2\,\pi\,n^2\,N\,C\,10^{-8}}{P},$$

hence, the absolute admittance of the circuit:

$$v = \sqrt{\rho^2 + \sigma^2} = \frac{C}{E} = \frac{P\,10^8}{2\,\pi\,n^2\,N} = \frac{b\,P}{N},$$

where

$$b = \frac{10^8}{2\,\pi\,n^2}$$ is a constant.

Thus:

"*The absolute admittance, v, of a circuit of negligible resistance is proportional to the magnetic reluctance, P, and inverse proportional to the frequency, N, and to the square of the number of turns, n.*"

In a circuit containing iron, the reluctance, P, varies with the magnetization, that is, with the E. M. F. Hence, the admittance of such a circuit is not a constant, but is variable also.

In an ironclad electric circuit, that is, a circuit whose magnetic field exists entirely within iron, as the magnetic circuit of a well-designed alternating current transformer, P, is the reluctance of the iron circuit. Hence, if μ = permeability, since,

$$P = \frac{\mathfrak{F}}{M},$$

and

$$\mathfrak{F} = L\,F = \frac{10}{4\,\pi}\,L\,H = \text{M. M. F.},$$

$$M = S\,B = \mu\,S\,H = \text{magnetism},$$

it is:

$$P = \frac{10\,L}{4\,\pi\,\mu\,S},$$

and, substituting this value in the equation of the admittance:

$$v = \frac{P\,10^8}{2\,\pi\,n^2\,N} = \frac{L\,10^9}{8\,\pi^2\,n^2\,\mu\,S\,N} = \frac{d}{N\,\mu},$$

where:

$$d = \frac{L\,10^9}{8\,\pi^2\,n^2\,S} = \frac{127\,L\,10^6}{n^2\,S}.$$

Thus:

"*In an ironclad circuit, the absolute admittance, v, is inverse proportional to the frequency. N, to the permeablity, μ, the cross-section, S, and square of the number of turns, n, and directly proportional to the length of the magnetic circuit, L.*"

The conductance is:

$$\rho = \frac{a}{N^{.6}\,E^{.4}}$$

the admittance:

$$v = \frac{d}{N\,\mu};$$

hence, the angle of hysteretic advance:

$$\sin a = \frac{\rho}{v} = \frac{a\,\mu\,N^{.4}}{d\,E^{.4}};$$

or, substituting for a and d:

$$\sin a = \mu\,\frac{N^{.4}}{E^{.4}}\,\frac{\eta\,L\,10^{5.8}}{2^{.8}\,\pi^{1.6}\,S^{.6}\,n^{1.6}}\,\frac{8\,\pi^2\,n^2\,S}{L\,10^9}$$

$$= \frac{\mu\,\eta\,N^{.4}\,n^{.4}\,S^{.4}\,\pi^{.4}\,2^{2.2}}{E^{.4}\,10^{3.2}};$$

or, substituting:

$$E = 2^{.5}\,\pi\,N\,n\,S\,B\,10^{-8}:$$

$$\sin a = \frac{4\,\mu\,\eta}{B^{.4}};$$

hence, independent of frequency, number of turns, shape and size of magnetic and electric circuit.

Thus:

"*In an ironclad inductance, the angle of hysteretic advance, a, depends upon the magnetic constants: permeability and coefficient of hysteresis, and upon the maximum magnetic induction, but is entirely independent of the frequency, of the shape and other conditions of the magnetic and electric circuit, and,* *therefore, all the ironclad magnetic circuits constructed of the same quality of iron, and using the same magnetic density, give the same angle of hysteretic advance.*"

"The angle of hysteretic advance, α, in a closed circuit transformer, depends upon the quality of the iron, and the magnetic density only."

"The sine of the angle of hysteretic advance equals four times the product of permeability and coefficient of hysteresis, divided by the .4th power of the magnetic density:

$$\sin \alpha = \frac{4 \mu \eta}{B^{.4}}."$$

If the magnetic circuit is not entirely ironclad, but the magnetic structure contains air-gaps, the total reluctance is the sum of the iron reluctance and the air reluctance:

$$P = P_1 + P_a;$$

hence, the admittance is:

$$v = \sqrt{\rho^2 + \sigma^2} = \frac{b}{N}(P_1 + P_a),$$

or:

"In a circuit containing iron, the admittance is the sum of the admittance due to the iron part of the circuit:

$$v_1 = \frac{b}{N} P_1,$$

and the admittance due to the air part of the circuit:

$$v_a = \frac{b}{N} P_a,$$

if the iron and the air are in series in the magnetic circuit."

The conductance, ρ, represents the loss of energy in the iron, and, since air has no magnetic hysteresis, is not changed by the introduction of an air-gap.

Hence, the angle of hysteretic advance of phase is:

$$\sin \alpha = \frac{\rho}{v} = \frac{\rho}{v_1 + v_a} = \frac{\rho}{v_1} \frac{P_1}{P_1 + P_a},$$

and is a maximum $= \frac{\rho}{v_1}$, for the ironclad circuit, but decreases with increasing width of the air-gap. The introduction of the air-gap of reluctance, P_a, decreases $\sin \alpha$ in the ratio $\frac{P_1}{P_1 + P_a}$.

In the range of practical application, from B = 2,000 to

$B = 12{,}000$, the permeability of the iron varies between 900 and 2,000 approximately, while sin α in an ironclad circuit varies in this range from .51 to .69. In air, $u = 1$.

If, consequently, one per cent. of the length of the iron is replaced by an air-gap, the total reluctance varies only in the proportion of $1\frac{1}{9}$ to $1\frac{1}{20}$, or by about six per cent.; that is, is practically constant, while the angle of hysteretic advance varies from sin $\alpha = .035$ to sin $\alpha = .064$. Thus ρ is already negligible compared with σ, and σ practically equal to v.

Hence:

"In an electric circuit containing iron, but forming an open magnetic circuit whose air-gap is not less than $\frac{1}{100}$ the length of the iron, the susceptance is practically constant and equal to the admittance, as long as saturation is not yet approached, and it is:

$$\sigma = \frac{P\,b}{N}, \text{ or: } s = \frac{N}{P\,b}.$$

The angle of hysteretic advance is small, below 4°, and the hysteretic conductance is

$$\rho = \frac{a}{E^{.4}\,N^{.6}}.$$

At a sine-wave of impressed E. M. F., the current wave is practically a sine-wave."

To determine the electric constants of a circuit containing iron, we shall proceed in the following way:

Let E = counter E. M. F. of self-induction;

then from the equation:

$$E = \sqrt{2}\,\pi\,n\,N\,M\,10^{-8},$$

where:

N = frequency,

n = number of turns,

we get the magnetism, M, and by means of the magnetic cross-section, S, the maximum magnetic induction:

$$B = \frac{M}{S}.$$

From B we get, by means of the magnetic characteristic of the iron, the M. M. F., F, in ampere-turns per cm. length, where

$$F = \frac{10}{4\,\pi}\,H,$$

H = M. M. F. in (C. G. S.) units.

Hence, if

L_1 = length of iron circuit, $F_1 = L_1 F$ = ampere-turns required in the iron,

L_a = length of air circuit, $F_a \; \frac{10\, L_a\, B}{4\,\pi}$ = ampere-turns required in the air,

hence,

$F = F_1 + F_a$ = total ampere-turns, maximum value, and

$$\frac{F}{\sqrt{2}} = \text{effective value.}$$

The exciting current is:

$$C = \frac{F}{n\ \sqrt{2}},$$

and the absolute admittance:

$$v = \sqrt{\rho^2 + \sigma^2} = \frac{C}{E}.$$

If F_1 is not negligible against F_a, this admittance, v, is variable with the E. M. F., E.

If:

V = volume of iron,

η = coefficient of hysteresis,

the loss of energy by hysteresis due to molecular magnetic friction is:

$$W = \eta\, N\ V B^{1 \cdot 6},$$

hence the hysteretic conductance:

$$\rho = \frac{W}{E^2},$$

and is variable with the E. M. F., E.

The angle of hysteretic advance is:

$$\sin \alpha = \frac{\rho}{v},$$

the susceptance:

$$\sigma = \sqrt{v^2 - \rho^2},$$

the equivalent resistance:

$$r = \frac{\rho}{v^2},$$

the reactance:

$$s = \frac{\sigma}{v^2}.$$

As conclusions we derive from this chapter:

1. In an alternating current circuit surrounded by iron, the current produced by a sine-wave of E. M. F. is not a true sine-wave, but is distorted by hysteresis.

2. This distortion is excessive only with a closed magnetic circuit transferring no energy into a secondary circuit by mutual inductance.

3. The distorted wave of current can be replaced by the equivalent sine-wave, that is, a sine-wave of equal effective intensity and equal power, and the superposed higher harmonic, consisting mainly of a term of triple frequency, can be neglected except in resonating circuits.

4. Below saturation, the distorted curve of current and its equivalent sine-wave have approximately the same maximum value.

5. The angle of hysteretic advance, that is, the phase difference between magnetism and equivalent sine-wave of M. M. F., is a maximum for the closed magnetic circuit, and depends then only upon the magnetic constants of the iron: the permeability μ and the coefficient of hysteresis η, and upon the maximum magnetic induction, by the equation:

$$\sin \alpha = \frac{4 \mu \eta}{B^{.4}}.$$

6. The effect of hysteresis can be represented by an admittance: $Y = \rho + j \sigma$, or an impedance: $U = r - j s$.

7. The hysteretic admittance, or impedance, varies with the magnetic induction, that is, with the E. M. F., etc.

8. The hysteretic conductance ρ is proportional to the coefficient of hysteresis η and to the length of the magnetic circuit L, inverse proportional to the .4th power of the E. M. F., E, to the .6th power of frequency N and of cross-section of the magnetic circuit S, and to the 1.6th power of the number of turns of the electric circuit n, thus expressed by the equation:

$$S = \frac{58 \eta L 10^3}{E^{.4} N^{.6} S^{.6} n^{1.6}}.$$

9. The absolute value of hysteretic admittance $v = \sqrt{\rho^2 + \sigma^2}$ is proportional to the magnetic reluctance: $P = P_1 + P_2$, and inverse proportional to the frequency N and to the square of the number of turns n, hence expressed by the equation:

$$v = \frac{(P_1 + P_2) 10^8}{2 \pi N n^2}.$$

10. In an ironclad circuit, the absolute value of admittance is proportional to the length of the magnetic circuit, and inverse proportional to cross-section S, frequency N, permeability μ, and square of the number of turns n:

$$v_1 = \frac{127\, L\, 10^6}{n^2\, S\, N\, \mu}.$$

11. In an open magnetic circuit, the conductance ρ is the same as in a closed magnetic circuit of the same iron part.

12. In an open magnetic circuit, the admittance v is practically constant, if the length of the air-gap is at least $\frac{1}{100}$ of the length of the magnetic circuit, and saturation is not approached.

13. In a closed magnetic circuit, conductance, susceptance and admittance can be assumed as constant in a limited range only.

14. From the shape and the dimensions of the circuits, and the magnetic constants of the iron, all the electric constants: ρ, σ, v; r, s, u, can be calculated.

II.—*Foucault or Eddy-Currents.*

While magnetic hysteresis or molecular friction is a magnetic phenomenon, eddy-currents are rather an electrical phenomenon. When passing through the iron, the magnetic field causes a loss of energy by hysteresis, which, however, does not react magnetically upon the field. When impinging upon an electric conductor, the magnetic field induces a current therein. The M. M. F. of this current reacts upon and affects the magnetic field more or less, and thus an alternating magnetic field cannot penetrate deeply into a solid conductor, but a kind of screening effect is produced, which makes solid masses of iron unsuitable for alternating fields, and necessitates the use of laminated iron, or iron wire, as the carrier of magnetism.

The eddy-currents are true electric currents, though flowing in minute circuits, and follow all the laws of electric circuits.

Their E. M. F. is proportional to the intensity of magnetization B, and to the frequency N.

Thus the eddy-currents are proportional to the magnetization B, the frequency N, and the electric conductivity γ of the iron, hence can be expressed by:

$$c = \beta\, \gamma\, B\, N.$$

The power consumed by the eddy-currents is proportional to their square, and inversely proportional to the electric conductivity, hence can be expressed by:

$$W = \rho \gamma B^2 N^2,$$

or, since $B N$ is proportional to the induced E. M. F., E, by the equation:

$$E = \sqrt{2} \pi S n N B 10^{-8}.$$

"*The loss of power by eddy-currents is proportional to the square of the* E. M. F., *and proportional to the electric conductivity of the iron:*

$$W = a E^2 \gamma."$$

Hence that component of the effective conductance, which is due to eddy-currents, is:

$$\rho = \frac{W}{E^2} = a \gamma;$$

that is:

"*The equivalent conductance due to eddy-currents in the iron is a constant of the magnetic circuit, independent of* E. M. F., *frequency, etc., but proportional to the electric conductivity of the iron* γ."

Eddy-currents cause an advance of phase of the current also, like magnetic hysteresis, by an *angle of advance*, β, but unlike hysteresis, eddy-currents in general do not distort the current wave.

The angle of advance of phase due to eddy-currents is:

$$\sin \beta = \frac{\rho}{v},$$

where v = absolute admittance of the circuit, ρ = eddy-current conductance.

While the equivalent conductance, ρ due to eddy-currents, is a constant of the circuit, independent of E. M. F., frequency, etc., the loss of power by eddy-currents is proportional to the square of the E. M. F., of self-induction, hence proportional to the square of frequency and the square of magnetization.

Of eddy-currents, only the energy component, ρE, is of interest, since the wattless component is identical with the wattless component of hysteresis, discussed before.

The calculation of the losses of power by eddy-currents is the following:

Let V = volume of iron,

B = maximum magnetic induction,

N = frequency,
γ = electric conductivity of iron,
ε = coefficient of eddy-currents.

The loss of energy per cm.^{3}, in ergs per cycle, is:

$$h = a \gamma N B^2,$$

hence, the total loss of power by eddy-currents is:

$$W = \varepsilon \gamma V N^2 B^2 10^{-7} \text{ watts},$$

and the equivalent conductance due to eddy-currents:

$$\rho = \frac{W}{E^2} = \frac{10 \varepsilon \gamma L}{2 \pi^2 S n^2} = \frac{.507 \varepsilon \gamma L}{S n^2},$$

where:
L = length of magnetic circuit,
S = section of magnetic circuit,
n = number of turns of electric circuit.

The coefficient of eddy currents, ε, depends merely upon the shape of the constituent parts of the magnetic circuit, that is, whether iron plates or wire, and thickness of plates or diameter of wire, etc.

The two most important cases are:
(*a*) laminated iron,
(*b*) iron wire.

a. Laminated Iron.

Let, in Fig. 14,
d = thickness of the iron plates,
B = maximum magnetic induction,
N = frequency,
γ = electric conductivity of the iron.

Then, if x is the distance of a zone, dx, from the center of the sheet, the conductance of a zone of thickness, dx, and one cm. length and width is, $\gamma\, dx$; and the magnetic flux cut by this zone is, Bx. Hence, the E. M. F. induced in this zone is:

$$\delta E = \sqrt{2} \pi N B x \text{ (C. G. S.) units.}$$

This E. M. F. produces the current:

$$d C = \delta E \gamma dx = \sqrt{2} \pi N B \gamma x dx \text{ (C. G. S.) units,}$$

if the thickness of the plate is negligible compared with the length, so that the current can be assumed as flowing parallel to the sheet, in the one direction at the one, in the other direction at the other side.

The power consumed by the induced current in this zone, dx, is:

$d\,W = \delta\,E\,d\,C = 2\,\pi^2\,N^2\,B^2\,\gamma\,x^2\,d\,x$ (C. G. S.) units or erg seconds, and, consequently, the total power consumed in one cm.^{2} of the sheet of thickness, d:

$$\delta\,W = \int_{-\frac{d}{2}}^{+\frac{d}{2}} d\,W = 2\,\pi^2\,N^2\,B^2\,\gamma \int_{-\frac{d}{2}}^{+\frac{d}{2}} x^2\,d\,k$$

$$= \frac{\pi^2\,N^2\,B^2\,\gamma\,d^3}{6} \text{ (C. G. S.) units,}$$

hence, the power consumed per cm.^{3} of iron:

$$w = \frac{\delta\,W}{d} = \frac{\pi^2\,N^2\,B^2\,\gamma\,d^2}{6} \text{ (C. G. S.) units or erg seconds,}$$

and the energy consumed per cycle and per cm.^{3} of iron;

$$h = \frac{w}{N} = \frac{\pi^2\,\gamma\,d^2\,N\,B^2}{6} \text{ ergs.}$$

Thus, the coefficient of eddy-currents for laminated iron is:

$$\varepsilon = \frac{\pi^2\,d^2}{6} = 1.645\,d^2,$$

where γ is expressed in (C. G. S.) units. Hence, if γ is expressed in practical units, or mho-centimetres, it is:

$$\varepsilon = \frac{\pi^2\,d^2\,10^{-9}}{6} = 1.645\,d^2\,10^{-9}.$$

Substituting for the conductivity of sheet iron the approximate value:

$$\gamma = 10^5,$$

we get:

Coefficient of eddy-currents for laminated iron:

$$\varepsilon = \frac{\pi^2}{6}\,d^2\,10^{-9} = 1.645\,d^2\,10^{-9}.$$

Loss of energy per cm.^{3} and cycle:

$$h = \varepsilon\,\gamma\,N\,B^2 = \frac{\pi^2}{6}\,d^2\,\gamma\,N\,B^2\,10^{-9} = 1.645\,d^2\,\gamma\,N\,B^2\,10^{-9} \text{ ergs}$$

$$= 1.645\,d^2\,N\,B^2\,10^{-4} \text{ ergs;}$$

or,

$$h = \varepsilon\,\gamma\,N\,B^2\,10^{-7} = 1.645\,d^2\,N\,B^2\,10^{-11} \text{ joules.}$$

Loss of power per cm.^{3} at frequency N:

$$w = N\,h = \epsilon\,\gamma\,N^2\,B^2\,10^{-7} = 1.645\,d^2\,N^2\,B^2\,10^{-11} \text{ watts},$$

and, total loss of power, in volume V:

$$W = V\,w = 1.645\;V\,d^2\;N^2\,B^2\,10^{-11} \text{ watts.}$$

Instance:

$d = 1$ mm. $= .1$ cm. $N = 100$. $B = 5{,}000$. $V = 1{,}000$ cm.^{3}

$$\epsilon = 1{,}645 \times 10^{-11},$$
$$h = 4110 \text{ ergs} = .000411 \text{ joules},$$
$$w = .0411 \text{ watts},$$
$$W = 41.1 \text{ watts.}$$

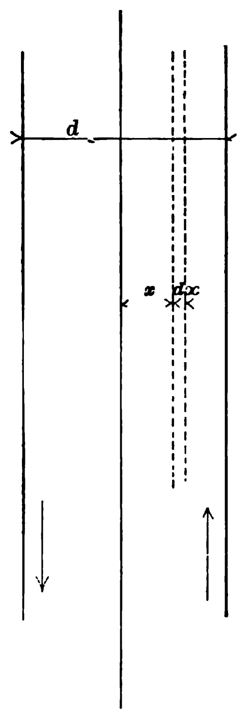


FIG. 14.

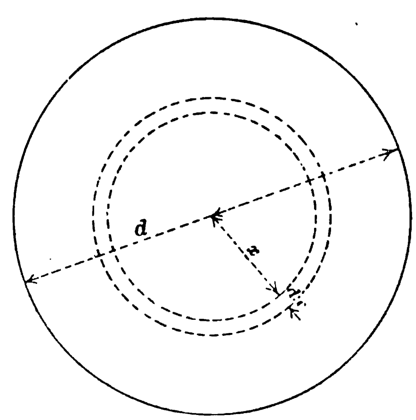


FIG. 15.

b. Iron Wire.—Let, in Fig. 15, d = diameter of wire; then, if x is the radius of a circular zone of thickness, dx, and one cm. length, the conductance of this zone is, $\frac{\gamma\,d\,x}{2\,\pi\,x}$, and the magnetic flux enclosed by the zone is $B\,x^2\,\pi$.

Hence, the E. M. F. induced in this zone is:

$$\delta E = \sqrt{2}\,\pi^2\,N\,B\,x^2 \text{ (C. G. S.) units,}$$

and the current produced thereby:

$$d\,C = \frac{\gamma\,d\,x}{2\,\pi\,x} \times \sqrt{2}\,\pi^2\,N\,B\,x^2$$

$$= \frac{\sqrt{2}\,\pi}{2}\,\gamma\,N\,B\,x\,d\,x \text{ (c. g. s.) units,}$$

hence, the power consumed in this zone:

$$d\,W = \delta\,E\,d\,C = \pi^3\,\gamma\,N^2\,B^2\,x^3\,d\,x \text{ (c. g. s.) units,}$$

and, consequently, the total power consumed in one cm. length of wire:

$$\delta\,W = \int_0^{\frac{d}{2}} d\,W = \pi^3\,\gamma\,N^2\,B^2 \int_0^{\frac{d}{2}} x^3\,d\,x$$

$$= \frac{\pi^3}{6^4}\,\gamma\,N^2\,B^2\,d^4 \text{ (c. g. s.) units.}$$

Since the volume of one cm. length of wire is:

$$v = \frac{d^2\,\pi}{4},$$

power consumed in one cm.³ of iron is:

$$w = \frac{\delta\,W}{v} = \frac{\pi^2}{16}\,\gamma\,N^2\,B^2\,d^2 \text{ (c. g. s.) units or erg seconds,}$$

and the energy consumed per cycle and cm.³ of iron:

$$h = \frac{w}{N} = \frac{\pi^2}{16}\,\gamma\,N\,B^2 \text{ ergs.}$$

Thus, the coefficient of eddy-currents for iron wire is:

$$\varepsilon = \frac{\pi^2}{16}\,d^2 = .617\,d^2,$$

or, if γ is expressed in practical units or mho centimetres $= 10^{-9}$ absolute units:

$$\varepsilon = \frac{\pi^2}{16}\,d^2\,10^{-9} = .617\,d^2\,10^{-9}.$$

Substituting:

$$\gamma = 10^5,$$

we get:

Coefficient of eddy-currents for iron wire:

$$\varepsilon = \frac{\pi^2}{16}\,d^2\,10^{-9} = .617\,d^2\,10^{-9}.$$

Loss of energy per cm.³ of iron, and per cycle:

$$h = \varepsilon\,\gamma\,N\,B^2 = \frac{\pi^2}{16}\,d^2\,\gamma\,N\,B^2\,10^9 = .617\,d^2\,\gamma\,N\,B^2\,10^{-9}$$

$= .617\ d^2\ N\ B^2\ 10^{-4}$ ergs,

$= \epsilon\ \gamma\ N\ B^2\ 10^{-7} = .617\ d^2\ N\ B^2\ 10^{-11}$ joules.

Loss of power per cm.³, at frequency N:

$w = N\ h = \epsilon\ \gamma\ N^2\ B^2\ 10^{-7} = .617\ d^2\ N^2\ B^2\ 10^{-11}$ watts,

and, total loss of power, in volume V:

$$W = V w = .617\ V\ d^2\ N^2\ B^2\ 10^{-11} \text{ watts.}$$

Instance:

$d = 1$ mm. $= 1$ cm. $N = 100$. $B = 5000$. $V = 1000$ cm.³

$\epsilon = .617 \times 10^{-11}$,

$h = 1540$ ergs $= .000154$ joules,

$w = .0154$ watts,

$W = 15.4$ watts, hence very much less than in sheet iron of equal thickness.

Comparison of sheet iron and iron wire.

If

d_1 = thickness of lamination of sheet iron, and

d_2 = diameter of iron wire, it is:

coefficient of eddies in sheet iron:

$$\epsilon_1 = \frac{\pi^2}{6}\ d_1^2\ 10^{-9};$$

coefficient of eddies in iron wire:

$$\epsilon_2 = \frac{\pi^2}{16}\ d_2^2\ 10^{-9}.$$

The loss of power is equal in both—other things being equal—if $\epsilon_1 = \epsilon_2$, that is:

$$d_2^2 = \frac{8}{3}\ d_1^2,$$

or,

$$d_2 = 1.63\ d_1.$$

That is:

The diameter of iron wire can be 1.63 times, or roughly $1\frac{2}{3}$ as large as the thickness of laminated iron, to give the same loss of energy by eddy-currents.

Alternating Current Transformer.

The relative proportions of wire and lamina are shown in Fig. 16.

The same formulas obviously apply to the eddy-currents in masses of any other material, substituting for γ the proper value.

As an instance of the calculation of ferric inductances, the general equations of the alternate current transformer may be given.

Let:

$Y_0 = \rho_0 + j\,\sigma_0$ = hysteretic admittance of primary coil,
$U_0 = r_0 - j\,s_0$ = impedance of primary coil,
$U_1 = r_1 - j\,s_1$ = impedance of secondary coil,

where the inductances, s_0 and s_1, refer to the flow of true self-induction, that is, that magnetism, which surrounds one of the transformer coils only, but not the other.

Let $a = \frac{n_0}{n_1}$ = ratio of turns of primary and of secondary coil.

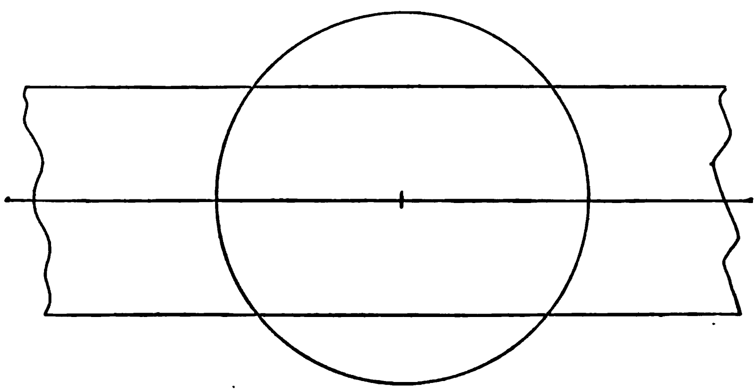


FIG. 16.

Then, denoting the terminal voltage of primary and of secondary coil by E_0 and E_1, and the E. M. F.'s induced in these coils by the magnetic flux surrounding them by E_0^1 and E_1^1, it is:

$$E_0^1 = a\,E_1^1.$$

Denoting the total admittance of the secondary circuit—including the internal impedance of the secondary coil—by:

$$Y_1 = \rho_1 + j\,\sigma_1$$

the secondary current is:

$$C_1 = Y_1\,E_1^1,$$

consisting of the energy component, $\rho_1\,E_1^1$, and the wattless component, $\sigma_1\,E_1^1$.

Hereto corresponds the component of primary current, by the ratio of turns:

$$C = \frac{C_1}{a} = \frac{Y_1\,E_1^1}{a}.$$

The primary exciting current (current at open secondary circuit) is:

$$C_{00} = Y_0 \, E_0^1$$
$$= Y_0 \, a \, E_1^1,$$

hence, the total primary current:

$$C_0 = C + C_{00} = \frac{E_1^1}{a} \, (Y_1 + a^2 \, Y_0),$$

and, the ratio of primary and of secondary current:

$$\frac{C_0}{C_1} = \frac{1}{a} \left(1 + \frac{a^2 \, Y_0}{Y_1}\right).$$

The terminal voltage of the secondary coil is:

$$E_1 = E_1 - U_1 \, C_1$$
$$= E_1^1 \, (1 - U_1 \, Y_1).$$

The terminal voltage of the primary coil is:

$$E_0 = E_0 + U_0 \, C_0$$
$$= a \, E_1^1 + \frac{E_1^1 \, U_0}{a} \, (Y_1 + a^2 \, Y_0)$$
$$= a \, E_1^1 \left(1 + U_0 \, Y_0 = \frac{U_0 \, Y_0}{a^2}\right),$$

hence the ratio of primary and of secondary terminal voltage:

$$\frac{E_0}{E_1} = a \, \frac{1 + U_0 \, Y_0 + \frac{U_0 \, Y_1}{a^2}}{1 - U_1 \, Y_1}.$$

That is, if, at the primary impressed E. M. F., E_0, the secondary circuit is closed by the admittance Y_1, it is:

Ratio of transformation of E. M. F.'s:

$$\frac{E_0}{E_1} = a \, \frac{1 + U_0 \, Y_0 + \frac{U_0 \, Y_1}{a^2}}{1 - U_1 \, Y_1}.$$

Ratio of transformation of currents:

$$\frac{C_0}{C_1} = \frac{1}{a} \left(1 + \frac{a^2 \, Y_0}{Y_1}\right)$$

where these ratios are complex quantities of the form:

$$p \, (\cos \tilde{\omega} + j \sin \tilde{\omega}),$$

thus denoting the numerical value of the ratio of transformation by the vector p, and the phase difference between primary and secondary circuit by angle $\tilde{\omega}$.

Discussion.

Dr. Bedell:—Mr. President, I would like to comment on the remarks of Mr. Steinmetz on the idea of the equivalent sine-wave.[1]

The distorted nature of actual current waves has been particularly emphasized in the valuable paper to which we have listened this morning. Although we know that this distortion exists, we still find it convenient to make what we call the "sine assumption." Now this sine assumption does not mean as commonly supposed, that we consider that the current is actually harmonic. When we assume a harmonic current we simply assume a harmonic current to which the actual current is equivalent. This has, I think, been already pointed out by Mr. Steinmetz as well as by Dr. Crehore and myself.[2] The sine assumption with this meaning has proved very useful in combining experimental and theoretical results, and is not open to the criticism which is often given, that we do not have perfect sine currents under ordinary circumstances.

I would like to question Mr. Steinmetz in regard to one other point; that is in regard to the hysteresis loss in the revolving armature as compared to the hysteresis loss in the transformer, and I would like to ask how he applies his law to the two cases.

Mr. Steinmetz:—With regard to the loss of energy by magnetic friction in a rotary magnetic field, as for instance in the revolving armature of a bipolar smooth core dynamo, I found no essential difference with the loss in an alternating field. But I found that occasionally the observed core loss in the armature of a machine is not the molecular magnetic friction only, but superimposed upon it are eddy-current losses in the iron, the shields, etc., and in the conductors, which losses are proportional to the square of the magnetization. Thus, the observed core loss sometimes rises with a power higher than 1.6, sometimes nearly approaching the square. But by laminating the iron very carefully, designing the mechanical construction so as to expose no solid metal to the alternating field, and shaping the conductors so as to exclude eddy currents, I always got curves very nearly proportional to the 1.6 power, like the one I show here for a variation of voltage up to 9,000 volts, that is, up to very high magnetic densities (about $B = 19{,}000$). There you see the curve of 1.6 power in drawn line, very closely representing the observed core losses. The points marked by crosses are the observed values of the power consumed by the generator less the friction of the belt. So I think the law holds for generators just the same, and therefore I believe the law applies not to the hysteresis loss, but to the loss by molecular magnetic friction, since in the generators we probably have no hysteresis. I took

1. Transactions, vol. xi, p. 46.

2. Geometrical Proof of the Three-ammeter Method of Measuring Power. *Physical Review*, vol. 1. No. 1, p. 61.

pains once to find out if there is a lag of the magnetism behind the resultant magnetizing force in a generator, which would distort the wave of electromotive force, but I did not find anything of the kind. I found no hysteretic lag. Thus the total loss of energy, which as you see here in this case is many kilowatts, is supplied directly by the mechanical power, in which way I am not able to say, but it is not in the form of a hysteretic loop, at least not a hysteretic loop of noticeable size.

PROF. ANTHONY:—I would like to ask one question simply to see whether I have properly understood Mr. Steinmetz. I understand him to mean when he speaks of equivalent sine-curves the several component sine-curves into which the distorted curve could be resolved.

MR. STEINMETZ:—No, I meant a true sine-wave of current of the same frequency as the fundamental, the same effective intensity as the total distorted wave, and shifted against the equivalent sine-wave of electromotive force by such an angle that its power in watts equals that of the distorted wave. I can say that the eqivalent sine wave is not identical with the fundamental sine-wave, except in the case where the sum total of higher harmonics is wattless, because the equivalent sine-wave includes the energy of the higher harmonics also, and thus the remainder, or the difference between distorted wave and equivalent sine-wave, generally includes a component of the same frequency as the fundamental.

MR. KENNELLY:—This paper seems to me to be valuable, first, for its bearing upon the subject of hysteresis and its nature, and, secondly, upon the practical determination of inductances or of equivalent inductances in coils containing iron, such as transformers. The main point, it seems to me, can be stated in a very few words. When the current is no longer a sinusoidal wave, if it becomes distorted by the action of iron in the circuit, it is a complicated wave such as shown at F in the Figs. 7 and 8, etc. But the ammeter or dynamometer which is used to measure that distorted current will show some effective current strength which might be attributable to a pure sinusoidal current. It would show a current strength in amperes which would be represented by the curve C, so that the real current F, whose shape can only be determined by a long series of experiments, has an equivalent representation in the dynamometer such as would be produced by a current of the pure sine shape of C. But if you do not carry the magnetization too high, the amplitude of the pure sine-wave C, such as the dynamometer, would lead you to suppose exists, and the amplitude of the actual distorted wave F are equal. This, if true, is an important and valuable proposition, because it gives you the maximum number of ampere-turns on the magnetic circuit, the maximum cyclic magneto-motive force. But it is pointed out that when you get beyond 10 kilogausses in your iron, you will no longer have this relation maintained. That is in agreement with the observations in Dr. Pupin's valuable paper read this morning, where it is shown that the harmonics of his primary currents remained proportional to the current strength if he did not go up too far in flux intensity, and that is bearing directly on this paper. If you do not go beyond 10 kilogausses you will probably have those two wave crests on the same line.

DR. BEDELL:—There is one point to which a little further attention might be given, and that is in regard to the lag of the current behind the electromotive force when the current and electromotive force are not harmonic. Those who have had occasion to make a study of currents which are not strictly harmonic and desire to find the phase relations, have doubtless met this question. The phase difference between the maximum values and zero values or any other values of the current and electromotive force are not the same. The use of the equivalent sine function is the solution of this question. We assume an equivalent electromotive force which is harmonic and has the same mean square value as the electromotive force which is not harmonic, and we do the same with the current. We then set these two with such an angle of lag between them that the power is the same. Now we can get our power from other measurements and by these measurements of the power, the current and the electromotive force, we thus have a measure of the angle of lag in degrees, which cannot be otherwise obtained when the currents are far from being harmonic. In other words, we say the power is $W = E\,I \cos \theta$. By measuring W, E and I, *we may find a value for the angle θ, whether the current is harmonic or not.*

MR. STEINMETZ:—I would like to point out one thing here, not to allow a misconception to arise. This dissolving of the distorted wave into an equivalent sine-wave, and a wattless remainder is not identical with the dissolving of it by Fourier's theorem into a series of sine-waves, because the equivalent sine-wave c is not the fundamental component of the total wave, but the wattless remainder of apparently triple frequency, shown here, may contain a term of simple frequency.

To fix a definition of this equivalent sine-wave, it is "a sine-wave of equal effective intensity and equal power with the true wave." If you take a wave of electromotive force, for instance, and a wave of current, then the higher harmonics may, but need not, be powerless. This is especially the case if you have the current distorted by hysteresis.

DR. PUPIN:—I might say a word or two on this paper of Mr. Steinmetz, a very interesting paper indeed. In the first place in studying these harmonics in the course of last year I had, especially, Prof. Rowland's paper of 1892 to guide me, in which a radically different view was taken from that of Prof. Fleming. Comparing these two views with my own work, it seemed to me that they could be reconciled to a certain extent in this way:

The hysteresis loop reminds us of two things: In the first place, of the loss of energy, and, in the second place, of the variation of permeability. Now Dr. Fleming ascribed the generation of harmonics to the action of hysteresis in general, not saying exactly what he meant by it. Hysteresis is a very broad term and may be made to mean a great many things. Prof. Rowland specified his view and ascribed the presence of harmonics to the variation of permeability. Both views, therefore, refer to the hysteresis loop for an explanation of the distortion of the current wave. In the course of a discussion[1] at a meeting of this INSTITUTE, I suggested that the distortion of alternating current waves could be very well studied by studying, with the aid of the hysteresis loop, the process of magnetization and demagnetization during each cycle. Mr. Steinmetz's method is exactly the method to which I referred at that time. I am sorry that Mr. Steinmetz has not explained the details of the method of his investigation and the data obtained by it, which enabled him to plot the harmonics of various frequencies from the hysteretic loop.

Another point that I would like to mention refers to what Mr. Steinmetz calls "molecular friction." The distinction between molecular friction and hysteresis does not seem quite clear from Mr. Steinmetz's paper. I have expressed my opinion on several occasions in the course of this and last year, that there are certain phenomena going on during each complete cycle of magnetization of iron which cannot very well be explained by Foucault current and hysteresis as commonly understood, but which phenomena seem to point out clearly the existence of additional passive resistances. Possibly Mr. Steinmetz means the same thing when he speaks of molecular friction. There is certainly a very marked difference between the action of iron when it forms a closed magnetic circuit and when it does not form such a circuit, especially in its damping action upon a resonating current. Again, certain kinds of iron may have a large hysteretic constant, but only a small damping constant, etc. These differences appear at all magnetizations, even at magnetizations due to telephonic currents, and are especially marked at higher frequencies. There is a certain magnetic sluggishness in every piece of iron, and it is my opinion that this sluggishness is not measured by the hysteretic action as ordinarily understood, nor by Foucault current losses. Now what this sluggishness is, it is difficult to tell. The invention of a new name like "molecular friction" certainly does not advance our knowledge one bit. It may retard it if the new name should lead us to believe that further inquiry into the matter will lead to nothing more than mere commonplace molecular friction.

MR. STEINMETZ:—I think Dr. Pupin is mistaken in his state-

1. See discussion of Dr. Bell's paper, "Practical Properties of Polyphase Apparatus." TRANSACTIONS, vol. xi, p. 46.

ment with regard to the name hysteresis. The word has a well-defined meaning. It was introduced merely to denote the lag of the magnetism behind the magnetomotive force, as the derivation of the word signifies, which lag causes the magnetism as function of an alternating M. M. F. to describe a closed curve, the "loop of hysteresis."

Afterward it was shown by Warburg and Ewing that the area of the hysteretic loop represents energy, and represents the energy expended by the magnetomotive force during the cycle of magnetism, and from this, the erroneous conclusion has been drawn that this hysteretic energy is the energy lost in the iron by molecular magnetic friction, that is, by changing the magnetic state of the iron. That is what I want to make clear—that this conclusion is wrong; that this energy expended by the magnetomotive force is not necessarily the energy wasted in the iron. The energy represented by the hysteretic loop or a part of it may be converted into mechanical motion, or the energy lost in molecular magnetic friction may be supplied by mechanical energy, and the hysteretic loop may collapse, or may expand considerably, so that between the area of the hysteretic loop and the loss of energy in the iron there is no direct relation. I have explained this quite fully and shown by tests in my second paper on hysteresis.[1] Since, however, it seems to have escaped attention, probably due to the length of aforesaid paper, I thought it advisable to discuss it again more fully in my present paper.

Now with regard to the changes of permeability and to hysteresis as producers of higher harmonics, the statement that hysteresis produces higher harmonics, is quite correct. It produces higher harmonics, but change of permeability does the same, or rather, hysteresis is nothing but a change of permeability. Take this case I show here on pages 575-7, Figs. 2 and 4. There you have the loop of hysteresis produced by the variable permeability. What Prof. Pupin means in his statement that hysteresis does not produce higher harmonics is probably that molecular magnetic friction does not necessarily cause higher harmonics, and with that I agree; higher harmonics of current appear only when the molecular magnetic friction causes a variation of permeability in the form of hysteresis. But beside this, there are undoubtedly still other causes, which produce higher harmonics, which are neither change of permeability nor hysteresis.

Of any sluggishness displayed by the iron in changing its magnetic state, I have never found any trace which could not be explained as the effect of the hysteretic loop, and thus do not believe that any such sluggishness or viscous hysteresis exists at ordinary frequencies of a few hundred cycles.

The difference in the action of a closed circuit transformer and an open circuit transformer is fully explained by the fact that the open circuit transformer is at open secondary circuit highly in-

1. TRANSACTIONS, 1892, vol. ix, chapter v, p. 711.

ductive; that is, the current passing through it is almost all idle or wattless current, having a small energy component only. In the closed circuit transformer the magnetizing current is so small that the exciting current is largely energy current—hysteretic energy current—the angle of lag being even at open secondary circuit only from 40 to 60 degrees. This explains that no resonance can be produced by a closed circuit transformer, since resonance presupposes a highly inductive circuit, which the transformer is not.

Can anyone inform me when the relation between the distortion of the alternating current wave and the hysteretic loop was first stated by Fleming?

Dr. Pupin:—It is in the second volume of his book.

Mr. Steinmetz:—If you go back, for instance, in our Transactions to Prof. Ryan's paper[1], I think it came out in 1889, he plotted the hysteretic loop from the wave shape of the current, thereby making use of the feature, that the distortion of the current wave is due to the hysteresis, and that the hysteretic loop can be reproduced from the distortion. What I did here was merely to reverse the process. But this has probably also been done before that. Thus I did not need to give a very explicit description. But I think the credit of having first shown this relation between distortion and hysteresis is due to Prof. Ryan.

Dr. Pupin:—I do not think that Prof. Ryan employed the hysteretic loop for plotting the various harmonics. If I remember correctly, the curves of current and electromotive force were plotted by sliding contact, and then the harmonics were determined by the ordinary method of harmonic analysis.

Mr. Steinmetz:—I think he did it directly from the shape of the wave of the current, not from the watt curve, if I am not mistaken. I really do not remember exactly.

Dr. Pupin:—Perhaps Dr. Bedell can tell us?

Dr. Bedell:—I think that the relation between hysteresis and the shape of the current curve was first brought out by Professor Ryan and described by him in his paper[2] on transformers before this Institute in 1889. In conjunction with Professor Merritt, he constructed a hysteresis loop from the curves of current and electromotive force taken by the method of instantaneous contact. From these curves for current and electromotive force, they did construct a watt curve, as Dr. Pupin states, but they made no use of this in determining the hysteresis loop, obtaining the latter directly from the instantaneous curves. That this relation between the current curve and the hysteresis loop existed had been pointed out a little before this time by Dr. Hopkinson,[3] who showed the relation by means of a graphical construction

1. Transactions, vol. vii. p. 1.

2. *Ibid.*

3. Hopkinson: "Induction Coils or Transformers." *Proceedings of the Royal Society*, Feb. 17, 1887. Also given on p. 184 of his re-printed papers.

involving three dimensions which was based upon some results obtained analytically from fundamental differential equations. As far as I am aware, however, it has not been until recently that Dr. Hopkinson has made any investigations in this direction. In a paper[1] published a year ago or so, he described an extended investigation in which hysteresis loops were obtained for different frequencies from curves taken by the method of instantaneous contact. This is, I think, the most complete investigation upon this line of work which has thus far been published; but it differs from the work of Ryan and Merritt only in its greater completeness.

In a paper[2] published about a year before the work done by Professors Ryan and Merritt, Dr. Sumpner showed a very pretty graphical construction for obtaining the current curve when we are given the electromotive force and a curve showing the relation between the current and the time-constant of the circuit. This is at least of considerable theoretical interest; but he could have carried it further. Furthermore, if I remember rightly, he did not take a different time-constant curve for his ascending and descending values. He did not accomplish by his method, however, that which was done by Ryan and Merritt, viz., the construction of a hysteresis loop from the current curve.

Dielectric hysteresis, as well as magnetic, affects the shape of the current curve. I have already had the pleasure of calling the attention of the INSTITUTE to this relation, and of describing a method for determining the hysteresis loop for a condenser. Such a loop is given in the TRANSACTIONS[3] for last year.

Each one of the papers I have referred to has contributed something of value to the question at hand, and due credit should be given to each of the several writers; but I think that to Professors Ryan and Merritt must be given the credit for the practical development of the subject. The harmonic analysis of these curves according to Fourier's theorem was worked out by them and is given by Dr. Fleming in the second volume[4] of his work on transformers. The fundamental together with the third and fifth harmonics were found to closely represent the actual distorted wave.

In conclusion I would say that I consider all this work of particular significance, combining, as it does, observed phenomena and mathematical analysis. Theoretical deductions are always based upon certain premises, and in many cases these premises have consisted of artificial conditions. The conclusions are rigorously true under the assumed conditions, but the conditions are unobtainable. We are acquiring greater ability in making our

1. Drs. J. and B. Hopkinson: London *Electrician*, Sept. 9, 1892. Also: "Gray's Absolute Measurements in Electricity and Magnetism," vol. ii, p. 752.

2. Sumpner: *Philosophical Magazine*, June, 1888, p. 468.

3. TRANSACTIONS, vol. x, p. 525.

4. "Alternate Current Transformer," vol. ii, p. 452.

conditions accord with facts. It has often happened that our conclusions are only true in case hysteresis be absent and the current is a true sine-wave. But this need not be: we may make quantitative assumptions as to the hysteresis present, and may assume the presence of such harmonics in addition to the fundamental wave as occasion demands; predetermination becomes possible, and our work becomes definite and exact.

A paper presented at the Eleventh General Meeting of the American Institute of Electrical Engineers, Philadelphia, May 18th, 1894. President Houston in the Chair.

EXPERIMENTS ON TWO-PHASE MOTORS.

BY DR. LOUIS DUNCAN, S. H. BROWN, W. P. ANDERSON AND S. Q. HAYES.

Within the last few years rotary field motors have been greatly improved, and the conditions for successful design are moderately well understood. The problem has been attacked mathematically, and results have been obtained which, while interesting, have not been submitted to the test of experiment. The mathematical treatment is difficult, unless many essential phenomena are omitted; indeed it is only lately that the solution of the case of a motor supplied from a constant potential circuit has been undertaken, and as this is the condition of actual practice the results even with their evident limitations are important and interesting. The phenomena that occur in the armatures of these motors are of special importance, but they have not yet been submitted to experimental investigation.

The experiments of which this article is a description were intended for the purpose of developing a method of obtaining the current and electromotive force curves of multiphase motors, and of applying it to a two-phase, two H. P. Tesla motor kindly furnished by the Westinghouse Electric and Manufacturing Company. While the results are probably correct for the machine tested, yet as the motor was small with inward projecting pole-pieces, the result will differ considerably from those that would have been obtained on a larger machine, or one without projecting pole-pieces. It was impossible for us to get any other machine, and the development of a method is, we think, of as great importance as the results themselves.

In a rotary field motor, if the resultant field is not exactly uniform, but presents some irregularities—then if the difference between the speed of the field and the speed of the armature is not a multiple of both, the armature electromotive force will not in general be a periodic curve, because if we consider an armature coil enclosing a maximum number of lines of induction, then when it again includes the maximum number, the field will be in a different position with respect to the poles, and its maximum value may be different. Or to put it another way, if the difference of the speeds of the field and armature is not a multiple of both, then any particular armature coil will not have the same relative position with respect to both the field and the pole-pieces in its successive positions of maximum induction. It is necessary then, in order to obtain periodic armature currents, that some form of gearing be employed.

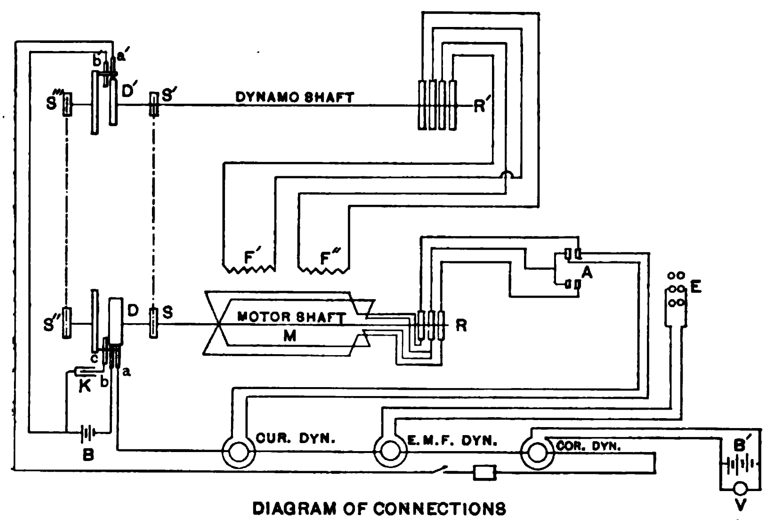


Fig. 1.

If large machines are to be tested, where it would be inconvenient to transmit a large amount of power by gearing, the motor may be loaded until the desired speed of test is approximately attained, when a very light gearing between the dynamo and motor will serve to keep this speed constant, the gearing serving simply to check any small tendency toward a change of speed. In our own experiments, the power to be transmitted was small, and the construction of the motor was such, that we had no room for a gearing and for a coupling to a load. We consequently geared the motor to the dynamo, the motor energy being given back to the dynamo.

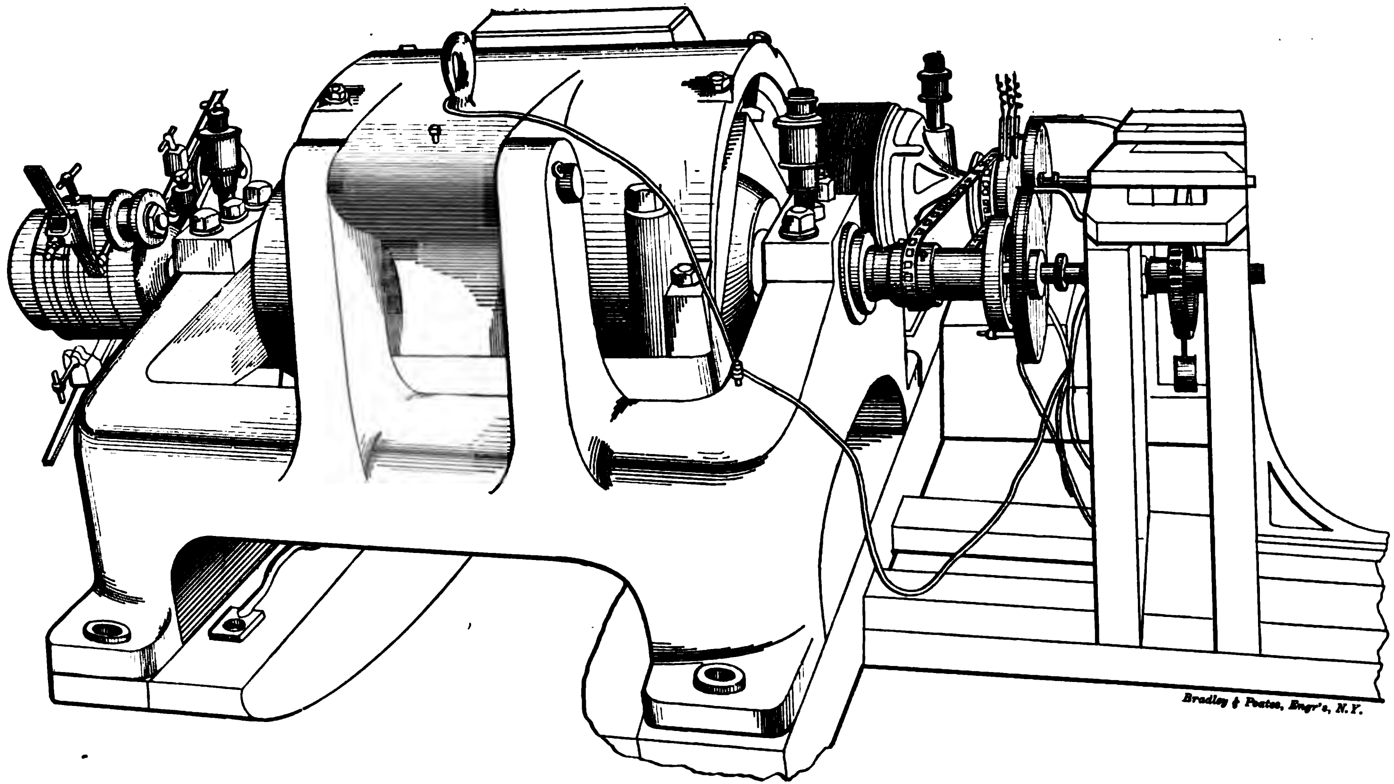


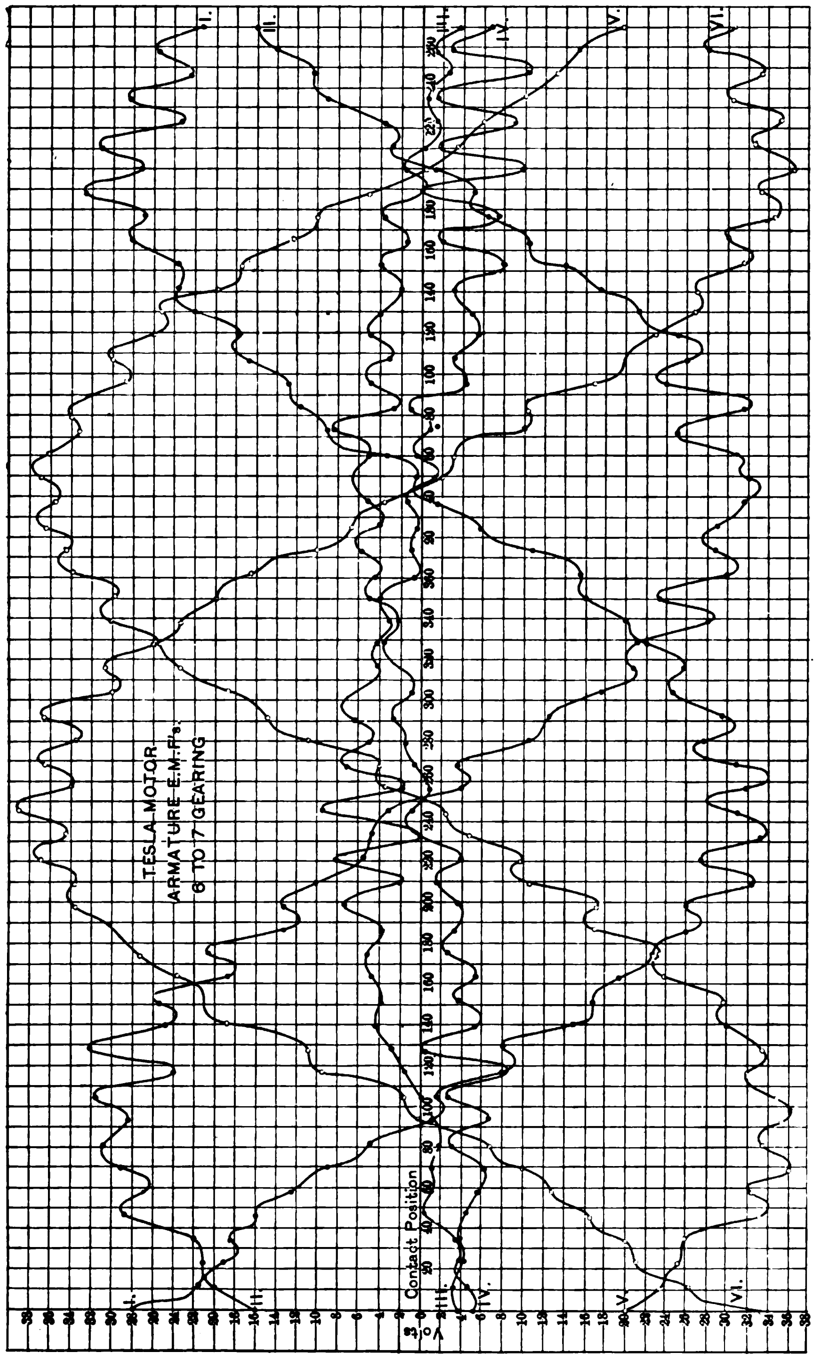
FIG. 2.

The apparatus experimented on, consisted of a 25 H. P. two-phase dynamo—an ordinary constant current machine supplied with four collecting rings—and one two-phase, 8-pole, 2 H. P. Tesla motor. The electromotive force of the dynamo, as may be seen from the curves, was practically a sine-curve. In our work the motor was not run up to its full capacity, as we were limited by the amount of power the gearing would safely transmit. We can not better describe the armature winding of the motor, than by quoting part of a letter written us by Mr. Charles F. Scott, of the Westinghouse company.

"The one you have, has, however, 41 slots. The odd slot was placed in this motor so that the relation of the armature teeth to the field poles was different in different parts of the circumference, and the forty (40) incipient dead points which might have occurred, were avoided by the addition of the extra slot. The winding consists of four (4) layers; the first and third are exactly similar and coincident in the slots occupied, and the second and fourth are similarly related. The four coils in each layer are connected in series and short-circuited."

The idea was to make the mutual induction of an armature coil with respect to the poles, a sine-curve. Of course with a limited number of slots, it is impossible to have the mutual induction of the field and armature, exactly a sine-curve. But it is possible to very nearly accomplish this, especially in large armatures, and the importance of it is beginning to be appreciated. Mr. Scott, of the Westinghouse company, was one of the first to appreciate the importance of the armature winding and the proper method of doing it, and he deserves much credit for his quiet and persistent work which has resulted in the production of excellent motors instead of voluminous papers.

It is of course true, that the period of the armature is the sum of the periods of the field and the armature. As we wished to obtain the curve of the armature current by the contact method, it became necessary to get contacts whose period was the sum of the field and armature periods. This was obtained by an arrangement of apparatus shown in Figs. 1 and 2. In the Fig 1, S and S^1 are the two sprocket wheels which gear the two machines together and give them the desired relative speeds. S'' and S''' are two others which gear the graduated disks which carry the brushes. D and D^1 are the two instantaneous contact disks, one mounted on each shaft, and a, b, c, a^1 b^1 are the wiping brushes. K is a condenser and B a charging battery; b and c make the circuit through the battery and condenser once every revolution,



I. COUNTER E.M.F. "B"
II. COUNTER E.M.F. "A"
III. EFFECTIVE E.M.F. "A"
IV. EFFECTIVE E.M.F. "B"
V. IMPRESSED E.M.F. "A"
VI. IMPRESSED E.M.F. "B"

FIG. 8.

thus keeping the condenser charged. When *a* and a^1 make simultaneous contact, the battery circuit is broken and the condenser is discharged through the movable coils of the dynamometer which are all connected in series. F and F^1 are the motor field coils. R and R^1 the rings of the motor and dynamo respectively. E is a double pole, double throw switch to which are connected the terminals whose potential difference curves are desired. M is the motor armature, and A is a switch in the circuit of the current instrument. B^1 is a battery which sends a steady current through the large coils of a dynamometer. This dynamometer is used as a correcting instrument, and the resistance in the condenser circuit is regulated to keep its deflection constant.

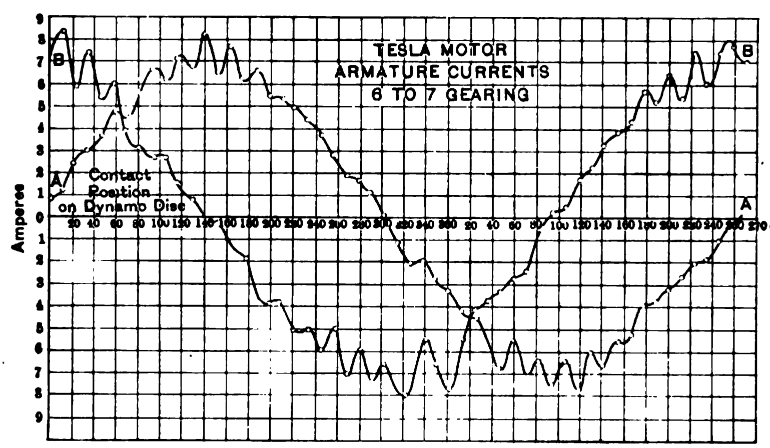


FIG. 4.

If the brushes are set together on the contacts, and then the two machines revolve with a given speed ratio, say, four to three, the brushes will again be simultaneously on the contacts, when the machines have made, respectively, four and three revolutions. If the ratio was seven to six, the machines would make seven and six revolutions before the contacts would again be coincident. In this way we obtained the needed instantaneous current.

After obtaining one point on the curve in this way, and wishing to obtain another point, we must shift our brushes through angular distances proportional to the speed ratio of the two machines; otherwise they would not make simultaneous contact again. Having shifted them in this ratio (say, if the ratio is six to seven, we would shift 10 degrees on the dynamo disks, and $\frac{6}{7}$ of 10 degrees on the motor disk), we obtain in another point on the curve. To accomplish this easily, we gear the brushes together in the same ratio as the armatures are geared, as is shown in Fig 1. Our gearing both for the brushes and armatures consisted of sprocket wheels and chains and was very satisfactory.

As the machine we tested was small, and was run much below its rated electromotive force, it was not, of course, particularly efficient, and as the armature efficiency is approximately the ratio of the armature speed to the field speed, this ratio was comparatively small, and the period of the armature current was

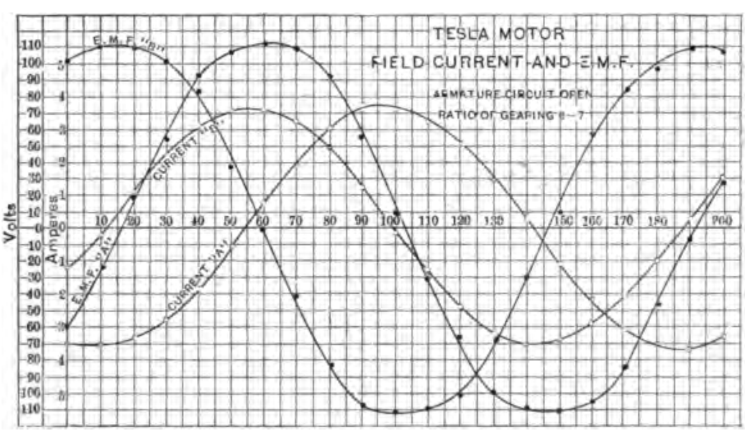


Fig. 5.

small, thus enabling us to use for measuring it a form of dynamometer which has been described before, and which was invented by one of us for obtaining such curves. It consists of a stationary coil carrying the current whose curve is to be obtained, and a movable coil through which passes an instantaneous direct current obtained by making the circuit on the armature disks before described. If this instantaneous current occurs when the alternating current is zero, we will get no deflection of the instrument. If it occurs when the alternating current is maximum, we will get a maximum deflection, and in general, the deflection will be proportional to the instantaneous value of the alternating current. The dynamometer used had a long period, and was well damped, and we had no difficulty in reading even when the period of the armature current was as much as one-quarter second.

If very efficient machines were to be tested, where the period of the armature current is very large indeed, then some electrometer method or a telephone method would be used, or the deflection of a galvanometer needle in the field of the current could be easily photographed.

The curves we have obtained are as follows: The electromotive force applied to the armature. Effective electromotive force of the armature. The counter electromotive force of the armature. The armature current. The value of field electromotive

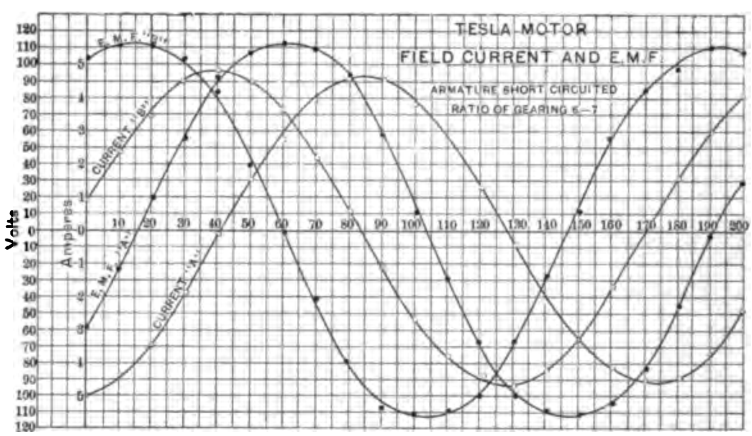


FIG. 6.

force and current for open and closed armature circuit. These for ratios of three to four and six to seven.

We also obtained the various currents and electromotive forces when the armature was held stationary, with and without resistance in the outside circuit. There are really three distinct sets of curves: those relating to the three to four gearing, those for the six to seven gearing, and those in which the armature was stationary. The angular positions do not correspond for the first two sets. For the second and third they very nearly correspond. This is due to the fact that we did not at first clearly appreciate the importance of permanently fixing the relative positions of the armatures of the two machines. Afterwards we made marks on each armature, and if for any reason we took off the gearing,

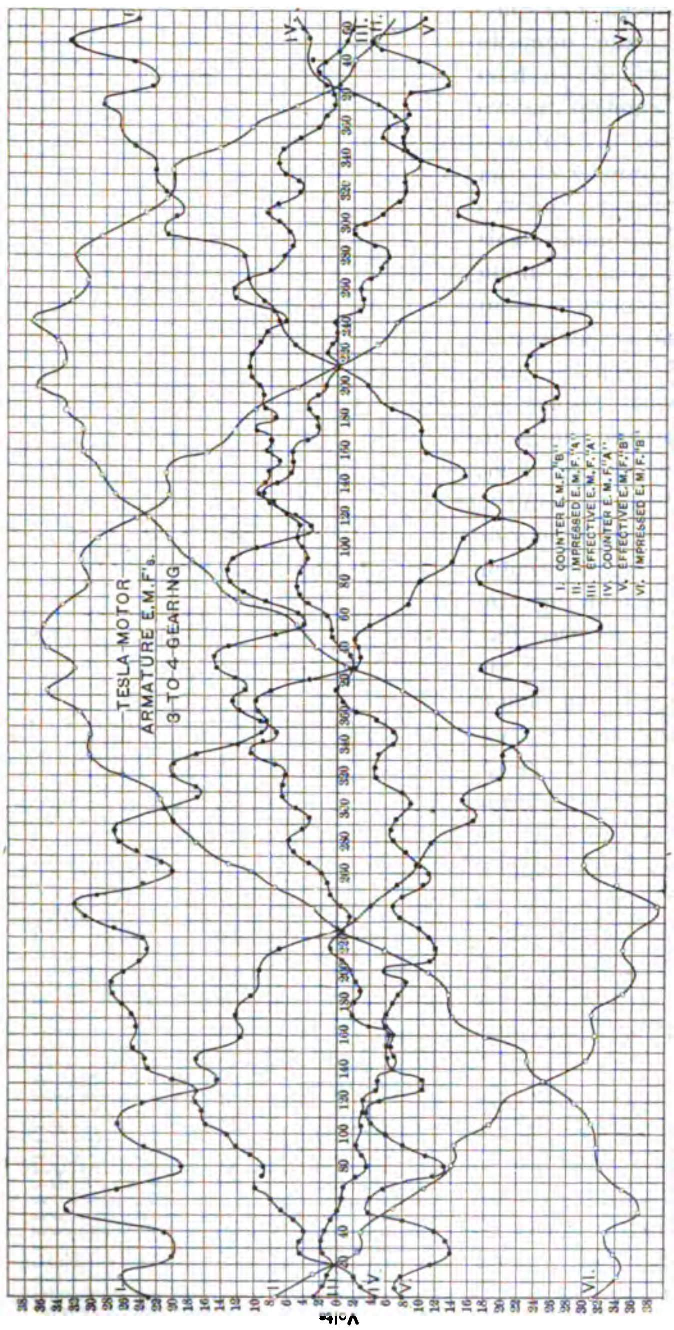

TESLA MOTOR
ARMATURE E.M.F.'s.
3-TO-4-GEARING
I. COUNTER E.M.F. "B".
II. IMPRESSED E.M.F. "A".
III. EFFECTIVE E.M.F. "A".
IV. COUNTER E.M.F. "A".
V. EFFECTIVE E.M.F. "B".
VI. IMPRESSED E.M.F. "B".
Volts

FIG. 7.

we replaced it so these marks came opposite points which we fixed on the frames of the two machines. It should also be remarked that the curves for the two sets of armature coils marked A and B should not in general present the same irregularities, as their relative positions with respect to the poles and the resultant field are different. The dynamo being a four-pole machine, this must be taken account of in calculating the angles on the base line. In Figs. 3 and 4 the contact positions should be multiplied by two. The length of an armature curve in terms of the positions of the dynamo brush should be $360 \times \frac{4}{2} = 720$ in the case of the three to four gearings, and $360 \times \frac{7}{2} = 1{,}260$ for the six to seven gearing.

We have not the same confidence in the results of the three to four gearings as for the six to seven gearing; the latter being taken for several sets of observations which checked very well.

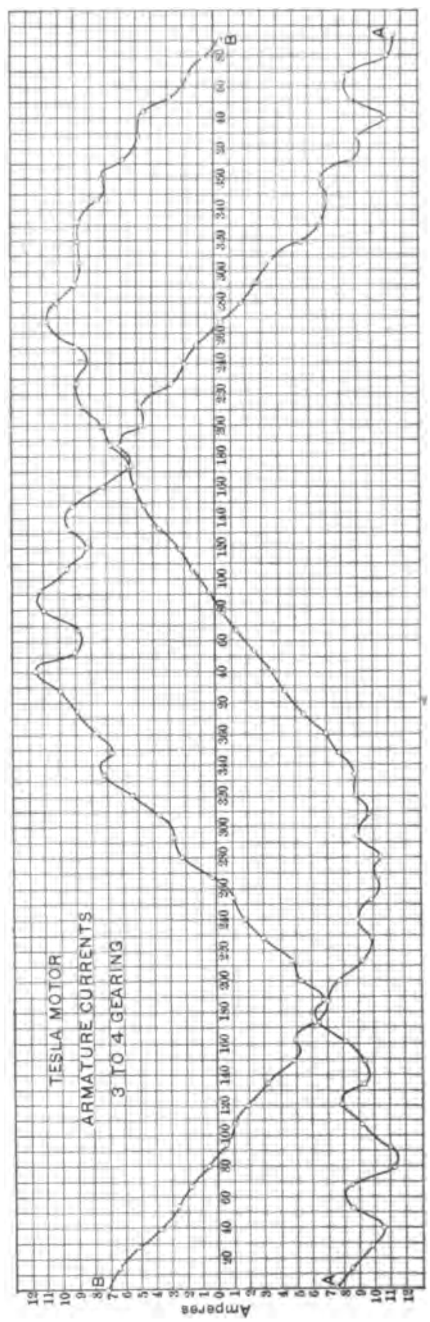


FIG. 8.

The curves of the applied electromotive force (curves v and vi, Fig. 3, and ii and vi, Fig. 7) are obtained in the following manner: The armature was held stationary, while the field revolved at its normal rate. The reading of our electromotive force dynamometer then gave us a point on the electromotive force curve. The armature was then moved through a given angle while the brush on the dynamo disk was moved through an angle corres-

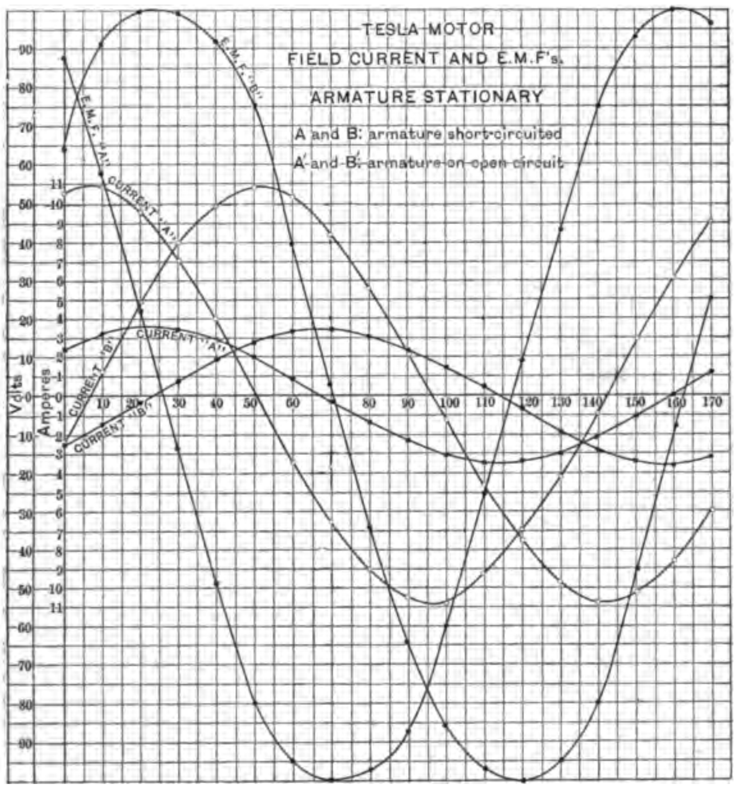


Fig. 9.

ponding to the ratio of gearing of the two machines. In this way another point was obtained on the curve. What we obtained was the electromotive force applied to the armature when there was no current flowing through it, and when the gearing was three to four or six to seven, according to the relative movement we gave the motor armature and the dynamo brush. It should be remarked that all of the armature curves marked electromotive force curves are obtained when the armature circuit is open, and therefore do not correspond to the actual condition of affairs when the armature is closed, as they do not contain the effects of armature reaction and self-induction.

The effective electromotive force of the armature given in curves III and IV, Fig. 3, and III and IV, Fig. 7, were simply measured by opening the armature circuit, and getting the poten-

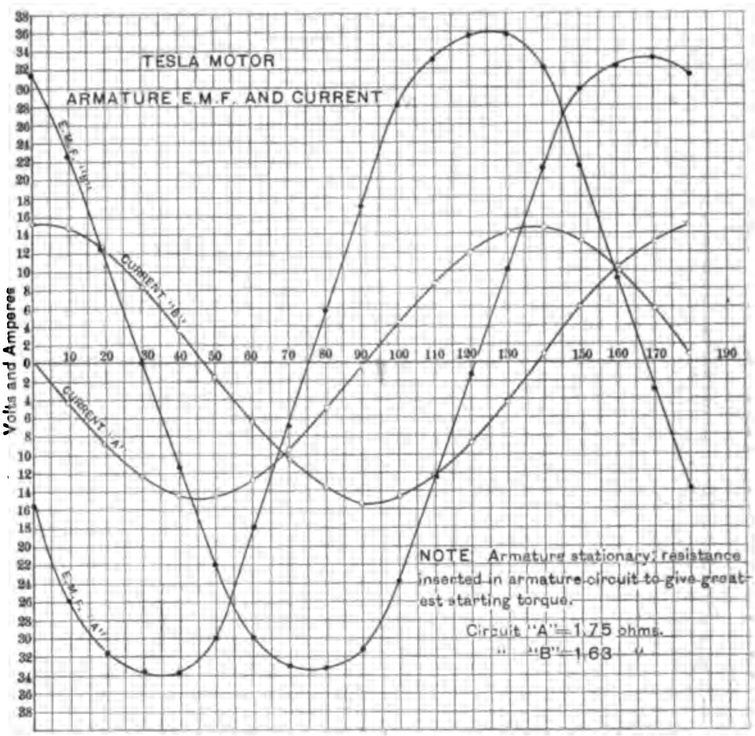


FIG. 10.

tial difference on the motor terminals when the motor was geared to the dynamo with ratios six to seven and three to four. The difference between these curves and the curves of impressed electromotive force, is the counter electromotive force, and is given by curves I and II, Fig. 3, and I and IV, Fig. 7. We could have obtained the counter electromotive force by supplying the fields with continuous currents whose ratio to one another would be that of the two-phase currents and varying the relative values of these currents as we vary the point of contact of our instantaneous current. This would have been a laborious task and was not necessary. It was not possible to obtain these quantities by direct observation when the armature circuit was closed, and when the machine was running under normal conditions.

In Fig. 4 is given the armature current of the motor with a gearing of six to seven, and in Fig. 8, is given the current when

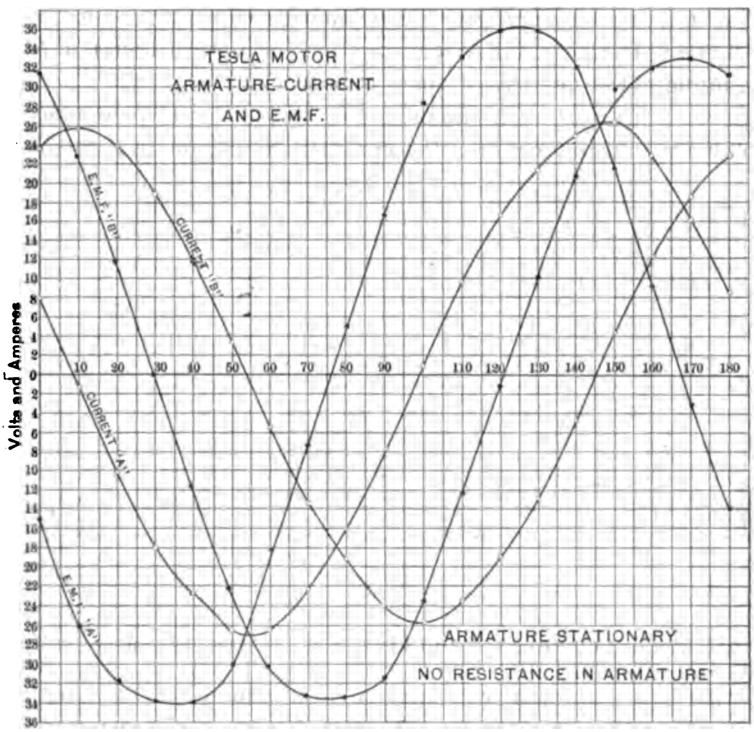


FIG. 11.

the gearing is three to four. If we compare these curves we will see that the current is more irregular with the higher efficiency than with the lower. Again comparing the curves of effective electromotive force for the two gearings, we see again that the irregularity is greater with the higher efficiency. If we consider that the current would follow this curve of effective electromotive force, but for the armature reactions and self-induction, we can see that these effects have their good as well as evil side.

The effective electromotive force is of course the difference between the applied and the counter electromotive forces, and if these are irregular, their difference becomes the more irregular as they are the more equal in value, that is, as the efficiency of the motor is higher; so that while in this machine whose maximum armature efficiency was made about 87 per cent., the irregularity is considerable, it would be very much exaggerated in a larger motor whose armature efficiency might be 97 per cent. or 98 per cent.; and in this case, great care should be taken to produce a perfect regular field. The effect of the armature reaction and self-induction is to decrease these irregularities.

If we consider for a moment the theory of the two-phase motor, we will remember that the armature efficiency is theoretically (leaving out lines in the field iron) $\frac{p^1}{p}$ where p and p^1 are respectively the angular velocities of the field and the motor armature. We will also remember that the lag of the armature current behind the effective electromotive force is taken as

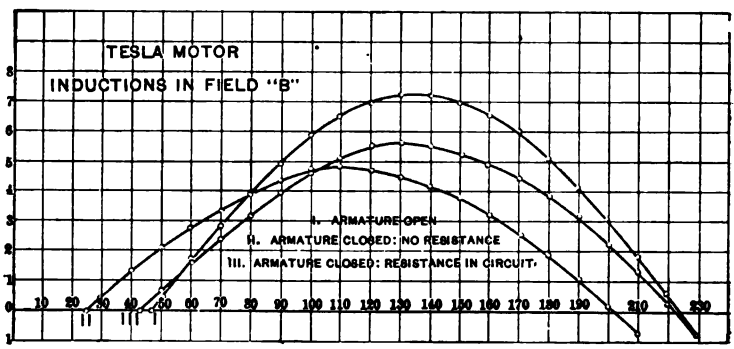


FIG. 12.

$$\tan \theta = \frac{(p - p^1)\, L}{r}.$$

If we look at the curves, however, obtained with the motor experimented on, we will see that the armature current is irregular, and therefore the actual heating would be greater than that calculated from sine curves; that is, the armature efficiency is always less, and the drop in speed is always greater than the theoretical value, and may be very much less if the armature current is irregular. As has been pointed out, this fluctuation in the armature current would be exceedingly great if the self-induction and armature reaction of the motor did not tend to wipe it out. The effect of the self-induction being to damp the most, the waves of shorter period, that is, the irregularities.

It seems to us that the most important curves we have obtained are those of applied and counter electromotive forces, effective electromotive force, and the armature current. They show at once the great importance of designing a machine whose applied and counter electromotive forces are both sine curves, and the method may be easily used to experiment on actual machines, and to find out if this condition is fulfilled. Again the comparison of the effective electromotive force with no armature current, with the fall of potential due to the armature current, shows us that there is some beneficial effect from self-induction in the armature. The principal results show that the special machine does not give us regular currents and electromotive forces, but it is to a large extent due to the fact that in these small machines considerations of economy make it necessary to wind the field coils on spools instead of winding them through slots cut in the field iron.

In designing a motor to give absolutely regular electromotive force curves we must take two things into account: The field due to the dynamo current must be regular, and the armature winding must be such that it will give a regular electromotive force in a regular field. To satisfy this condition in the field windings, projecting pole-pieces should certainly be avoided. To satisfy the condition in the armature, would require an infinite number of armature windings, but it can be practically satisfied in the larger machines with a reasonable number of windings. It must not be understood that it is specially easy to accomplish this. Figs. 5 and 6 give us the input of the motor at full load and with an open armature with a six to seven gearing. Figs. 9, 10 and 11 show the field and armature electromotive forces and currents when the armature is stationary, and in one case short-circuited, and in the other case has outside resistance in its circuits. Fig. 12 gives in arbitrary units the induction through a coil wound around one of the pole-pieces, under the conditions stated.

We do not wish to add to an already lengthy paper, a prolonged discussion of the curves we have given, especially as we hope to be able to experiment on a larger machine, the results from which will be of much greater importance; but we think that the curves we have given show that the design of rotary field motors requires careful experimental study and we believe that such a study may be easily made, even for machines of large capacity.

DISCUSSION.

MR. STEINMETZ:—Some time ago my attention was also drawn to these irregularities, especially when working with a single phase induction motor, where these irregularities were comparatively large. Having no means at my disposition to make instantaneous readings, I arrived at the solution of the problem in a somewhat different way, by theoretical reasoning, and I think my results agreed with those in the present paper. I found that these higher harmonics superimposed upon the main wave of the armature current are in reality not higher harmonics in the common sense of the word, but are due to the fact that the armature current is a complex current produced by the superposition of two currents of different frequencies.

If in an induction motor of frequency N, the relative slip of armature is denoted by K, that is, the difference of speed between armature and synchronism N is $K\ N_1$, then in the armature will be induced two E. M. F.'s, one of the frequency of $K\ N$, the other of the frequency $(2—K)\ N$. The relative values of these E. M. F.'s will depend upon the construction of the motor, and in general the second E. M. F., that of higher frequency, will be small compared with the E. M. F. of lower frequency. These E. M. F.'s produce currents of the same frequency in the short-circuited armature; with the difference, however, that the current produced by the E. M. F. of the higher frequency will be very much smaller, due to the higher self-induction produced by the higher frequency. Thus while the E. M. F. wave of the armature may show the effect of the superposed higher frequency very markedly, the current wave will show it in a greatly reduced manner. This is very nicely borne out by the curves shown in the paper.

I may add that the relative proportion of the two waves depends upon the ratio of K to $2—K$, and thus consequently with the reduction of K, that is, with the closer approach of the motor to synchronism, these higher harmonics will more and more disappear.

The conditions under which tests were made, do not quite represent actual working conditions of the induction motor, since in actual work, the slip, that is, the value of K, is usually considerably less than in these tests, so that the higher harmonics of current will be less in practical operation.

MR. C. F. SCOTT:—I have been very much interested in Dr. Duncan's method, especially as I had the opportunity of seeing the apparatus and his collection of dynamometers for making all the measurements simultaneously. The small variation in the curves are in the form of higher harmonics which result from the construction of the motor itself. We are apt in these theoretical discussions to leave out the bearing of the exact form and construction of the apparatus. The assumption, for instance, which has been made that the wave form given by a smooth core armature is necessarily a sine-wave is not a correct deduction, because the wave is of course due to the distribution of the magnetic field and also the distribution of the winding, and it so happens that as machines are usually built, smooth core machines do give sine-waves very approximately. The statement has been made by Prof. Anthony that either the two-phase or the three-phase combination should give uniformly revolving fields, that is, fields in which the polar line revolves uniformly and retains a constant value. That again assumes a certain form of construction. If we were to use a machine of four distinct poles, as a two-phase machine—

Prof. Anthony:—I was assuming that the wave was a true sine-wave.

Mr. Scott:—Very good. Let us take true sine-waves and put the two currents around the poles of the four-pole motor. Then it is probable that in the center of the space, the position which would be occupied by the shaft of the machine, we would get a uniformly revolving field, because there would be two fields at right angles, varying according to the sine law. But if we consider a position at or near the ends of the poles, then we may have a coil which in moving will embrace the lines which come from one circuit at one time, from both circuits together at another time, and from the second circuit alone at another time. Therefore we do not have the combination of the fields for both circuits at all times. For instance when one circuit is at its maximum strength, we may embrace all its lines in the coil and not begin to embrace any lines of the other circuit until a certain time has elapsed, possibly corresponding to 15 or 20 degrees, so that there will be a change in the induction through the coil, because one circuit has begun to decrease in its value before the other has begun to thread any of its lines through the coil. So that even with true sine-waves we may not in actual machines, unless special attention is given to construction, produce the effects of a uniform revolving field.

With regard to the influence of harmonics in the rotating field, I will have to differ from the statement which was made that the field would be found to be uniform in its intensity although harmonics were present. Several years ago I was engaged in the testing of some motors, the first motors of the Tesla type, by the way, which were used for commercial work. I had gone over the designs and had tested some preliminary experimental motors and thought I had everything covered with regard to the construction of the motors. A number had been made and in the test of the first one the motor had been carrying its load for an hour or more when I found an unduly high temperature. I found the speed had fallen from synchronism fully fifty per cent. more than it should have fallen, and the motor instead of coming up to a definite temperature kept going up on what was a rather straight line. I had a second generator started, and transferred the motor from the generator that had been running, to the new one. The speed immediately came to what it should be, and the temperature began to fall, although the load was the same. The heating of the motor with one generator was equivalent to what would have been obtained by carrying half or two-thirds additional load with the second generator. The second generator, the good one, gave a curve which was approximately a sine-curve. The other generator gave a wave form which was measured and found to be very much depressed at the middle, where the sine-curve is maximum, showing that the third harmonic was very strongly present.

An analysis of the effect of the third harmonic in the rotating field shows some rather interesting relations. The third harmonic evidently tends to a synchronous speed which is three times as great as the fundamental, so that if we have present a third harmonic, we are tending of course to two speeds of rotation, the speed of the fundamental and one three times as fast. This third harmonic, however, is negative in its tendency to rotation. That is, the fundamental produces rotation in one direction, and the third harmonic not only tends to produce a different speed, but it is in the opposite direction. This can be very simply shown by drawing a sine-wave and its third harmonic, and also a second wave which is a quarter-phase from the first, together with its harmonic. You will notice that if the first fundamental comes before the second one, that the third harmonic of the first comes after that of the second one. Consequently the fundamentals tend to rotation one way, and the third harmonics in the opposite direction. By different relative values of the two we may get almost anything. We may get normal rotation or no rotation at all, or rotation at normal speed in the opposite direction, or twice normal speed or three times normal speed.

With regard to the higher harmonics in ordinary commercial working, it is to be remembered that those which are produced by iron cores are produced in what may be called the leakage current which flows around these cores, commonly transformer cores, for the purpose of magnetizing that part of the apparatus; and that these higher harmonics will usually be but a small per cent. of that so-called leakage or magnetizing current. Again magnetizing current in commercial apparatus is in general but a small per cent. of the current at full load, usually only a very few per cent., so that the higher harmonics are only a minor effect in the magnetizing current, and this is to be but a few per cent. of the full load current for which the apparatus is in general designed. Therefore we need not be greatly afraid of the influence of those harmonics in producing trouble in commercial circuits. There are conditions and times when they must be carefully guarded against and profoundly respected.

A beautiful and simple method of showing higher harmonics was called to my attention not long ago by a young man in our laboratory, and is well worth considering. A thin iron plate (one about six inches wide and 18 or 20 inches long gave the best result) is placed in an alternating field set up by current passing through a coil. The familiar tone of the dynamo itself is heard. If the plate be bent slightly, the sound changes. At first it is a sort of a general hum, but by bending, the different harmonics are brought out and may predominate. The octave, and the third, and so on, running up the scales give their different musical tones very clearly and very purely, and by quickly changing the plate you can run through the scale with almost the beauty and clearness of a musical instrument. It might be possible to take all these sounds which are produced by the alternating current, by using some good resonator for giving all the tones together, and then pick them out and thus analyze the different harmonics by means of acoustic apparatus.

Mr. Steinmetz:—It is indeed true that in induction motors we can get higher harmonics, but I think the foremost improvement in the modern induction motor, the improvement which has made this motor the engineering success which it now is, was the elimination of these higher harmonics. If you take one of the old type of induction motors with four poles, two excited by the current of one phase, and two by the current of the other phase, then, when the one current is maximum, the other current is zero, and its corresponding poles are practically dead. Thus the total flux crosses through the air-gap of one pole. In the moment, however, when both currents are equal, the cross-section of the magnetic flux consists of two air-gaps. Since the magnetic reluctance essentially consists only of air reluctance, you see that in such a motor it will vary by nearly 50 per cent., and the result is that no matter whether you use a sine-wave of impressed E. M. F. or not, you must get higher harmonics. This, however, no longer applies to the present induction motor.

Now with regard to the effect of higher harmonics in an induction motor, I cannot agree with the generality of the statement made by Mr. Scott. It is indeed true that in the quarter-phase induction motor whatever action is produced by the triple harmonic, will be of the nature of a backward torque of triple frequency, tending to turn the motor in the opposite direction, and thereby retarding its motion and reducing its efficiency.

This, however, does not apply to the three-phase induction motor.

Let, in Fig. 13, be shown the three waves of a three-phase circuit, as 1, 2, 3. Then the triple harmonic of 1 as shown by *a* in Fig. 13 coincides with the triple harmonic of 2 and 3, that means no matter whether triple harmonics are present or not, they can in a three-phase motor, never give a backward torque, but balance each other while the motor is at rest. If the motor is revolving, these triple harmonics must give the same action as the current in a single phase motor, that is, combine to a torque in the direction of rotation.

Thus to conclude, the triple harmonics will tend to turn a quarter phase motor backward, while in the three-phase motor, they will not have any effect at all, or will assist the fundamental wave, which induced me to prefer the three-phase system to the quarter-phase system, although I do not think that the effect of the triple harmonic is sufficiently serious in a properly designed motor to cause, even in quarter phase motors supplied from iron-clad generator, any serious reduction of output or efficiency.

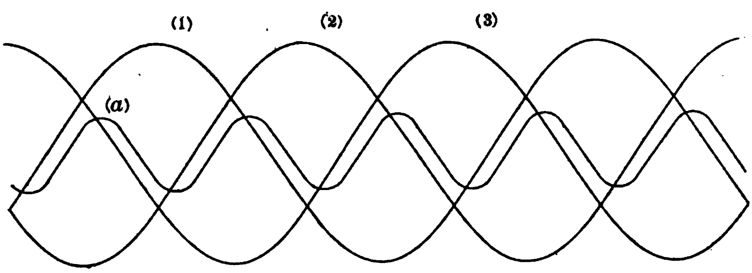


FIG. 13.

DR. PUPIN:—In reply to Mr. Scott's remark I wish to state that in a rotary magnetic field the intensity practically remains constant under conditions specified in my paper. These conditions correspond to practical conditions, when there is no polyphase motor on the circuit. It would never do to exterpolate and guess what would happen under some other conditions. I stated in the conclusion of my paper that rotary magnetic fields produced by reasonably well constructed machines are not accompanied by fluctuations in their intensity. What I mean by a reasonably well constructed machine is simply a machine which produces an electromotive force that may be a complex harmonic; but the upper harmonics are weak in comparison with the fundamental. In the particular machine that my students experimented with, there were upper harmonics, there were the third, the fifth, the seventh and even the ninth, but the rotary magnetic field was nevertheless of constant intensity. As soon as we depart from conditions met with in practice, we can, if we wish, introduce all sorts of complications as in the case described by Mr. Scott, where the generator had a very strong third harmonic. I do not think that Mr. Scott can produce that phenomenon by any reasonably well constructed machine which the Westinghouse company is putting on the market.

THE PRESIDENT:—If there is no further discussion on this paper, the Secretary wishes me to make a statement.

It is probably apparent to you that this meeting has had the largest attendance and the most continuous interest in the various sessions of any general meeting we have ever had. It gratifies me, both as your President and as a resident of Philadelphia, that this is true.

The report of the Committee appointed to prepare a series of resolutions expressing the thanks of the INSTITUTE for courtesies received is now in order. Is Mr. Binney prepared to report these resolutions?

Mr. Binney, Chairman of the Committee, reported the following resolutions:—

Whereas, upon the occasion of the Tenth Anniversary of the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS, and during the period of the convention held at Philadelphia, May 15th to May 18th, 1894, the INSTITUTE and its members severally have received in all manner of ways the most gratifying hospitality and overwhelming courtesies; and

Whereas, it is the sense of this meeting that the INSTITUTE is deeply appreciative and grateful therefor; now, therefore, be it

Resolved, that a Committee of three be appointed to prepare and present to those from whom such remarkable kindnesses have come, letters of thanks in the name of the INSTITUTE, and accompanied by a copy of this resolution; and be it further

Resolved, that it is greatly regretted that the business of this convention, and the necessary limitation of time have in some instances prevented the INSTITUTE from accepting offered favors.

[The resolutions were adopted.]

It was then voted that a proposed paper on "Reactance," by Mr. Charles P. Steinmetz and Dr. Frederick Bedell, be accepted as read by title at this meeting, subject to the approval of the Committee on Papers, Meetings and Editing.

The general meeting then adjourned.

SMOOTH AND TOOTHED CORE ARMATURES.

[Reply to Discussion, by A. D. Adams, see page 465 *et seq.*]

The statement that "In order to allow for the waste armature-field due to this core-leakage, the exciting power has to be increased in about the same degree as the lessening of the magnetic resistance of the air-gap would otherwise decrease it" seems to be entirely at variance with the known facts.

There is some leakage from tooth to tooth, the amount varying with the type, but a toothed armature can readily be so designed that with given number of armature inductors and given field magnet, the ampere-turns required on field magnet for given speed, are much less than when a smooth core armature is used; in fact the question usually is, how far the ampere-turns on field may be reduced, and excessive sparking avoided. It is no doubt possible to build generators with either toothed or smooth core armatures, that will not spark under extreme changes of load with the brushes in one position, but it is not hard to find many of both types in operation which do not meet this condition. The .45-inch air-gap from iron to iron, cited as an example of what may be done in a smooth core armature machine of about 30 K. W. would not of course remain the same in similar machines of several hundred K. W. capacity. As the total energy used in field winding of even smooth core armature machines of 100 K. W. or more capacity is usually not more than 1½ per cent. it is hardly possible that a "very much higher efficiency" can be obtained by the use of toothed armatures in such machines.

The main question for which answer was sought in my paper is whether the same results can be obtained in bipolar machines, more cheaply with toothed or smooth core armatures. To those engaged in the manufacture of these machines as a money making enterprise, the answer to this question is much more than a mere matter of taste. The toothed armature for the same capacity must cost more, for while about the same amount of copper, and only a little more iron are required, the additional labor to form the teeth, and cost of tools for same, is a considerable item. Unless the toothed amature can effect some saving in other parts of these machines to offset its own greater cost, the smooth core will have the preference.

In machines under 10 K. W., at about 1500 revolutions per minute, the air space required by winding and clearance is usually greater than that necessary for sparkless operation, and the saving in metal of field core and winding will probably more than offset the greater cost of toothed armatures in these machines. In bipolar dynamos of 20 K. W. or more capacity, at 1200 to 1500 revolutions per minute, the air space required for winding and clearance is usually less than that necessary for sparkless operation. Good builders in this country and Europe are in the main using smooth core armatures in medium and large bipolar dynamos and we can hardly expect to see this practice reversed unless some means not now in use is provided to produce sparkless commutation.

Troy, N. Y., Oct. 3d, 1894.

Memorial to the United States Congress.

Prepared and Submitted by the Committee on Units and Standards.

STATEMENT.

The American Institute of Electrical Engineers is a national organization to promote the arts and sciences connected with the production and utilization of electricity.

It has enrolled among its members all the prominent electrical workers in the United States and has besides many well known electricians of other countries as foreign members.

Its Transactions during the past decade, constitute a complete history of electrical progress, as recorded in the language of the principal promotors of electric science, and are the only publications of the kind in the North American Continent.

At a general meeting of the American Institute of Electrical Engineers, in Philadelphia, on the 15th of May, 1894, the following resolution was unanimously adopted.

Preamble and Resolutions.

Whereas, a bill, numbered H. R. 6,500, has been presented to the Honorable House of Representatives, providing for the legalization in the United States of America by the United States Government, of certain units and standards, and,

Whereas, these units and standards are certain electrical units and standards adopted by the chamber of official delegates to the International Electrical Congress, held at the World's Columbian Exhibition, at Chicago, 25th of August, 1893, which delegates were appointed by the various governments of Europe and America, from among their most eminent electricians, and

Whereas, the principal object of convening the said International Electrical Congress and said chamber of official delegates was to arrive at an international agreement concerning electrical units and standards; and,

Whereas, the United States Government joined the other nations in appointing official delegates to attend said Congress for the purpose of discussing said units and standards and for their recommendation to the various governments for adoption and legalization; and,

Whereas, these international electrical units, as adopted at the said International Electrical Congress: to wit, the international ohm, the international ampere, the international volt, the international coulomb, the international farad, the joule, the watt, and the henry, have already been adopted as legal standards by several of the European Governments; and,

Whereas, these units and standards have received the hearty approval of electricians in all parts of the world,

Therefore, be it resolved by the American Institute of Electrical Engineers, in general session assembled: That it respectfully petitions the Honorable Senate and House of Representatives of the United States to pass and enact the said bill H. R. No. 6,500, and duly to declare the same to be the legal units throughout the United States of America and its Territories.

A paper presented at the Eleventh General Meeting of the American Institute of Electrical Engineers, Philadelphia, May 18th, 1894, President Houston in the Chair.

REACTANCE.

BY CHARLES PROTEUS STEINMETZ AND FREDERICK BEDELL.

The term "reactance,[1]" which has been used and advocated by the writers and others, and which has been officially adopted at our suggestion by the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS at its Philadelphia meeting, is one which assumes particular importance now that the term "inductance" is defined as synonymous with the "coefficient of self-induction," usually symbolized by the letter "*L*." Reactance is similar in many respects to resistance, but the electromotive force used in overcoming reactance consumes no power, for it is at right angles to the current.

The impressed electromotive force in an alternating current circuit may be divided into two components: First, the *power electromotive force* in the direction of the current, and, second, the *reactive electromotive force* in quadrature with the current to overcome the reactance. The reactive electromotive force is the product of the current and the reactance. *The reactance is, accordingly, equal to the component of the impressed electromotive force at right angles to the current, divided by the current.* Reactance is measured in ohms.

The reactive electromotive force in the circuit may be due to self or mutual induction, to capacity or to some outside counter electromotive force produced by a motor or other device. In general, in any alternating current circuit,

1. This term was first suggested by M. Hospitalier (see *L' Industrie Electrique*, May 10, 1893,) and was proposed officially (see *Bulletin*, June, 1893,) by the committee appointed by the Société Internationale des Electriciens to consider the Congress proposals of this INSTITUTE. (See TRANSACTIONS, vol., x., p. 418.) We take pleasure in expressing our appreciation of the praiseworthy efforts of Prof. Hospitalier and the Société Internationale in their advocacy of this and other conventions of nomenclature.

$$\text{Impressed E. M. F.} = \sqrt{\text{Power E. M. F.}^2 + \text{Reactive E. M. F.}^2};$$

that is, the impressed electromotive force is the vector sum of the electromotive force which transmits power and the reactive electromotive force.

Reactance tends to cause a phase difference between current and electromotive force. If θ represents this angle of phase, we have the relation,

$$\tan\,\theta = -\,\frac{\text{reactance}}{\text{resistance}}.$$

When θ is negative, the current lags behind the electromotive force; when θ is positive, the current is in advance of the electromotive force. The expression for the instantaneous value of the current may be written.

$$i = I \sin\,(\omega\, t + \theta),$$

or

$$i = \frac{\text{Impressed E. M. F.}}{\text{Impedance}} \sin \left\{ \omega\, t - \text{arc tan} \frac{\text{reactance}}{\text{resistance}} \right\}.$$

A few illustrations of particular cases will make the use of the term reactance more clear. For simplicity in these illustrative examples, we will consider that no iron is embraced by the circuit.

Circuits Containing Resistance and Inductance.—In a simple circuit containing resistance and non-ferric inductance, the reactance is equal to $L\,\omega$; that is, it is $2\,\pi$ times the product of the inductance and frequency. The impedance being the vector sum of the resistance and reactance, is in this case

$$\text{Impedance} = \sqrt{R^2 + L^2\,\omega^2},$$

where $\omega = 2\,\pi \times$ frequency. In this case all the power is used in overcoming resistance, and the power electromotive force is equal to the ohmic electromotive force, $R\,I$. The reactive electromotive force is equal to the inductive electromotive force $L\,\omega\,I$; hence

$$\text{Impressed E. M. F.} = \sqrt{\text{Power E. M. F.}^2 + \text{Reactive E. M. F.}^2};$$

or,

$$\text{Impressed E. M. F.} = \sqrt{\text{Ohmic E. M. F.}^2 + \text{Inductive E. M. F.}^2}.$$

The impressed electromotive force in this case is the vector sum of the electromotive forces necessary to overcome resistance and inductance.

A consideration of Figs. 1 and 2 (in which positive direction is counter clockwise) will show that the reactance and reactive electromotive force are *positive* in the case of a circuit containing inductance. The impressed electromotive force which is the sum of the two, is, therefore, in advance of the current, which is in the same direction as the power or ohmic electromotive force. The current is indicated by a closed arrow in the figures. The same, however, may be otherwise expressed if we take the impressed electromotive force as our direction of reference, by saying that the *current lags behind* the electromotive force by the angle θ, which angle is negative, therefore, for circuits with inductance according to the relation

$$\tan\theta = -\frac{\text{reactance}}{\text{resistance}} = -\frac{L\,\omega}{R}.$$

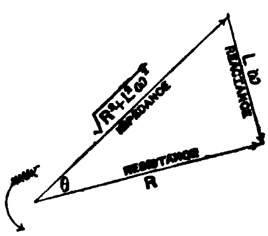


FIG. 1.

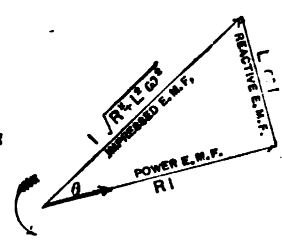


FIG. 2.

The value of the current at any instant is given by the equation

$$i = \frac{E}{\sqrt{R^2 + L^2\,\omega^2}} \sin\left(\omega\,t - \text{arc}\tan\frac{L\,\omega}{R}\right).$$

Circuits Containing Resistance and Capacity.—In such circuits the reactance is equal to $-\frac{1}{C\,\omega}$, where $C =$ capacity, and $\omega = 2\,\pi \times$ frequency, that is, it is negative. The reactive electromotive force is also negative, being equal to $-\frac{I}{C\,\omega}$. This negative reactance gives a positive value to the angle θ, and the current is accordingly in advance of the electromotive force. We have then

$$\text{Impedance} = \sqrt{R^2 + \frac{1}{C^2\,\omega^2}},$$

and

$$\tan\theta = -\frac{\text{reactance}}{\text{resistance}} = +\frac{1}{C\,R\,\omega},$$

similar to the corresponding equations above. The instantaneous value of the current is given by the equation

$$i = \frac{E}{\sqrt{R^2 + \frac{1}{C^2 \omega^2}}} \sin \left(\omega t + \text{arc tan} \frac{1}{C R \omega} \right).$$

These relations are shown in Figs. 3 and 4. They assume the absence of dielectric hysteresis.

Circuits Containing Resistance, Inductance and Capacity.— In a circuit containing inductance and capacity the reactive electromotive force is the sum of the inductive electromotive force, and the condenser electromotive force; where the inductance is non-ferric and dielectric hysteresis absent, this is, $L \omega I - \frac{I}{C \omega}$. The reactance is similarly the sum of two terms, $L \omega - \frac{1}{C \omega}$;

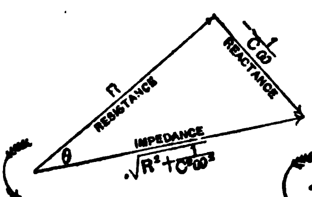


FIG. 3.

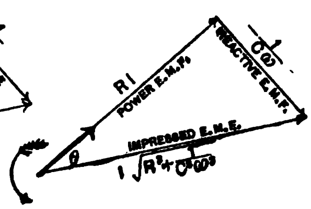


FIG. 4.

thus, the reactance is positive or negative, according to whether the inductive electromotive force is greater or less than the condenser electromotive force. Whether the current is behind or ahead of the impressed electromotive force depends upon the same condition, for

$$\tan \theta = - \frac{\text{reactance}}{\text{resistance}} = \frac{1}{C R \omega} - \frac{L \omega}{R}.$$

The value of the current at any instant is

$$i = \frac{E}{\sqrt{R^2 + \left(L \omega - \frac{1}{C \omega}\right)^2}} \sin \left\{ \omega t + \text{arc tan} \left(\frac{1}{C R \omega} - \frac{L \omega}{R} \right). \right\}$$

The diagrams in Figs. 5 and 6 illustrate the case of a single circuit containing resistance, inductance and capacity. In the case here represented, the condenser electromotive force is greater than the inductive electromotive force, and the current is, therefore, in advance.

Circuits Containing Mutual Induction.—To further exemplify the use of the term reactance, let us consider a circuit containing a transformer. Besides the electromotive forces already discussed, we must consider the back electromotive force due to the influence of the secondary upon the primary circuit. This may be resolved into two components, one in the same direction as the current, and the other in quadrature with it, which will form part of the power electromotive and reactive electromotive forces, respectively. The electromotive force introduced by a motor is treated in the same way.

These illustrations will suffice to show the method by which any electromotive force in a circuit is resolved into two components, one in the direction of the current, which, therefore, transmits power, and the other at right angles to the current, which represents no power, but simply overcomes the reactance.

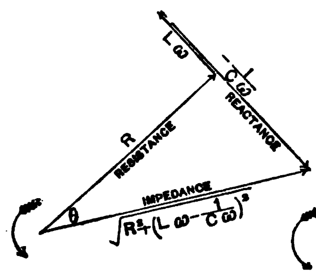


FIG. 5.

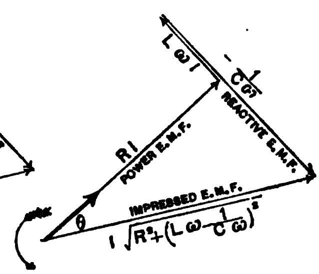


FIG. 6.

It is to be borne in mind that the terms here given may be used in all alternating current circuits, whether the current is harmonic or not; for, when the current is not strictly harmonic, we may consider it equivalent to an harmonic current. The *equivalent* harmonic current and equivalent harmonic electromotive force have the same square root of mean square values as the actual current electromotive force, and have such relative phase positions that the same power is transmitted.

APPENDIX.

THE DEFINITION OF REACTANCE.

[Communicated by the authors, October 31, 1894]

At the Philadelphia meeting of the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS, May, 1894, the name "reactance" was officially adopted by the INSTITUTE and defined in accordance with a paper on Reactance communicated by us and reprinted in most of the electrical periodicals. The action of the INSTITUTE was unanimous and its definition has met with general approval. Objection to it, however, has been raised in an article[1] recently communicated to a French periodical. Since we cannot agree with the arguments forwarded in this article, it will be in place for us to state our views thereon.

The definition of reactance adopted by the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS is:

"Reactance is the component of electromotive force at right angles to the current, divided by the current"

By this definition the term reactance is the quotient of the reactive E. M. F. divided by the current, where the reactive E. M. F. includes all wattless E. M. F.'s, whether due to inductance, capacity, polarization or counter E. M. F. of any kind, as of synchronous motors, and excludes all energy components, as would be introduced by motors, transformers, hysteresis, etc.

In objecting to this definition, the writer referred to takes a position essentially as follows:

(1.) The term reactance shall include the effects of self-induction and capacity only.

(2.) It should always be defined by the equation

$$I = \frac{E}{\sqrt{R^2 + K^2}};$$

Where: $I =$ current;
$R =$ ohmic resistance;
$E =$ electromotive force;
$K =$ reactance.

(3.) For harmonic currents

$$K = \omega\, L - \frac{I}{C\,\omega};$$

Where: $L =$ self-inductance;
$C =$ capacity;

$$\omega = \frac{2\,\pi}{T} = \text{pulsation.}$$

(4.) The term "reactance" has a right to exist only because it is a constant of the circuit. Defined, however, as the quadrature component of E. M. F. divided by the current, it is the complex resultant of different reactions and not a constant of the circuit. All terms in "ance" should denote constants of the circuit, and whenever used in a generalized meaning, an additional term should be added, as "apparent reactance."

We may state that when investigating the question of properly defining the term "reactance," we have fully considered the

1. "A propos de la Reactance," by Professor André Blondel. *L'Industrie Electrique*, Sept. 25, 1894.

position taken by this writer. We have, however, come to the conclusion that his definition is not tenable, but is contradictory, for the following reasons:

(1.) Neither in the one nor in the other definition is "reactance" a constant of the circuit, except in circuits containing no iron. In reality, circuits nearly always contain iron, and in such circuits the reactance can be considered as approximately constant only in a very limited range. When extended over a greater range of E. M. F. or of current, the self-inductance, and thus the reactance, varies. The same applies to most of the other quantities ending in "ance," as impedance, reluctance, and permeance, and thus the statement that quantities in "ance" should be constants of the circuit, is against the adopted practice and not fulfilled by either definition.

(2.) Where iron is present, the statement for harmonic currents

$$K = \omega L - \frac{I}{C \omega}$$

contradicts the definition of K by the equation

$$I = \frac{E}{\sqrt{R^2 + K^2}},$$

R being taken as the ohmic resistance and K as reactance. This is due to the presence of hysteresis. These relations do hold in the absence of iron, and such cases were taken by us in our paper to illustrate the definition for simple cases; the fundamental definition, however, should in our opinion be sufficiently general to include circuits with iron. Consider an harmonic current flowing in a circuit embracing iron; for simplicity assume capacity absent. The last equation would not give any direct relation between the inductance and reactance, such as would be obtained from the equation just preceding, but would make reactance still a more complex quantity, by including therein not only the effect of self-induction but that of hysteresis as well. Furthermore, it would lead to the result that the reactive E. M. F. is not wattless, but includes an energy component, an idea quite foreign to the term.

These considerations obliged us to discard any indirect definition of reactance by means of the term inductance, and to adopt the more direct definition analogous to the definition of resistance.

The definition of resistance is Ohm's law:

$$R = \frac{E}{I},$$

where E and I represent an unvarying E. M. F. and current, respectively.

Considering E and I as an harmonic electromotive force and an harmonic current—or in the case of alternating quantities which are not simple harmonics, as their equivalent harmonic values, that is, values of equal square root of mean square value and equal power—the above equation gives the definition of impedance:

$$\text{Impedance} = \frac{E}{I}.$$

Our proposed definition for reactance is analogously written:

$$\text{Reactance} = \frac{\text{reactive } E}{I}.$$

Further, we may define the apparent[1] resistance of the circuit by the expression

$$\text{Apparent resistance} = \frac{\text{power } E}{I}.$$

In the absence of expenditure of energy outside the electric conductor. this quantity coincides with the true ohmic resistance.

These quantities are thus defined directly, and in a uniform manner. How far these quantities are constants of the circuit depends upon the circumstances; in any case reactance and impedance depend upon the frequency.

The use of "equivalent" values for quantities is employed in the paper on "The Law of Hysteresis, III." In the discussion of this paper at the Philadelphia meeting the significance of these values is pointed out.

The phenomena taking place in an alternating current circuit cannot fully be represented by the terms:—ohmic resistance, inductance, and capacity, but a further term has to be introduced, representing the losses of energy outside of the electric conductor, as hysteresis, etc., and the most satisfactory way to do this appears to us to be the generalization of the term "resistance" and to denote by it "apparent resistance," or by some similar term.

Wherever the reactance is generalized to include active counter E. M. F.'s., it may well be distinguished by the denotation "equivalent reactance."

We would emphasize the utility of *two rectangular components*, whether E. M. F.'s or currents are resolved. Reactance should always, in our opinion, be associated with that which represents no expenditure of energy, the reactive E. M. F. being at right angles to the current and the reactive current (the wattless current) being at right angles to the E. M. F. Whether we resolve currents or E. M. F.'s depends upon the problem in hand; each method has its advantages. If we resolve the current, we may write:

$$(\text{Current})^2 = (\text{power current})^2 + (\text{reactive current}).^2$$

Divided by E, this gives:

$$(\text{Admittance})^2 = (\text{conductance})^2 + (\text{susceptance})^2,$$

1. The term apparent resistance is here used in contradistinction to the true ohmic resistance. Whether the word apparent is the best one for generalizing the term resistance is an open question; we so use the word in the present communication.

for a simple case; in general, for conductance and susceptance we should write apparent conductance and apparent susceptance. Admittance, conductance and susceptance are thus used as the inverse correspondents of impedance, resistance and reactance, and may be added as vector quantities. Many alternating current problems are much simplified by this treatment. It is important, however, to employ components which are at right angles to each other, and for this reason the definition of the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS seems preferable. It is in this point that the definition is fundamentally different from that of the French writer already referred to. In the absence of hysteresis losses, the definitions would be the same, applying the term equivalent reactance to the case where counter E. M. F.'s other than those due to capacity and self-induction, are present. To conclude, then, we may say that in the absence of iron we may define reactance in terms of inductance and capacity, as this writer has done, and as has been done by us in the illustrative examples in our paper; the fundamental definition, however, should, in our opinion, remain in the general form adopted by the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

In a note to our original paper, we have called attention to the first suggestion of the term reactance by M. Hospitalier in May, 1893, and the recommendation of the committee appointed by the Société Internationale des Electriciens to consider the programme for the Chicago Congress, 1893. The term is a happy one; it is international, and uniformity in its use is to be desired.

In our opinion the best definition for "reactance" is that adopted by the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS. In this, as in all matters, there is however room for difference of opinion. The reasons for thus defining the term have not before been published, but we believe that when they are duly considered, the action of the INSTITUTE will meet with international approval.

October 31, 1894.

[COMMUNICATED BY PROF. H. J. RYAN, NOVEMBER 21ST, 1894.]

I like the definition of "reactance" as put forth by the authors of this paper. Had we learned to use alternate currents first, this relation between the constant properties of a circuit, the current and E. M. F. would first have been understood instead of Ohm's law:

$$\text{Impedance} = \frac{E}{I}.$$

Experience would next have taught us that this impedance relation is made up of two fundamental component relations: the one is a power relation, and the other, a wattless relation. Because of our early familiarity with direct currents we learned long ago to write the power relation as:

$$\text{Resistance} = \frac{\text{power } E}{I},$$

and, now, if we accept this definition of "reactance' the second and equally important component of the impedance law will take the same simple form

$$\text{Reactance} = \frac{\text{reactance } E}{I}.$$

Associate Members Elected, and Transferred.

Meeting of Council, June 20, 1894.

Name	Address	Endorsed by
Archbold, Wm. K.	Westinghouse Electric and Mfg. Co., 620 Atlantic Ave., Boston, Mass.	E. L. Nichols. Frederick Bedell. F. B. H. Paine.
Blood, John B.	Assistant Engineer, Railway Dept. General Electric Co., Schenectady, N. Y.	Louis Bell. J. B. Cahoon. C. P. Steinmetz.
Bennett, Edwin H., Jr.	Electrician and Engineer, Diehl & Co., Elizabeth, N. J., and 17 E. 33d St., Bayonne, N. J.	Philip Diehl. G. A. Hamilton. R. W. Pope.
Bliss, George S.	Electrical Engineer. Central District and Printing Telegraph Co., Telephone Bldg., Pittsburg, Pa.	H. Lemp, Jr. H. V. Hayes. Thorburn Reid.
Brady, Frank W.	Post-Graduate Student, and Laboratory Assistant, Purdue University, Lafayette, Ind.	H. B. Smith. W. E. Goldsborough. L. S. Boggs.
Caldwell, Francis C.	Assistant Professor of Electrical Engineering, Ohio State University, Columbus, O.	B. F. Thomas. E. L. Nichols. H. J. Ryan.
Chesney, C. C.	Electrician, Stanley Laboratory, Pittsfield, Mass.	Wm. Stanley. W. B. Tobey W. D. Weaver.
Childs, Arthur Edwards,	*B.Sc. M.E.E.E.* Electrical Engineer, Westinghouse Elec. and Mfg. Co., 302 Girard Bldg., Philadelphia. Pa.	C. A. Bragg. J. A. Seely. E. G. Willyoung
Criggal, John E.	Electrician, Davis Electrical Works, 143 Tenth St, Springfield, Mass.	Edwd. Weston C. F. Whittemore. J. W. Hyde.
Croxton, A. L.	Electrical Engineer, Standard Electric Co., Midwinter Fair, San Francisco, Cal.	W. F. C. Hasson. Wynn Meredith. Sidney Sprout.
Eddy, H. C.	Salesman, Western Electric Co., Room 22, 170 La Salle St., Chicago, Ill.	H. F. Albright. H. H. Wait. C. D. Crandall.
Gossler, Philip G.	Assistant Electrical Engineer, United Electric Light and Power Co., 107 Montague St., Brooklyn, N. Y.	Fremont Wilson. Jos. Wetzler. T. C. Martin.
Hutchinson, Frederick L.	Westinghouse Electric and Mfg. Co., Newark, N. J.; 355 Morris Ave., Elizabeth, N. J.	E. L. Nichols. H. J. Ryan. Frederick Bedell.
Harris, George H.	Superintendent, Electric Car Shops, Birmingham, Ala.	A. F. McKissick. Chas. H. Smith. Cecil P. Poole.
Knox, Frank H.	J. G. White & Co., 2116 N. Charles St., Baltimore, Md.	C. G. Young. Louis Duncan. J. G. White.
Lilley, L. G.	Electrical Inspector, Underwriters' Association of Cincinnati, Wyoming, O.	J. A. Cabot. T. J. Creaghead. H. Ward Leonard. A. R. Foote.
Lloyd, Herbert	General Manager, Electrical Engineer and Chemist, The Electric Storage Battery Co., Drexel Bldg., Philadelphia, Pa.	A. E. Kennelly. E. J. Houston. E. G. Willyoung.

MALIA, JAMES P.	Electrician, Armour & Co., 5314 Union Ave., Chicago, Ill.	C. C. Haskins. M. O'Dea. L. Gutmann.
MAYRHOFER, JOS. CARL	Electrical Engineer, 165 West 82d Street, New York City.	Jos. Wetzler. T. C. Martin. G. M. Phelps.
OUDIN, MAURICE	Electrical Engineer, General Electric Co., Schenectady, N. Y.	C. P. Steinmetz. A. L. Rohrer. H. G. Reist.
PLUMB, CHARLES	Proprietor and Electrician, The Chas. Plumb Electrical Works, 89 Erie St., Buffalo, N. Y.	E. F. Phillips. F. P. Little. Joseph Sachs.
SCHEIBLE, ALBERT	Secretary and Assistant, with George Cutter, 486 North Park Ave., Chicago, Ill.	Geo. Cutter. Fred. DeLand. B. J. Arnold.
STEARNS, JOEL W., JR.	Treasurer, Mountain Electric Co., Box 1545, Denver, Col.	G M. Phelps. L. B. Stillwell. E. J. Houston.
STEPHENS, GEORGE	General Superintendent, Canadian General Electric Co., Ltd., Peterboro, Ont.	H. T. Hartman. W. Rutherford. John Langton.
WINAND, PAUL A. N.	Engineer and Superintendent, Schleicher, Schumm & Co., 3200 Arch St., Philadelphia, Pa.	E. G. Willyoung. Carl Hering. C. O. C. Billberg.

Total 25.

TRANSFERRED FROM ASSOCIATE TO FULL MEMBERSHIP.

Approved by Board of Examiners, March 20th, 1894.

ALBRIGHT, H. FLEETWOOD	Electrical Engineer, Western Electric Co., Chicago, Ill.
DANIELL, FRANCIS G.	Electrical Engineer, State Street H. R. R. Co., New Haven, Conn.
DUNN, GANO S.	Electrical Engineer, of the Crocker-Wheeler Electric Co., Ampere, E. Orange, N. J.
HASKINS, CARYL D.	General Electric Co., Boston Mass.
HOLMES, FRANKLIN S.	Electrical Engineer, with C. H. Davis, [120 Broadway, N. Y.
MANSFIELD, ARTHUR NEWHALL	Assistant Electrician, American Telephone and Telegraph Co., New York City.
MAYER, GEORGE M.	1306 Polk St., San Francisco, Cal.
MOORE, D. MCFARLAN	Electrical Engineer, General Electric Co., 44 Broad St., New York City.
SHEA, DANIEL W.	Assistant Professor of Electrical Engineering and Physics, University of Ill., Champaign, Ill.
SPAULDING, HOLLON C.	P. O. Box 454 Exeter, N. H.
STEVENS, W. LE CONTE	Professor of Physics, Rensselaer Polytechnic Institute, Troy, N. Y.
WAIT, HENRY H.	Assistant Electrical Engineer, Western Electric Co., Chicago, Ill.
WARNER, ERNEST P.	Electrical Engineer, Western Electric Co., Chicago, Ill
WATERMAN, F. N.	Electrical Engineer, Westinghouse Electric and Mfg. Co., 120 Broadway, New York City.
WIRT, CHARLES	Consulting Engineer, and Proprietor The Wirt Laboratory, 56 Fifth Ave., Chicago, Ill.

Total 15.

THE AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

New York, September 19, 1894.

The 89th meeting of the Institute was held this date at 12 West 31st Street, and was called to order at 8:20 P. M. by Vice-President Crocker.

THE CHAIRMAN (Vice-President Crocker):—The first business is the reading of the names of the associate members elected by the Council this afternoon.

The Secretary read the list, as follows:—

Name.	Address.	Endorsed by.
BAKER, GEO. O.,	Local Superintendent, General Electric Co., 44 Broad St.; residence, 450 W. 23rd St., New York City.	J. C. Bennett. D. McF. Moore. W. G. Whitmore.
BERG, ERNST JULIUS,	Engineer, General Electric Co.; residence, 540 Liberty St., Schenectady, N. Y.	C. P. Steinmetz. H. M. Hobart. H. G. Reist.
BLANCHARD, CHARLES M.,	714 Girard Building, Philadelphia, Pa.; residence, 4565 Pulaski Ave., Germantown, Pa.	E. J. Houston. E. G. Willyoung. A. E. Kennelly.
BOILEAU, WILLARD E.	Superintendent and Electrician, Brush Electric Light & Power Co., Columbus, Ga.	A. M. Schoen. A. E. Worswick. Ralph W. Pope.
BRADY, E. D. A.,	Consulting and Constructing Engineer; Lock P. O. Box 132, Waterbury, Conn.	H. Ward Leonard. F. A. Pattison. Ralph W. Pope.
BROWN, EDWARD D.,	District Inspector, American Telephone and Telegraph Co., 18 Cortlandt St., New York City: residence, 75 Hicks St., Brooklyn, N. Y.	F. A. Pickernell. G. A. Hamilton. A. N. Mansfield.
CHASE, HARVEY STUART,	Electrical and Mechanical Engineer, 136 Liberty St., New York City.	H. Ward Leonard. A. S. Vance. Ralph W. Pope.
COMPAGNIE, GEORGE BOUNE,	Chief Engineer, Antwerp Hydro-Electric Supply Co., Antwerp, Belgium.	A. G. Inrig. Ralph W. Pope. G. Sacco Albanese.
CREWS, J. W.,	Manager, Southern Bell Telephone and Telegraph Co., Telephone Exchange, Norfolk, Va.	C. E. McCluer. M. B. Leonard. E. W. Trafford.

CUSHING, HARRY COOKE, JR.,	Electrical Inspector, Boston Board of Fire Underwriters, 55 Kilby St.; residence, 259 Beacon St., Boston, Mass.	Caryl D. Haskins. Geo. F. Curtiss. S. Dana Greene.
DARLINGTON, FREDERIC W.,	Consulting Electrical and Mechanical Engineer, 508 Girard Building, Philadelphia, Pa.	C. A. Bragg. Louis Duncan. E. G. Willyoung.
DELANCEY, DARRAGH,	Manager of Works, Eastman Kodak Co., Rochester, N. Y.	Francis R. Hart. J. P. B. Fiske. Leonard C. Wason.
DRYSDALE, WILLIAM A.,	Consulting Electrical Engineer, Hale Building, Philadelphia, Pa.; residence, Overbrook, Pa.	E. J. Houston. W. J. Hammer. A. E. Kennelly.
DYER, FRANCIS MARON,	Associate Engineer with Chas. L. Eidlitz, 10 W. 23rd St.; residence, 160 W. 129th St., New York City.	F. Broadnox. L. Stieringer. J. C. Hatzel.
EDEN, MORTON EDWARD,	Electrical Inspector, the Underwriters' Association of the Middle Department, Philadelphia, Pa.; residence, 83 Fourth Ave., Pittsburg, Pa.	E. Braddell. L. Knowles Perot. T. C. Martin.
EGLIN, WM. C. L.,	Chief of Electrical Department, Edison Electric Light Co., 909 Walnut St.; residence, 4230 Chester Ave., Philadelphia, Pa.	W. D. Marks. C. W. Pike. E. G. Willyoung.
EIDLITZ, CHAS. L.,	10 W. 23rd St.; residence, 1125 Madison Ave., New York City.	F. Broadnox. L. Stieringer. J. C. Hatzel.
ELLICOTT, EDWARD B.,	Superintendent of Construction, Western Electric Co., 4438 Ellis Ave., Chicago, Ill.	E. M. Barton. C. D. Crandall. T. D. Lockwood.
ERICKSON, F. WM.,	Electrical Engineer, with C. L. Livingston, 713 Penn Ave.; residence, 5812 Parker St., Pittsburg, Pa.	E. G. Waters. S. B. Paine. R. W. Pope.
FULLER, FRANK G.,	Salesman with W. R. Brixey, 203 Broadway, New York City; residence, Meriden, Conn.	G. M. Phelps. F. R. Colvin. W. R. Brixey.
GERSON, LOUIS JAY,	President, The Gerson Electrical Co., 4303 Walnut St., Philadelphia, Pa.	C. W. Pike. Carl Hering. E. J. Houston.
GRISSINGER, ELWOOD ARISTIDES,	Electrical Engineer, Mechanicsburg, Pa.	E. J. Houston. A. E. Kennelly. W. E. Geyer.
HOLLERITH, HERMAN,	Washington, D. C.	T. C. Martin. Jos. Wetzler. R. W. Pope.
HUBLEY, G. WILBUR,	Electrical Engineer, Louisville Electric Light Co., Louisville, Ky.	C. F. Scott. L. B. Stillwell. Alex. J. Wurts.
HUNT, ARTHUR L.,	Electrician, Utica State Hospital, Utica, N. Y.	T. C. Martin. W. W. Nicholson. R. W. Pope.
KAMMEYER, CARL E.,	Western Manager the *Electrical Engineer*, 1439 Monadnock Block, Chicago, Ill.	Charles Wirt. B. J. Arnold. Geo. B. Shaw.
LA ROCHE, FRED. A.,	President and Manager, La Roche Electric Works, American and Diamond Sts.; residence, 2235 N. 16th St., Philadelphia, Pa.	E. G. Willyoung. E. J. Houston. A. E. Kennelly.

LYMAN, CHESTER WOLCOTT,	Manager, Herkimer Paper Co., Herkimer, N. Y.	Alex. J. Wurts. C. F. Scott. O. B. Shallenberger.
LYMAN, JAMES,	Student in Electrical Engineering at Cornell University, 39 Eddy St., Ithaca, N. Y.; residence, Middlefield, Conn.	Fred. Bedell. H. J. Ryan. E. L. Nichols.
MEDINA, FRANK P.,	Electrician, Pacific Postal Telegraph Co., 534 Market St., San Francisco, Cal.	F. F. Barbour. C. L. Cory. J. A. Lighthipe.
MYERS, L. E.,	Secretary and Treasurer, Electrical Installation Co., Monadnock Building, Chicago, Ill.	T. C. Martin. Gilbert Wilkes. Jos. Wetzler.
POTTER, HENRY NOEL,	Electrician, Laboratory of Westinghouse Electric and Mfg. Co., Pittsburg, Pa.	R. B. Owens. E. G. Willyoung. E. J. Houston.
PRICE, CHAS. W.,	Editor the *Electrical Review*, 18 Park Row, New York City; residence, 223 Garfield Place, Brooklyn, N. Y.	G. A. Hamilton. G. M. Phelps. R. W. Pope.
REED, HARRY D.,	Electrician, Bishop Gutta Percha Co., 420 E. 25th St., N.Y. City; residence, 88 N. 9th St., Newark, N. J.	W. E. Geyer. G. A. Hamilton. H. A. Reed.
RICHARDSON, ROBERT E.,	Electrical Engineer, Pierce & Richardson, 3827 Forest Ave., Chicago, Ill.	R. H. Pierce. B. J. Arnold. Edw. Caldwell.
ROBERTS, WM. H.,	Assistant Engineer, South Covington and Cincinnati Street Railway Co., 15 Harrison St., Cincinnati, O.	J. A. Cabot. T. J. Creaghead. A. L. Searles.
ROLLER, JOHN E.,	Lieut. U. S. N., in charge of Inspection and Installation, U. S. Navy Yard, New York; residence, 515 Clinton Ave., Brooklyn, N. Y.	C. P. Steinmetz. F. R. Upton. F. Tischendoerfer.
ROWLAND, ARTHUR JOHN,	Professor of Electrical Engineering, Drexel Institute; residence, 4007 Powelton Ave., Philadelphia, Pa.	H. S. Hering. E. J. Houston. Louis Duncan.
SHIELDS, W. J.,	Professor of Electrical Engineering, University of Vermont, Burlington, Vt.	E. L. Nichols. Fredk. Bedell. C. P. Matthews.
SLADE, ARTHUR J.,	Student Electrical Engineering, Columbia College; residence, 62 E. 66th St., N. Y. City.	F. B. Crocker. M. I. Pupin. W. H. Freedman.
SMITH, FRANK E.,	Chief Electrician, Edison Light and Power Co., 229 Stevenson St., San Francisco, Cal.	F. F. Barbour. C. L. Cory. J. A. Lighthipe.
STEVENS, J. FRANKLIN,	Secretary and Treasurer, La Roche Electric Works, American and Diamond Sts.; residence, 1419 Walnut St., Philadelphia, Pa.	E. G. Willyoung. E. J. Houston. A. E. Kennelly.
TAIT, FRANK M.,	Superintendent Catasauqua Electric Light and Power Co., 731 3rd St., Catasauqua, Pa.	R. W. Pope. H. A. Foster. J. W. Lattig.
VARLEY, THOMAS W.,	Electrician, The Okonite Co., Ltd., Passaic, N. J.	G. A. Hamilton. G. W. Gardanier. Jas. Hamblet.

Total, 44.

THE CHAIRMAN:—The time is somewhat limited, and we will pass directly to the reading of the papers. The first paper is entitled "A Study of the Residual Charges of Condensers and their Dependence upon Temperature," by Frederick Bedell and Carl Kinsley. This paper is to be read by title only, in the absence of the authors, as it has been printed and in the hands of the members for some time before the meeting. The paper is now open for discussion, if there are many remarks to be made upon it. The subject is rather in the line of pure physics, but is nevertheless of considerable practical importance nowadays as condensers are coming more and more into use in connection with alternating current apparatus.

A paper presented at the 89th Meeting of the American Institute of Electrical Engineers, New York, Vice-President Crocker in the Chair, Chicago, Professor Stine in the Chair, September 19th, 1894.

A STUDY OF THE RESIDUAL CHARGES OF CONDENSERS, AND THEIR DEPENDENCE UPON TEMPERATURE.

BY FREDERICK BEDELL, PH.D., AND CARL KINSLEY, A.B., M.E.

When a condenser is subjected to a difference of potential for some time, part of the charge received is absorbed by the dielectric and a part remains upon the surface of the condenser plates.[1] This latter part becomes discharged when the terminals of the condenser are connected together through a conducting circuit, and the condenser appears to be in a perfectly neutral condition. The absorbed charge, however, still remains in the condenser and will gradually come to the surface in the form of a residual charge of the same sign as before. If this be discharged, and the condenser be allowed to stand insulated, a second residual charge will collect in the same way as the first. A series of residual charges with rapidly diminishing values may thus be formed. Therefore, the condition of a condenser is dependent upon its past charges, some of which may have been held by the dielectric for weeks or even months before appearing upon the surface; a former charge of one sign may entirely neutralize a subsequent charge of opposite sign and smaller value. It has been justly said that the past history of a condenser must be known before it can be trusted.

The object of the present investigation was to determine this

1. The part absorbed depends directly upon the duration of the time of charge; the part remaining on the surface is practically independent of the length of charge, for the condensers used in the present investigation. This was shown in a preliminary investigation by Messrs. W. M. Craft and H. B. Henderson, who showed the first discharge, as measured by the throw of a ballistic galvanometer, to be independent of the length of charge.

soaking-in effect of dielectrics. A study was made,[1] first of the successive residual discharges of a neutral condenser and then of the effects produced by previously charging the condenser in the opposite direction. Figs. 1–10 show some results from this part of the investigation. The absorption by the dielectric was next studied by allowing a charged condenser to stand insulated, and discharge through its own dielectric. The effect of a previous negative charge upon this absorption and upon the apparent resistance of the condenser was thus studied, as shown in Figs. 11 and 12. An investigation was then made of the influence of initial potential upon the discharge curves and the insulation resistance, the results being shown in Fig. 13. An examination of temperature effects was then undertaken. The influence of tem-

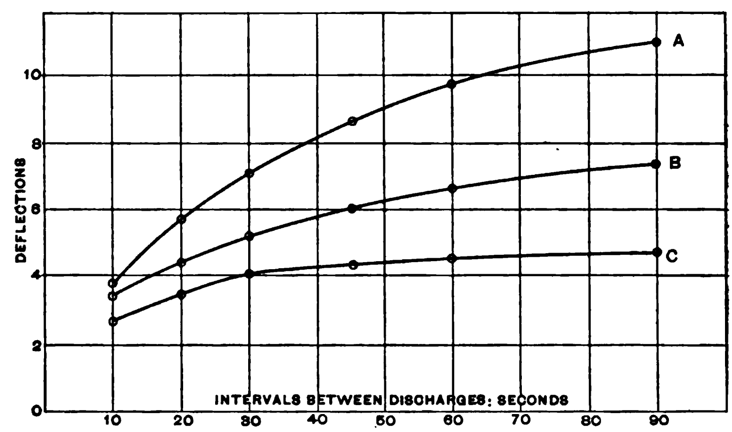


FIG. 1.—First Residual Discharge. Curve A for no previous charge; curve B for a previous negative charge for 30 seconds; curve C for a previous negative charge for 60 seconds.

perature changes upon discharge curves through various commercial oils, and upon the resistance of the oils (see Figs. 14, 15 and 16), was ascertained preliminarily to similar experiments upon condensers. Finally, the influence of temperature upon the absorption by the dielectric of a condenser was obtained by a study of the successive residual discharges (Figs. 17 and 18), and by discharge curves (Fig. 19) at different temperatures. We will discuss separately the several parts of investigation.

1. The condensers used throughout these experiments were furnished by Mr. Stanley in the spring of 1893, and were used in the experiments upon "Hedgehog Transformer and Condensers," described in a paper before this INSTITUTE, Oct. 18, 1893. For their construction see TRANSACTIONS, vol. x., p. 514.

The Effect of a Previous Negative Charge upon Successive Residual Discharges.

Previous to the final charge, the condenser was subjected, for this series of experiments, to a charge of opposite sign. The final charge was in every case in the same direction (arbitrarily called positive) and for the same length of time, viz., 30 seconds. Experiments were made with previous negative charges, *i.e.*, charges opposite in direction to the final charge, of different duration. The value of the positive or negative potential to which the condenser was charged was in all cases about 114 volts. After the final positive charge of 30 seconds, the condenser was

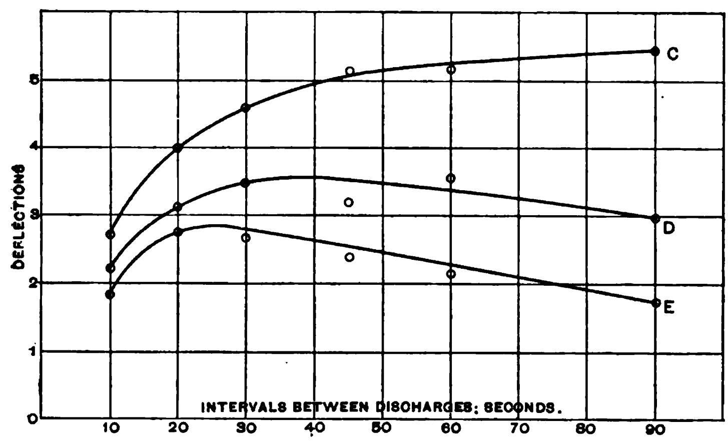


Fig. 2.—First Residual Discharge. Previous negative charge as follows; curve C 60 seconds; curve D 90 seconds; curve E 120 seconds.

allowed to stand insulated for five seconds, and was then short-circuited for 15 seconds. After this discharge by short-circuiting, the condenser was allowed to stand insulated for definite intervals of time, during which the absorbed charges worked out to the surface. It was then discharged through a galvanometer, the throw of which furnished a comparative measure of the discharges. Successive residual discharges were thus obtained by allowing the condenser to stand after each discharge for an interval of time, during which the residual charges worked to the surface. The first, second, third and fourth residual discharges were obtained with equal intervals of time between the succes sive discharges. This was repeated, allowing different intervals of time between the discharges, and the results are shown in Figs. 1–8. In these figures, curves A, B, C, D and E represent the residual discharges corresponding to different lengths of previous negative charge. In the case of curve A, there was no previous charge of any kind, the condenser being as nearly neutral as possible when the final positive charge of 30 seconds was given. The measurements for curve B were made upon the same condenser, which was given a previous negative charge for 30 seconds, was then allowed to stand insulated for five seconds, and then short-circuited for 15 seconds previous to the final posi-

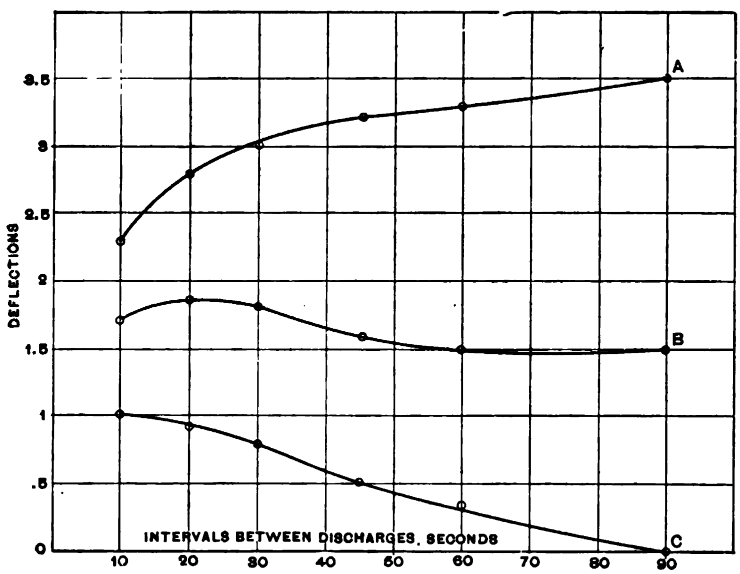


FIG. 8.—Second Residual Discharge.

tive charge. For curve C, the condenser received a preliminary negative charge for 60 seconds, stood insulated for five seconds and was then short-circuited for 15 seconds previous to the final positive charge. For curves D and E, the previous negative charges were 90 and 120 seconds respectively, all other operations being as before. The absorption, due to a positive charge, deflected the needle to the right, and such deflections are plotted, above the X-axis; deflections to the left, due to a negative charge, are plotted below the axis. In taking curves D and E, the sensitiveness of the galvanometer was changed, and curve C was, therefore, determined twice, in order to show the relation between curves A and B and curves D and E.

The effect of the previous negative charge in partially or

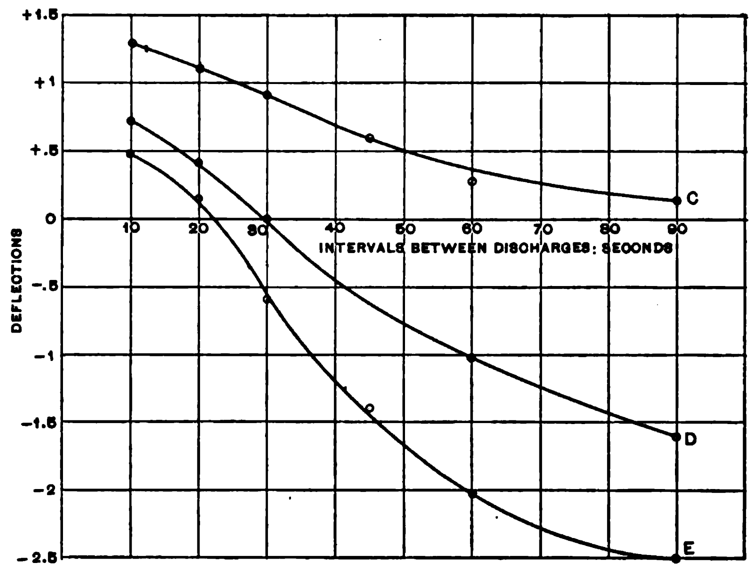


FIG. 4.—Second Residual Discharge.

wholly neutralizing the final positive charge is clearly seen by an inspection of the curves. This influence becomes more and

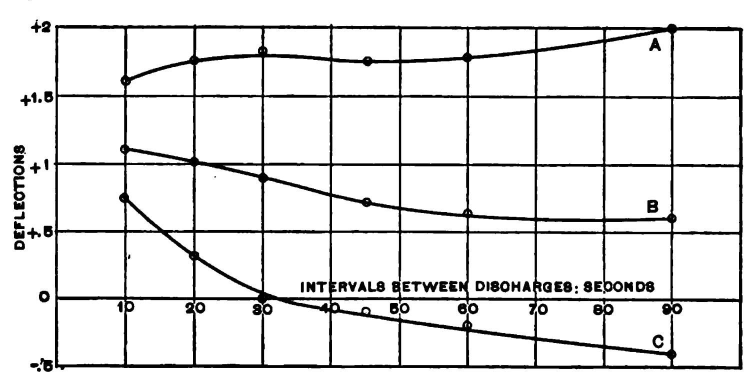


FIG. 5.—Third Residual Discharge.

more marked with each successive discharge, and still more apparent as the intervals between the discharges are made greater. This would naturally be so, since the previous residual charge has time to work out and neutralize the absorption due to the final charge.

In the first residual discharge curves, Figs. 1 and 2, the previous charge has evidently the most influence where the intervals between discharges are the longest, and although the discharges are all positive, it is not at all unlikely that curves D and E, at any rate, would be brought to zero, or even made negative by increasing the intervals between discharges.

In the curves for the second residual discharges, Figs. 3 and 4, curve C is brought to zero, while curves D and E actually cross the X-axis, indicating a negative discharge; that is, one opposite

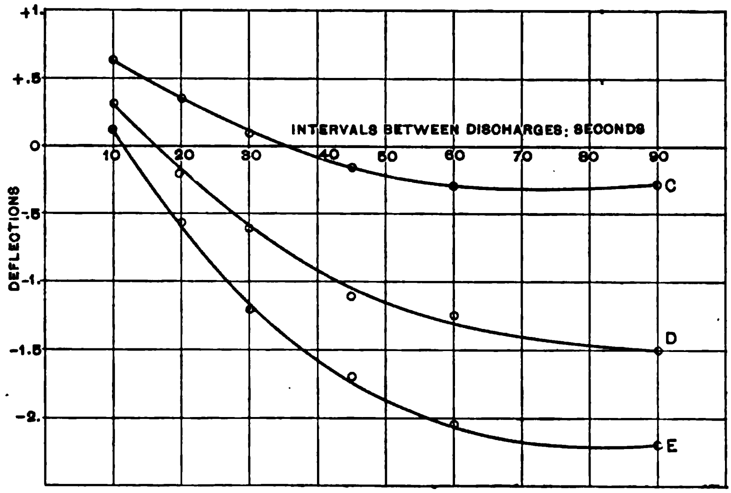


FIG. 6.—Third Residual Discharge.

in direction to the final charge. The evident turning again of the lower part of the curves indicates that the previous negative charge is becoming weakened.

All these effects are more marked in the curves for the third residual discharges, Figs. 5 and 6, where in the case of curves D and E, the 10-second interval is the only one giving a residual discharge in the direction of the final charge. These curves are very nearly of the form which they would have if the final positive charge had not been given to the condenser at all, and the residual discharges were due only to the previous negative charge as is more plainly seen by inverting the figure. As is usual, the fourth residual discharge curves are rather flat, but curves D and E, as they indicate the presence of merely a negative charge, resemble the usual second discharge curves and are considerably concaved towards the X-axis.

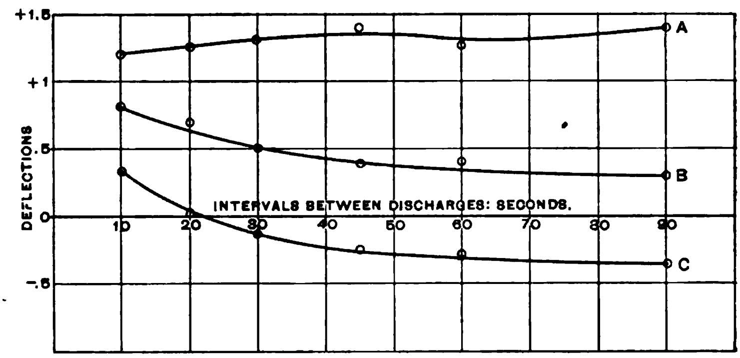


FIG. 7.—Fourth Residual Discharge.

The fifth and sixth discharge curves were obtained, but were not sufficiently marked to warrant reproduction; they merely emphasized the foregoing observations.

The curves shown in Figs. 9 and 10 are obtained from the preceding curves, and show the effect of a change in the length of the previous negative charge given to the condenser before the final charging. The residual discharges, as measured by the throw of

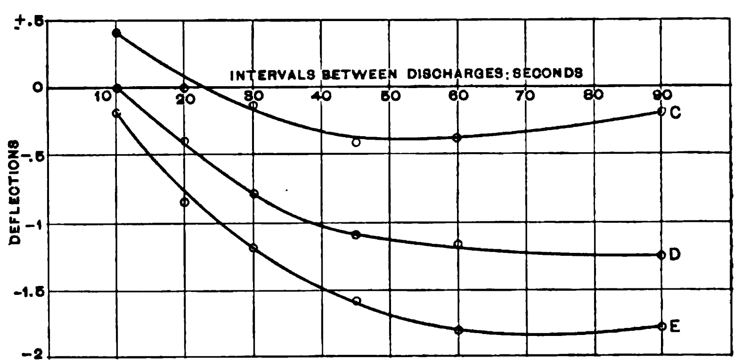


FIG. 8.—Fourth Residual Discharge.

the galvanometer, are represented as ordinates, and the length of the previous negative charges as abscissæ. Two sets of curves are drawn, obtained with intervals between the successive discharges of 45 and 90 seconds, respectively. In the case of the curves corresponding to intervals of 90 seconds, the residual discharges decrease with the increase of the previous negative charge, more rapidly than for the corresponding curves taken with 45-second intervals between the successive discharges. This

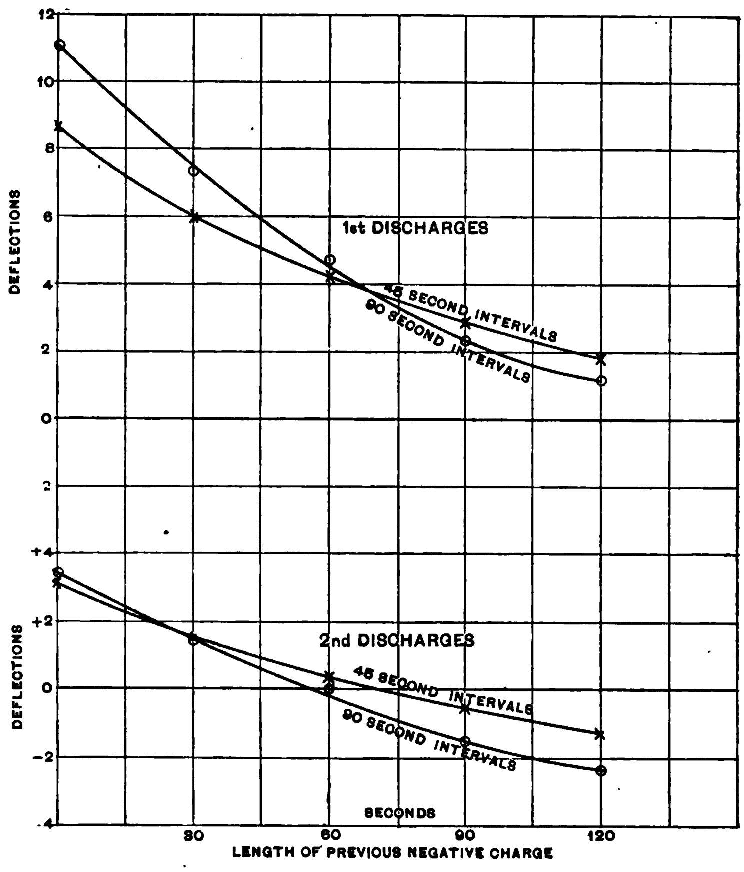


FIG. 9.—Effect of Previous Negative Charge upon Residual Discharges.

is in accordance with the interpretation of the previous results. These curves show plainly that the absorption of the previous negative charge becomes greater as the time of this charge is increased; and the smoothness of the curves would indicate that the dependence of absorption upon time, follows some definite law. The curves would probably be asympotic to some line determined by the condition of the condenser and the nature of the dielectric. This line would then represent the limit to the amount of absorption.

The reflex nature of the curves, particularly of D and E, Figs. 4 and 6, suggested that they might be made to again cross the axis

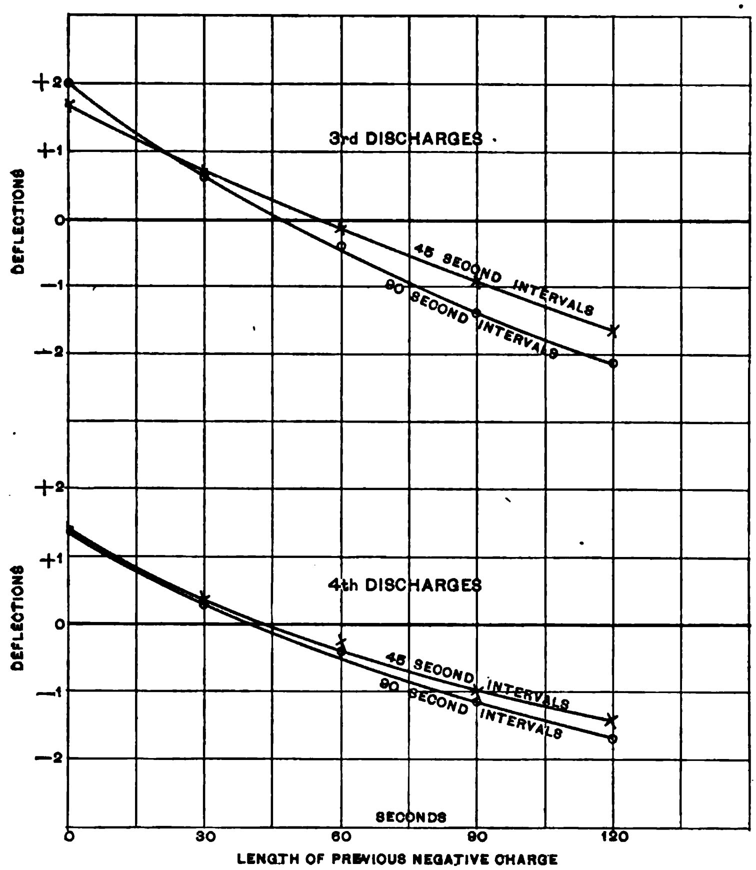


FIG. 10.—Effect of Previous Negative Charge upon Residual Discharges.

of X if three charges were given them, the first being in the direction of the final charge. This was done, and upon short-circuiting the condenser and then taking residual discharges at 10-second intervals, the galvanometer first showed positive, then negative, and finally again positive residual discharges.

With longer intervals between discharges it was found that residual charges of opposite sign more thoroughly neutralized each other. Hence, a condenser, if charged in both senses for different periods, would show before long a residual discharge merely in the direction of the greatest absorption. If the period were made long enough this would likewise leak away, and the curve would approach the axis of X; although, probably, the condenser would never become absolutely neutral.

The Effect of Absorption upon the Discharge Curves.

The curve for the discharge of a condenser through its own dielectric deviates widely from an exponential curve on account

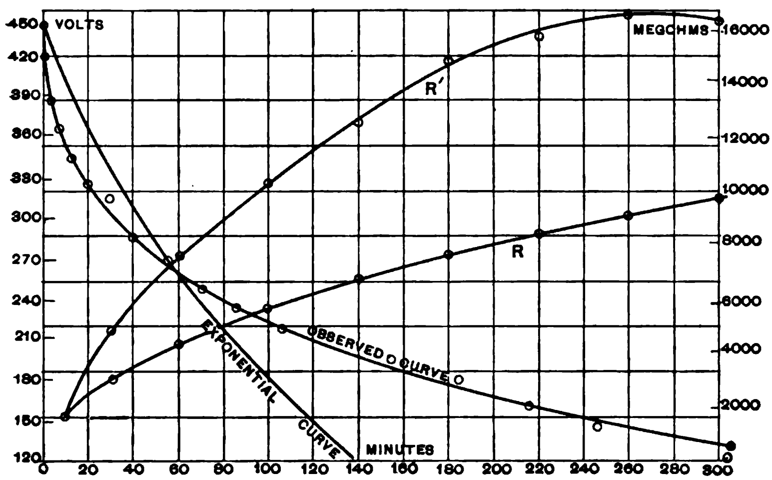


Fig. 11.—Condenser Discharge Curve and Computed Resistance. (Condenser No. 3.)

of the absorption of charge in the dielectric; for, as a condenser discharges, this residual charge works out to the surface and becomes effective. The form of the discharge curve depends likewise upon previous charges, whether of the same or of opposite sign. The resistance of a condenser is similarly dependent upon this absorption.

The discharge curves of the condenser were obtained by measuring the difference of potential at its terminals, by means of a multicellular voltmeter, as the condenser was allowed to discharge through its own dielectric. The leakage through the voltmeter was found to be negligible. Assuming that the discharge of the condenser follows the exponential law, we have

$$V_t = V_0 \varepsilon^{-\frac{t}{RC}}$$

where V_0 represents the initial potential; V_t the potential at some time t counted from the beginning of discharge; C, the capacity of the condenser; R the insulation resistance, and ε the base of the Napierian logarithms. This gives

$$R = \frac{t}{C \log_\varepsilon \frac{V_0}{V_t}}.$$

This value of the resistance is the value which would be true if

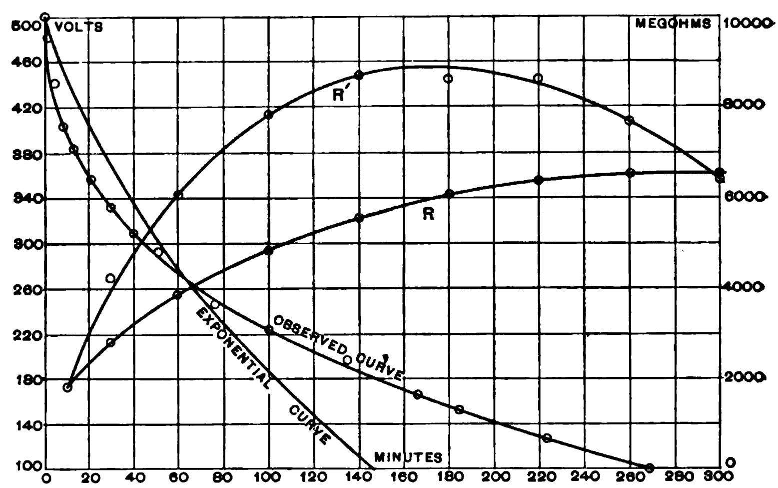


FIG. 12.—Condenser Discharge Curve and Computed Resistance. (Condenser No. 3.)

the discharge curve were exponential from the beginning of discharge to the time t.

The resistance may be otherwise computed by taking brief intervals of time and assuming the curve to be exponential for this time. Thus, if V_1 and V_2 are the potentials at the times t_1 and t_2, we have

$$R' = \frac{t_2 - t_1}{C \log_\varepsilon \frac{V_1}{V_2}}.$$

The value of the resistance thus obtained, which we will call R', is a closer approximation than the resistance R obtained above, and becomes more nearly a true value for the apparent resistance of a condenser as the interval between t_1 and t_2 is shortened.

In Fig. 11 is given the observed discharge curve from a condenser, which, although used for some time, had always been charged in the same direction. An exponential curve is also plotted with the assumed constant resistance of 4,250 megohms. The derived curves showing the variation of the resistances R and R', obtained as explained above, are also given.

This condenser was left short-circuited for 88 hours, and then charged in the opposite direction, and the curve of discharge obtained as above. The results are shown in Fig. 12. The previous charge affects the shape of the discharge curves, but its influence is more clearly seen in the curves for the variation of resistance. The increase in apparent resistance, shown in Fig. 11, is due, as has already been explained, to the absorption in the dielectric. In Fig. 12 the resistance increases at first, due to the absorption of the final charge, and then decreases as the previous absorption of opposite sign begins to be effective. From this it is evident that the soaking-in effect, which is the cause of the usual rise of resistance, must have acted in the opposite direction, and so must have hastened instead of hindering the discharge. Where the curve for resistance R', calculated for short intervals of time from the discharge curve, is horizontal, the two absorbed charges neutralize each other, the resistance is constant and the discharge curve is truly exponential.

The exponential curves, of course, cross the actual discharge curves when the resistance of the condenser is the same as the constant resistance assumed for the exponential curve. The resistance assumed in the case of an initially neutral condenser should be so small that the exponential curve would lie entirely below the observed discharge curve, inasmuch as the apparent resistance is increased by the coming out of the charges previously absorbed.

The Influence of Initial Potential upon the Average Resistance.

To show more definitely that the departure of the discharge curve from an exponential form, and the variation of resistance were due to absorption, and were consequently functions of the time rather than potential, discharge curves were taken through a wide range of initial potential, and the average resistance calculated by taking the mean values of the resistances obtained after 60, 160 and 260 minutes, according to the two methods above explained. Two condensers were used to check the accuracy of the work. The results agree throughout, and show a practically constant resistance throughout the range of initial potential employed, viz., from 150 to 450 volts, as shown in Fig. 13. The condensers were designed to stand a pressure of 500 volts, so we may conclude that while the insulation remains un-

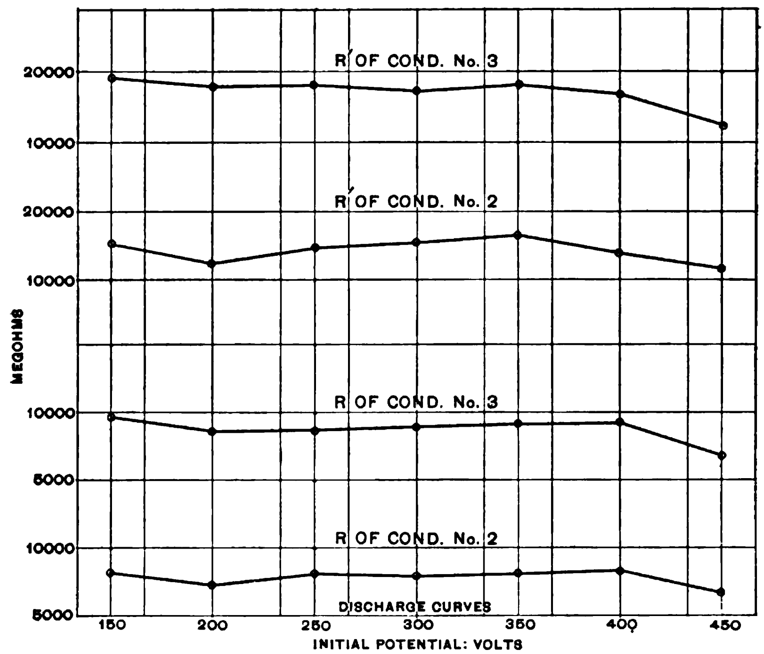


FIG. 13.—Average Resistance Corresponding to Different Initial Potentials.

strained, the changes which occur are due to what may be termed local causes—such as the conditions of the atmosphere and of previous charge—rather than to the initial potential of the condenser.

THE EFFECT OF TEMPERATURE UPON THE RESISTANCE OF OILS.

The foregoing results show the effects of absorption in a solid dielectric. The results which follow show that the absorption in ordinary dielectrics, as shown by the residual discharges

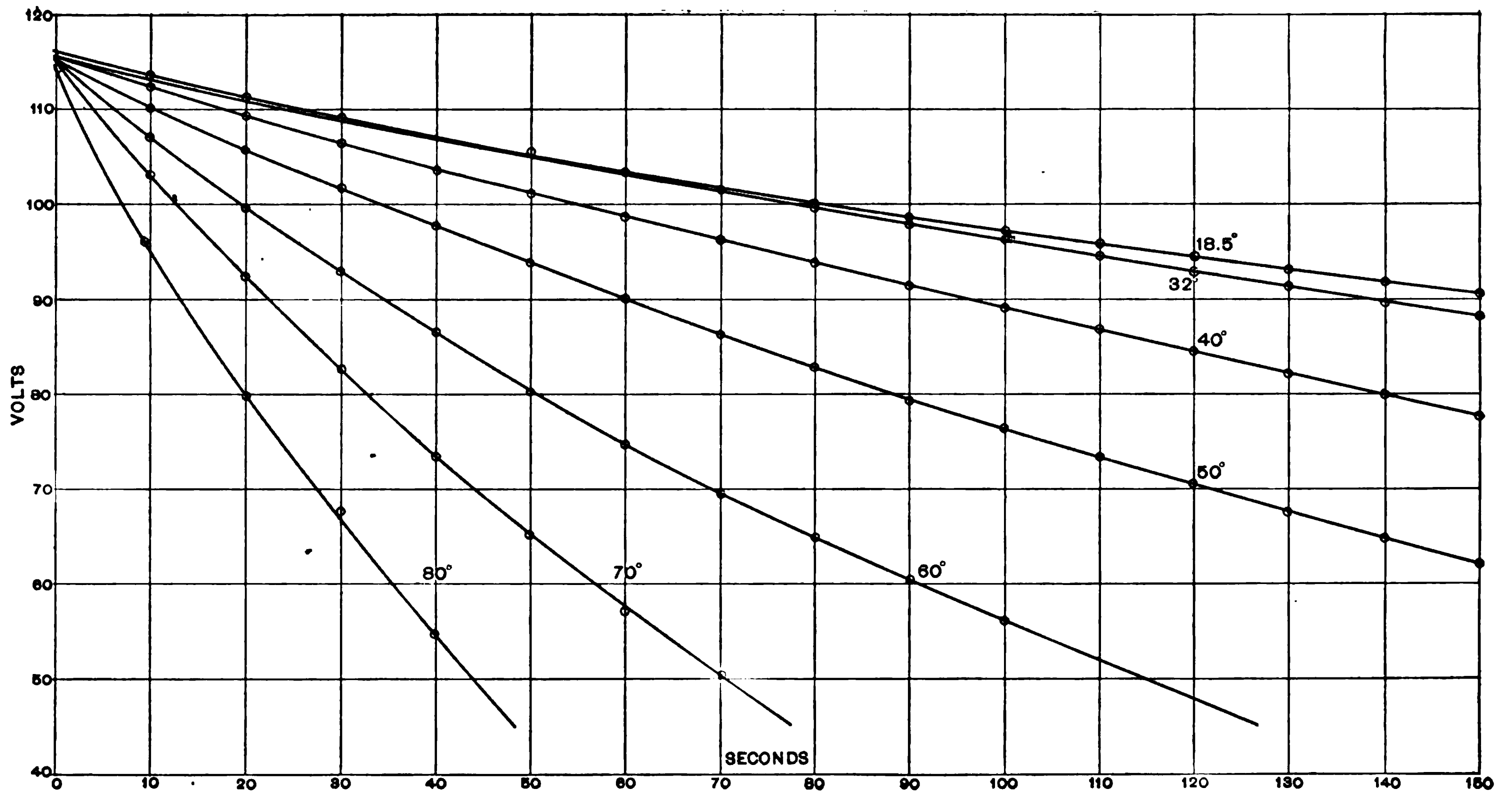


FIG. 14.—Discharge through Vacuum Oil at Different Temperatures.

and the curves of discharge of a condenser through the dielectric, are closely dependent upon temperature changes, and that

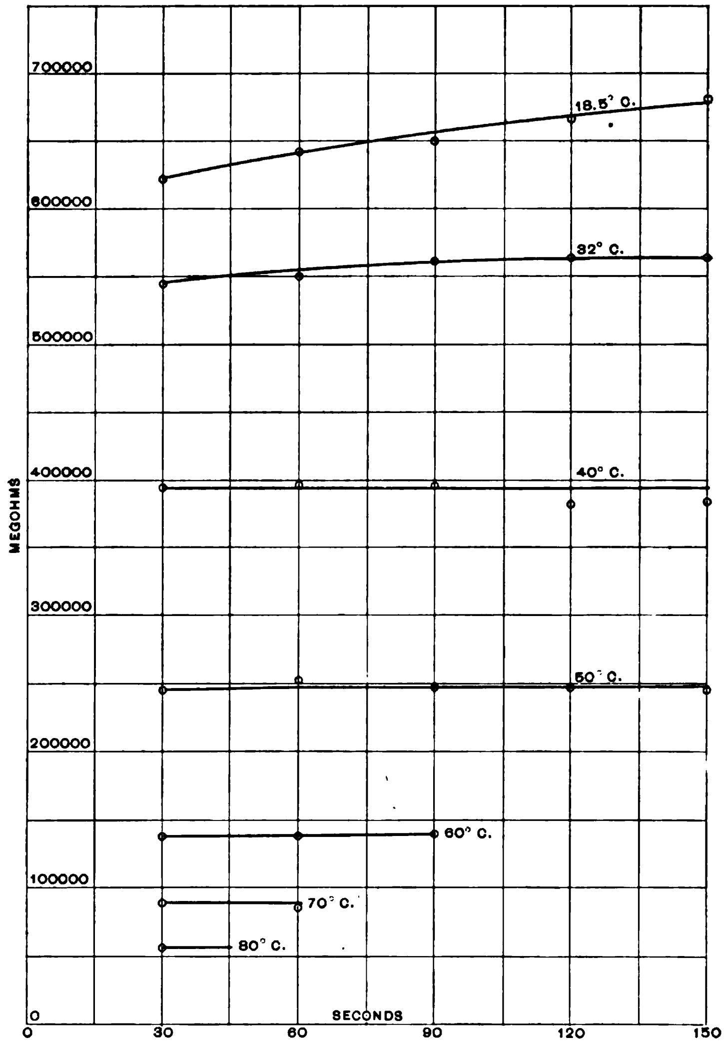


FIG. 15.—Resistance of Vacuum Oil at Different Temperatures.

the effects of absorption and residual discharge disappear entirely at high temperatures. An investigation of the effect of temperature changes upon the resistance of various commercial oils was made preliminarily to a similar investigation upon solid dielectrics. The experiments for the determination of these temperature effects were conducted, under the direction of one of us, by Mr. E. G. Gilson and Mr. E. G. Mason, and are described at length in their report[1] upon the work.

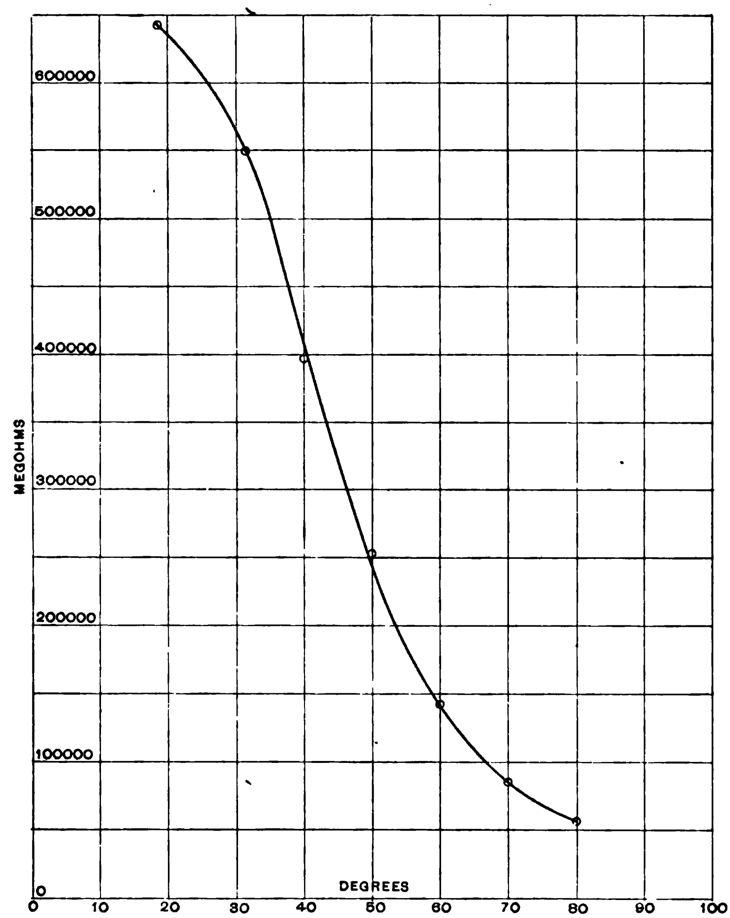


FIG. 16.—Resistance of Vacuum Oil at Different Temperatures after 60-second Electrification.

In perfect oils there should be no absorption of charge, and the curves for the discharge of a condenser through such oils should follow the exponential law. Two surface plates were im-

1. See MS. thesis in the Cornell University Library "Effects of Temperature upon Dielectrics," by E. G. Gilson and E. G. Mason.

mersed in the oil experimented upon; these plates were 25.25 cm. long by 17.5 cm. wide, and were separated by three hard-rubber pillars 1.47 cm. high. The resistance of the oil was determined by discharging a condenser of known capacity through the oil by connecting the terminals of the condenser to these two surface plates. The potential at the terminals of the condenser was measured by a multicellular voltmeter, and the resistance computed according to the exponential law. The resistance of the rubber pillars, the multicellular voltmeter and of the condenser insulation were so small compared with that of the oil, that no correction for them was found necessary.

The results for different oils were similar, and curves are, therefore, given for vacuum oil alone.

Fig. 14 shows the curves for the discharge of a condenser through vacuum oil at different temperatures. The temperature of the condenser and its capacity were constant. The resistance of the oil computed from the discharge curves according to the first method already described, is plotted in Fig. 15. For the lower temperatures there is a slight increase in the resistance during the discharge. This has already been commented upon in the case of solid dielectrics, and indicates that there has been some absorption, possibly due to impurities in the oil. That the resistance is more nearly constant, and the discharge curves exponential at the high temperatures, indicates that absorptive effects disappear at these temperatures.

Fig. 16 shows the resistance of vacuum oil at different temperatures after an electrification[1] of 60 seconds. This decrease in resistance is most rapid between 30° and 60° where the resistance diminishes over 80 per cent. The same phenomena were observed in the experiments with boiled linseed oil, petroleum oil and cylinder oil. The results are given in the accompanying tables.

RESISTANCE IN MEGOHMS OF 1 CU. CM. OF VACUUM OIL.

Time in Secs.	18.5°	32°	40°	50°	60°	70°	80°
30	622,000	547,550	395,000	247,500	139,200	91,250	58,600
60	643,500	549,500	396,500	252,500	140,100	88,000	
90	649,900	560,000	396,600	249,500	142,000		
120	666,000	564,500	384,000	249,000			
150	682,000	564,000	383,500	245,000			

1. This word as here used may be misleading. It is used to denote the time the condenser stands electrified after being separated from the source of potential

RESISTANCE IN MEGOHMS OF 1 CU. CM. OF CYLINDER OIL.

Time in Secs.	20°	40°	50°	60°	70°	80°	90°
30	141,100	136,000	78,200	60,300	40,850	36,830	37,420
60	141,000	140,950	78,750	63,200	42,700	38,750	39,300
90	162,950	142,500	80,350	64,200	53,600	39,550	40,450
120	165,800	146,900	82,100	63,950	44,220	40,300	40,500
150	185,200	148,200	83,600	64,900	44,550	40,850	41,650

RESISTANCE OF BOILED LINSEED OIL.

Time in Secs.	Resistance in Megohms.		
	20°	30°	40°
10	....	1,263	770
30	2,310	1,181	1,084
60	2,340		

RESISTANCE OF PETROLEUM OIL.

Time in Secs.	Resistance in Megohms at				
	13°	20°	30°	40°	50°
30	44,700	38,900	28,150	20,200	14,200
60	45,650	39,990	28,210	19,740	
90	46,550	40,850	28,720		
120	47,150	40,200			
150	47,550				

TEMPERATURE EFFECTS UPON SOLID DIELECTRICS.

For this investigation the same condensers were used as in the experiments described in the earlier parts of this paper. An investigation of the dielectric hysteresis of these condensers and the effects of temperature upon capacity have already been described at length in a paper[1] in the *Physical Review*, where a detailed description of the condensers is also given.

A determination of the capacity of the condenser was made in connection with these earlier experiments, and also during the present investigation a year later. The values are here given for comparison.

Condenser.	No. 2.	No. 3.	No 4.
Capacity (Microfarads) in 1893	1.571	1.52	1.539
" " " 1894	1.592	1.535	1.554

1. Bedell, Ballantyne and Williamson "Alternate-Current Condensers and Dielectric Hysteresis," *Physical Review*, vol. i., No. 2.

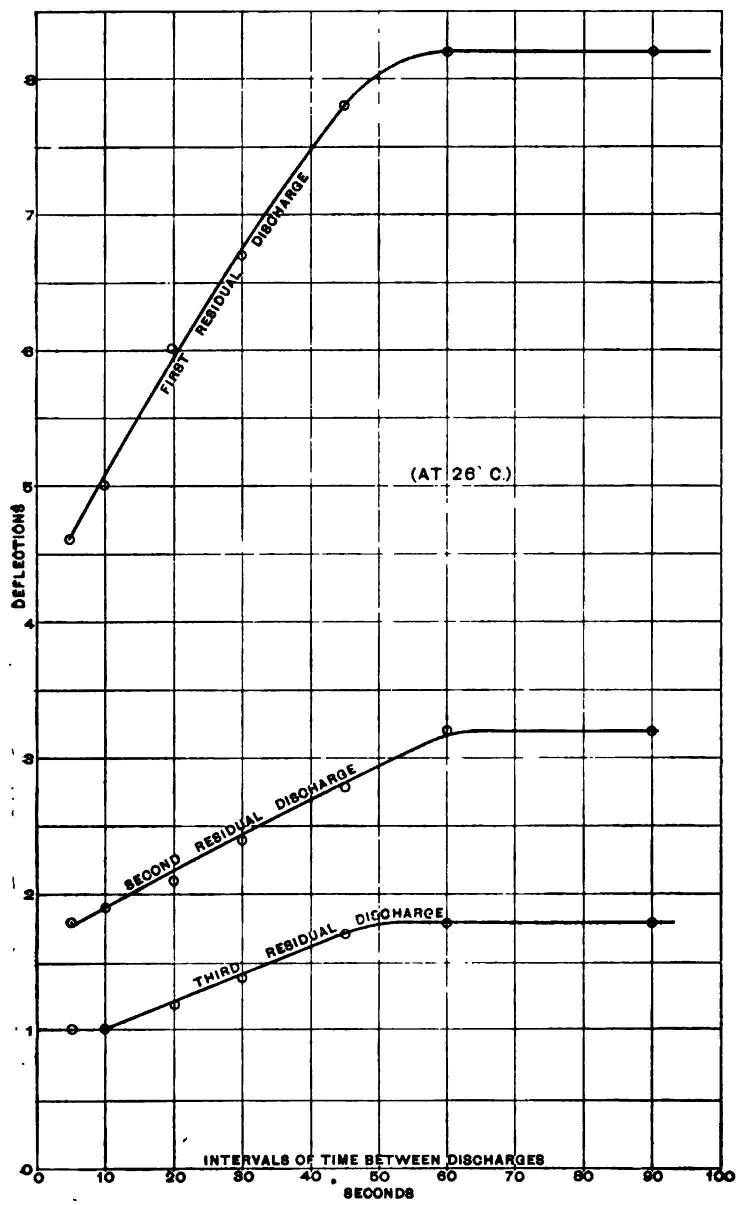


Fig. 17.—Residual Discharges of Condenser at 26° C.

The measurements were made in each case by the method of mixtures, but with different apparatus so that some of the variation may be due to the change of standards. The condensers, however, had stood severe treatment, being in continual use for a year under all sorts of conditions and a slight change of capacity might be looked for.

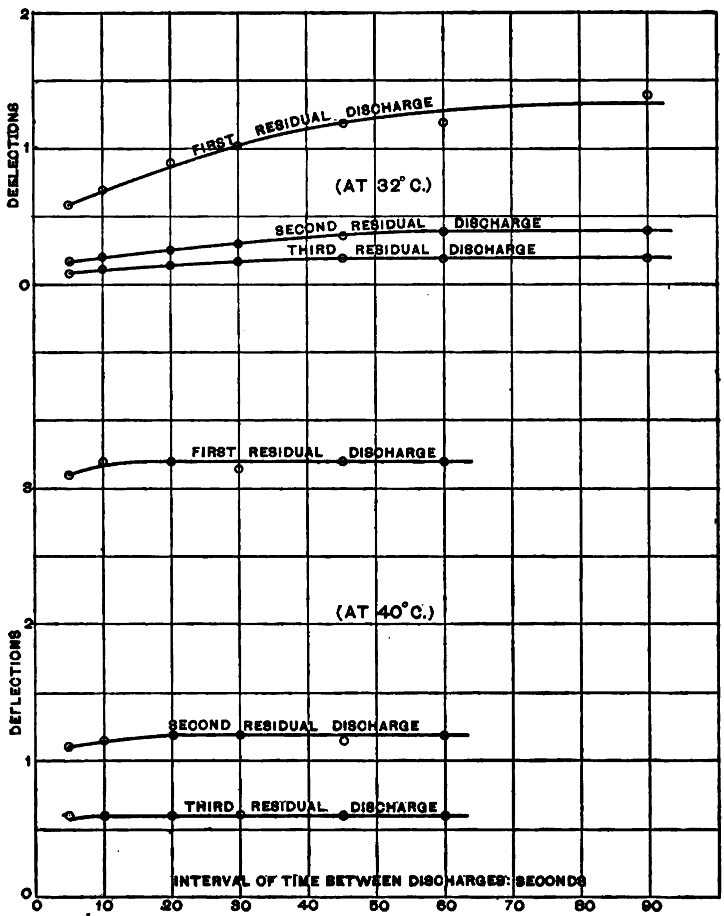


Fig. 18.—Residual Discharges of Condenser at 32° C and 40° C.

Temperature Effects upon Residual Discharges.—The first, second, and third residual discharges were obtained at different temperatures by the method used in the experiments already described. The sensitiveness of the galvanometer was not kept constant so that the deflections cannot be compared so as to show the relative values of the absolute quantity of electricity constituting the residual discharges under different conditions. The shape of the curves plotted in Figs. 17 and 18, however, indicates the relative amount of absorption at different temperatures, which is the point in question.

The residual discharges at 26° C. show a large amount of absorption, as it took long intervals between discharges to equalize the residual discharges.

Residual discharges at 32° C. give curves much flatter; the interval between discharges does not largely affect their amount.

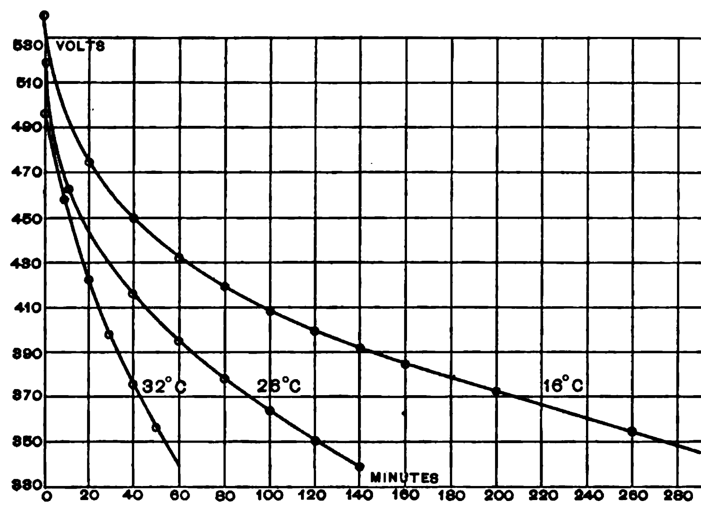


FIG. 19.—Condenser Discharge Curves at Different Temperatures.

Residual discharges at 40° C. give remarkably flat curves, the intervals between discharges having, practically, no effect upon the discharges. This indicates that there was but little absorption. Whatever residual charge there was, so readily passed out, that a large increase in the intervals between discharges did not affect the quantity of the residual discharge.

These experiments show conclusively that absorption, as shown by residual effects, disappears at high temperatures.

Temperature Effects upon Discharge Curves and Insulation Resistance.—The insulation resistance of a condenser was obtained by allowing the condenser to discharge through its own insulation, and then solving the usual exponential equation for the resistance. The capacity of the condenser at different temperature was known as the result of the experiments already referred to.

The discharge curves at different temperatures are given in

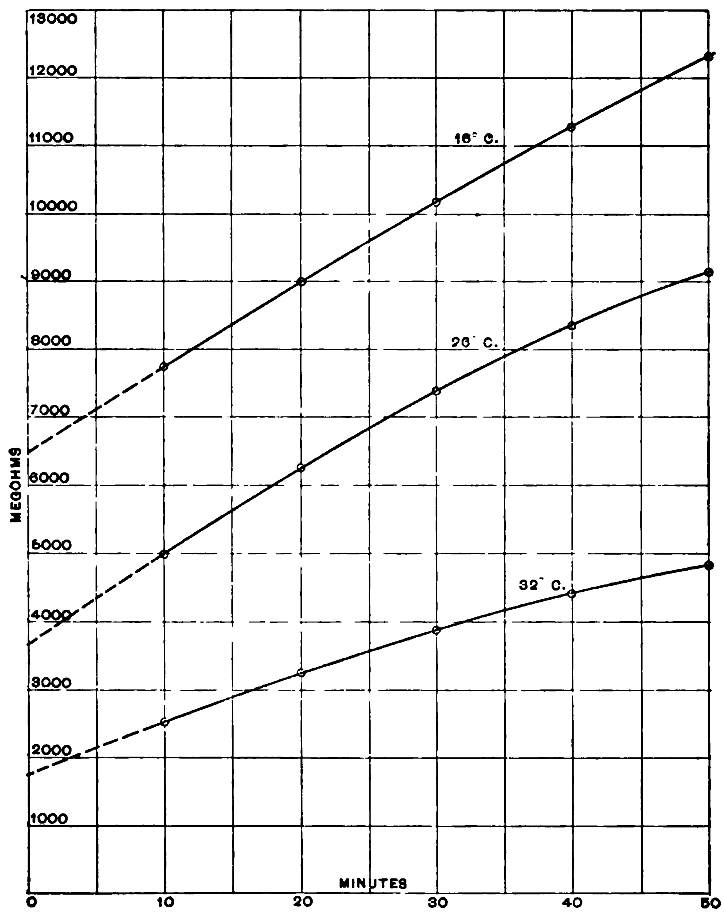


FIG. 20.—Condenser Resistance after Different Times of Electrification at Different Temperatures.

Fig. 19; the differences, are due, however, to change in both capacity and resistance.

The variation in resistance during the discharge, computed according to the first method before described, is shown in Fig. 20, and is to be compared with Fig. 15 which shows the corresponding results for vacuum oil. Fig. 20 shows that the resistance remains more nearly constant at high temperatures. Inasmuch as this variation of resistance during discharge is due to absorption, it is evident that there is less and less absorption as

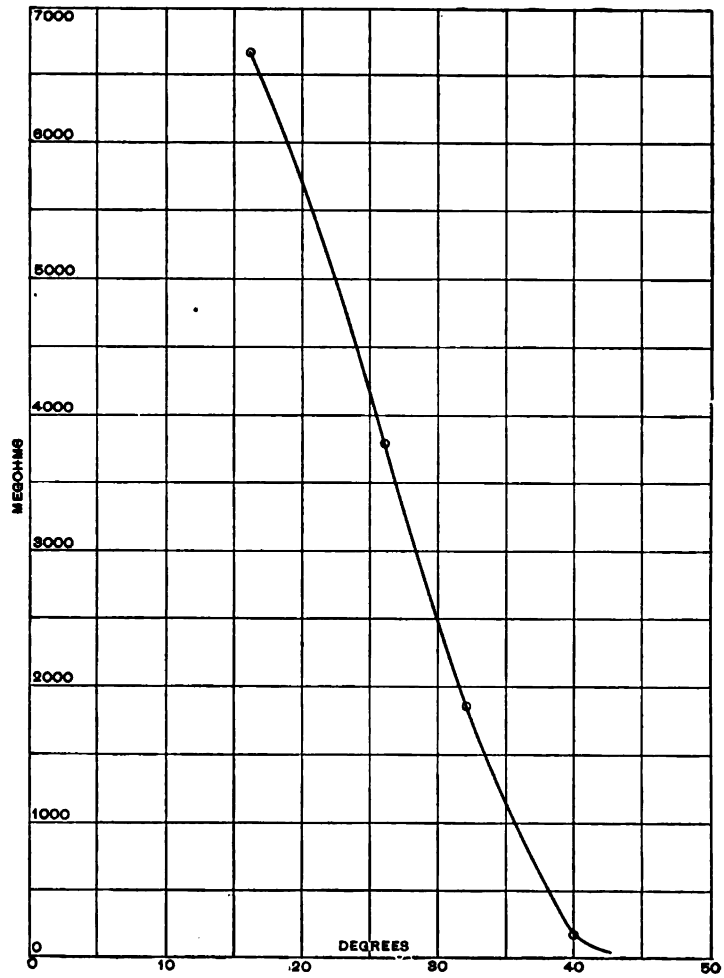


FIG. 21.—Condenser Resistance for Different Temperatures after three Minutes Electrification.

the temperature rises, a conclusion already reached from a study of residual discharges.

The condenser resistance at different temperatures, after three minutes of electrification,[1] is given in Fig. 21, and is in form quite similar to the corresponding curve for vacuum oil (Fig. 16). The sudden bend in a curve after 40° corresponds to the rapid changes in capacity at these temperatures. Between 42° and 50°

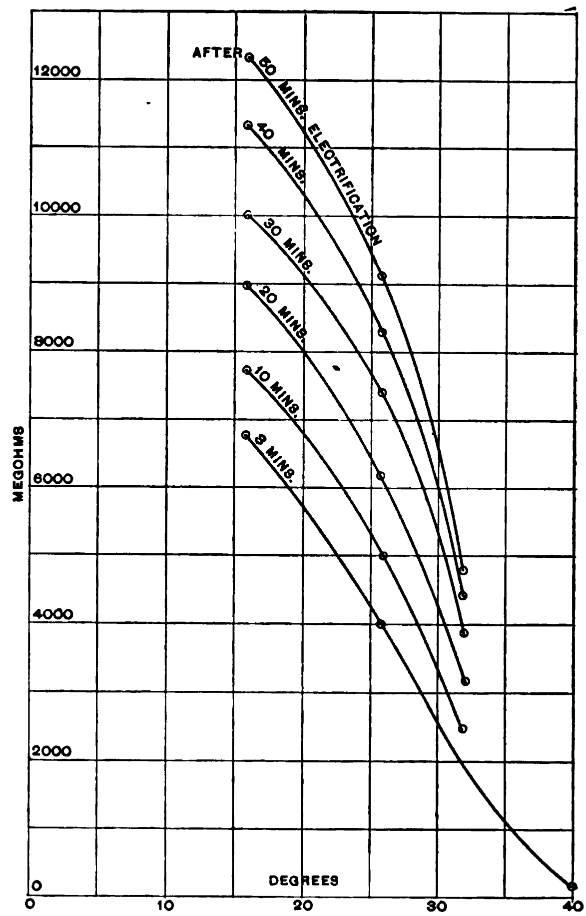


FIG. 22.—Condenser Resistance for Different Temperatures after Different Times of Electrification.

the capacity increases very rapidly. When the capacity becomes infinite, the resistance will, of course, be zero.

Mr. M. A. Hess read a paper before La Société Francaise de Physique, a review of which appeared in *L'Electricien*, January,

1. See preceding note.

1894, in which he says in speaking of the phenomena of absorption: "If the product of the capacity and resistance of different "portions of the dielectric sheet are equal, there will be no "residual discharges. . . . . . . It has been observed that the "insulation diminishes when the temperature rises. . . . . From "this it is seen that there is a temperature at which the pheno- "mena of the residual charge disappears."

In Fig. 22 curves are drawn after different times of electrification similar to the curve in Fig. 21. These emphasize the results already discussed. These curves come nearer and nearer together at high temperatures as absorption is eliminated, and apparently would coincide at some temperature between 40° C. and 50° C., the limiting temperature the condenser could stand.

The experiments given above would indicate that in the case of the paraffin condenser, absorption will never be eliminated until the capacity becomes infinite. The temperature was raised until the resistance became only $\frac{1}{20}$th of its normal value. The absorption, though diminishing, was very definite in amount, and the condenser would evidently break down before the residual discharges entirely cease.

Conclusion.

The action of a condenser is dependent, in a marked degree, upon its previous history; so much so, in fact, that its previous charges may be of more importance in determining its action under certain circumstances than charges received later. In that case the previous absorption, after neutralizing the absorption due to the final charge, may give rise to residual discharges which increase with time up to their previous initial value.

Absorption gives rise to the phenomena of residual charges and causes the condenser to depart from the exponential law in discharging through its own dielectric. The increase in apparent resistance in the condenser during discharge is associated with this effect of absorption. Previous charges modify these results, increasing or decreasing them according to whether the previous charges were in the same or opposite sense.

The resistance of pure oils is constant at any one temperature, but falls off rapidly with the temperature. There are, accordingly, no absorption or residual effects in pure oils.

In solid dielectrics the effects of absorption are diminished as the temperature is increased, as shown by the residual discharges and by the changes in the insulation resistance.

This investigation can in no wise be considered as comprehensive. Many of the results here described are already known, but they have not before been presented together, so as to show the relation between them. The fact that the previous condition of the condenser has such an influence upon its action, causes considerable embarrassment in an investigation of this sort, inasmuch as a condenser used once under certain conditions may be practically useless in the same investigation for further experiments. One of the condensers experimented upon, retained its past charges after being short-circuited for a month, and it was necessary to employ another condenser which was neutral, for further experiments.

Cornell University, June, 1894.

THE CHAIRMAN:—As there appears to be no discussion, we will pass to the next paper, which is entitled "The Electric Brake in Practice," by Elmer A. Sperry, of Cleveland, Ohio, whom I now take pleasure in introducing to you.

MR. SPERRY:—In connection with this paper, I wish to thank the Excelsior Electric Company, who have kindly loaned us the series-wound motor before you, and also the Weston Electrical Instrument Company, who have furnished us with these two beautiful illuminated ammeters by means of which we are all able to so easily observe the current furnished the motor in accelerating, and also the current produced in the brake circuit by the transformed motor after it is converted into a dynamo.

A paper presented at the 89th Meeting of the American Institute of Electrical Engineers, New York, Vice-President Crocker in the Chair, Chicago, Professor Stine in the Chair, September 19th, 1894.

THE ELECTRIC BRAKE IN PRACTICE.

BY ELMER A. SPERRY.

Enterprise in the United States for the public convenience of her citizens is truly phenomenal, and reaches its highest expression in conveniences for travel. The per capita investment in the cities and larger towns of this country for electric street railroad work is much larger than is often realized; in some no less than $55. is invested for each man, woman and child, for accommodation in intramural travel. The enormous aggregate sum thus invested has given a stimulus to engineers, resulting in wonderful advancement and refinement in means for mobilizing the thousands of tons of human freight that are hourly seeking transport. Vast skill and almost untold resources have been devoted to perfecting means for overcoming inertia, and quickly accelerating cars and trains which are ever increasing as to their weight and capacity, with patrons constantly more exacting as to speed and smoothness of operation, and especially as to safety. It is proverbial that while these advancements have been going forward, comparatively no attention has been paid to the mechanism for retarding, and quickly but smoothly bringing the cars and trains to a stop.

The popular appreciation of this point is illustrated by the growing frequency of its discussion by the daily press in our larger cities. The following is an extract from an editorial recently appearing.

"The number of deaths, due to motor cars, has alarmingly in-"creased since the trolley system liberated the slow-going horse "and mule. Not that the deaths are due to the electric current. "The 'deadly trolley' notion has properly been exploded. It is "the car wheels that are doing the deadly work. Now that the "motors and trucks have been in a measure standardized, mana- "gers of large roads, especially, are being asked by their patrons "why they do not adopt proper precautions so that the death-rate "may be cut down. There is no one part of electric or cable "railroading so important as the ability to stop the cars quickly. "At such times hand-brakes show their inherent inadequacy."

As the practice of the steam railroad engineer as to road beds, speeds and weights are gradually approached, his experience in the solution of this most important problem of braking should be studied with care. In so doing, however, the features wherein the street car problem is differentiated from the steam road problem should be kept prominently in mind. We must remember that the locomotive engineer is provided with a complete, expensive and somewhat intricate apparatus with which to de-energize his train. The periods of its application are comparatively infrequent, and as to personal capabilities, he is schooled and trained for years before ever being allowed to touch either the throttle or the air valve. While working, his pressure gauges allow him to adjust the brake application to a nicety. Moreover, the time allowed for the total retardation and final stopping of his train is usually very long as compared with that allowed in street railway practice.

Of the two, the street railway problem is the more exacting, and in the hands of far less experienced operators; and yet we are told that the mechanism involved in its solution must bear only a small ratio to the cost of the total equipment. Owing to the recently developed relation between effective braking and the accident and damage account, the purchaser will be spurred on to careful investigation as to the brake applied to his cars, and will be willing to spend sufficient time and money to effectually control the retarding, as well as the accelerating of the car. To be sure, as little expense should be incurred here as possible, but enough to fully meet the requirements. Especially is this true, now that ample power for applying the heaviest brakes is at hand, and even if used in a wasteful and extravagant manner in the largest amount possible, is entirely without cost, and its means of control already at hand. The importance of this proposition should at once command the attention of all, and commend the problem as such to the engineer.

Examination of accounts of the electric street railway companies of our large cities reveals the fact that the item of damage, already very great, is one of growing importance. Investigation of the circumstances and detailed statements of numerous items taken at random from the damage account, point at once and in no uncertain way to the inefficiency of the present hand brake. In many instances, could the car or train have been stopped within a comparatively short distance, the accident and resultant damages would have been entirely averted. The first investigation led to others with the same result, and in consequence the writer is prepared to show that nearly 85 per cent. of the accidents directly occuring, are due to the inefficient operation of brakes. The growing frequency of accidents and the constantly increasing demands of the public for damages, are indications that have not been made to impress the mind of the engineer, or I am sure adequate means would have been forthcoming for the correction of so grave a fault inherent in all the present systems of power-operated street cars. Some of our municipal authorities are taking action with reference to the increasing frequency and severity of accidents, and although no thoroughly scientific investigation of the matter has been published, yet it is a startling fact that with the present hand brake, no electric or other equipment to-day stands provided with anything in the line of an emergency brake. In so grave and urgent a case, what can be done in the line of remedy? The question naturally arises, will any system of braking worked upon or in connection with the ordinary wheel of a vehicle be sufficient for the stop required? What is the maximum efficiency obtainable by the brake working through the wheels? Is it sufficient to arrest the car before accident in case of emergency? Can it be made in any event a sufficient accident preventer? The popular notion that most accidents are due to brake failure is true, but in a way that is little understood, the failure being one of degree. It may not be known that under proper and standard conditions any car or train may be brought from a speed of 10 miles an hour to absolute rest inside of 10 feet. It is not generally appreciated that the wheel brake has ample capacity to accomplish this. The former investigations of the writer with reference to adhesion under conditions of acceleration and retardation, climbing and descending hills, afford ample proof that the rail adhesion through the wheels gives the wheel brake more than capacity sufficient to accomplish this result. For instance, assuming any weight and load, say 17,000 lbs., the stored up energy, 64,426 ft. lbs. can with ease be dissipated within 20 ft. for the ordinary equipment, and less than half of this distance, or a little over 9 ft. under conditions of coupled drivers, or if the wheels are compelled to revolve in unison. This latter consideration will be seen to have quite an unexpected as well as pertinent relation to the problem. It will be seen by a glance at the figures that the center of gravity of the mass, is far above the wheel contact with the rail. The retarding effect takes place on the base line in a plane below that of the center of gravity, really the farthest projection downward of the mass as a whole. A sudden stop operating on this base line tends to pitch the upper portion forward, bringing nearly all of the weight, and with it the retarding capacity, upon the front pair of wheels. The brakes on the rear pair in the ordinary equipment will have but little effect; if, however, by any practicable method they are coupled to the forward drivers, the brakes on this rear pair still remain active and of full effect. This is true even if the back pair should be lifted clear of the track. With increased weight upon the forward drivers comes ample increased adhesion, thus preserving the full tractive effort of the total weight intact for purposes of stop, which is impossible in the ordinary equipment. These effects are all aggravated in case of short wheel base. The present tendency toward a longer wheel base is a step decidedly in the right direction and should be encouraged. The effect of shifting the load in reference to the axles will be especially noticed in descending hills, as shown in Fig. 1, where the momentum of the rapidly retarded mass tends to shift the load centers still further forward, in some cases almost wholly onto the front drivers. It will be seen to have a greater effect than in *ascending* grades, where in stopping, the inertia tends to correct the position of the shifted load, whereas going down, in stopping, the momentum as stated tends to still further aggravate the condition. The practical effect of this may be seen whenever an ordinary street car mounted upon springs of fair resilience is quickly stopped. The car will be seen to suddenly right itself, having been pitched forward in the process of stopping, (see Fig. 2) the front springs being depressed and the rear springs extended. These considerations all go to show in a new light the advantages to be derived from coupled drivers for general street railway service.

Bearing these facts in mind let us turn for an instant to the ordinary hand brake. The ratio in the brake levers will be found in the modern trucks to be anywhere from 6 to 11½, averaging about 8½ to 1. The lever arm of the brake staff will be found to be anywhere from 6 in. to 13 in. Assuming 11 in. as the average, the radius from the center of the brake chain to the center of the brake staff will be 1¾ in., giving thus 6.28 to 1, or a total leverage of 53.4 to 1 from the operating handle to the brake beam. Two elements now have to be assumed. First, the friction coefficient of the brake shoes acting upon chilled wheels. Second, the power upon the brake staff. The writer has endeavored to cover both of these unknown quantities by actual experiment, giving the results in the tables. Table I. was taken by a dynamometer being fastened directly to the brake staff handle in line of the pull of the motorman, a cast-iron brake wheel 16 in. diameter from center to center of a 1½ in. rim bearing the handle. A number of experienced motormen were invited to test their strength upon this handle and careful readings were taken. It was noticed that the right arm of the more experienced motormen was much more developed than the left, a fact which I think has been pointed out before. The extent of this development in the forearm is certainly quite marked, showing that the gripping muscles and weight of the body are the elements brought most into play.

TABLE I.

No. of motorman.	Weight of motorman	Gradual pull with one hand.	Jerk with both hands on hand wheel.	Emergency jerk with both hands on hand wheel.
6	140	112	135	275
58	200	135	275	385
264	287	145	235	312
62	175	125	212	285
123	153	125	245	310
60	185	150	200	300
26	170	150	275	350
96	155	135	210	325
246	135	110	175	325
287	135	125	250	350
66	160	125	250	405
4*	176	100	200	400
266	185	175	250	375
		Av. 131.7	224	338.23

*9 years in service.
Right arm, circumference of forearm, 12¾ in.
" " biceps, 14¼ "
Left arm, " " forearm, 11⅝ "
" " biceps, 13⅝ "

Columns 1 and 2 indicate respectively the number of the motorman and his weight.

Column 3 indicates the greatest possible steady pull with the right hand on the handle, bringing into play all possible weight of the body.

Column 4 indicates the same conditions as column 3, the motorman in this case grasping the hand-wheel with both hands, some motormen using gloves to prevent their hands slipping. None of the men could maintain these values for more than one half second.

In column 5 is indicated an "emergency stop." The motormen were told to "break the machine" if possible. In this test the body was braced, sometimes with the knee against the dasher rail, the needle registering the highest jerk usually given with a sudden lunge of the body.

It will be seen that the power applied by the steady pull of the average motorman is about 131.7, and can be made to run up in case both hands and the weight of the body are used, to about 224 average, but this value cannot be maintained. The average values of column 5 cannot be used in these calculations, for the reason that although they show the pressure it is possible to reach by jerking upon the hand-wheel, these pressures cannot be maintained and therefore cannot be depended upon for braking effect. The tests show that the full power that can be maintained upon the brake lever for a sufficient length of time for the purposes in hand, does not exceed an average of 180 lbs.

TABLE II.

Speed rev. per minute 33 in. wheel.	Brake pressure.	Traction.	Coefficient.
Varying, 150	900 lbs.	87.4 lb.	9.7 per cent.
125	900	91.7	10.2
100	900	99.8	11.1
78	900	118.	13.2
56	900	133.	14.8
38	900	150.4	16.6
20	900	154.	17.1
4	900	174.6	19.4
Constant, 105	300	29.4	9.8
	500	50.5	10.1
	750	91.	12.
100	1150	125.	11.2
	1500	178.	12.
	2200	305.	14.4
94	3780	488.	13.2

Table II. was obtained as shown in Fig. 3. A brake shoe that had been run in service about three or four days was taken with the axle carrying its cooperating wheel, lifted out of the truck and placed between the centers of a lathe, the load upon the brake shoe accurately measured, and the shoe held from movement around the wheel by a dynamometer. Every precaution was taken to avoid handling the periphery of the wheel or the face of the brake shoe, and even the dust was left upon it so as to conform as nearly as possible to the normal conditions of practice.

Tracing our 180 lbs. application to the brake beam with allowance for loss by friction, we have 3,840 lbs. applied to each of the two shoes which upon the chilled surfaces are found under ordinary circumstances by Table II. to give a coefficient of about 12 per cent. This would give a retarding effect of 460 lbs. which is less than one-third that easily obtainable were the power needful for its application at hand. The coefficient under these conditions must have been about 37 per cent. to realize anything like the total value of the retarding effect of the wheel. This under condition of chilled and glazed surface is entirely out of the question, showing at once the necessity of power in the application of brakes, if anything like their full value and use is to be obtained. This is also amply borne out in practice, as those who have tested this point well know that under ordinary conditions it is next to impossible to slip the wheels of a motor car by the hand brake.

In the electric brake, on the other hand, the fact that the truck parts are not bound up and locked into a solid mass by the enormous pressures of the heavy brake levers and shoes is found to prevent racking and straining the truck as well as jumping the track and curves when the brakes are set. The axles and truck parts are perfectly free for easy and normal movement even when the brake is exerting its full power. Great reduction of wear at the pedestal journals is also found, owing to entire absence of all of the usual heavy pressures of the brake shoes. We can all see that by applying adequate power and control to the wheel brake, this element of the equipment may be raised to the position of an indispensable safeguard, the value of which can only be appreciated as its hitherto undeveloped resources are brought out, demonstrated, and rendered simple and easy of application and control. In practice the greatest necessity for maximum brake application exists at the higher speeds. From Table II., it will be noticed that at just this point the failure is greatest, the coefficients being least; increasing as the speeds decrease. This has always constituted the one grave fault of the air brake in railway service. The intensity of its application should be greatest when the speed is greatest, and decrease as the speed drops off. As will be shown farther on, this point has been fully covered in the electric brake, which is the first time that the varying application has ever been embodied in practice, and especially in such a manner as to perform its important function automatically.

A plan, especially one pertaining to electrical matters, after having been proven mathematically to be feasible, is far from being realized. Many are the practical difficulties to be surmounted before a thoroughly commercial, or anything like a standard apparatus has been produced. Especially is this true in the electrical field. Many subtle influences and energies are at work which well nigh overwhelm the experimenter. In a new field but few precedents are at hand, and these are apt to be extremely unreliable and in the nature of a blind leader of the blind. The practical application of the electric brake, although probably no exception to the general rule, amply illustrates the wide distance to be spanned between the conception of the idea and the commercial apparatus itself. For years the writer has believed that electricity was vastly preferable to any other force for the application and control of brakes. Working first on the solution of the continuous brake problem for railway trains, he built his first electric brake apparatus in '82 and has studied and experimented on the problem in its various phases almost continuously since that date, with more or less encouragement in the line of substantial progress. As to its application to electric cars, the apparatus was successfully applied on some double truck cars in Illinois, one of these cars weighing as much as 12 tons. The first equipment, similar to that shown in Fig. 4, was constructed some five years since. This apparatus has been constantly undergoing alterations and been experimented with, until for the past 18 months a constantly increasing number of electric cars, equipped with it, have been in regular service, some of these running with change of motormen on each of 13 daily trips, the same motorman having the car once in about three days, making it impossible for the men to become familiar with the operation of the brake. During this time one car has made upwards of 70,000 miles, hauling a trailer about 48,000 miles, during some special weeks of test, making from 178 to 220 miles daily. It is only under such rigorous conditions of actual operation that rapid progress can be made in reduction to practice. All machinery or apparatus must pass this ordeal successfully before it can be brought into thoroughly commercial shape.

At the mention of electric brakes the engineer at once admits that they should be entirely feasible, and usually adds that there is plenty of electrical energy at hand from the central station to retard and control as well as to propel the car. This, however, is not the method undertaken by the writer. To employ the central station current for operating the brakes would be to limit very materially their usefulness and certainty of operation. The braking current, although used at comparatively infrequent intervals, and then only for a short period, should for this reason be absolutely certain and unfailing in its action, and not subject to any "heart failure" of the central station, or sudden cessation caused by the opening of the circuit breakers, the interruption of the line, the flying off of the trolley, failure of the fuse, or failure at other more or less vulnerable points. The electric brake under discussion has been operated over a year on equipment upon different roads, from electricity generated independently of the trolley connection, the braking current not being derived from the central station but produced by the power of the moving car, which power it is desired to get rid of, or destroy. The brake thus operates equally well with the trolley off, and, as will be understood from the following description, the trolley current has nothing at all to do with the car while the brake is being operated, except possibly to maintain the light circuit. The electric brake at the same time is entirely independent of the hand brakes, which may or may not be present upon the equipment. The braking action being altogether independent of the ordinary brake shoes, it is not found necessary to employ them in connection with the electric brake, although in the earlier forms they were used, and in the case of trail cars, especially in heavy service and on grades, some engineers prefer to use them at the present time, in connection with apparatus such as shown in Fig. 4. The current employed by the writer for operating the brakes is developed by automatically turning the motor or motors into generators. As these are driven forward by the moving car they develop current which is controlled as to intensity by the starting rheostat of the car. The braking current is thus produced at the expense of the mechanical energy stored up in the moving car, which, being consumed causes a retardation and final stopping of the mass as a whole. The current so generated may be furthermore led through a brake magnet as above seen, to apply the brake shoes; it may arrest the motion of the car direct by magnetic adhesion, or develop heavy retarding currents in the moving metallic mass by magneto-induction. When an active local circuit is used, the latter method is usually employed for reasons which will be made more apparent.

In developing this system, the point which seemed fraught with the most difficulty, and which has finally received the simplest solution of any in connection with the problem, was that of obtaining always and with absolute certainty, sufficient current at the lowest speeds without the aid of the trolley current. Teaser coils were at intervals resorted to, maintaining connection with the trolley circuit. "Artificial teasers" were also used, being a device by means of which the trolley circuit was entirely done away with, and which worked well. Observations made from time to time in connection with these experiments led to an exhaustive investigation of residual magnetism, in consequence of which, structural means were adopted to utilize to the full, the residual magnetism of the motor. This supply is constantly being renewed with every energizing of the car. This method was found to be the simplest as well as the most effective. The connections, and in fact the whole arrangement of the electric brake upon the car, is extremely simple. This is shown by the fact that only one small extra wire needs to be run to the controller in addition to the ordinary wiring of the standard equipment without the electric brake. The certainty of operation is evinced by the fact that at present writing, over 150 of the equipments have been placed, which are making upwards of 10,000 miles daily in regular service. Early in the experimentation a phenomenon was observed in reference to the persistence of the current even after the motor had stopped. This is due to the slow action of the decreasing magnetization, taken together with the reaction or self-induction effect of the fields, and any brake coil or coils that may be in the circuit. The movement of the magnetic lines, which persist after, and in fact long after the motion of the motor has ceased, generates potential. In many instances it is possible to draw an arc from the rupture of the brake circuit one second after the motion has ceased, showing the presence of current in the local circuit.

Fig. 5 has been developed from the average stop to show the curve of current in reference to the motion, the black line indicating the period of motion during the application of the brake, and the curve indicating the current intensity and its duration. The current flowing after motion ceases, though small, is found exceedingly useful in holding the car from starting itself, even on quite a heavy grade, as only a small quantity of energy added to the already great friction of quiescence will prevent the car from starting. This persistency of current is also found useful to kill or destroy the magnetism of the brake mag-

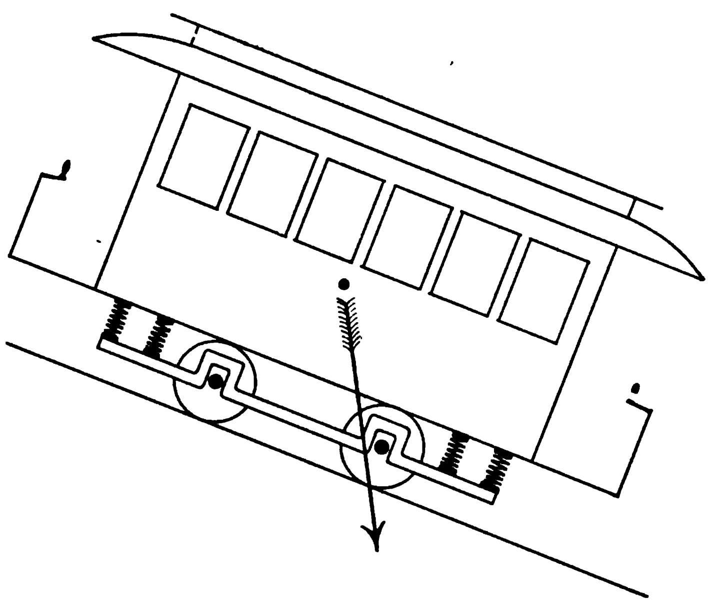


FIG. 1.

net, in case it is desired to suddenly move the car forward again. The tendency on the part of the windings at the moment of rupture to generate an opposing E. M. F., tends to suddenly free the magnet from its face, a purely accidental feature, which is of great value and utility in this connection. The wonderful energy of the withdrawal of the lines of force, being in its manifestations a phenomenon of magnetic viscosity, is illustrated by the following fact: With a perfectly dry track, a great force is required to shear the adhesion and start the wheels slipping. A car going down a grade under these conditions where no brake magnets are present, will, with a sudden application of the electric brake, generate sufficient current to not only arrest the motion of the wheels, but start them going in the opposite direction, the reverse motion being maintained through an interval truly remarkable, in some instances running as high as one and one-half seconds. It will be borne in mind that all the above phenomena are entirely independent of the central station current, the trolley connection having been severed before the brake is applied.

The current required to be developed to stop a car when no other braking apparatus is used, is found to be only a fraction of that required to accelerate the car in the same interval. This may be easily illustrated by the lines in diagram Fig. 6, A being the

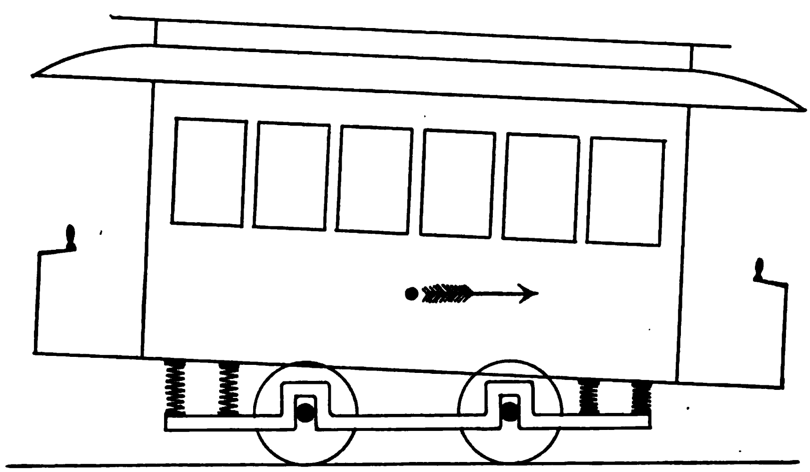


FIG. 2.

electrical energy applied in a given acceleration; B the resulting mechanical energy stored in the car after deducting all the wastes in the motor, and between the motor and the momentum; C the average mechanical energy in the car at the time of applying the brake; D being the electrical energy required to be developed for retardation after the efficiency losses have all been provided for out of the quantity C. Thus it will be seen that the so-called efficiency losses act in a two-fold sense, between A and B and between C and D, to reduce the amount of current required to be generated for braking purposes.

As to the effect of the electric brake on the the total temperature of the motor the following experiments were made. A car and trailer were operated over the line in regular service 41.1 miles without the brake. The atmospheric temperature was noted every half-hour during the test, and the temperature carefully taken of all parts of the motor at the end of the run. The succeeding day a similar run was made with the same trailer over the same track and in the same length of time, but with the electric brake in use, braking direct on the simple local circuit without brake magnets. The difference in the average atmospheric temperatures during the two days was six and one-half degrees, and the difference in the average temperature of the motor parts was seven degrees, making only a difference of one-half of one degree Centigrade as the total increase of temperature from the use of the brake. Observations in reference to the heat in the

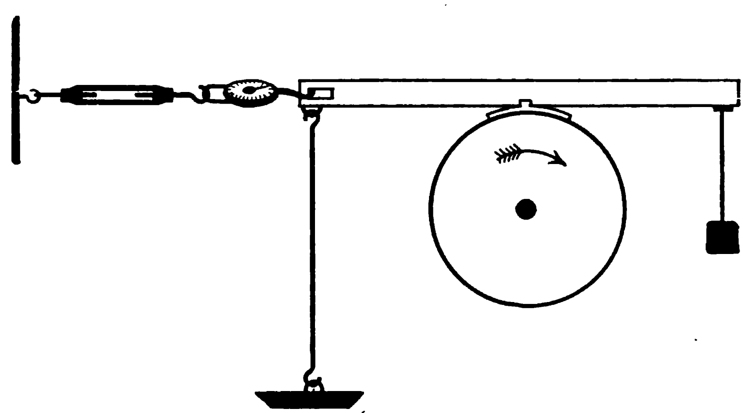


FIG. 3.

rheostat were made, although no temperatures were taken, and no difference could be observed in reference to the heating of this portion of the equipment. The explanation will be found in the comparatively small amount of current as seen above, and the relative infrequency of its application, and short duration at the time of each application. The following wattmeter readings have also been taken :

	1st Trip.	2nd Trip Trailer.
Reading of wattmeter, leaving Lake View,	392,538.9	392,542.25
Reading of wattmeter, end of round trip...	392,542.25	392,547.1
Number of full stops........	55	53
Number of slow-ups......................	42	37
Time....................................	1:40	1:35
Difference in reading.................. ..	3 35	4.85

Constant of wattmeter equals "22"; total watts both trips, 180.4.

Diagram No. 7 indicates the connection of the wattmeter in the circuit.

Automatic resistances were even at one time used in the endeavor to relieve the motorman of *all* responsibility in connection with the control in applying the brake, but superlative simplicity was found to be much more desirable than the superlatively automatic, and the automatic devices were seen to be entirely

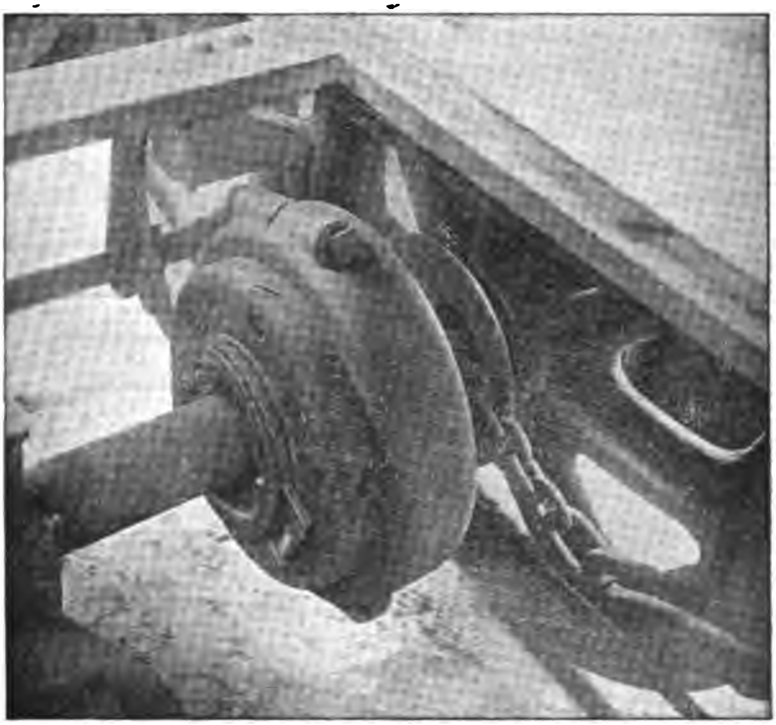


FIG. 4.

unnecessary, the apparatus as at present constructed being of great simplicity.

The diagram of the braking current in Fig. 5, shows the automatic decrease in the intensity of the brake application so desirable with the decreasing speed referred to above. As the speed decreases, the generator runs more slowly and consequently produces less and less current.

With this style of brake, the life of the wheels is increased from two to three-fold, thus affording a saving in the item next in cost to the electric maintenance itself, to say nothing of the entire saving in brake shoes. This is emphasized in the fact that the brake shoes are being constantly besmeared with sand and grit thrown from the wheels, and when in this condition, they are brought against the wheels with the tremendous pressures noted above. A better method could hardly be devised for reducing both wheel and shoe. We little realize the great number of brake applications necessary in a day's run. Careful record has been kept of this point, giving in three days an average of 1377 brake applications per day for a run of about 164 miles.

Another interesting feature in this connection is that a flat wheel from skidding is an impossibility. It will readily be seen that should the wheels stop, the generator connected with the

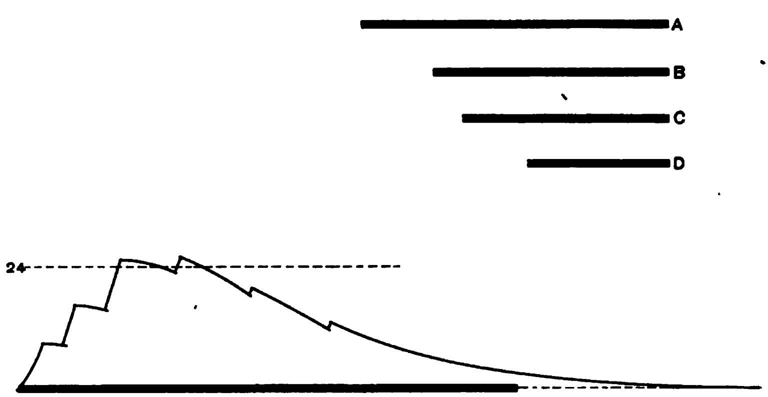


FIG. 5. FIG. 6.

axles ceases to produce current, and none therefore exists to farther apply the brake, and though they may be sliding forward on the rail, yet the wheels continue to rotate more or less, and constantly present new surfaces for the sliding contact.

The braking action is two-fold and is especially efficient. The rotating armature of the motor, instead of tugging ahead by its momentum, is itself pulling back, and more or less powerfully braking the car through the gears by the retarding effort of the magnetism of its field while generating the braking current. The power required therefore to perform this work is taken from energy of the moving car which it is desired to destroy; not only is the car thus retarded, but the electric brakes arrest the motion of the wheels direct, with a force that is remarkably powerful and under perfect control of the motorman.

Two forms of braking magnets are used, one for winding up a brake chain usually employed in connection with the trailer, shown in Fig. 4, and another for directly arresting the motion of the axles, one magnet only being used in connection with each axle, as shown in Figs. 8, 9 and 10. These magnets are truck-mounted, not an ounce of their weight being directly on the axle, and are so supported that their gravity acts to automatically retract them from the brake face, see Figs. 10 and 11, the latter showing the link standing out of the vertical. The brake face is automatically lubricated to a slight degree, receives a high polish and does not cut or rapidly wear. The brake is noiseless in its

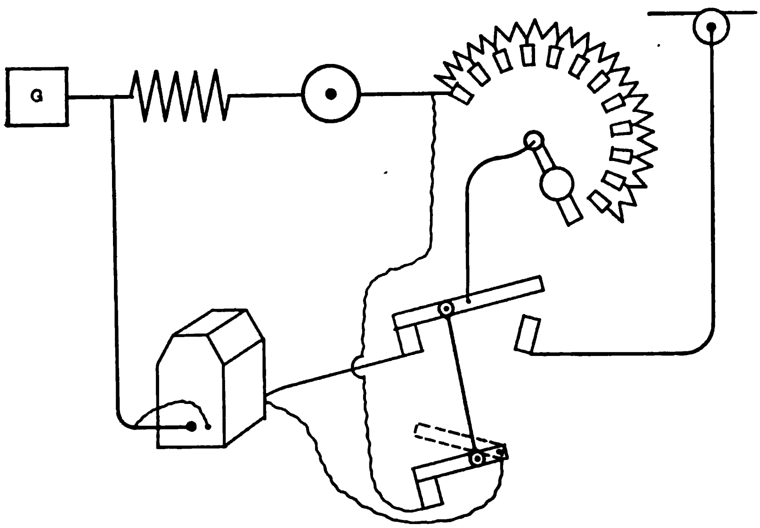


FIG. 7.

operation. It will be seen from the cuts, that inasmuch as it does not revolve, no commutating or contact device is necessary. Its crescent form accomplishes important technical functions and also eliminates the necessity of pulling off a wheel for its attachment, removal or inspection. The brake is shown in position on truck in Fig. 12. Its face is *solid unbroken metal* with no grooves or interstices for catching grit or sand, which in part explains the absence of wear above referred to. The brake magnet is practically indestructible, a few turns of stout wire constituting its one coil entirely enclosed and sealed in metal. No harm nor moisture can reach it. As to moisture, it is immaterial, as the E. M. F. at which it works is extremely small, seldom reaching six volts. The lubricator for the brake surface is dry, not sticky or adhesive, and does not gather sand or dirt and retain it upon the braking face. No mechanical pressures whatever are employed to arrest the car, and hence no strain or shoulder-wear comes upon the journals. In constructing the brake magnets their proportions and the arrangement of the magnetic circuits received considerable study. It was during

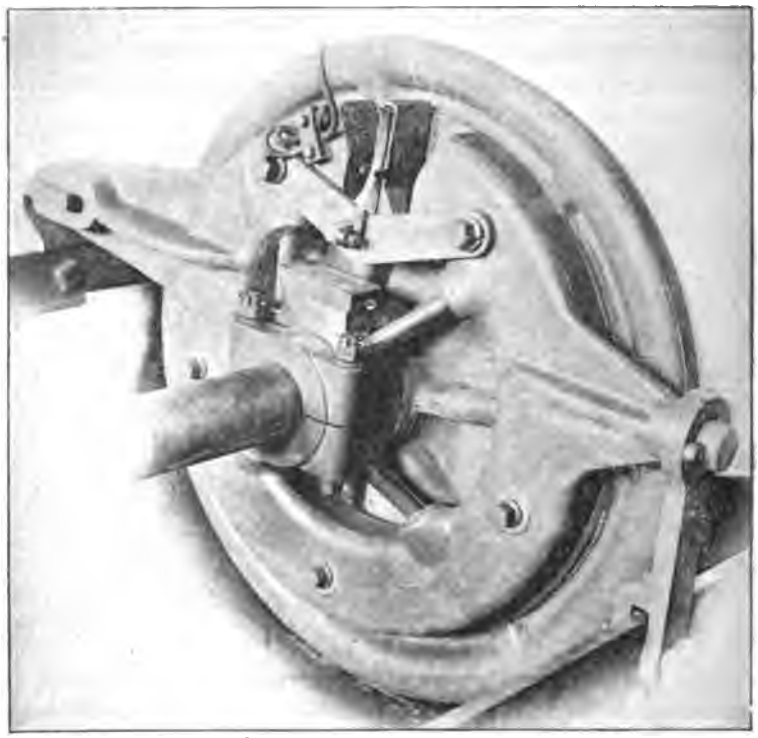


FIG. 8.

some preliminary experiments that an unexpected phenomenon was noticed; namely, that the retarding effect when speed is an element, is very much more than would have been expected from the co-efficient of friction due to magnetic attraction or adhesion, this latter being a known and definite quantity. Farther experiment, made to ascertain the cause, showed it to be due to Foucault or eddy currents set up in the masses. The conditions and structure of the brake magnet were therefore varied in a number of particulars, especially such as would be expected to give the greatest result in Foucault currents produced. The result was immediately successful. It was found that the retarding effect of the brake magnet is due very much more to the generation of these currents than to the direct effect of the coefficient of friction resulting from direct magnetic adhesion, the amount of which I find can be relied on accurately when employed by itself. Some of the forms of brake magnets experimented with were provided with numerous poles of opposite polarity, which were worked upon three different kinds of armature, two of

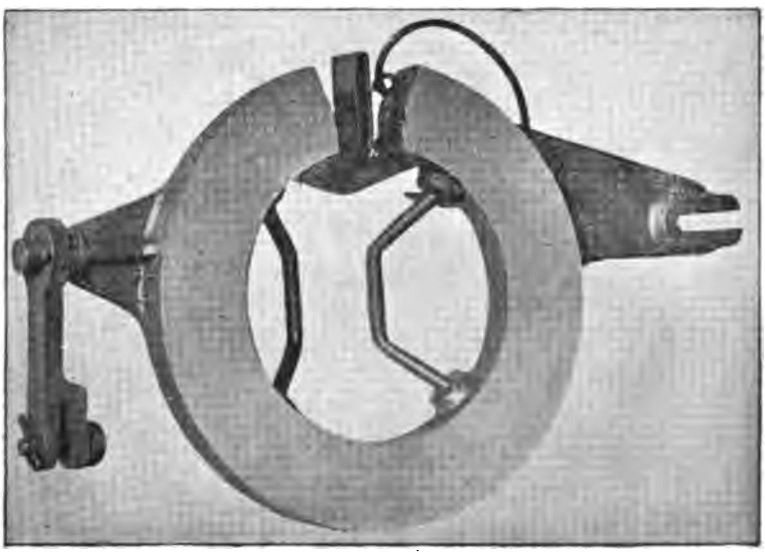


FIG. 9.

which had radial teeth of different number relative to those in the magnet. and one being a plain disk armature. The toothed armature, while it causes a series of sudden jerks and is also unsatisfactory in the total retardation resulting from a given input is found also upon rupture of the magnetic circuit to impart to the magnet coil certain counter E. M. F.'s which materially cut down the current supply and thus the capacity of the device. A magnet formed of a continuous disk with an annular groove sunk in its face, is found to give very satisfactory results, but is much heavier and requires an armature twice as heavy for a given number of lines as the double circuit magnet shown in the figures.

Furthermore, the relative rotation between such a magnet and its armature affords no point in the masses where the lines are interrupted or changed, and the Foucault currents, or reactionary effect set up, is very much inferior to those in a magnet where a gap or cessation of magnetic stress is continuously produced. As a result of these investigations the crescent form shown in the

FIG. 10.

figures has been adopted, the opening in the crescent giving the effect referred to, as well as affording an excellent method of attachment and removal of the brake magnet, and at the same time supplying a gap for easily reaching the face for inspection and lubrication. A lubricator, see Figs. 8 and 9, is shown as occupying this space. The belief that the extra retarding phenomenon is that of Foucault or other eddy currents is borne out by the fact that a conducting lubricant such as graphite is found to considerably increase the effect, also metal filling between the polar faces is almost indispensable for the best result, while at the same time effectually protecting the coil from all damage. These observations would seem to indicate that the eddy currents, however produced, circulate in both masses near the surface, and traverse back and forth across the air-gap whenever ample provision is made to allow them so to do. The practical value of the combined action of all these forces in increasing the

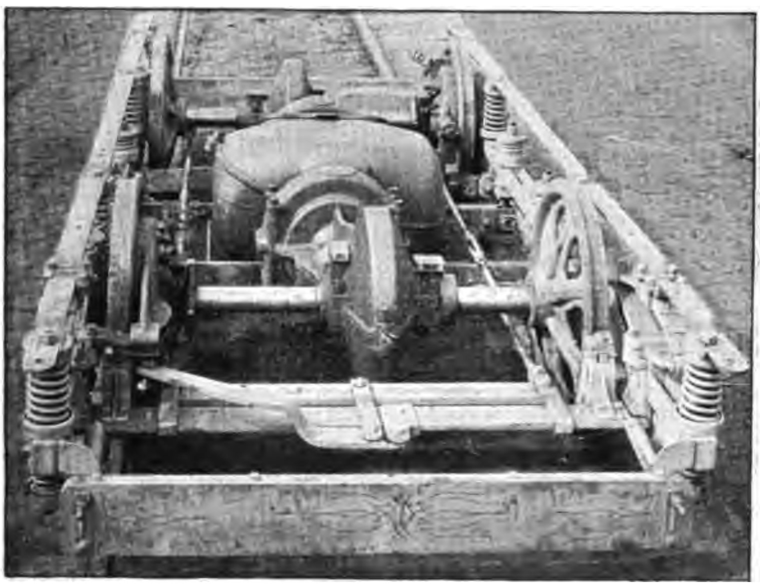


FIG. 11.

retarding effect, results in necessitating but a small magnet, and a smaller current expenditure considering the work performed.

By reference to Table III., the result in retardation gained through the eddy or other currents may be plainly seen, column A indicating the retarding effect which should be expected from a friction coefficient of 10 per cent. between the lubricated surfaces due to magnetic attraction of the lines actually circulating; and column B indicating the values of retardation actually obtained on the dynamometer.

That the important positions and trying circumstances under

TABLE III.

Amperes.	Volts.	"A" Pull due to magnetic adhesion or traction. Friction coefficient 10 per cent.	"B" Pull on brake chain obtained Graphite lubrication.
5	1	7.6 lbs.	125 lbs.
9	1.8	18.3	300
9.5	1.9	36.4	608
15	3.	121.	1976
16	3.2	149.	2432
20	4.	158.4	2584
23	4.6	167.	2736
25	5.	186.	3040
31	6.2	207.	3385
35	7.	213.	3490
35.5	7.3	214.	3500
41	8 5	223.	3650

The assumed values are ba ed on a traction of 28.26 lbs. per square inch for 45 000 lines per square inch, being the assumed values at the knee of the curve easily recognized as occurring between 16 and 20 amperes in the table.

which the motorman often labors are appreciated, may be seen by the space devoted to it in the daily press. An editorial in an evening paper reads thus:

"One trembles to think what consequences may follow if a "motorman gets rattled or has a fainting fit when trying to wind "up his hand brake in time. Muscles grow weary and relax at a "critical time. Even when a man is in fine working condition, the "strain upon him is severe when compelled to handle the grip "lever or current controller and hand brake simultaneously."

As to the arrangement for application and control of the brake by the motorman, about a year ago the writer made the following statement:

"Considering the inexperience of the operator and the responsibility which at times well nigh overwhelms him, I think that, as engineers, we should be willing to set a very high mark to be attained in the ideal brake for electric street railway service, namely: the use of but a *single controlling handle for everything;* starting, accelerating, retarding and braking the trailer, or trailers and all. Let the motorman have nothing to think of except one handle, and two-thirds of the accidents now occurring will be prevented. Let this handle require no more exertion in its operation than the present controlling handle. Let the motorman fulfil his function with as little physical exertion as possible; he will then have a greater reserve for mental application when necessary. A motorman required to exert an enormous amount of brute force, constantly grinding at the brake, has but little life left to apply in case of emergency. I agree with a prominent writer on this subject, where he says that a multiplicity of handles is fatal in time of emergency."

At the time the above was written, equipments controlled as therein set forth, namely, by the use of a *single controlling handle for everything*, had been in operation for upwards of a year. The methods employed for accomplishing this have been varied, but the form most in use at the present time is that shown in Fig. 13, where the resistance contacts are employed in a two-fold manner; the controller handle is made to operate back and forth over the same contacts for controlling both the application of the current to the motor and braking the car. A self-correct-

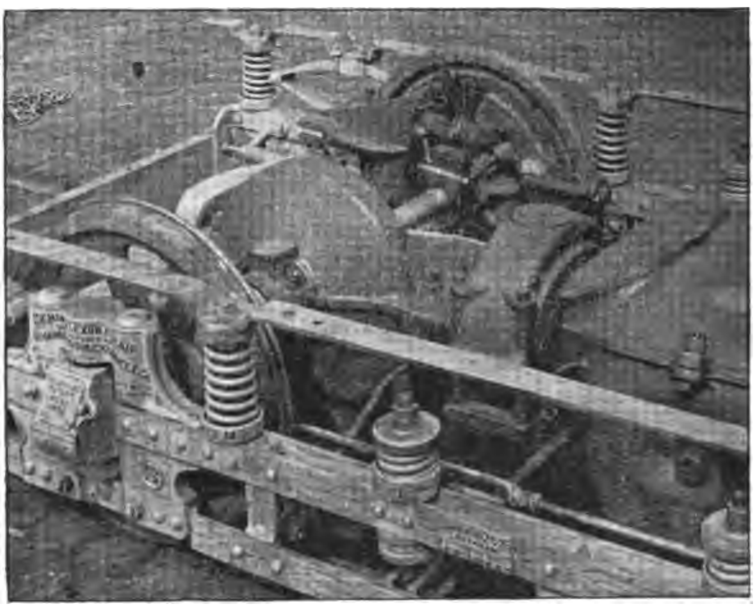


FIG. 12.

ing and interlocking device is also provided, shown at A, A′, Fig. 14, so if the motorman does not throw the handle clear over, the transformation is completed automatically before the movement of the lever can reach the operating contacts.

At one point where a number of equipments were started last summer a newspaper correspondent described the operation of the brake as follows:

"The connecting beam was taken from the trolley wire several "times and the car brought to a sudden stop with the electric "brake alone, within three-quarters of a car length. And it was "not a sharp, jerky stop, but something as if the car had run "into a big feather tick."

The smoothness of even a sudden stop by the electric brake is quite aptly described by this droll statement. It seems as though the car was running into an air cushion.

It will thus be seen that the brake is automatic and does its work without any special act or even the knowledge of the

FIG. 18.

motorman. He simply works a single handle back and forth and electricity "does the rest." Suppose the motorman wishes to stop his car, he turns off the current by simply swinging the lever over to the right. This operation is made to automatically convert the motor into a special dynamo for generating currents at very low speeds, and also simultaneously to cut off all connection with the trolley current. The brakes are then applied by sim-

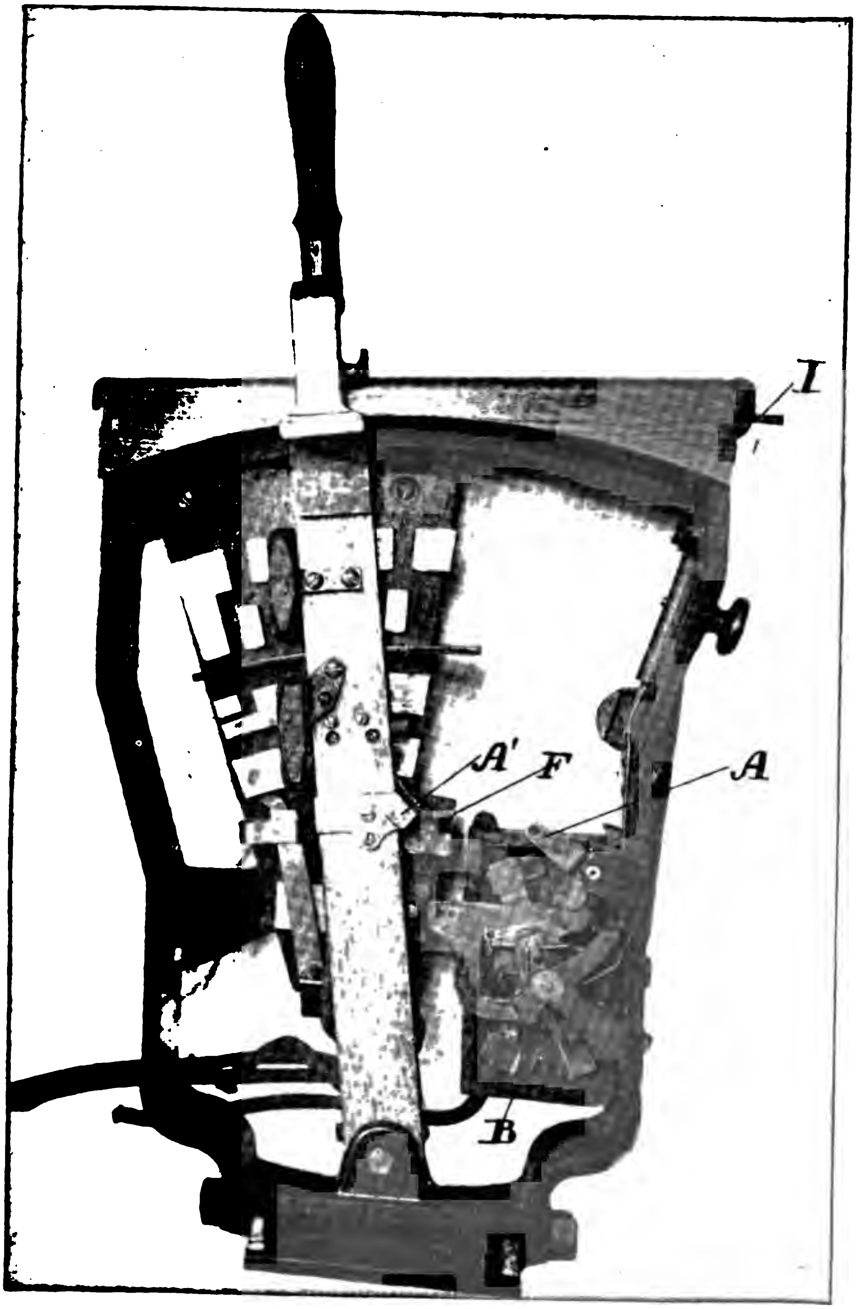


FIG. 14.

ply swinging the handle back over the path it has just traversed; the farther it is swung to the left, the stronger the brakes are applied. The act of releasing or letting off the brakes again automatically re-establishes connection with the trolley, and reconverts the dynamo into a motor. Fig. 15 shows the switches used in part for the conversion of the motor, and also the finger F mounted upon the lever forming a part of the alternate stroke operating device. The trolley brake-switch B is shown open in Fig. 14 and closed, with dimensions, in Fig. 16. The same rheostat and contacts are applied to control the motor while running the car, and also to control the slight amount of current generated by the transformed dynamo which is sufficient to brake the train.

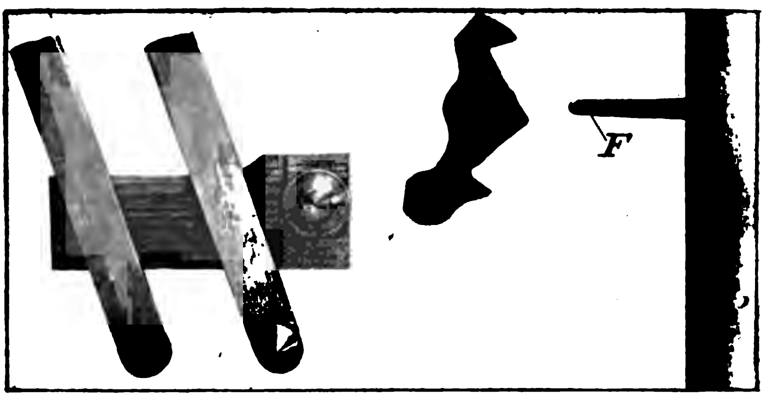


FIG. 15.

The motorman cannot turn on the current before the brakes have been released, nor can he apply the brakes before the current has been turned off. This is a result of construction, and constitutes a feature of merit in the new electric brake, effecting an economy in current and a saving in wear and tear. Freeing the conductor of all care in this connection, and leaving the braking of the train—including trailer or trailers and all—solely in the hands of the motorman by placing at his command a power with which he may with the utmost ease accomplish his task, constitutes an important advance in the art of control of electric railway equipment.

It has been found that the electric brake is practically incapable of abuse by any motorman, an advantage which never before has been attained in any power brake, due to the fact above named, that no amount of over-application can cause flattening of the wheels, or any harm whatever to any part of the equipment through their locking and skidding.

The application of the brake, its regulation and also the running of the car, all by a single operating handle, is a result that has only been accomplished by the closest application. It has involved much pioneer work, and been rendered possible through the use of new mechanical movements original with the writer. The principal moving parts are simple and durable, being only two in number. The arrangement may be easily seen in the

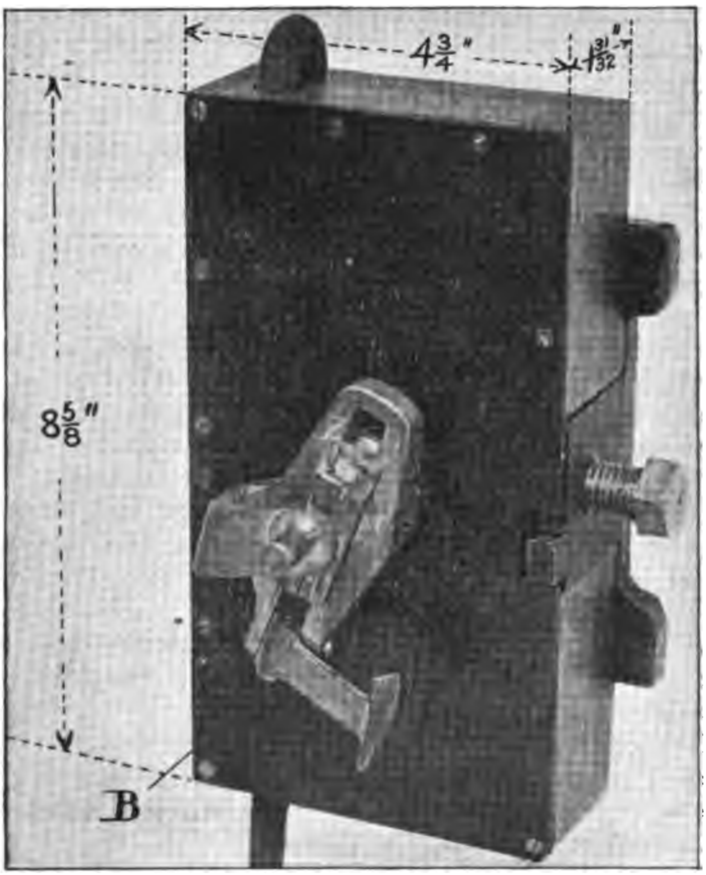


FIG. 16.

controller and parts exhibited, as well as in Figs. 14, 15 and 16. The various portions of the controller have been subjected to the severest tests possible, one test made in '93 consisting in 518,700 consecutive brake applications without appreciable wear, the parts being in regular service at the present time. An indicator at the right side of the controller at the top (see I Figs. 13 and 14) shows when the controller is in condition for brake, or is being so used.

Operating the brake in this manner, it will at once be seen that the system is one of the utmost certainty of operation, surer even than the hand brake, air or other power brake, from the fact that every time the car runs, the motor, which is depended on for the braking action, receives a test, and its fitness and capacity for the next brake application is constantly being demonstrated. On the other hand, the motorman never knows whether his hand brake is sure to operate when called upon for the next application. An accident came under the observation of the writer on Case avenue hill, in Cleveland. Here the last hand brake application was successful in every way, but before the brake was called into action again, a nut dropped off from a brake rod upon the truck, rendering the brake useless. The motorman continued to wind on his brake staff, and before he realized what the trouble was, his car was going at a tremendous speed into a short curve at the foot of the hill. There were a number of casualties and also six demands for damages as a result. With the air or other power brake, this liability to failure is increased in direct proportion to the complexity and number of parts.

The advantages found to result from the practical use of the electric brake as compared with former brake systems; its qualities as an accident preventer, as well as its general commercial value, may be recapitulated as follows:

1. The certainty of its operation.

2. The enormous power at instant command and under perfect control.

3. The absence of all power absorption at moneyed cost from the central station.

4. Its high efficiency, being far superior to compressed air; amply proven in numberless instances where electricity has replaced air. (The air requires a direct application of energy, amounting to an immense aggregate power-absorption during the

day from the central station; the working parts of the air machinery are attached to the car axles and require a large quantity of energy, not only while compressing, but at other times as well.)

5. Its extreme simplicity.

6. Observed saving in wheels, two to three-fold.

7. Entire saving in brake shoes.

8. Lubrication of brake face; very little wear of either wheel or magnet.

9. Absolute silence of operation and release. (No hissing to frighten horses on streets.)

10. The low E. M. F. at which it operates.

11. The ease of its application and control.

12. Conserving strength, and prolonging the usefulness and life of the motormen.

13. The smoothness of its operation.

14. The fact that its use cannot cause flat wheels.

Discussion.

The Chairman:—I consider Mr. Sperry's paper very important, as all electrical engineers must feel it has been a reproach to our profession, that electric cars have killed so many people in the last few years. In fact, such accidents have been much more fatal than those due to the direct effect of the electric current; that is to say, the number of persons killed by electric current, strictly speaking, is probably far less than those killed by electric cars which were not stopped in time. It seems to me that anything in the direction of alleviating this trouble is welcome, and something which seems to be so effective as this, is particularly welcome. Discussion of the paper is now in order.

Mr. Joseph Wetzler:—I would like to ask Mr. Sperry if he has noticed any additional wear on the teeth of the gear, due to the action of the brake? In ordinary braking by hand, when the current is shut off the wear practically ceases. But with the motor acting as a generator, the teeth would again come into action, the wear, of course, being on the opposite flanks of the gear teeth.

Mr. Sperry:—In response to the gentleman's question I would say that we have noticed no wear whatever, and the reason for it is this, and it is made quite apparent in Table III. If you will notice, in this table the currents mentioned are really the currents used in the braking application, and they hardly more than consume the stored up energy of the moving masses of the motor. You can see that the watts delivered to the brake are very small indeed. The ordinary braking current employed to de-energize both the car and the trailer is only about fourteen amperes; it is hardly anything, you see, compared with the current used to accelerate the same car and trailer, which would probably require 40 or 50 amperes.

Mr. C. S. Bradley:—Mr. Chairman, Mr. Sperry has brought this out so well that I do not dare to criticize it, but I would like to ask him a question that occurs to me, and it is this: Does the motorman ever get rattled and throw that switch clear over and throw off his brake, and bring in the motor current, and then throw it back again, and in that movement undertaking to stop the car, when really he is throwing the motor on?

Mr. Sperry—Is your question this: Does the motorman throw the lever off a little way and then throw it on again without going clear over, thus? [illustrating]. My experience is that an excited motorman will try to throw the lever over too hard and through the end of the controller box if he could.

Mr. Bradley:—But a motorman, in the event of a great emergency and under excitement, is sometimes apt to make two motions, one forward and one backward in succession.

Mr. Sperry:—I should think it possible, but I never knew it to occur, however.

Mr. W. J. Hammer:—I would like to ask Mr. Sperry a question. Mr. Sperry speaks of the braking effect of the Foucault currents on the generator. I would like to ask him what his estimate is of the percentage which is due to the Foucault current. A little over two years and a half ago, when I was in England, I had the pleasure of witnessing some experiments made by Mr. Henry E. Walter, a member of the Institute, and formerly Chief Electrician of the Edison Machine Works, in Goerck Street, and afterwards at Schenectady. He made a brake in which he employed nothing but the Foucault currents generated, and in the experiments referred to, magnets were suspended underneath the cars, placed close to the rail, and by passing a heavy current through them he produced Foucault currents which he proposed to use solely for the braking effect on the car. That seems somewhat in line with the experiments which Mr. Sperry has been making, and, as he refers to the importance of it, I would like to ask him about what percentage of the braking effect he considers is due to that in his brake. These experiments of Mr. Walter's that I saw were only preliminary, and were conducted on a small scale.

Mr. Sperry:—I would ask the gentleman, in the experiments referred to, was the air-gap open? There was no contact.

Mr. Hammer:—No sir.

Mr. Sperry:—Of course the resistance there is something enormous. We have found that the *reduction* of the resistance at the air-gap is a great point in fully utilizing these induction currents. Table III, in the paper, will give the exact percentage of any point on the curve that you may want. Taken at the knee, which occurs at 16 to 18 amperes, the pull due to magnetic adhesion is 160, and the pull actually obtained from both was 2,584. You can readily see that the Foucault currents here were by all manner of means the greater component. Just what percentage I have not figured.

The Secretary:—I would like to inquire of Mr. Sperry as to the reception which such a radical change in the form of brake-shoe has received at the hands of the railroad men who have been using the ordinary brake-shoe for so many years?

Mr. Sperry:—I could better answer that inquiry by referring you to our sales department. The reception of it at the hands of engineers, wherever they have been found, has been very enthusiastic; but as to the average railroad purchasing agent, he looks upon it as a brake-shoe that is liable to cost more than the ordinary brake-shoe, and he is a little afraid of it on that account, losing sight of the saving in wheels which is from two to three fold. The reception of the brake at the hands of the motormen has been very enthusiastic. There has, however, been one exception. A motorman by the name of Wm. Kerslake, thought it was a dangerous thing, because if they put this kind of a brake on all the cars the company would be hiring women to run them. It must of course be remembered that it takes the place of two brake-shoes and a lot of brake mechanism.

Mr. Joseph Sachs:—I would like to ask Mr. Sperry something in reference to Table III. I notice in looking through that table that the effects obtained from magnetic adhesion, or attraction of the brake-shoe to the magnet is but a small part of the entire retarding effect. It would seem to be possible to entirely brake the car by means of a non-magnetic armature, and simply use the currents generated therein to brake with. Apparently the greatest retardation is due to the brake magnet and shoe, acting as a dynamo. It would seem from your experiments that the friction due to magnetic attraction could be disregarded.

Mr. Sperry:—I should say that such a thing would be perfectly possible. Suppose *these* two circles were upon the two sides of the core, and suppose *that* was a non-magnetic material, where would you get your lines, to commence with? You will be obliged to have enormous current to get any circulation, and of course it is the lines of force that do the business after all. The air-gap *there* would be so great that it would be putting a coil of wire down against a magnet and expecting a heavy flux. Of course there would be hardly any appreciable magnetism. But as to the question, given the flux, and of course the retardation would certainly be there, as has been shown.

Mr. Max Osterberg:—I would like to add one, to the advantages pointed out by the speaker which seems of rather striking commercial value. The authorities in a great many cities limit the maximum speed of the cars in the business districts to eight miles per hour, as with a higher speed sudden stops in cases of emergency become impossible. It would not be long before the maximum limit would be raised, if we could convince those entrusted with framing the laws that with an electric braking system, cars running at ten miles an hour can be stopped quicker than at present running at eight. There are about 2,600 cars running in New York, and if every car can run ten instead of eight miles, that is 25 per cent. faster, then 20 per cent. or 520 of the total number of cars can be done away with. Counting the wages of motorman and conductor at $5.00 per day, it amounts to $2,600.00 a day, or $955,000.00, pretty nearly one million dollars per year, which the car companies in New York City alone would save.

Mr. Sachs:—The point I wanted to bring out is in regard to the wear of the gears, as Mr. Wetzler asked before. I notice that the actual braking energy, supplied by the motor, is very small. The brake acts like a separately-excited dynamo, the exciting current being furnished by the motor.

The braking effect of the motor through the gears, when acting as a dynamo, is very small, compared with the braking energy of the brake magnet and shoe, acting as a dynamo. I believe I am right in that supposition.

MR. BRADLEY:—Mr. Chairman; this magnet seems to be something that I never have heard of before. I have often thought of such a magnet. I would like to ask Mr. Sperry exactly how the winding runs; where is the coil located and where are the terminals?

MR. SPERRY:—The coil is first wound in a large hoop, then folded back upon itself and made to surround the crescent-shaped core. The recess is contracted at the face, the coil is secured in place by a plastic material surrounding same, the upper portion of the slot being filled with metal forming a smooth metallic surface. The terminals are brought to the surface in the form of flexible wires one of which is usually again reimbedded.

MR. ROBERT MCA. LLOYD:—Mr. Sperry has given us this evening the result of an able research, and shown us some ingenious devices; but, Mr. Chairman, they do not satisfy me that the brake problem is solved, and for three principal reasons. First, it seems to me that this residual magnetism does not offer any security while holding a car on a steep gradient. Second, Mr. Sperry has said that this apparatus is much less liable to disorder than the familiar mechanical brake, but it is not clear to me why electrical devices should be any more free from fault than mechanical devices, and so I do not see why this brake should be more perfect than a windlass brake. There may be some trouble in the switch box, or some difficulty in the motor itself, or a brush might be injured so that there would be no exciting current in the magnet. Then, in the third place, when you stop the wheels you have not necessarily stopped the car, and a great many of the accidents which occur in our cities are because the tracks are covered with a soapy kind of mud that will let a car slide along fifty feet. It is true, as Mr. Sperry has said, that most brakes are not capable of locking the wheels, but even if the wheels are locked on such a rail as we have in many of our cities, a car will slide a considerable distance, leaving grade out of account. So that in order to help ourselves, we may have to pay more attention to the track, and to the wheels themselves; —a turned steel tire is of course better for traction purposes than a chilled wheel. Then we ought to have both a mechanical and an electrical brake on a car, because in going down a hill if anything should happen to such a brake as this, you would be lost, and the same would be true also of a mechanical brake. It seems to me that pressure might be applied to these disks by a windlass, or some other mechanical contrivance, and make the brake just as efficient as if it were held by magnetism. On the other hand, I would like to say in answer to one of the previous speakers, that I do not think it would be practicable to use a combination of that sort as a generator, besides if you are going to do that, you might as well as well use the armature of the dynamo or the motor as a brake, and brake through the gears. Of course, that presents a great many difficulties, and it is not a good way of braking a car, the gears, however, not being the greatest source of difficulty. Then I would like to say that an improvement might be made in putting the disks on the armature shafts where there is gearing. I have tried some of these disk brakes and think this is by far the best one I have ever seen, and I believe Mr. Sperry will work it out to much greater perfection.

Mr. Sachs:—I would like to answer the previous speaker in regard to using the motor and braking the cars by means of the gears. I think that is just the point that has been tried at various times, and the point that Mr. Wetzler brought out. I think if a car were braked by means of simply short-circuiting the motor, or using it as a dynamo and retarding the car through the teeth of the gear, that in a very short time you would have no gears. I think, therefore, the principle that Mr. Sperry has brought out is a much superior one. If the retarding effect of a current in a magnetic field is used, it is certainly better to obtain this effect as Mr. Sperry does, than by the motor itself through the gears. I think that the experience with the electric motor, used as a braking dynamo, has not been very successful and I believe one of the principal reasons has been that the wear and tear upon the gears has been so great that your gears would not last.

Mr. Frankland Jannus:—Mr. President; I have understood all through the paper of Mr. Sperry that his device was intended to bring a car to a stop, and there it ended. I supposed that he intended to have a mechanical brake in addition to this. For example, if a long hill is to be traversed, the electric-brake will stop the car, but then of course the motorman will want to use his hand-brake in order to hold the car. Is not that the idea? If there is any way of using this device to let the car down, I think a little explanation of that would be very interesting.

Mr. Sperry:—Mr. Lloyd mentioned tracks covered with a soapy kind of mud and the wheels of the car sliding along fifty feet, etc. I would like to call attention to the fact, that in all large cities where these conditions are likely to exist, the railway companies usually use sand to give the wheels better adhesion and this incidentally helps on the brakes. For instance, to-day, when the rainfall has been constant for twelve hours, I have no doubt, but that the Broadway cable road near us, has used a number of tons of sand. Now of course their cars are propelled without traction of the wheels in any sense, still they use sand for the purpose of making their brakes effective. This is a remedy commonly adopted for slimy rails, and without it there are conditions of track where no brakes would be of service. The ordinary hand-brake may be used as a duplicate brake apparatus if required. I have, however, yet to see the first man who will wear himself out on the hand-brake, when he can brake the car by simply pushing the lever and allowing electricity to do his work. As to the certainty of its acting, as I said in my paper, I do not see how anything can be more certain; every time you apply the current you necessarily test its capacity for the next brake application. In the thousands of miles per day that this brake is now running in this country, I do not know of a single failure. If a car will not run, it should be put in order, but if it runs, it brakes. As to coming down hills, that is doubtless a point that I have not made clear. The brake will not make a full stop unless you want to stop. We are operating on a long and steep hill (a mile and a quarter, I think) in Waterbury, Connecticut, not far from New York City, and if anyone is sufficiently interested in its operation he may go up there and ride all day up and down the hill, and he will see that it performs its work well. The point that I have not made clear, I believe [illustrating with the apparatus,] and one which you can readily understand, is that I can hold the amperage in the brake circuit anywhere I choose by simply manipulating the lever thus. When I apply the brake I will now cause it to hold the amperes at some given place, and that means that the car is retarded at a certain rate or pace. [Illustrating]. Now you see I hold it right there.

MR. JANNUS:—Suppose the car has come to a full stop on the hill, what then?

MR. SPERRY:—As I said in the paper, the residual magnetism producing current, will hold the car for a time as you can see, and perhaps you did see the last time; this is very much more marked on a large motor than on this one. [Illustrating.] Of course the hand-brake is on the car if you wish to hold it indefinitely.

MR. E. A. MERRILL:—I would like to ask as to one point. About two weeks ago I was in an Eastern city where there are a number of grades. They were running two motor cars, pulling three trailers all in one train. Near the top of an eight per cent. grade the fuse blew on one of the motor cars, and the other motor car was not able to hold the train, which started back down hill. Now I would like to ask Mr. Sperry what would be the action of the electric-brake in a case like that?

MR. SPERRY:—The electric-brakes work equally well after the fuse has blown. The blowing of the fuse in the case cited would not have made any difference whatever with the electric-brake.

MR. MERRILL:—In the case I refer to, they caught the train and held it by the ordinary hand-brake. This was an eight per cent. grade, running up to nine per cent. at the top. There were three trailers attached to two motor cars, the two motor cars acting in conjunction. My question is, what would have been the result if they had had your electric-brakes and found that they were useless to hold the train?

MR. BRADLEY:—The motion of the cars in itself applies this brake.

MR. MERRILL:—My point is this. This electric-brake will undoubtedly stop the train, but how are you going to hold the train on a grade like that after you have stopped it?

MR. SPERRY:—That depends on how long you wish to stop, and has been before explained.

MR. MERRILL:—Also, can that brake be used on an ordinary emergency stop? There is a great difference between the emergency stop and the ordinary stop. In an ordinary stop it is of great advantage to have the adhesion increased as the speed decreases; that is, to have the brake pressure decrease. In an emergency stop it is very necessary for that adhesion to remain constant or to increase. Now with the electric-brake the stop is very gradual; the retardation is practically uniform. With the hand-brake or with the air-brake you can hold the brakes set, and the stop becomes relatively more rapid as the speed decreases.

MR. SPERRY:—In reply I would say that it is in emergency service that the full beauty and effectiveness of the electric brake is brought out. By its use the car can be brought almost to rest before the motorman can get the slack out of his hand-brake chain. There is nothing more instantaneous than the electric brake. There is no appreciable time lost. You see, the motorman puts it on instantly, and there is no time lost in winding up the slack chain as in a hand-brake as stated.

MR. SACHS:—I should like to ask another question: What would happen if the motorman suddenly stopped his car on a down grade, stopping the wheels, so that the motor generated no more current, would not the retardation cease and the car start again? I would like to know whether the hand-brake would not then have to be applied, or whether the electric brake would take care of itself?

MR. SPERRY:—Such a condition cannot exist. If the wheel stops, very little or no current is then circulating in the brake circuit, and the wheels start again to roll, and the generator to produce the braking current.

MR. SACHS:—Under the above condition, if the speed is very low, perhaps the motor would not generate current enough to energize the brake sufficiently, and the car would continue moving down grade slowly.

MR. SPERRY:—It is entirely automatic. It will generate and stop itself at very low speed, hardly moving.

MR. SACHS:—Is it not possible for the wheels to skid along for a few feet?

Mr. Sperry :—No sir.

Mr. Sachs :—Then I understand you to say that you cannot hold the car on a down grade with your electric-brake alone.

Mr. Sperry :—Oh, yes, we can. I illustrated that a few moments ago.

The Chairman :—I think that this point has been pretty well discussed, and the hour is quite late. But if there are any further remarks on some new point that has not yet been brought out, we should be glad to hear them.

Mr. Merrill :—I would like to ask one more question. There are a great many statistics showing the space within which a car can be stopped with various brakes. I would like to ask Mr. Sperry if he has any statistics of that sort by which his system can be compared with other systems in use.

Mr. Sperry :—The Westinghouse air-brake people from their latest tests, give data showing that they only utilized about fourteen per cent. of the available adhesion of the rail, whereas we utilize it nearly up to the limit. I judge from this that with a given speed we could stop in say less than one-fourth of the distance that they require, or did require in the tests named.

The Chairman :—If there are no further remarks, a motion to adjourn will be in order.

[On motion the meeting then adjourned.]

Communications Received After Adjournment.

Mr. E. A. Merrill :—It is to be regretted that Mr. Sperry has made no tests to determine in what distances cars can be brought to a standstill under given conditions of weight, speed and track, and especially for emergency stops, for this would, in the minds of practical men, go far in determining the merits of the system as compared with other methods. Also that we have no definite information as to its ability to control cars on steep grades, as this question is one of the first to arise wherever grades occur. I think Mr. Sperry has failed to make proper allowances, in his comparisons with the Westinghouse tests, for the two principal factors of weight and speed. In stopping the car, a certain amount of energy must be dissipated, and it is evident that there is a limiting rate per ton weight, which cannot be exceeded without hazarding the safety of the car and passengers; it is quite possible not only to reach but to exceed this rate with the Westinghouse air-brake for any speed we meet in street railroad practice; therefore, *speed for speed and weight for weight* an electric car cannot safely be stopped in a less distance, the present limit for an 800-ton train from 30 miles per hour, is about 325 feet on an emergency application; with a corresponding rate of energy dissipation per ton weight, an allowance of one second for shutting off the current and applying the brakes, the distance required for bringing an 8-ton electric car to a full stop from an initial speed of 12 miles per hour is 39 feet, or a little less than one-eighth the former distance; such a comparison, however, is manifestly unfair, for the 800-ton train can also be stopped in 39 feet under similar conditions, and the 8-ton electric car will require 377 feet for a full stop, from an initial speed of 30 miles per hour; if the rate of energy destruction were greater, of course this distance would be less.

MR. SPERRY:—Having noted the above communication, I would add that the tests mentioned in connection with emergency service are given in the general statement found on top of page 685. The limiting rate of energy dissipation does not seem to trouble railway managers very much for strictly emergency purposes, and certainly cannot be more severe than reversing the motor under full speed, which is almost a universal practice in cases of the most urgent necessity. The statement that electric cars cannot be safely stopped in less distance than ordinary railway cars I have found not to be true, and probably for the reason that the strain is not required to be transmitted through the swiveling bolt, or bolster of the truck high above the wheel contact upon the rail, which has been found to be the first place to give way in emergency stopping in railway service. On the street car the masses are more resiliently supported, especially on the four-wheel car, where the stripping of the car body from the truck is a far more difficult matter than with the swiveling truck. I have found in practice that an emergency stop with the electric-brake can be made under the conditions, and within practically the distances named in the paper. The easing off of the curve of retardation at both ends, making it an O. G. curve rather than a straight line at a declining angle gives by far the easiest stop. This curve I have found is the one naturally produced by the electric-brake, and is probably the condition which yields the sensation of a cushioned stop.

MR. W. E. HARRINGTON:—I think without peradventure of a doubt that the paper as read by Mr. Sperry is one that covers a subject vital to the interests of every electric railway manager. The question of the proper braking of a car should really be viewed from an emergency standpoint, and further, the form or method of braking employed, should be such that it be always in use and not known as an emergency brake? The present method of reversing a motor in case of an emergency is absurd, and usually results in opening the magnetic circuit-breaker at the power station controlling the particular division the car may be on, or breaking gears and probably springing shafts. The above results are so usual, that particular instructions are given to motormen never to reverse, except in cases of extreme emergency, as, for instance, where life is in danger. The recent and established principles of electric railway engineering, which now make it necessary to place magnetic cut-outs on the feeders leading out from the switchboard of our power stations, has rendered it absolutely necessary to have other methods of stopping our cars without depending on the power station for so doing. An accident occurred recently at York, Pa., where a life was lost, simply because the magnetic cut-outs opened at the power station when the motor circuit was reversed. I mention this case particularly as it had the rather peculiar effect on the local management of their considering the advisability of going back to the unreliable and station-destroying fuse-wire of ancient history. The facts are so numerous and so convincing that the braking of a car should be self-contained and entirely independent of the power station, that it may be considered axiomatic. The above position is further emphasized when we consider the fact that in the next two years it will be as common to see magnetic cut-outs on our cars, as it is at present to see such devices in our power stations. I think that the plan and method as employed by Mr. Sperry are correct and practicable, and after a few minor details not insurmountable, are overcome, or rather remedied, the system will be commercially successful.

Mr. Joseph Sachs :—There is one point in reference to the form of electric brake, described by Mr. Sperry, which would appear to be more or less of an objection under certain conditions; that is the fact that it is impossible to completely stop the car for any length of time on a down grade by simply using the electric brake as herein described. During the discussion at the meeting of the Institute I attempted to bring out this point, but it appears to me from the answers made by Mr. Sperry to my questions, that I was not properly understood. I assumed a condition where an obstruction made it necessary for the motorman to stop his car instantly and hold it on a down grade, but I could not see how this could be accomplished with the form of brake described. It is true that the car can be brought to a *stop* by the arrangement Mr. Sperry describes, but I can see no way in which it can be *held* on a down grade without the use of some additional device. After the car has been brought to a stop by changing the motor into a dynamo, and throwing it across the brake magnet, the retardation of the brake will also be stopped, as no more current will then be generated by the motor. As a natural consequence, therefore, the car will start again and stop, start again, and stop, and so on until it gets to the bottom of the grade, or, perhaps, after the first stop, will run down the grade at perhaps such speed as to prevent enough current being generated by the motor to effect sufficient retardation in the brake to hold it. It would seem therefore, to me, that in case of an accident under the conditions given, that it would become an absolute necessity to provide some means whereby the car can be held any length of time. It is true that this can be accomplished by applying the hand-brake after the car has been brought to its first stop, but this would seem to be objectionable. It would seem to be a simple manner to so arrange the brake that current could be supplied to it to hold the car under any conditions. While I think that Mr. Sperry's device of the greatest interest and value to electric traction, it would seem that the feature which I have described would be an objection but one which could no doubt, readily be obviated, even without the use of the hand-brake.

Mr. Sperry:—The gentleman is referred to previous answers in the discussion.

Discussion in Chicago.

The meeting of Western members was held at Armour Institute, Chicago, September 19th, 1894.

Professor Stine having read a telegram announcing the appointment of Mr. B. J. Arnold as Secretary, Mr. Arnold after a short address, in which he thanked the members for their support, announced the first business to be the appointment of a chairman for the meeting, and suggested the name of Professor Stine, who was unanimously elected.

The Chairman:—I will call your attention to the paper prepared for this evening: "The Electric Brake in Practice." In the absence of the author, Mr. Elmer A. Sperry, who is reading the paper at the New York meeting to-night, Mr. L. H. Rogers, the General Manager of the Sperry Electric Company, will read the paper for us.

The Chairman:—The paper is an exceedingly interesting one, and I am sure that we ought to have a full and interesting analysis of it, and I will call on Mr. Kammeyer to open the discussion.

Mr. Carl Kammeyer:—My familiarity with electric railways is so slight that I do not feel exactly competent to start this discussion. The paper has been of great interest to me, and I cannot help seeing the importance of this subject. I also admire the especially able manner in which Mr. Sperry seems to have solved the problem, and it seems to me that if I had an electric railway, the first thing I should do would be to order one or two of these brakes for each car; perhaps one would be enough. Mr. Grier here wants to know—he didn't like to ask the question himself—whether that would not be a good device to let a car slide down hill. Would it not be a good idea to let a car go down in that way?

Mr. Rogers:—Yes; if you did not want the car to go very fast.

Mr. Grier:—Not very long ago the question of automatic brakes in connection with the inclined plane railway upon which many Chicago people ride after "shooting the chutes" was brought to my attention. We put a brake on the track, but there was nothing electric about it. Now, it seems to me, if we had a brake of that kind on the car, if the cable should snap, the car could go down only just so fast, because when it began to go faster, the brake would become stronger and stronger. When the brake caught, the car could only go at a certain speed, and when it reached the bottom, it would drop as if it were going into a feather tick.

THE CHAIRMAN:—It occurs to me that this device might be capable of extension to an elevator in its fall. I presume it might be worked by the use of an independent device.

MR. KAMMEYER:—I think Mr. Arnold would be very competent to enlighten us upon this question. I recollect that he had a device somewhat similar to Mr. Sperry's.

MR. B. J. ARNOLD:—I have given this electromagnetic clutch question some study, and some time ago devised a plan for attaching direct coupled generators to their engines or other prime movers in such a manner as to allow any one, or all of the generators to be driven from either power unit, and I presume that this is what Mr. Kammeyer refers to. The form of electromagnet which has been described here this evening is similar in form and action to that device, although it is applied to a different use, so there can be no real similarity in them. I recognize in this brake two main parts, as follows: First, the motor under the car, which becomes a generator when the brakes are applied, and, second, the electromagnet or coil, whose function is to absorb the energy produced by the motor-generator and utilize it toward overcoming the momentum of the car by gripping or clasping the revolving disk on the car wheel. It seems to me that the retarding effect caused by the drag of the armature on the wheels after the motor has been converted into a generator should be a sufficient brake without the use of the electromagnet at all, unless the magnet is used simply as a resistance or reservoir, as it were, to consume the current generated by the armature, and prevent its having to work an open circuit. I presume that Mr. Sperry has discovered other practical objections to working the motor on open circuit, than the usual one, which is the serious sparking at the brushes, and has adopted the coil to eliminate the difficulty.

MR. ROGERS:—The generation of the current is excessive, and tends to heat the armature to quite a considerable degree, when you attempt to stop the cars through the gears alone, and then the wear on the gears and pinion is considerable. It will do admirably. You can stop the car just as quickly that way as you can do this; but the point is to get the electromotive force and current as low as possible, and get the maximum braking effect.

MR. ARNOLD:—The idea is, that this electromagnet shall absorb the energy from the armature and assist in braking without putting the entire work on the armature and gears, as I understand Mr. Rogers.

MR. ROGERS:—Yes, that is right.

MR. ARNOLD:—There is another thing which must be seriously considered by any one who is contemplating the adoption of electric brakes for railway purposes, and that is the relative cost of maintaining this brake in comparison with the cost of other brakes. It strikes me that the cost of maintenance would be less in this brake than it would be for brake-shoes and wheels; but I can see in this apparatus some delicacy, inasmuch as the stationary and rotating disks must revolve very closely together. It certainly is a first class thing in the following respects: It is dependent upon no external power; it is not dependent upon the trolley in any way, and the latter point alone removes the worst objection that has been brought against other electric brakes, namely that in case of an emergency, when you want the brakes badly, they may become wholly inoperative if the current should be cut off the line, or the trolley wheel leave the wire. There have been a number of electric brakes devised upon the principle of using the current from the trolley; but they are objectionable for the above reason, and amount to no brake at all in an emergency

There is one objection which it seems to me might be urged against this brake, and that is this:—Suppose an armature should become burned out so that your generator is disabled. In case of an emergency stop, you would have no brake except by hand. However, in justice to the apparatus, it should be stated that this would be very unlikely to happen, and not nearly so likely as the liability of the trolley leaving the wire or the current leaving the line.

Mr. Rogers:—If the armature were burned out, I do not think the car would be going at a very high rate of speed.

Mr. Arnold:—It might be going down hill. There is another thought occurs to me in regard to going down the chute or inclined plane referred to by Mr. Grier. I hardly think this brake would retard the car enough, because the grade or incline is so great, that were the wheels to be skidded, there would be but slight retarding effect upon the wheels, and the car would slide down anyway, as the adhesion of the wheels to the rails would not be sufficient to prevent it.

Mr. Rogers:—What is the grade?

Mr. Grier:—Twenty-five per cent.

Mr. Arnold:—You can see that the cars would go down there, brake or no brake.

Mr. Rogers:—This is not a brake for sudden drops

Mr. Grier:—Take the viaducts in this city. Sometimes the the cars have a very heavy load. They have sometimes 250 people on the motor car and the two trailers, and sometimes more. Now, that is almost enough of a load to cause an accident by the burning out of the armature if anything is defective, which would make the motor ineffective, inoperative, and the car would begin to slide backwards down hill. How would you stop that?

Mr. Rogers:—If the source of power is disabled, there is no brake.

MR. GRIER:—You always have the hand-brakes there. But on the viaduct at Sixty-first Street, during the World's Fair, if the armature had burned out going up the hill, it might have been very disastrous.

MR. ROGERS:—Some cities are compelling two brakes now on electric cars. In Cincinnati they compel two brakes on each car. On one of the hills there, a car ran away and hit a telegraph pole, and the pole cut the car in two and killed several people.

MR. GRIER:—What happens if the fuse connected with the control blows out?

MR. ROGERS:—There is no fuse in the local circuit. I will say here that the auxiliary brake that they adopted in Cincinnati would make an interesting picture. It is a long rod with a piece of hard wood on one end of it, held by a nut and washer. The conductor is compelled to stand on the rear step when going up a grade, and at every stop of the car he is expected to get off and shove that under the rear wheel. That is in Cincinnati, a city of 300,000 inhabitants. They operate 500 cars on that system, and yet that is what the conductor is compelled to do.

MR. GRIER:—That is the old device of putting a chunk of wood under the wheel to prevent its slipping.

MR. ROGERS:—The conductor's duties are prescribed minutely, and he is required to put that log under the rear wheel. He is not allowed to collect fares when on the grade.

I will just state that we sold one of these equipments, and after they had tried it they would not allow the brake to start in service, giving as a reason that it was a perfect device, and the Council would compel them to put one on each of their 500 cars. They would not even let it start.

MR. KAMMEYER:—You speak of a low voltage being employed, as low as six volts. I take it that the only magnetism used is the residual magnetism of the field of the motor. Is it not?

MR. ROGERS:—All you want is to generate enough voltage to send that current through the wire.

MR. KAMMEYER:—It would seem that after cutting off the trolley current, the car would still have enough speed to give you certainly more than six volts.

MR. ROGERS:—The residual magnetism of the field is all that is used.

MR. CALDWELL:—I think the low voltage is due to heavy resistance in the rheostat.

MR. KAMMEYER:—It goes through the rheostat?

MR. ROGERS:—Yes. If it depended entirely upon the residual magnetism, that would necessitate cutting the field out, and leaving only the armature in circuit.

MR. ARNOLD:—I would like to ask Mr. Caldwell what the E. M. F. of this motor is, when converted into a generator and run at a speed of five miles an hour. Does it generate enough voltage to pump any current back into the line, if you gave it an opportunity? We have talked and dreamed about the possibility of giving back energy to the line on electric roads, but we have never yet done it practically on city service, and here seems to be a pretty good chance to see whether it can be done or not.

Mr. Caldwell:—I am pretty certain that it could not be done in this case from, the fact that we cut the trolley line entirely free.

Mr. Arnold:—Suppose you left it on?

Mr. Rogers:—If it were connected with the trolley at all would not the larger potential sway as it were?

Mr. Arnold:—That is the point. I do not think that it can generate a strong enough E. M. F. for a sufficient length of time to be effective when series motors are used.

Mr. Rogers:—It would be necessary to go above 500 volts, and I do not believe it possible to generate more than this.

Prof. Stine:—It would have to run faster as a generator than it did as a motor. I would like to ask the question whether you would like to state just how much wire is buried here in this brake coil. Can you? What I want to know is the number of ampere-turns.

Mr. Rogers:—I will make a guess, if Mr. Caldwell will. I think it is about 60 or 70 turns of No. 10 wire. I can get it exactly for you.

Prof. Stine:—Another thing. In the swinging of the brake do you have any spring to ensure contact when not in use, or to keep approximate contact. I would like to ask just here this question: How far away the brake can swing from contact with the wheel to be effective? What is the greatest distance? I am after the end play of the axle. How do you compensate for that to ensure the fact that the brake will be close enough to the wheel, and through what maximum distance will that brake work?

Mr. Caldwell:—I could not say what is the maximum distance it would work through, but in my experience I have had the face of the magnet removed almost a quarter of an inch from the wheel. That was due to the collar on an axle having become loose and slipping off a little and allowing the magnet to fall away. Now it would act effectively over that space and draw the magnet right up to the wheel.

Prof. Stine:—Is it so swung that its own weight will keep it up against the wheel?

Mr. Rogers:—No. It will keep it away. It is a little out of center, so its own weight will tend to keep it away.

Prof. Stine:—There is never a maximum of a quarter of an inch allowed?

Mr. Caldwell:—No.

Mr. Rogers:—I think we have allowed one-sixteenth.

Mr. Grier:—Have you any data as to the cost of replacing wheels when using the ordinary brake-shoe?

MR. ROGERS:—Do you mean, what will new wheels cost?

MR. GRIER:—When you ride in a car which has been some time in service, you will hear the thump, thump of the flat wheel. Now, of course, the replacing of those wheels is an expense. We have been examining this brake as to its electrical and mechanical construction, but it is important to examine it in its commercial aspect. The very point you will have to bring to the attention of those people in Cincinnati is this: You use brake-shoes on your cars; you will have to buy 500 of these equipments, if the Council find that they are the best thing in the market. Now they reply that they can't stand the expense; and if you can show them that the cost of replacing wheels worn out by the action of the ordinary brake-shoe on the rim, and the cost of replacing the brake-shoes is more than the investment required to put on these new brakes, you will put the matter in a very different light.

I find that a car runs 55 miles a day on the average, and they make 400 to 500 applications of the brake-shoe. Now, the wear and tear is considerable; the grinding is considerable, and I should judge from riding over old equipments in Chicago that the wear on the wheels is considerable, and it seems to me these considerations would more than offset the cost of new brakes.

MR. ROGERS:—That is one of the advantages of the new brake, but I learn in dealing with these parties that it is very hard to convince them that some new way of spending money will be advantageous. They become accustomed to the expenditure of money in a certain way; the expenditure of it for wheels and brake-shoes is a known quantity with them, and they prefer to pursue the even tenor of their way; and that old argument must be presented in a very emphatic manner, before it will have any weight with them.

MR. GRIER:—While any advance in electrical science is of advantage to the public generally, yet it is also for the welfare of the electrical profession; and when we know of a good thing such as this, we should have such data as will enable us to present forcible argument to others for the adoption of the improvements. In pushing such things forward, we are indirectly benefiting ourselves, and we should be able to present our case strongly. Now, if in discussing this particular matter, we could say to a man that it cost $2,000 or $3,000 a year to replace car wheels, and we have an electric device that will save a great part of that, we shall make a good point. We are not interested pecuniarly in this brake at all, but we are interested in the development of the electrical business.

MR. ROGERS:—That was one of the main points in Mr. Sperry's mind in developing this brake: the saving of car wheels.

A MEMBER:—Car wheels do not cost very much now. They are pretty cheap.

MR. ROGERS:—The saving in the wheels is said by Mr. Bradley, of Waterbury, Conn., to be three-fold. He writes to our company—he has seventeen of these equipments and thirty-four brakes—and states that it is his candid belief that the wheel will last three times as long, using this electric brake instead of the ordinary hand brake.

MR. GRIER:—Another item of cost sometimes lost sight of is the cost of changing. It always costs two or three dollars to remove the old wheels and put the new wheels on.

MR. ROGERS:—The men in this city wish to have it demonstrated on their own lines before they are satisfied. The saving of money for car wheels is an old story, and it does not have the effect it might at first be expected to have.

MR. ELBERT F. NORTON:—My knowledge of street railway practice is extremely limited, but there is one point connected with the paper which has not been commented upon, and it is one which I think would appeal very forcibly to a railway company; that is the point of emergency stops. Take one accident —of course the price paid is never publicly known—but a person may recover $5,000 damages, and it would seem to me the saving of that $5,000 would purchase a considerable number of these equipments.

DR. WALTER LOBACH:—I wish just to refer to the question whether it is possible to send from a current of 500 volts, energy back to the same line which furnished the current for the motor. I believe that it is well known that motors which are run at a certain speed to give a certain amount of mechanical energy, when used as dynamos always need much higher speed to furnish the same voltage. If one is using shunt motors it may be possible by increasing the strength of the field to increase the tension so much that current is flowing back to the line, especially if there is added a new source of energy as is the case when cars are descending hills.

MR. ROGERS:—I was about to suggest that if it were possible to put some "juice" back into the line from each moving car, if we had a system of that kind and provided enough cars and a sufficient number of hills, some erratic "Pennock" would evolve a system of street car propulsion without any generating station at all.

PROF. STINE:—There is one thought occurs to me in regard to the desirability of using a portion of this energy generated in this machine when it becomes a motor. It strikes me that the mean effective pressure would exist for such a small period of time that it would be utterly impossible to think of attempting any such plan. It would strike me that there is but a moment when you have that effective pressure, and without some new scheme of distribution I believe it would be impossible in any case to utilize it for that purpose.

MR. KAMMEYER:—I do not think that any engineer will claim to-day that a 500 volt motor when brought to a stop, or virtually brought to a stop, is able to pump any current back into a 500-

volt circuit. It seems to me that the counter E. M. F. that a motor develops, would act as resistance to the flow from the trolley. There is no doubt about that. And whether that would be a gain or not I do not exactly see. It seems to me that it would, because it would act as a resistance.

PROF. STINE:—I think that unless the volts were sensibly strengthened, you could never get up a greater pressure than possibly ten per cent. less, say 450 volts.

I would like to ask for some further information as to the part assigned to the Foucault currents. It seems to me that a very high efficiency is claimed for them. In Table III of magnetic adhesion or traction, the pull due to magnetic traction or adhesion is given in the first case as 7.6 pounds. Now am I to understand that the 125 pounds given in Column B is what is actually obtained with the brake, or that the excess pull between those two is due to the Foucault currents. I have not taken the pains to decipher it very carefully.

MR. ROGERS:—That is my understanding of it.

PROF. STINE:—I do not understand it. I do not attempt to dispute the figures, but it seems to me to be enormous.

MR. ROGERS:—On page 701 Mr. Sperry attempts to answer that. He says: "By reference to Table III the result in re- "tardation gained through the eddy or other currents may be "plainly seen, Column A indicating the retarding effect which "should be expected from a friction co-efficient of 10 per cent. "between the lubricated surfaces due to magnetic attraction of "the lines actually circulating, and Column B indicating the "values of retardation actually obtained on the dynamometer."

PROF. STINE:—Now it seems to me that it is a question somewhat similar to the old one that came up when it was claimed that you could get better traction from a car using the single trolley current passing through the wheel to the rail. That theory was virtually smiled down, but I declare that these results seem to show that there is something more in that than was allowed. Of course, the Foucault currents that would result from the fluctuation of the current might play a greater part than we imagine in traction.

MR. ROGERS:—I think the conditions are different.

PROF. STINE:—They are different, but they have a similarity.

MR. ROGERS:—Increased traction due to connection between the rail and the wheel, as you say has been smiled down, but it is great if the rail is magnetized.

PROF. STINE:—I do not say that there is much of it, but these tables would lead us to give more credence to those claims than has been given. But as I understand it, you get your iron in contact and the Foucault currents do the rest.

MR. ROGERS:—Yes. But consider the amount of surface there as compared with the impact of the wheel against the rail, which is almost infinitesimal.

PROF. STINE:—You can't compare them; they are out of all ratio. I simply wanted to know why the part played by the Foucault currents seemed so high. There might be a great deal said about this device. If successful, it would seem likely to do for the electric service, what the Westinghouse air-brake did for the steam service.

[After a vote of thanks to Mr. Rogers for the paper of the evening, the meeting adjourned.]

THE AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

New York, October 17, 1894.

The 90th meeting of the Institute was held this date at 12 West 31st Street, and was called to order at 8 P. M. by President Houston.

The Secretary read the minutes of the meeting of September 19th, which upon motion were approved.

THE PRESIDENT:—The Secretary will please read the names of those who have been elected associate members at the meeting of Council to-day.

The Secretary read the following list:—

Name.	Address.	Endorsed by.
COLE, JOHN R.,	Sales Agent, New York Insulated Wire Co., 102 Sacramento St., San Francisco, Cal.	Geo. P. Low. F. C. Colville. Sidney Sprout.
DAVIS, HAROLD MCGILL,	Advertising Manager, *Electric Power*, 86 Cortlandt St., N. Y.; residence, 312 Clinton St., Brooklyn, N. Y.	H. L. R. Emmet. H. A. Foster. Joseph Sachs.
DOMMERQUE, FRANK J.,	Chief Draughtsman, Chicago Telephone Co.; residence, 71 Potomac Ave., Chicago, Ill.	C. H. Wilson. A. V. Abbott. A. S. Hibbard.
DUNCAN, THOMAS,	Electrician, Laboratory Fort Wayne Electric Corporation, 407 Broadway, Fort Wayne, Ind.	F. S. Hunting. E. A. Barnes. C. S. Bradley.
DUNN, KINGSLEY G.,	Electrician, Palace Hotel, San Francisco, Cal.	Sidney Sprout. Geo P. Low. Wynn Meredith.
ETHERIDGE, CHAS. LOCKE,	Chicago Telephone Co.; residence, 4714 Kenwood Ave., Chicago, Ill.	R. H. Pierce. A. V. Abbott. A. S. Hibbard.
EYRE, M. K.,	Assistant to Manager of Lamp Sales, General Electric Co., Harrison, N. J.; residence, 43 S. Washington Sq., New York.	John W. Howell. W. D. Weaver. Henry W. Frye.
FORD, JAMES S.,	Electrician, Chicago Telephone Co., Engineering Department, Chicago, Ill.	C. H. Wilson. A. V. Abbott. A. S. Hibbard.

HADLEY, ARTHUR L.,	Assistant Electrician to Chief Electrician and Gen'l Sup't, Fort Wayne Electric Corporation, 149 Griffith St., Fort Wayne, Ind.	E. A. Barnes. F. S. Hunting. A. L. Searles.
JONES, ARTHUR W.,	Representative for So. Africa, General Electric Co., Port Elizabeth, So. Africa.	John W. Kirkland. Maurice Oudin. Theodore Stebbins.
KELLER, CHAS. L.,	Chicago Telephone Co., Chicago, Ill.	C. H. Wilson. A. V. Abbott. A. S. Hibbard.
LARNED, SHERWOOD J.,	Electrical Engineer, Chicago Telephone Co., 203 Washington St., Chicago, Ill.	C. H. Wilson. A. V. Abbott. A. S. Hibbard.
NOXON, C. PER LEE,	Contracting Electrical Engineer, 628 Mission St., San Francisco, Cal.	Geo. P. Low. Sidney Sprout. F. C. Colville.
O'CONNELL, J. J.,	Telephone Engineer, Chicago Telephone Co.; residence, 76 Eugene St., Chicago, Ill.	C. H. Wilson. A. V. Abbott. A. S. Hibbard.
OLIVETTI, CAMILLO,	Ingegnere Industriale, Ivrea, Italy.	Wm. J. Hammer. R. N. Baylis. Ralph W. Pope.
RHODES S. ARTHUR,	Electrician, Chief Testing Dep't, Chicago Telephone Co., Chicago, Ill.; residence, 429 N. Pine Ave., Austin, Ill.	C. H. Wilson. A. V. Abbott. A. S. Hibbard.
ROYLANCE, L. ST. D.,	Electrical Engineer, with W. L. Brown, 2636 Howard St., San Francisco, Cal.	Geo. P. Low. Sidney Sprout. Fred'k G. Cartwright.
SACKETT, WARD M.,	Assistant Chief Draughtsman, Chicago Telephone Co., residence, 3249 Groveland Ave., Chicago, Ill.	C. H. Wilson. A. V. Abbott. A. S. Hibbard.
SANDERSON, EDWIN N.,	New England Manager, Westinghouse Electric Mfg. Co, 620 Atlantic Ave., Boston, Mass.; residence, Newton Centre, Mass.	F. B. H. Paine. W. K. Archbold. C. A. Terry.
SLATER, FREDERICK R.,	Assistant Superintendent of Buildings and Grounds, Columbia College; residence, 103 W. 48th St., N. Y. City.	Frederick Bedell. Harris J. Ryan. C. P. Matthews.
STRAUSS, HERMAN A.,	Electrical Engineer. Westinghouse Electric & Mfg. Co., 29 Plane St.; residence, 10 Clay St., Newark, N. J.	Philip A. Lange. Chas. F. Scott. O. B. Shallenberger.

Total, 21.

In the absence of the author the Secretary announced the title of the first paper before the meeting upon "The Theory of Two and Three-Phase Motors," by Lieut. Samuel Reber, of Fort Riley, Kan.

A paper presented at the Nintieth Meeting of the American Institute of Electrical Engineers, New York, President Houston in the Chair, and Chicago, Lieut Samuel Rodm n in the Ch ir, October 17th, 1894.

THEORY OF TWO AND THREE PHASE MOTORS.

BY LIEUT. SAMUEL REBER.

The complete mathematical solution of the two and three phase motors with the coefficients of self-induction as variables, and the effect of magnetic leakage taken account of, is extremely difficult, if not impossible. But an approximate solution in which the change of magnetic properties of the iron and magnetic leakage are neglected, the coefficients of self-induction considered constant, while the mutual-induction between the armature and field circuits follows a sine law, and the field supposed to be without projecting pole-pieces, is quite easy. We will then proceed to the solution with these assumptions, remarking that the two-phase formulæ are those of Professor H. A. Rowland, while the three phase formulæ and tables are our own.

Use the following notation:

L...... Coefficient of self-induction of one armature circuit.

L'..... Coefficient of self-induction of one field circuit.

c' c'' c''' Field currents.

$c_{\prime}$ $c_{\prime\prime}$ $c_{\prime\prime\prime}$ Armature currents.

E...... Maximum E. M. F. applied to field circuit.

p and p' are Maxwell's electro-kinetic momenta of one armature and one field circuit.

b...... 2π times the complete periods of field circuit.

f...... 2π times the complete periods of armature circuit.

ϑ...... Angular position of the armature.

M..... Maximum value of coefficient of mutual-induction between a field and an armature circuit.

V...... Velocity of rotating field

v...... Velocity of rotating armature.

λ ...... Length of one complete wave of magnetization or angular distance subtended by four (or six in the case of three-phase) poles.
I ...... Impedance of one armature circuit.
I' ..... Impedance of field circuit.
$C\ c$ .... Maximum currents in one field and armature circuit.
$R\ R'$ ... Armature and field resistance.
ψ and φ. Lag in the two circuits.

We may write at once the following equations:

Two-phase.	Three-phase.
$c' = C\cos(b\,t + \psi)$.	$c' = C\cos(b\,t + \psi)$.
$c' = C\sin(b\,t + \psi)$.	$c'' = C\cos(b\,t + \psi + 120)$.
$c_{,} = c\cos(f\,t + \varphi)$.	$c''' = C\cos(b\,t + \psi + 240)$.
$c_{,,} = c\sin(f\,t + \varphi)$.	$c_{,} = c\cos(f\,t + \varphi)$.
	$c_{,,} = c\cos(f\,t + \varphi + 120)$.
	$c_{,,,} = c\cos(f\,t + \varphi + 240)$.

The angular position of the armature at any moment will be $a\,\vartheta$ where $a = \frac{2\,\pi}{\lambda}$, and in the case of the two-phase motor the mutual-induction between one armature circuit and the two field circuits adjacent will be $M\cos a\,\vartheta$ and $M\sin a\,\vartheta$, and for the other armature circuit will be $-M\sin a\,\vartheta$ and $M\cos a\,\vartheta$. For the three phase they are $M\cos a\,\vartheta$, $M\sin(150 - a\,\vartheta)$, $M\sin(210 - a\,\vartheta)$, and $M\cos(a\,\vartheta + 120)$, $M\sin(210 - a\,\vartheta)$, $M\sin(270 - a\,\vartheta)$.

For p and p' we have for the two-phase

$$p = L\,c_{,} + M\,c'\cos a\,\vartheta + M\,c''\sin a\,\vartheta,$$
$$p' \quad L'\,c' + M\,c_{,}\cos a\,\vartheta - M\,c_{,,}\sin a\,\vartheta,$$

substituting and reducing since $b\,t - f\,t = a\,\vartheta$.

$$p = L\ \ c\cos(f\,t + \varphi) + M\,C\cos(f\,t + \psi),$$
$$p' = L'\,C\cos(b\,t + \psi) + M\,c\cos(b\,t + \varphi).$$

For the three-phase

$$p = L\,c\cos(f\,t + \varphi) + \tfrac{3}{2}\,M\,C\cos(f\,t + \psi),$$
$$p' = L\,C\cos(b\,t + \psi) + \tfrac{3}{2}\,M\,c\cos(b\,t + \varphi).$$

For the two-phase motor, the equations for the other two circuits are the same as the first, except sine is substituted for cosine. In the three-phase the values will differ by 120 degrees in the cosine functions. Passing to the exponential form, and in the two-phase system multiplying the sine values by i, the imaginary unit, and adding, Maxwell's equations become

$$R\ c\ \varepsilon^{i(f t + \varphi)} + \frac{d p}{d t} = 0. \qquad (1)$$

$$R'\ C\ \varepsilon^{i(b t + \psi)} + \frac{d p'}{d t} = E\ \varepsilon^{i b t}. \qquad (2)$$

If a condenser of capacity q be added in series to the armature circuit, L will be changed to $L - \frac{1}{f^2 q}$.

Substituting, differentiating and reducing equations (1) and (2) for the two-phase reduce to

$$\left[R + i\left(L f - \frac{1}{f^2 q}\right)\right] \varepsilon^{i\varphi} + i f\ M \frac{C}{c}\ \varepsilon^{i\psi} = 0. \qquad (3)$$

$$(R' + i\ b\ L')\ \varepsilon^{i\psi} + 2\ M b \frac{c}{C}\ \varepsilon^{i\varphi} = \frac{E}{C}, \qquad (4)$$

divide (3) by $\varepsilon^{i\varphi}$ and reduce we have

$$\varepsilon^{i(\psi - \varphi)} = \frac{c}{2 f M C}\left[R + 2\left(L f - \frac{1}{f q}\right)\right],$$

since $\varepsilon^{i(\psi - \varphi)} = \cos(\psi - \varphi)\ t + \sin(\psi - \varphi)$ we have at once

$$\cos(\psi - \varphi) = -\left(L f - \frac{1}{f q}\right)\frac{c}{M f C}, \qquad (5)$$

$$\sin(\psi - \varphi) = R\frac{c}{M f C}, \qquad (6)$$

$$\tan(\psi - \varphi) = -\frac{R}{L f - \frac{1}{f q}}. \qquad (7)$$

Squaring and adding (5) and (6) and reducing, we may write

$$\frac{c}{C} = \frac{M f}{\sqrt{R^2 + \left(L f - \frac{1}{f q}\right)^2}} = \frac{M f}{I} \qquad (8)$$

Eliminating $\varepsilon^{i\psi}$ from (3) and (4), and writing

$$R R' + b f\left[M^2 - L'\left(L - \frac{1}{f^2 q}\right)\right] = A,$$

$$b L' R + f R'\left(L - \frac{1}{f^2 q}\right) = B,$$

we have

$$E^{i\varphi} = \frac{-\frac{E}{C} I (B + i A).}{A^2 + B^2} \qquad (9)$$

hence,

$$\sin \varphi = \frac{-\frac{E}{C} A I}{A^2 + B^2} = - \frac{A}{\sqrt{A^2 + B^2}}, \qquad (10)$$

$$\cos \varphi = \frac{-\frac{E}{C} B I}{A^2 + B^2} = - \frac{B}{\sqrt{A^2 + B^2}}, \qquad (11)$$

$$\tan \varphi = \frac{A}{B}.$$

Squaring (10) and (11), and adding and reducing

$$\frac{E}{C} = \frac{\sqrt{A^2 + B^2}}{I}. \qquad (12)$$

Solving (5) and (6) by aid of (10) and (11)

$$\sin \psi = \frac{A f \left(L - \frac{1}{f^2 q}\right) - B R}{I \sqrt{A^2 + B^2}}, \qquad (13)$$

$$\cos \psi = \frac{A R + \left(L f - \frac{1}{f q}\right) B f}{I \sqrt{A^2 + B^2}}, \qquad (14)$$

$$\tan \psi = \frac{A f \left(L - \frac{1}{f^2 q}\right) - B R}{A R + f B \left(L - \frac{1}{f^2 q}\right)}. \qquad (15)$$

For the three-phase by the same method of solntion

$$\sin (\psi - \varphi) = \frac{2 c R}{3 C M f} = \frac{R}{I}. \qquad (14)$$

$$\cos (\psi - \varphi) = \frac{-2 c}{3 C M f}\left(L f - \frac{1}{f q}\right), \qquad (15)$$

$$\tan (\psi - \varphi) = - \frac{R}{L f - \frac{1}{f q}}. \qquad (16)$$

$$\frac{c}{C} = \frac{3 M f}{2 \sqrt{R^2 - \left(L f - \frac{1}{f q}\right)^2}} = \frac{3 M f}{2 I} \qquad (17)$$

$$A = R R' + b\, t \left[\frac{9 M^2}{4} - L' \left(L - \frac{1}{f^2 q} \right) \right]$$

$$B = b L' R + f R' \left(L - \frac{1}{f^2 q} \right).$$

$$\sin \varphi = - \frac{\frac{E}{C} A I}{A^2 + B^2} = - \frac{A}{\sqrt{A^2 + B^2}}, \tag{18}$$

$$\cos \varphi = - \frac{\frac{E}{C} B I}{\sqrt{A^2 + B^2}} = - \frac{B}{\sqrt{A^2 + B^2}} \tag{19}$$

$$\tan \varphi = \frac{A}{B}. \tag{20}$$

$$\frac{E}{C} = \frac{\sqrt{A^2 + B^2}}{I}. \tag{21}$$

$$\sin \psi = \frac{A f \left(L - \frac{1}{f^2 q} \right) B R}{I \sqrt{A^2 + B^2}}. \tag{22}$$

$$\cos \psi = \frac{A R + B f \left(L - \frac{1}{f^2 q} \right),}{I \sqrt{A^2 + B^2}} \tag{23}$$

$$\tan \psi = \frac{A f \left(L - \frac{1}{f^2 q} \right) - B R}{A R + B f \left(L - \frac{1}{f^2 q} \right)}. \tag{24}$$

In the case of the two-phase motor the total work done on the motor will be

$$E C \cos \psi = E^2 \frac{A \ R + B f \left(L - \frac{1}{f^2 q} \right)}{A^2 + B^2} \tag{25}$$

$$= C^2 \frac{A R + B + \left(L - \frac{1}{f^2 q} \right)}{I^2}.$$

The current heating in the field circuits is

$$\frac{C^2 R'}{\frac{\pi}{b}} \int_{\frac{\pi}{b}}^{0} \sin^2 b\, t + \frac{C^2 R'}{\frac{\pi}{b}} \int_{-\frac{\pi}{2 b}}^{\frac{\pi}{2 b}} \cos^2 b\, t$$

$$= \tfrac{1}{2} C^2 R' + \tfrac{1}{2} C^2 R' = C^2 R'.$$

Likewise for the armature circuits the heating is $C^2 R$.

$$C^2 R' + c^2 R = C^2 \left(\frac{R M^2 f^2 + R' I^2}{I^2}\right) \quad (26)$$
$$= E^2 \left(\frac{R M^2 f^2 + R' I^2}{A^2 + B^2}\right)$$

The total energy transformed into mechanical work less the hysteresis loss in the fields is:

$$E C \cos \phi - (c^2 R + C^2 R') = E^2 \frac{R M^2 f (b - f)}{A^2 + B^2} \quad (27)$$
$$= C^2 \frac{R M^2 f (b - f)}{I^2}$$

The ratio of this to the $c^2 R$ loss of the armature is $\frac{b - f}{f}$ (when no condenser is used) or $\frac{v}{V - v}$, hence the armature efficiency is $\frac{v}{V}$, when hysteresis loss in the field is neglected.

In the three-phase motor the work done by the currents on the motor is:

$$\tfrac{3}{2} E C \cos \phi = \tfrac{3}{2} E \frac{A R + B f \left(L - \frac{1}{f^2 q}\right)}{A^2 + B^2}$$
$$= \tfrac{3}{2} C^2 \frac{A R + B f \left(L - \frac{1}{f^2 q}\right)}{I^2}$$

The current heating is:

$$\tfrac{3}{2} (c^2 R + C^2 R') = \tfrac{3}{8} C^2 \left(\frac{9 M^2 f^2 R + 4 R' I^2}{I^2}\right)$$
$$= \frac{3 E^2}{8} \left(\frac{9 M^2 f^2 R + 4 R' I^2}{A^2 + B^2}\right)$$

The total energy transformed in the armature less the hysteresis loss in the fields is:

$$\tfrac{3}{2} E C \cos \phi - \tfrac{3}{2} (c^2 R + C^2 R')$$
$$= \frac{27}{8} E^2 \frac{R M^2 f (b - f}{A^2 + B^2} = \frac{27}{8} C^2 \frac{R M f (b - f)}{I^2}$$

and the ratio of this to the $c^2 R$ loss in the armature is as before $\frac{b - f}{f}$.

The angular torque is equal to the work divided by the velocity, and is in the case of the two-phase motor

$$E^2 \frac{2\pi}{\lambda} \frac{R M^2 f}{A^2 + B^2} = C^2 \frac{2\pi}{\lambda} \frac{R M^2 f}{I^2},$$

and for the three-phase

$$\frac{27}{8} \frac{2\pi}{\lambda} E^2 \frac{R M^2 f}{A^2 + B^2} = \frac{27}{8} C^2 \frac{2\pi}{\lambda} \frac{R M^2 f}{I^2}.$$

The starting torque can be increased by changing either the resistance or condenser; representing the quantities as starting by the sub o, we have the ratio of the starting torque to the running torque as follows for both cases:

$$\frac{T_o}{T} = \frac{E_o^2 R_o b (A^2 + B^2)}{E^2 R^2 f (A_o^2 + B_o^2)} = \frac{C_o^2 R_0 b I^2}{E^2 R^2 f I^2}.$$

At a certain speed the torque is a maximum, and the motor, if pushed in its work beyond this speed will stop. To prevent this, the motor should not be pushed to a point more than half the maximum torque. This speed will be given by finding what value of f will make T a maximum, and there will be two solutions depending on whether E or C is constant. To simplify the solution make the following abbreviations:

$$l = L - \frac{1}{f^2 q} \qquad l_o = L - \frac{1}{b^2 q},$$

$$K = \frac{R}{l f} \qquad K_o = \frac{R_o}{l b} \qquad m^2 = \frac{M^2}{l L'} \qquad K'_o = K' = \frac{R'}{b L'},$$

then

$$A^2 + B^2 = b^2 f^2 l^2 L'^2 [(K K' - 1 + m^2)^2 + K^2 + K'^2]$$

$$I^2 = l^2 f^2 (K^2 + 1).$$

$$I_o^2 = l^2 f^2 (K_o^2 + 1).$$

$$T = \frac{2\pi}{\lambda} C^2 \frac{M^2}{l} \frac{K}{K^2 + 1}$$

$$= \frac{2\pi}{\lambda} E^2 \frac{K m^2}{b^2 L' [(K K' - 1 + m^2)^2 + K^2 + K'^2]}.$$

$$W \text{(work)} = C^2 \frac{K M^2 (b - f)}{l (K^2 + 1)}$$

$$= E^2 \frac{K m^2}{L' [(K K' - 1 + m^2)^2 + K^2 + K'^2]} \frac{b - f}{b^2}.$$

For the three-phase the same solution, except $m^2 = \dfrac{9 M^2}{4 l L'}$, the torque reduces to this form

$$\frac{27}{8}\frac{2\pi}{\lambda}C^2\frac{M^2}{l}\frac{K}{K^2+1}$$

$$=\frac{27}{8}\frac{2\pi}{\lambda}E^2\frac{K\,m^2}{b^2\,L'\,[(K\,K'-1+m^2)^2+K^2+K'^2]}.$$

It is evident from the similarity of the formulæ that the two and three phase motors have the same properties and differ only in the constants, hence what follows will apply to both kinds of motors.

CONSTANT CURRENTS.

T is a maximum when

$$f^2=\frac{R^2}{2\,L^2}-\frac{1}{L\,q}\pm\sqrt{\frac{3}{L^2\,q^2}+\left(\frac{1}{L\,q}-\frac{R^2}{2\,L^2}\right)^2}.$$

If there is no condenser we have at once the condition for maximum torque

$$R=f\,L.$$

Consequently the armature resistance must be adjusted till the condition is fulfilled to obtain the maximum torque. The maximum torque can be obtained at starting by a proper resistance, as at starting $f=b$. The armature velocity for maximum torque in terms of the rotating field velocity is:

$$V\left(1-\frac{1}{b}\sqrt{\frac{R^2}{2\,L^2}-\frac{1}{L\,q}\pm\sqrt{\frac{1}{R\,q^2}-\frac{1}{L\,q}-\frac{R^2}{2\,L^2}}}\right)^2$$

When $b\,L-R=0$ the torque is a maximum at the start and will decrease rapidly as the speed increases, likewise when $b\,L<R$ the torque at the start is greatest and will decrease rapidly as the speed goes up. Such motors will start well, will have a low efficiency and will regulate poorly as the decrease of torque will increase if speed is too slow. When $b\,L>R$ the point of maximum torque comes near the point of maximum speed, giving good regulation but they will have poor starting qualities unless resistance is introduced at the start and then cut out.

The maximum value of the torque when $f\,L=R$ is

$$\frac{2\pi}{\lambda}\frac{C^2\,M^2}{2\,L}$$

for the two, and

$$\frac{27}{8}\frac{2\pi}{\lambda}\frac{C^2\,M^2}{2\,L}$$

for the three-phase motors which is independent of the speed, and depends only on the proper adjustment of the armature resistance.

The equations of torque with no condenser reduce to

$$T = \frac{2\pi}{\lambda} C^2 \frac{M^2}{l} \frac{K}{K^2 + 1}$$

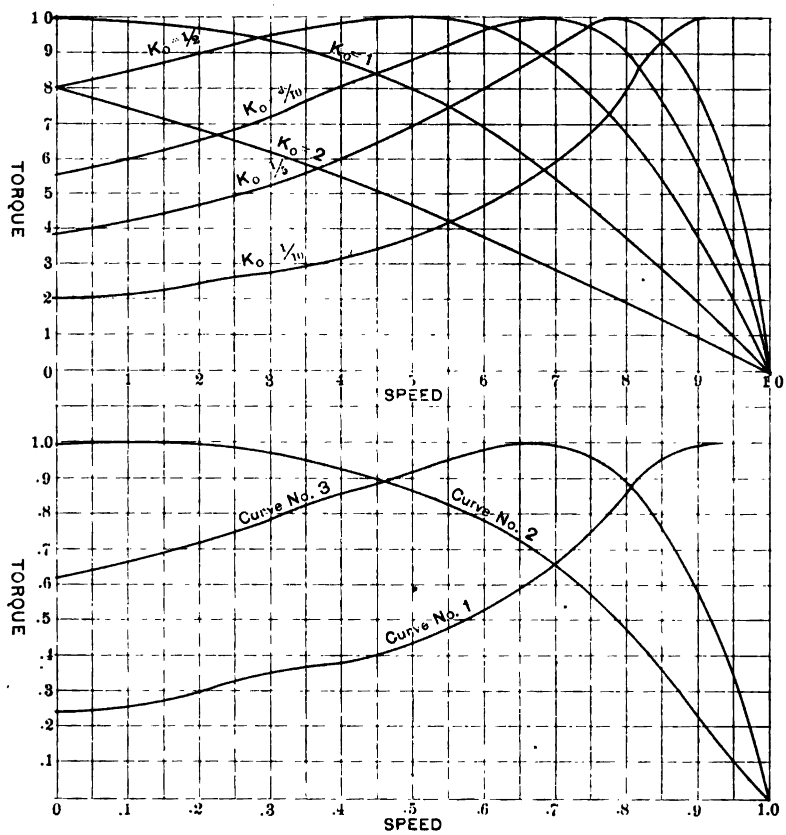


FIG. 1.

for the two-phase, and

$$T = \frac{27}{8} \frac{2\pi}{\lambda} C^2 \frac{M^2}{l} \frac{K}{K^2 + 1}$$

for the three-phase. The variable part of which is $\frac{K}{K^2 + 1}$ which may be put in the form

$$\frac{K_0\left(1-\frac{v}{V}\right)}{K_0^2+\left(1-\frac{v}{V}\right)^2}=\frac{1}{\frac{K_0}{1-\frac{v}{V}}+\frac{1-\frac{v}{V}}{K_0}}.$$

Representing the value of V by unity, and expressing the torque in terms of the maximum as unity we have the following table:

TABLE I.

$\frac{v}{V}$	$K_0 = 1$ $T.$	$K_0 = \frac{1}{2}$ $T.$	$K_0 = \frac{3}{10}$ $T.$	$K_0 = \frac{1}{5}$ $T.$	$K_0 = \frac{1}{10}$ $T.$	$K_0 = 2$ $T.$
0.0	1.00	.80	.55	.38	.20	.80
.1	.99	.85	.60	.42	.22	.75
.2	.98	.90	.66	.47	.25	.69
.3	.94	.95	.72	.53	.28	.62
.4	.88	.98	.80	.60	.32	.55
.5	.80	1.0	.88	.69	.38	.47
.6	.69	.98	.96	.80	.47	.38
.7	.55	.88	1.0	.92	.60	.29
.8	.38	.69	.92	1.0	.80	.20
.9	.20	.38	.60	.80	1.0	.10
1.0	.00	.00	.00	.0	.00	.00

Taking the safe working torque at .5 the maximum torque, the value of $\frac{v}{V}$ will be for the various values of K_0 as follows:

$$K_0 = 2, \frac{v}{V} = .48;\ K_0 = 1, \frac{v}{V} = .73;\ K_0 = \frac{1}{2}, \frac{v}{V} = .87;$$

$$K_0 = \frac{3}{10}, \frac{v}{V} = .92;\ K_0 = \frac{1}{5}, \frac{v}{V} = .94;\ K_0 = \frac{1}{10}, \frac{v}{V} = .97.$$

The ratio $\frac{v}{V}$ likewise gives the armature efficiency and consequently K_0 should be as small as possible, and ought not to be greater than about $\frac{3}{10}$, even in small motors. The diagram shows the necessity of a starting resistance in high efficiency motors and the way they regulate. Fig. 2 shows some curves of three-phase motors built by Steinmetz, and described by him in the March TRANSACTIONS of this year.

If the condition of maximum torque is satisfied, the ratio of starting to working torque with constant current does not depend on the frequency, but only on the working out-put which can only be increased by change of size and not design.

If the motor is forced beyond the point of maximum torque it is not only liable to stop, but to make the starting torque by comparison smaller. For a given current the maximum torque and work depend on the ratio of $\frac{M^2}{L}$. If there is no magnetic

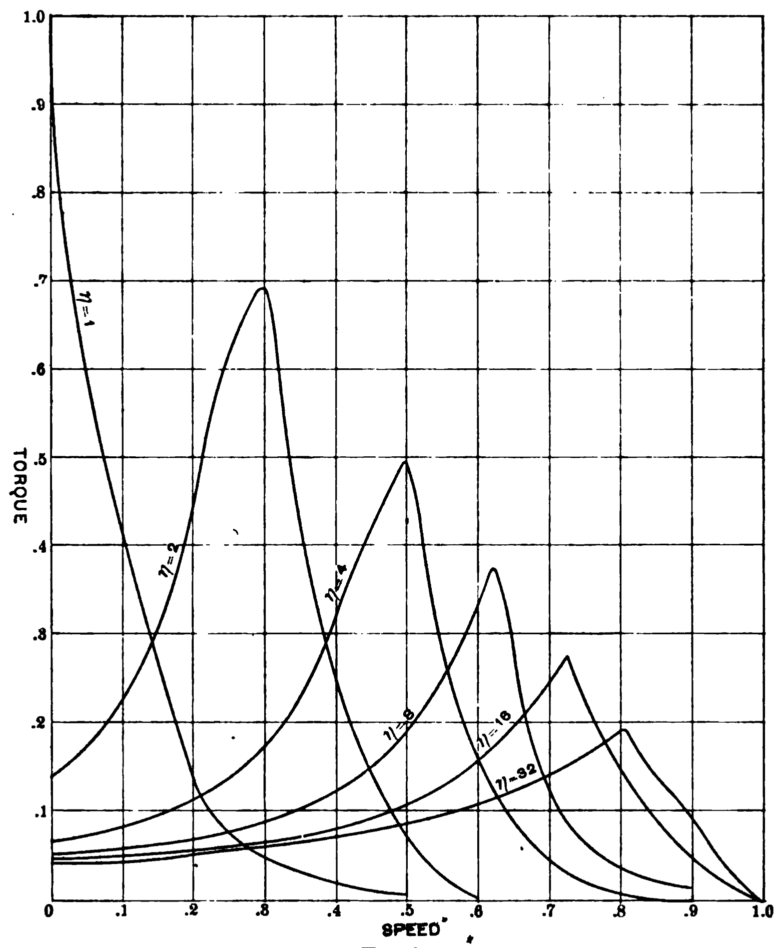


FIG. 2.

leakage $M^2 = L\,L''$, L'' being that part of the field self-induction interior to the machine, L therefore cannot be diminished without decreasing the output or increasing L'' at the same time. If the field be increased, the same output can be obtained with a smaller value of L, but this increases the electromotive-force and does not change the properties, as these depend on the ratio K, and not on the self-induction and resistance alone. If there is leakage as there always is, $M^2 < L\,L'$ which reduces the output.

Condenser in Armature Circuit.

The ratio of the torque of a motor with a condenser in the armature to one without, is

$$\frac{R^2 + L^2 f^2}{R^2 + f^2\left(L - \frac{1}{f\,q}\right)^2}$$

whose maximum value is $1 + \frac{1}{K^2}$. This maximum by proper adjustment of the condenser can be thrown at any value of the armature velocity. The value of f which makes the above ratio a maximum is $f^2 = \frac{1}{2\,L\,q}\,(1 + \sqrt{2\,R^2\,L^2\,q + L^2})$, hence by

TABLE II.

$\frac{v}{V}$	$n = 1.$	$n = 2.$	$n = 4.$	$K_0 = \frac{1}{8}.$ $n = 8.$	$n = 16.$	$n = 32.$
0.0	10.00	1.37	.66	.49	.43	.40
.1	4.21	2.25	.83	.57	.49	.41
.2	1.32	4.54	1.15	.70	.57	.52
.3	.48	6.95	1.77	.90	.67	.59
.4	.20	2.48	3.24	1.23	.83	.69
.5	.09	.70	5.06	1.95	1.10	.88
.6	.04	.21	1.76	3.36	1.61	1.11
.7	.01	.05	.47	2.24	2.47	1.54
.8	.003	.01	.07	.36	1.52	1.91
.9	.0004	.006	.03	.12	.46	.92
1.0	.000	.000	.00	.000	.00	.000

adjusting the value of q, f can be made anything we please.

Calling the maximum starting torque without a condenser l, the starting torque with a condenser is

$$\frac{2\,b\,L\,R}{R^2 + f^2\left(L - \frac{1}{f^2\,q}\right)^2}$$

times as great, and will be a maximum when $L = \frac{1}{f^2\,q}$ which reduces the factor to $\frac{2}{K_0}$. This can have a large value if the resistance of the armature is small. Thus we see that the starting torque can be increased without changing the resistance, and this increase of torque is obtained, not by increasing the current in the field circuits, but only in the armature. When the condition $L = \frac{1}{b^2 q}$ is satisfied, the torque at any velocity with a condenser is using the same unit as before

$$\frac{2K}{K^2 + \left(1 - \frac{b^2}{f^2}\right)^2},$$

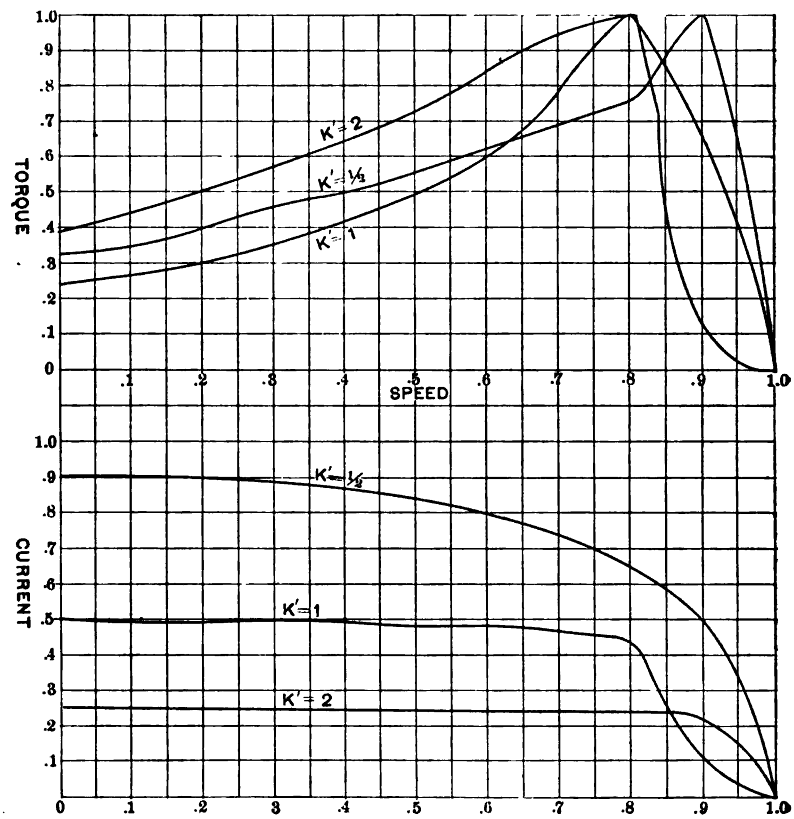


FIG. 8.

where

$$K = K_0 \frac{v}{V - v},$$

and

$$\frac{b^2}{f^2} = \frac{v^2}{(V - v)^2}.$$

To illustrate, let us take the case where $K_0 = \frac{1}{2}$, and compare it with the same case without a condenser, the general formula in writing for q, $\frac{n}{b^2 L}$ is

$$= \frac{2K}{K^2 + \left(1 - \frac{1}{n}\frac{b^2}{f^2}\right)^2}.$$

Table II. is calculated by this formula.

The curves show that the condenser has only a local effect on the torque and a small change in velocity renders it useless, though this defect could be overcome by a variable condenser. The advantage is that the motor can be run at a slow speed with increased torque without overheating. As the field current has

TABLE III.

$\frac{v}{V}$	CURRENT.			TORQUE.		
	$K' = \frac{1}{2}$.	$K' = 1$.	$K' = 2$.	$K' = \frac{1}{2}$.	$K' = 1$.	$K' = 2$.
0.0	1.81	1.00	.50	.33	.24	.39
.1	1.80	.99	.50	.35	.26	.45
.2	1.78	.99	.50	.40	.31	.51
.3	1.76	.99	.50	.46	.36	.58
.4	1.73	.98	.50	.50	.41	.65
.5	1.64	.97	.50	.56	.50	.74
.6	1.58	.97	.50	.60	.60	.85
.7	1.41	.94	.49	.65	.79	.94
.8	1.22	.88	.48	.75	1.00	1.00
.9	1.00	.22	.44	1.00	.10	.66
1.0	0.00	.00	.00	.00	.00	.00

been supposed to be constant there is no extra heating of the field circuits; this will require special means for keeping the field current constant. By use of a variable condenser the speed can be made to increase to any extent up to synchronism and there is no overheating at low speeds, or waste of power by the insertion of resistance to increase the torque. A variation of the capacity from one to four, changes the armature speed from zero to one-half of the rotating field velocity and an increase of condenser capacity to 25 varies the armature to 80 per cent. of synchronism at the point of maximum torque.

Constant E. M. F.

Neglecting the change of the magnetic properties of the iron at high magnetization, the torque and work of the motor vary at the square of the field current. With constant E. M. F., when the motor is at rest or just starting, the current is very large and if there are other motors or lamps in the exterior circuit, they are rendered unsteady at the moment of starting of the first motor, this however may be corrected by a secondary transformer. The formulæ for the current and torque for E constant are

$$C = \frac{E}{b\,L'}\sqrt{\frac{K^2+1}{(K\,K'-1+m^2)^2+K^2+K'^2}}.$$

$$T = E^2\,\frac{2\,\pi}{\lambda}\,\frac{1}{b^2\,L'^2}\,\frac{K\,m^2}{(K\,K'-1+m^2)^2+K^2+K'^2}$$

for the two-phase, and

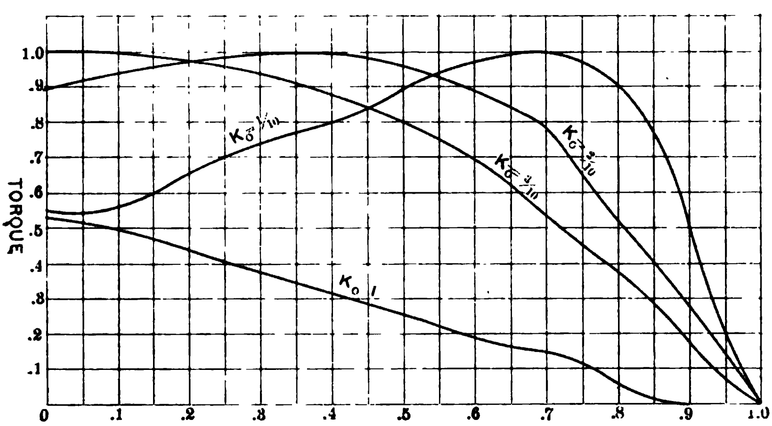


FIG. 4.

$$T = \frac{27}{8}\,E^2\,\frac{2\,\pi}{\lambda}\,\frac{1}{b^2\,L'^2}\,\frac{K^2+1}{(K\,K'-1+m^2)^2+K^2+K'^2}$$

for the three-phase.

To keep down the heating of the fields, the field resistance must be small, hence K' must be small compared with unity. When the self-induction external to the machine in the field circuit and the magnetic leakage are small, m^2 is very nearly equal to unity. There is always some self-induction in the dynamo or transformer so that m^2 is rarely less than $\frac{1}{2}$, for a transformer equal in self-induction to the motor $m^2 = \frac{1}{2}$. If there is no condenser, the torque is a maximum for $K = \frac{K'^2 + (m^2-1)^2}{K'^2+1}$, hence K

must vary from 0 to $\frac{1}{2}$, which gives R varying from 0 to $\frac{1}{2} f L$. as the condition of maximum torque. The added starting resistance must be less than in the case of constant current.

Table III. and curves, show the variation of current in the field circuits and the torque when the field resistance is changed, but $K_0 = \frac{1}{5}$ for the armature: m_2 is taken at .75 the torque is expressed in terms of the maximum as unity.

To show the effect of changing the armature resistance I have taken the case where $K' = .2$ $m^2 = .75$ and calculated for $K_0 = \frac{1}{10}, \frac{2}{10}, \frac{3}{10}$ and 1. The value of $K_0 = \frac{3}{10}$ gives the maximum at the start (see Table IV. and curves).

We may conclude that the two and three-phase motors have in general the same properties, and that the most important relation is that of R to $f L$. The lower the field and armature resistance are, the higher the efficiency of the armature and the nearer to synchronism is the point of maximum torque. We see that high efficiency motors require a starting resistance, and that in such motors the torque can be readily regulated by adjustment of the armature resistance and we are enabled to throw the point of maximum torque at any speed we desire. It is likewise apparent that the smaller the magnetic leakage the more efficient the motor.

TABLE IV.

$\frac{v}{V}$	$K_0 = \frac{1}{10}$.	$K_0 = \frac{2}{10}$	$K_0 = \frac{3}{10}$.	$K_0 = 1$.
.0	.55	.89	1.00	.53
.1	.56	.94	.99	.50
.2	.65	.97	.98	.54
.3	.74	.99	.95	.38
.4	.80	1.00	.88	.32
.5	.89	.95	.80	.16
.6	.97	.89	.70	.22
.7	1.00	.74	.54	.18
.8	.91	.53	.37	06
.9	.53	.28	.17	.008
1.0	.00	.00	.00	.000

If the frequency is supposed to vary, the speed and output are greatly changed. K_0 varies inversely as the frequency, so the motor is improved for higher frequencies as far as this relation effects it. Increasing the frequency n times in a motor without a condenser, the velocity will be increased n times, K_0 decreased n times, the maximum torque is not affected, while the hysteresis is increased n times nearly, the output of the motor will be increased nearly n times if the motor is run at a given percentage of the maximum torque. Hence it is an advantage to increase the frequency till the hysteresis heating becomes too great, or the motor runs too fast. If the efficiency is to be kept constant, the work will then vary as n^2 and the field and armature currents will be increased $\sqrt{n}$ times. Hence increase in speed gives an advantage in output till the machines begins to overheat. With a condenser the same facts hold, but with this advantage that an increase in the frequency greatly reduces the size of condenser necessary. A high frequency motor will weigh less than a low frequency one. When the clearance is large, the advantage of high frequency motors is not so decided, and if very large the low frequency motors are better if weight is no consideration. The higher frequency motor will contain less iron and more copper. If we vary the size of the motor, keeping the magnetization constant, output and hysteresis vary directly as the weight; the heating of the field and armature circuits, directly as the increase of size.

DISCUSSION.

DR. M. I. PUPIN:—Mr. President, I read with much pleasure this very interesting paper of Lieutenant Reber, and, in the absence of the writer himself, I venture to present his case by a few remarks which are intended as an introduction into the discussion of the subject matter of this paper. I hope that the discussion itself will be taken up by others who have a better practical knowledge of the subject than I have.

One thing which struck me as particulary meritorious in this paper is the method by which the proposed problem is treated. The author states clearly the problem before him, namely, the theory of the two and three-phase induction motors, and the conditions for which he proposes to discuss the solution of the problem. He then proceeds from the very fountain-head of the solution of all problems of this class, namely, from Maxwell's equations, which are given on page 733, under (1) and (2). A rather interesting aspect of Maxwell's equations (1) and (2) presents itself to my mind, and with your kind permission I shall mention it. They correspond to Newton's second law of motion in mechanics. This law states, as is well-known, that the rate at which the momentum of a moving body varies is equal to the effective force. Similarly, so Maxwell's equations tell us, that at which the electro-kinetic momentum of an electrical circuit varies, is equal to the effective electromotive force. The analogy is complete. One of the great merits of Maxwell's method of presentation is the strong emphasis of the analogy between the fundamental relations which underly the phenomena of electrical current and those of mechanical motion.

The electro-kinetic momentum of an electric circuit must, of course, be calculated first. This is done on page 732. There we have p, the electro-kinetic momentum of one of the armature circuits, and p_1 the electro-kinetic momentum of one of the field circuits. The electro-kinetic momentum is simply the total number of interlinkages of the magnetic lines of induction which the circuit has. The rate, then, at which these interlinkages vary, is equal to the impressed electromotive force, minus the passive resisting force due to ohmic resistance. This part of Lieut. Reber's paper, though it occupies two pages only, is by far the most difficult, and the difficulties have been overcome by a method of the Maxwellian school. The remaining part contains the working out of the several details of the solution, and is necessarily moulded with due deference to practical considerations.

These considerations contain questions referring to the power, the efficiency, and the torque. These are the three most important elements in our study of the working of a motor, and to these Lieut. Reber devotes his attention in the remaining part of his paper.

Now, to calculate the power we must know—in alternating currents—not only the current and the electromotive force current, but also the difference in phase between them. The impressed electromotive forces being given, we have to calculate from Maxwell's equations (1) and (2) the currents and the differences in the phase between the currents and the impressed electromotive forces. Lieut. Reber simplified the problem in the following way: he eliminated hysteresis, Foucault currents and magnetic leakage. That allowed him then to express the steady state of the currents by the same mathematical functions as the impressed electromotive force; that is simple harmonics given on p. 732.

Next comes the calculation of the difference of phases between the currents and the impressed electromotive forces, which is done from equation (3) to (24). The power supplied to the motor is there calculated, and subtracting from this the value of heat losses gives the power transformed into mechanical power, which divided by the armature speed gives the torque, p. 736. Mechanical power obtained divided by electrical power supplied, gives the efficiency (p. 737).

The torque of an induction motor, and, for that matter, of any other motor, gives the practical engineer more anxiety than any other element. He is quite willing to sacrifice a considerable part of even the efficiency for the sake of the torque. Lieut. Reber does well to devote a very large portion of his paper to this side of the question. His method of investigating this point is very clear indeed. It is *a priori* evident that in an induction motor the torque at any moment will depend on the respective values of the currents in the armature and in the field, and also on the differences in the phases between them, that is practically on the phase of the armature current, hence, if we are to vary the torque for a given armature speed, we can do it by varying the phase of the armature current. That, it seems to me, is self-evident and needs no mathematical discussion.

Now there are only three ways by means of which we can vary the phase of the armature current; that is, the frequency of the armature current being given, we can vary the phase by varying one of the three fundamental quantities of the armature circuit, that is, capacity, self-induction, or resistance. A variation of the self-induction is out of the question as a simple consideration will show. Then there are left their resistance and capacity. The torque therefore, of an induction motor can be varied at any given speed, by varying the resistance of the armature or the electrostatic capacity. The phase depends on the ratio between the reactance and the resistance. Hence, if we choose to leave the reactance constant, the only thing to vary then is the resistance. If however, the armature circuit contains a condenser, then the reactance may be varied without varying the self-induction or the resistance. This ratio of reactance to resistance, must have a perfectly definite value if we are to have a maximum torque for a given speed. It is this ratio which the author calls K, and he points out very distinctly indeed that this quantity is one of the most important ones to consider, at any rate looking upon the motor in the light in which he proposed to regard it. He expresses then the power and the torque in terms of this quantity *K*, so that in order to bring out the conditions under which a given motor at a given speed will run with a given torque or give a certain power, he simply has to consider the variation of the torque with respect to this one variable *K*.

Two particular cases are considered. A constant current induction motor with one of two devices in it. A device for varying the resistance of the armature, and another for varying the electrostatic capacity, that is, a condenser in series with the armature.

The relation of reaction to resistance which will give the maximum torque at a given speed in a constant current motor is given on p. 738. If I understand correctly Dr. Bell's paper on induction motors, this resistance-varying device has been found practically to work quite satisfactorily. It is very curious to observe here that about nine or ten years ago, when Dr. John Hopkinson revived the question of running alternators in parallel, his theoretical deductions suggested that the best design for alternators intended to run in parallel will make the resistance of the alternators equal to the reactance, exactly the same relation which gives the maximum torque in induction motors. The theory of the induction motor throws a new light on the other question, that is to say, an alternator will run best in the parallel, at a given frequency when it exerts the maximum torque for a given difference of potential. The maximum torque is obtained with a condenser in the armature circuit when the armature circuit and the induced electromotive force are in resonance. The case of constant potential motors is then discussed in the same manner, and the results arrived at are similar to those just mentioned.

The whole theory here presented is, as I said, extremely simple. A great deal has been written upon this subject, especially by Professor Arnold, of Carlsruhe, who, I think, has published a book on the subject lately. But I must say the way in which Lieut. Reber has treated the question seems to me to be much simpler than anything that has been done so far. The theory which he gives us ought to prove of great practical utility by reason of its simplicity. Of course, the whole problem is dealt with for ideal conditions only. That is, there is no magnetic leakage, there is no hysteresis, there are no Foucault currents; so that all the numerical results obtainable from the author's theory will be modified to a greater or less extent by these three ever-present imperfections, and will have to be allowed for by the designer.

Of course a great many people will say, what is the use of a theory which simply treats of an ideal case that never presents itself in practice? They said the same thing when the theory of the alternating current transformer was brought forward to the notice of practical engineers. They called it the "phantom theory" of a "phantom transformer," implying often by that name that the theory had little or no practical value. And yet somehow or other, when the relations, obtained for purely ideal conditions, were compared with actual experimental tests, it was found that they agreed pretty well with them. Of course those men who had always had a grudge against "iron-less" mathematics and against the so-called text-book mathematicians and text-book physics, and against all electrical discussions which did not smell of the shop, when they found that the phantom equations agreed well with the practical results, were quite surprised and did not understand the "miracle." For instance, I do not know that I ought to touch upon this much discussed point: some people had a great deal of mistrust against the supposition that impressed E. M. F., produced by most alternators can be treated as a pure sine. It was not a pure sine, and therefore all results obtained on the hypothesis just mentioned were of no use. The curve was not exactly a pure sine, but quite often nearly so. Practical results, however, agreed with the theory, because a distorted electromotive force utilizes its fundamental component only when it works an iron core closed magnetic circuit transformer, and when the transformer load is not too small. This is especially true of old frequencies like the Westinghouse frequency. There is something new; something that we ought to inquire into, and inquiring, we shall undoubtedly find, new properties of an iron core closed magnetic circuit.[1] So in the case of this ideal theory of the induction motor, although its results may differ considerably from the actual facts, it can be safely accepted as a first approximation to actual truth as a first guide. Then, if there should be differences between the ideal and the practical conditions, as there certainly will be, we will be able to measure these differences, to inquire into the intensity of the causes producing them, and in that way obtain more definite knowledge of all the activities which are concerned in this particular machine, the induction motor.

MR. STEINMETZ:—I have read Lieut Reber's valuable paper and was very much interested in it, and when in the following I shall take rather a critical view of it, I beg you to understand that I do not depreciate the value and importance of the paper, but after Dr. Pupin has shown us the good features of the paper, I shall now endeavor to designate its weak points.

First, with regard to the neglect of hysteresis in the paper, I may say that in induction motors, and especially smaller ones, the hysteretic loss is one of the largest of all the losses, and thus should not be neglected in a complete theory.

1. For fuller information see M. I. Pupin: "Resonance Analysis of Alternating Currents, etc.", TRANSACTIONS, p. 528.

I may further draw the attention to the raggedness of the curves given in the paper, and state that this is not inherent to the theory, but merely due to the fact that the curves were plotted from tables calculated to two decimals only. The neglect of the third decimal becomes noticeable in the curve.

On page 737, it is stated that the motor should not be pushed to a point more than half the maximum torque. This general statement I consider wrong. Let us rather say that at full load the motor must have sufficient margin of torque to carry it over any overload, which it may possibly be exposed to in practical operation. How much this is, greatly depends upon the circumstances; in a motor driving a ventilating fan, it is entirely useless to allow such a large margin, since such a motor by the nature of its load cannot be overloaded. On the other hand, a railway motor allowing 100 per cent. margin over its rated torque could never work satisfactorily, but 500 or even 1000 per cent. margin would come nearer the requirements. In general, in most cases of practical application, 100 per cent. margin is too much and 50 per cent. quite enough. The same applies to the starting torque. Thus I have all the standard induction motors designed to give a starting torque and maximum torque 50 to 60 per cent. in excess of the running torque, while wherever more or less is desired, the motor is considered as special.

With regard to the effect of the frequency, I would like to discuss somewhat more a statement on the last page. As it is worded there, though correct, it may be greatly misleading. It is: "Increasing the frequency n times in a motor without a con-"denser, the velocity will be increased n times, K_0 decreased n "times, the maximum torque is not affected, while the hysteresis is "increased n times nearly, the output of the motor will be in-"creased nearly n times, if the motor is run at a given percentage "of the maximum torque. Hence it is an advantage to increase "the frequency till the hysteresis heating becomes too great or the "motor runs too fast." And then it is stated further along, "A "high frequency motor will weigh less than a low frequency one." This is not the case. Instead of "a high frequency motor," it should be said "a high speed motor." The quantity which is decided upon and determined first in a motor, is its *speed*. If now you take a motor running first at one frequency and then run it at twice the frequency but the same speed, by doubling the number of poles, you find in general, the opposite true. Then the high frequency motor is less favorable. So in a motor with a given number of poles, the gain by increasing the frequency is merely due to the increase of speed, and if a motor is improved by doubling the frequency and doubling the speed, it is still more improved by doubling the speed but retaining the same frequency, by halving the number of poles.

The main objection, however, which I have against the paper is that it is *by far too mathematical.* Mathematics is a very nice thing indeed, and the most powerful weapon science has, and having done a little work myself in mathematics, pure and applied, I may be allowed to say that pure mathematics may go as high as desired in purely scientific interests, but if you apply mathematics to an engineering problem, then it is proper to ask; what is the use of the results? or, can we make any application of it for engineering practice?

There are two ways of solving an engineering problem by theory; either by forcing a solution by bringing the whole power of analysis to bear upon it, or to arrive at the solution by a simple way, by a short cut, we may say.

Now taking the present paper: In the equations here, the quantity, K, appears several times. This quantity K is defined on page 737. It is a quotient whose terms are differences of quotients. so you see that the expression, if you substitute backwards to the orig nal constants, will be so complicated that it is hardly possible to make any use of it.

And now coming back to this quantity, L, the coefficient of self-induction. Even this is not a simple quantity, because it contains two entirely heterogeneous terms. L is here used in the sense used in former times in theoretical investigations, as referring to the number of interlinkages of the circuit with the flux produced by the circuit. In the primary circuit, the current will set up a magnetic field which will pass around the circuit with a given number of interlinkages, and the total product, for unit current, of the number of interlinkages into the number of turns is the coefficient of self-induction. Now this product has two heterogeneous effects; a part of the flux passes around the primary coil without passing around the secondary coil. This chokes off the electromotive force and reduces the output of the motor. The other part passes around the primary coil and around the secondary coil, and does the work. You see, consequently, that no conclusions can be drawn from the quantity, L, because it contains two terms, one representing the work done, and the other choking or reducing the work. Therefore, where the quantity, L, enters, you cannot separate the useful part. You cannot draw any conclusions how to design a motor from the equations where L enters.

DR. PUPIN:—There is no leakage, and, therefore, you are stepping beyond the limits of the paper.

MR. STEINMETZ:—Then I take back what I said beyond the limits, but keep to my first objection that the method is too theoretical and too limited.

It assumes no magnetic leakage. Now the maximum output of an induction motor is essentially determined by the magnetic leakage or self-induction, while resistance plays a lesser part. Conseqently by neglecting the magnetic leakage, the paper neglects the principal factor, which determines the maximum output of the motor.

However, I do not know whether the magnetic leakage is really excluded in the paper, because—

DR. PUPIN:—If you will look at the beginning of the paper, you will find that it so states in the sixth line.

MR. STEINMETZ:—If there is no magnetic leakage the product of the two L's is equal to the M^2. I think I saw a statement here that it may be equal or larger. In this case this objection does not hold.

However, by separating the L in these two parts, representing the useful flux and the flux of self-induction, or rather in this case, I would say by introducing a second quantity, L^1, as the magnetic leakage, you can make the equation more complete.

Now, I have made the statement that the paper appears to me too mathematical. The induction motor is such a simple apparatus, just as simple as the alternating current transformer, that it should be assumed *a priori* that the theory of the induction motor should be much simpler. Moreover, I think the theory of the induction motor ought to include the hysteresis, and ought to include the stray field of magnetic leakage, because the hysteresis is the largest factor in efficiency, and the leakage is the limiting factor in the output. The theory ought to make no assumption as to how many phases there are, and if you will allow me, I will make a few sketches explaining that. I do not intend to make use of anything more mathematical than Ohm's law, and the equation that the power is the electromotive force times current. But I will use complex quantities, which, I assume, are now generally known.

THEORY OF THE INDUCTION MOTOR.

In the following sketch of a theory of the induction motor, no further assumptions shall be made but Ohm's law and the power equation:

$$P = E\,C.$$

The theory will be general, that is, independent of the number of phases in field and in armature, etc., and shall include, and not neglect, the effect of hysteresis, magnetizing current, of self-induction or magnetic stray field, etc.

The method of complex quantities shall be used, however, which, I presume, can be considered now as universally known and understood.

Capitals shall denote complex or vector quantities, small letters represent absolute values.

Let, in an induction motor,

Y = primary admittance, per circuit,[1]

1. $Y = \rho + j\,\sigma$ is the ratio of total exciting current, or current at no load or synchronism, to the E. M. F., ρ its power component, σ its reactive component. Thus:

U = primary impedance,[1]
$U_1 = r_1 - j\,k\,s_1$ = secondary impedance,
where:
k = slip, or ratio of secondary frequency to main frequency,[2]
a = ratio of secondary turns to primary turns (ratio of transformation).

Since the reactive component of secondary impedance depends upon the slip, k, the two components of secondary impedance have been introduced:

$$U_1 = r_1 - j\,k\,s_1,$$

where:
$s_1 = 2\,\pi\,N\,L_1$ = secondary reactance at rest, or full frequency.

Counting the time from the moment where the mutual magnetic flux, or useful magnetism (flux interlinked with both, primary and secondary circuit) passes through zero, the magnetic flux is denoted by:

$$M = j\,m.$$

Thus the E. M. F. induced by this flux in the primary circuit is:

$$E = -\,e,$$

where e is the "counter E. M. F. of the motor."

The E. M. F. induced in the secondary circuit is:

$$E_1 = -\,a\,k\,e,$$

since the secondary circuit cuts the same flux, m, at the lower frequency, $k\,N$.

Hence the secondary current is:

$$C_1 = \frac{E_1}{U_1} = -\,\frac{a\,k\,e}{r_1 - j\,k\,s_1}.$$

$$\rho = \frac{\text{hysteretic energy current}}{\text{E. M. F.}},$$

$$\sigma = \frac{\text{magnetizing current}}{\text{E. M. F.}},$$

$$v = \sqrt{\rho^2 + \sigma^2} = \frac{\text{total exciting current}}{\text{E. M. F.}},$$

ρ and σ are calculated from dimensions and material of the motor, and determined experimentally by ammeter and wattmeter readings at no load.

1. $U = r - j\,s$, where r = (equivalent) resistance, $s = 2\,\pi\,N\,L$ = reactance of primary circuit, the latter referring, however, to the magnetic flux of self-induction, or stray magnetism, only, that is the flux produced by the primary circuit and interlinked with the primary circuit only, but not with the secondary circuit. Thus s does not include the mutual inductance, or the useful magnetism, M. The same applies to U_1.

r and s are calculated from the dimensions of the motor and observed experimentally by ammeter and wattmeter readings at standstill with short-circuited armature.

2. $k = 0$ at synchronism,
$k = 1$ at standstill,
$k\,N$ = frequency of armature current

Hereto corresponds in the primary circuit the current:

$$C_1^1 = - a\ C_1$$
$$= \frac{a^2\ k\ e}{r_1 - j\ k\ s_1}.$$

The primary exciting current is:

$$C_0 = Y\ e.$$

Hence the total primary current:

$$C = C_0 + C_1^1;$$

or, substituted:

Primary Current:

$$C = e\left\{ Y + \frac{a^2\ k}{r_1 - j\ k\ s_1}\right\}. \qquad (1)$$

The E. M. F. consumed by the primary impedance is:

$$E^1 = C\ U$$
$$= e\ U\left\{ Y + \frac{a^2\ k}{r_1 - j\ k\ s_1}\right\}.$$

Thus the total primary impressed E. M. F. is:

$$E_0 = E_1 + e;$$

or, substituted:

Primary Impressed E. M. F.:

$$E_0 = e\left\{1 + U\ Y + \frac{a^2\ k\ U}{r_1 - j\ k\ s_1}\right\}. \qquad (2)$$

The power transmitted into the secondary circuit is:

$$E\ C_1^1 = e\ a\ C_1.$$

The power wasted by the secondary current is:

$$E_1\ C_1 = k\ e\ a\ C_1$$

hence, the output, or the net power of the motor is:

$$P = E\ C_1^1 - E_1\ C_1$$
$$= e\ a\ C_1\ (1 - k);$$

or, substituted:

$$P = \frac{e^2\ a^2\ k\ (1 - k)}{r_1 - j\ k\ s_1},$$

and rationalized:

Power:

$$P = \frac{e^2\ a^2\ k\ (1 - k)\ r_1}{r_1^2 + k^2\ s_1^2}. \qquad (3)$$

The torque of the motor is:

$$T = \frac{b\,P}{1 - k},$$

where b is a constant depending upon frequency, N, and number of poles, p:

$$b = \frac{550}{.746\,\pi\,p\,N} \text{ in pounds at 1 foot radius.}$$

Substituting P gives:
Torque:

$$T = \frac{b\,e^2\,a^2\,k\,r_1}{r_1^2 + k^2\,s_1^2}. \qquad (4)$$

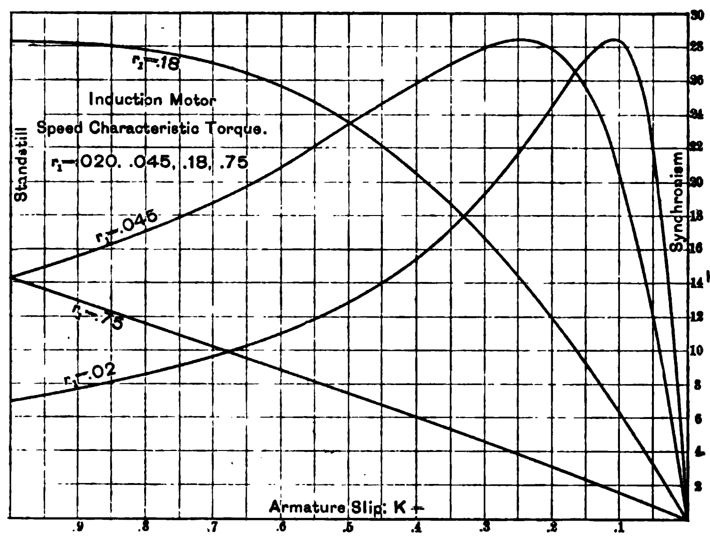


FIG. 5.

These four equations are the fundamental equations of the induction motor, giving current, impressed E. M. F., power and torque as functions of the slip, k, and the counter E. M. F., e.

Thus, at constant impressed E. M. F., E_0, we eliminate e by equation (2), and get from equations (1), (3), (4) the current, power and torque, as functions of the slip, k, that is the speed, and eliminating again k by equation (1), equations (3) and (4) give power and torque as functions of the primary current, at constant impressed E. M. F. In general, it is preferable, however, to retain k and e as parameters.

The armature resistance, r_1, appears explicit in the equations, so that the effect of a change of r_1 can directly be seen from the equations.

Substituting:

$$k = 0,$$

gives the condition of synchronous running, or of no load:

$$C = e\, Y$$
$$E_0 = e\,(1 + U\, Y),$$
$$P = 0,$$
$$T = 0.$$

Substituting:

$$k = 1$$

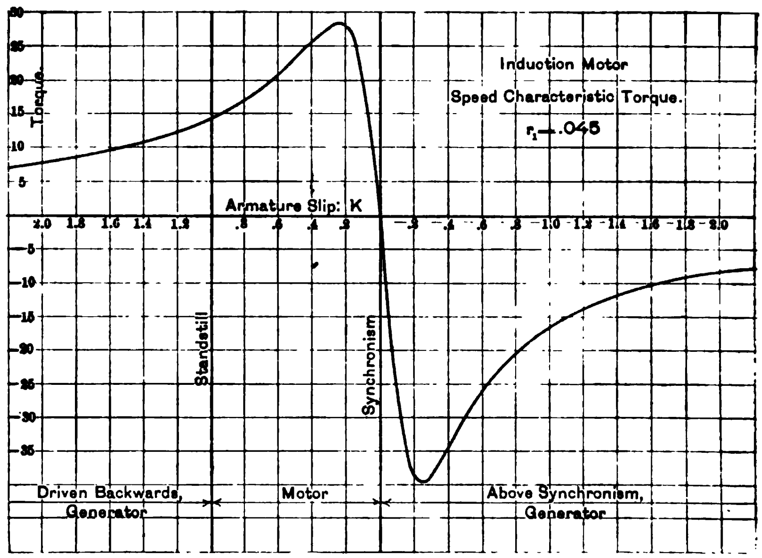


FIG. 6.

gives the conditions in starting, or at rest:

$$C = e\,\left\{ Y + \frac{a^2}{r_1 - j\, s_1} \right\}$$
$$E_0 = e\,\left\{ 1 + U\, Y + \frac{U\, a^2}{r_1 - j\, s_1} \right\}$$
$$P = 0$$
$$T_0 = \frac{b\, e^2\, a^2\, r_1}{r_1^2 + s_1^2}$$
$$\frac{d\, T_0}{d\, r_1} = 0,$$

gives the armature resistance, r_1, which gives maximum starting torque.

Substituting:

$$k < 0$$

gives the conditions of running above synchronism:

$$P < 0$$
$$T < 0;$$

that is, the machine gives a backwards torque and consumes power, that is, its primary circuit acts as electric generator.

$$k > 1$$

gives the conditions of backwards running:

$$P < 0$$
$$T > 0;$$

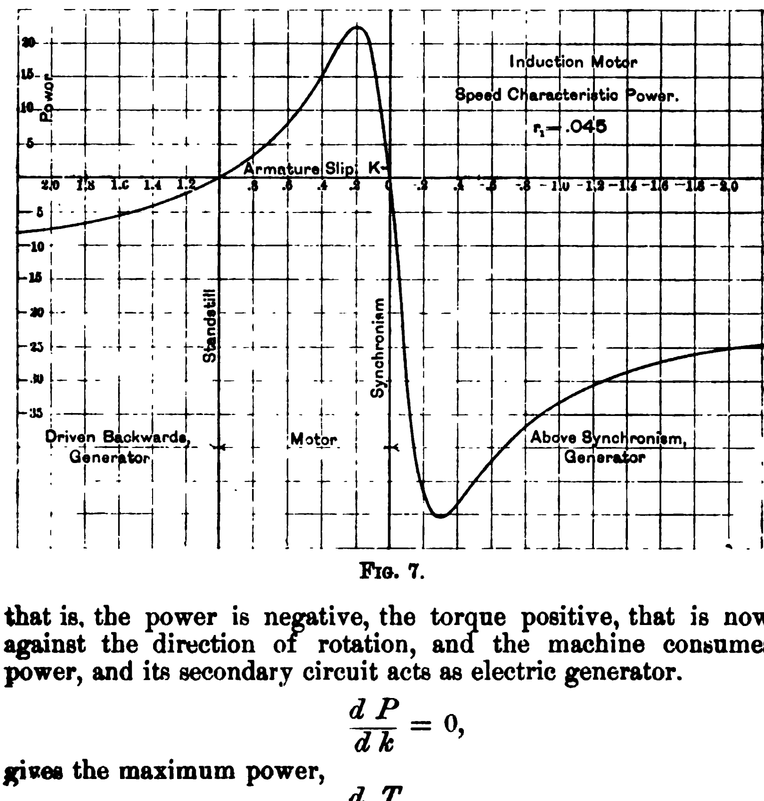


Fig. 7.

that is, the power is negative, the torque positive, that is now against the direction of rotation, and the machine consumes power, and its secondary circuit acts as electric generator.

$$\frac{d\,P}{d\,k} = 0,$$

gives the maximum power,

$$\frac{d\,T}{d\,k} = 0,$$

gives the maximum torque of the motor.

From the four fundamental equations the behavior of the induction motor under all conditions of load, etc., can be calculated and plotted in curves.

The values of equations (3) and (4) refer to one circuit only. Thus the total power (resp. torque) of the induction motor is $= 3\ P$ in a three-phase, $= 2\ P$ in a quarter-phase, and $= P$ in a monocyclic induction motor.

The general shape of such motor characteristics is shown in Figs. 5, 6, 7, which give the torque and the power of a three-phase motor, at various armature resistances: $r_1 = .02$; $.045$; $.18$; $.75$ ohms, as function of the speed, that is, the slip, k, with the constants:

$$
\begin{aligned}
Y &= \rho + j\,\sigma = .03 + .2\,j, \\
U &= r - j\,s = .03 - .1\,j, \\
U_1 &= r_1 - j\,k\,s_1 = r_1 - .08\,k\,j, \\
a &= 1,
\end{aligned}
$$

110 volts between the lines, hence

$$E_0 = 63.5.$$

There you have all the quantities. No assumptions are made whether it is a three-phase, single-phase, or any other induction motor. I do not make any assumption either how the armature is wound.

MR. A. E. KENNELLY:—Mr. President and Gentlemen: I think that after what Mr. Steinmetz has told us concerning the advisability of having the paper before us include the question of hysteresis and other losses, we ought to feel very grateful to the author for having left them out, because to what extent the paper would have run, had those more difficult questions been included, it is difficult to say. But I, for one, am opposed to other than engineering methods for treating engineering subjects. I believe that the method which has just been discussed by Mr. Steinmetz before us on the blackboard is far more capable of giving us the engineering properties of induction motors than are the methods based on Maxwell's equations. When we consider the work done in lifting a weight from the ground, it is not necessary to start with Newton's second law of motion. Similarly when we move a dynamo, we need not start with Maxwell's electro-kinetic momentum of a circuit. These things are true and beautiful, but we need not always be bringing them into print. The difficulties in the problem that confront the engineer are sufficiently great without adding to them analytical difficulties in dealing with the subject. I think the paper might have been shortened and its utility to engineers greatly enhanced if many of the difficulties which enshroud that subject had been cleared away and a simple exposition given to us.

Discussion at Chicago, Oct. 17th, 1894.

[Lieut. Samuel Rodman in the Chair.]

Prof. W. M. Stine:—We have this evening papers by Lieut. Samuel Reber and Mr. Charles Proteus Steinmetz, and I have been requested to read the papers, but presume you have had them before you long enough to become familiar with their contents. They are simply mathematical deductions for the most part—a crystallizing of some of the mathematical deductions and facts already known concerning this class of motors. The papers are of course valuable as mathematical contributions, and as forming a part of these Transactions, we must consider them as having considerable value. We will ask you to consider them as read, that is, to pass over the verbal reading of the papers and leave them in your hands for discussion.

It is a matter very much of this kind; we have here known factors in connection with the design and operation of these motors. There is no question involved directly as to the construction or operation of these motors, and we could, by reading the papers, point out the relations of torque on starting, and the relations involved in retaining sychronism, getting step and getting out of step, but prefer to leave that to the discussion. I have had quite a number of slides prepared illustrating the work of Tesla, and the work of S. P. Thompson, and in this way I think we can develop the theory of synchronism and magnetic rotation involved in these motors.

[Prof. Stine here used the slides in his discussion and explained same.]

Mr. H. J. Sage:—The papers under discussion have been in my hand so short a time, it will be impossible for me to intelligently discuss them. But in glancing over that of Lieut. Reber I notice in the opening sentences that his deductions refer to motors whose fields do not have projecting poles. It has been my experience that in general, motors with smooth field cores are the better. I am of the opinion that in such motors alone can be secured a uniform rotating field, which is most necessary for high efficiency in operation. With such a field core, a winding may be designed in which the magnetic fields, due to the separate circuits, shall combine to give a resultant uniform field rather than a sum total of the various fields, which, is not necessarily uniform. And it is easily proved mathematically in the case of triphase motors, that such a resultant field, in which the components combine according to the law of parallelogram of forces is uniform. It seems to me too much attention cannot be paid to the air-gap, and also the resistance of the field and armature circuits. The former should be as short as possible; the lower the latter, the more efficient the motor, and the nearer will the speed and maximum torque approach to synchronism. A simple variable resistance in the armature circuit is a convenient method of regulating speed and maximum torque, and also is much more economical than the resistance boxes necessary in continuous current practice. Then, too, in most cases an induction motor may be reversed under full speed and full load with impunity. The little care required by them when running, the ease of speed regulation, and the absence of brushes are points greatly in their favor. The greatest objection to them appear to me to be the false currents at starting and the difference of phase under nearly all conditions between electromotive force and current. Fortunately greater familiarity with the conditions exacted, and more appropriate designs, are reducing these effects to a minimum, and soon, I trust, their effects may be neglected.

A paper presented at the Nintieth Meeting of the American Institute of Electrical Engineers, New York, President Houston in the Chair, and Chicago, Lieut. Samuel Rodman in the Chair, October 17th, 1894.

THEORY OF THE SYNCHRONOUS MOTOR.

BY CHARLES PROTEUS STEINMETZ.

The following theory of the synchronous motor was written somewhat over a year ago, but was not intended for publication. Since, however, through Prof. S. P. Thompson's paper, entitled "Some Advantages of Alternating Currents," read before the British Association, the synchronous motor has been brought into discussion again, it may be of interest to communicate this sketch.

While by the use of the method of complex quantities, the analytical treatment may be shortened somewhat, I consider it preferable to give the theory essentially in its original form.

I shall discuss one circuit only; the results apply however to the polyphase synchronous motor as well. In the latter case the volts, amperes, watts, etc., are these quantities per phase of the system, so that for instance in a three-phase synchronous motor the total power is three times the value per phase, introduced in the discussion.

Let $u = \sqrt{r^2 + s^2}$ = impedance of the circuit of (equivalent) resistance r and (equivalent) reactance $s = 2 \pi N L$, containing the impressed E. M. F. e_0 [1] and the counter E. M. F. e_1 of the synchronous motor, that is the E. M. F. induced in the motor armature by its rotation through the (resultant) magnetic field.

Let c = current in the circuit (effective values).

1. If e_0 = E. M. F. at motor terminals, u = internal impedances of the motor; if e_0 = terminal voltage of the generator, u = total impedance of line and motor; if e_0 = E. M. F of generator, that is, E. M. F. induced in generator armature by its rotation through the magnetic field, u includes the generator impedance also.

The mechanical power delivered by the synchronous motor (including friction and core loss), is the electric power consumed by the C. E. M. F. e_1, hence:

$$p = c\, e_1 \cos (c_1\, e_1), \qquad (1)$$

thus:

$$\left.\begin{aligned} \cos (c_1\, e_1) &= \frac{p}{c\, e_1} \\ \sin (c_1\, e_1) &= \sqrt{1 - \left(\frac{p}{c\, e_1}\right)^2}. \end{aligned}\right\} \qquad (2)$$

The displacement of phase between current c and E. M. F. $e = u\, c$ consumed by the impedance u is:

$$\left.\begin{aligned} \cos (c\;\; e) &= \frac{r}{u} \\ \sin\;\, (c\;\, e) &= \frac{s}{u} \end{aligned}\right\} \qquad (3)$$

Since the three E. M. F.'s acting in the closed circuit:

e_0 = E. M. F. of generator,
e_1 = C. E. M. F. of synchronous motor,
$e = u\, c$ = E. M. F. consumed by impedance,

form a triangle, that is, e_1 and e are components of e_0, it is (Fig. 1):

$$e_0^2 = e_1^2 + e^2 + 2\, e\, e_1 \cos (e_1, e) \qquad (4)$$

hence,

$$\cos (e_1, e) = \frac{e_0^2 - e_1^2 - e^2}{2\, e_1\, e} = \frac{e_0^2 - e_1^2 - u^2\, c^2}{2\, u\, c\, e_1}, \qquad (5)$$

since, however, by diagram:

$$\begin{aligned} \cos (e_1, e) &= \cos (c, e—c, e_1) \\ &= \cos (c, e) \cos (c, e_1) + \sin (c, e) \sin (c, e_1) \end{aligned} \qquad (6)$$

substitution of (2), (3) and (5) in (6) gives after some transposition:

$$e_0^2 - e_1^2 - u^2\, c^2 - 2\, r\, p = 2\, s\, \sqrt{c^2\, e_1^2 - p^2}, \qquad (7)$$

the *Fundamental Equation of the Synchronous Motor*, relating impressed E. M. F., e_0; C. E. M. F., e_1; current, c; power, p, and resistance, r; reactance, s; impedance, u.

This equation shows, that at given impressed E. M. F. e_0 and given impedance $u = \sqrt{r^2 + s^2}$, three variables are left, e_1, c, p of which two are independent. Hence, at given e_0 and u, the current c is not determined by the load p only, but also by the excitation, and thus the same current c can represent widely different loads p, according to the excitation; and with the same load, the current c can be varied in a wide range, by varying the field excitation e_1.

The meaning of equation (7) is made more perspicuous by some transformations, which separate e_1 and c, as function of p and of an angular parameter φ.

Substituting in (7) the new coordinates:

$$\left.\begin{aligned} x &= \frac{e_1^2 + u^2 c^2}{\sqrt{2}} \\ y &= \frac{e_1^2 - u^2 c^2}{\sqrt{2}} \end{aligned}\right\} \text{ or, } \left\{\begin{aligned} e_1^2 &= \frac{x + y}{\sqrt{2}} \\ u^2 c^2 &= \frac{x - y}{\sqrt{2}} \end{aligned}\right\} \qquad (8)$$

we get

$$e_0^2 - x\sqrt{2} - 2\,r\,p = 2\,\frac{s}{u}\sqrt{\frac{x^2 - y^2}{2} - u^2 p^2}, \qquad (9)$$

substituting again

$$\left.\begin{aligned} e_0^2 &= a \\ 2\,u\,p &= b \\ r &= \epsilon\,u \\ \text{hence,} \qquad s &= u\sqrt{1 - \epsilon^2} \\ 2\,r\,p &= \epsilon\,b \end{aligned}\right\} \qquad (10)$$

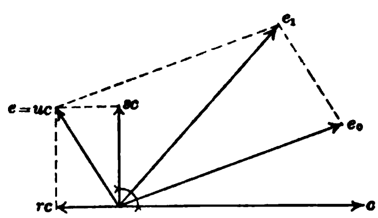


FIG. 1.

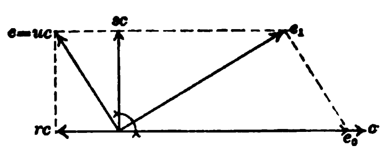


FIG. 2.

we get

$$a - x\sqrt{2} - \epsilon\,b = \sqrt{(1 - \epsilon^2)(2\,x^2 - 2\,y^2 - b^2)}, \qquad (11)$$

and, squared

$$\epsilon^2 x^2 + (1 - \epsilon^2)\,y^2 - x\sqrt{2}\,(a - \epsilon\,b) + \frac{b^2(1 - \epsilon^2)}{2} + \frac{(a - \epsilon\,b)^2}{2} = 0, \qquad (12)$$

substituting

$$\left.\begin{aligned} \epsilon\,x - \frac{(a - \epsilon\,b)\sqrt{2}}{2\,\epsilon} &= v, \\ y\sqrt{1 - \epsilon^2} &= w, \end{aligned}\right\} \qquad (13)$$

gives, after some transposition,

$$v^2 + w^2 = \frac{(1 - \epsilon^2)}{2\,\epsilon^2}\,a\,(a - 2\,\epsilon\,b), \qquad (14)$$

hence, if

$$R = \sqrt{\frac{(1-\varepsilon^2)(a - 2\,\varepsilon\, b)\, a}{2\,\varepsilon^2}}, \tag{15}$$

it is

$$v^2 + w^2 = R^2, \tag{16}$$

the equation of a circle with radius R.

Substituting now backwards, we get, with some transpositions:

$$\{r^2(e_1^2 + u^2 c^2) - u^2(e_0^2 - 2\,r\,p)\}^2 + \{r\,s\,(e_1^2 - u^2 c^2)\}^2 = s^2 u^2 e_0^2 (e_0^2 - 4\,r\,p). \tag{17}$$

the *Fundamental Equation of the Synchronous Motor* in a modified form.

The separation of e_1 and c can be effected by the introduction of a parameter φ by the equations:

$$\left.\begin{aligned} r^2(e_1^2 - u^2 c^2) - u^2(e_0^2 - 2\,r\,p) &= s\,u\,e_0\,\sqrt{e_0^2 - 4\,r\,p}\,\cos\varphi \\ r\,s\,(e_1^2 - u^2 c^2) &= s\,u\,e_0\,\sqrt{e_0^2 - 4\,r\,p}\,\sin\varphi \end{aligned}\right\} \tag{18}$$

These equations (18), transposed, give

$$\begin{aligned} e_1 &= \sqrt{\frac{1}{2}\left\{\frac{u^2}{r^2}(e_0^2 - 2\,r\,p) + \frac{u\,e_0}{r}\left(\frac{s}{r}\cos\varphi + \sin\varphi\right)\sqrt{e_0^2 - 4\,r\,p}\right\}} \\ &= \frac{e_0\,u}{r}\sqrt{\frac{1}{2}\left\{\left(1 - \frac{2\,r\,p}{e_0^2}\right) + \left(\frac{s}{u}\cos\varphi + \frac{r}{u}\sin\varphi\right)\sqrt{1 - \frac{4\,r\,p}{e_0^2}}\right\}}. \end{aligned} \tag{19}$$

$$\begin{aligned} c &= \sqrt{\frac{1}{2}\left\{\frac{1}{r^2}(e_0^2 - 2\,r\,p) + \frac{e_0}{r\,u}\left(\frac{s}{r}\cos\varphi - \sin\varphi\right)\sqrt{e_0^2 - 4\,r\,p}\right\}} \\ &= \frac{e_0}{r}\sqrt{\frac{1}{2}\left\{\left(1 - \frac{2\,r\,p}{e_0^2}\right) + \left(\frac{s}{u}\cos\varphi - \frac{r}{u}\sin\varphi\right)\sqrt{1 - \frac{4\,r\,p}{e_0^2}}\right\}}. \end{aligned} \tag{20}$$

The *Parameter Equations of the Synchronous Motor*:—

The parameter φ has no direct physical meaning, apparently.

These equations (19) and (20), by giving the values of e_1 and c as functions of p and the parameter φ enable us to construct the *Power Characteristics of the Synchronous Motor*, as the curves relating e_1 and c, for a given power p, by attributing to φ all different values.

Since the variables v and w in the equation of the circle (16) are quadratic functions of e_1 and c, the *Power Characteristics of the Synchronous Motor are Quartic Curves.*

They represent the action of the synchronous motor under all conditions of load and excitation, as an element of power transmission even including the line, etc.

Before discussing further these Power Characteristics, some special conditions may be considered.

A. Maximum Output.

Since the expression of e_1 and c [equations (19) and (20)] contain the square root $\sqrt{e_0^2 - 4\,r\,p}$, it is obvious that the maximum value of p corresponds to the moment where this square root disappears by passing from real to imaginary, that is,

$$e_0^2 - 4\,r\,p = 0,$$

or,

$$p = \frac{e_0^2}{4\,r} \qquad (21)$$

This is the same value, which represents the maximum power transmissible by E. M. F. e_0 over a non-inductive line of resistance r, or more generally, the maximum power which can be transmitted over a line of impedance $u = \sqrt{r^2 + s^2}$ into any circuit, shunted by a condenser of suitable capacity.

Substituting (21) in (19) and (20), we get,

$$\left.\begin{aligned} e_1 &= \frac{u}{2\,r}\,e_0 \\ c &= \frac{e_0}{2\,r} \end{aligned}\right\} \qquad (22)$$

and the displacement of phase in the synchronous motor,

$$\cos\,(e_1,\,c) = \frac{p}{c\,e_1} = \frac{r}{u}$$

hence,

$$\tan\,(e_1,\,c) = -\,\frac{s}{r} \qquad (23)$$

That is, the angle of internal displacement in the synchronous motor is equal but opposite to the angle of displacement of line impedance,

$$\begin{aligned} (e_1,\,c) &= -\,(e,\,c) \\ &= -\,(u,\,r) \end{aligned} \qquad (24)$$

and consequently,

$$(e_0,\,c) = 0 \qquad (25)$$

that is, the current c is in phase with the impressed E. M. F. e_0.

If $u < 2\,r$, $e_1 < e_0$ that is, motor E. M. F. < generator E. M. F.
If $u = 2\,r$, $e_1 = e_0$ that is, motor E. M. F. = generator E. M. F.
If $u > 2\,r$, $e_1 > e_0$ that is, motor E. M. F. > generator E. M. E.

In either case, the current in the synchronous motor is leading.

B. Running Light, P = O.

When running light, or for $p = o$, we get, by substituting in (19) and (20),

$$\left.\begin{array}{l} e_1 = \frac{e_0\, u}{r} \sqrt{\frac{1}{2}\left\{1 + \frac{s}{u}\cos\varphi + \frac{r}{u}\sin\varphi\right\}} \\ c = \frac{e_0}{r} \sqrt{\frac{1}{2}\left\{1 + \frac{s}{u}\cos\varphi - \frac{r}{u}\sin\varphi\right\}} \end{array}\right\} \qquad (26)$$

Obviously this condition can never be fulfilled absolutely, since p must at least equal the power consumed by friction, etc., and thus the true no load curve merely approaches the curve $p = 0$, being however rounded off, where curve (26) gives sharp corners.

Substituting $p = 0$ into equation (7) gives, after squaring and transposing,

$$e_1^4 + e_0^4 + u^4 c^4 - 2\, e_1^2\, e_0^2 - 2\, u^2\, c^2\, e_0^2 + 2\, r^2\, c^2\, e_1^2 - 2\, s^2\, c^2\, e_1^2 = 0. \quad (27)$$

This quartic equation can be resolved into the product of two quadratic equations,

$$\left.\begin{array}{ll} e_1^2 + u^2\, c^2 - e_0^2 + 2\, s\, c\, e_1 = 0 & \text{generator.} \\ e_1^2 + u^2\, c^2 - e_0^2 - 2\, s\, c\, e_1 = 0 & \text{motor.} \end{array}\right\} \quad (28)$$

which are the equations of two ellipses, the one the image of the other, both inclined with their axes.

The minimum value of C. E. M. F. e_1 is, $e_1 = 0$ at $c = \frac{e_0}{u}$ (29)

The minimum value of current c is, $c = 0$ at $e_1 = e_0$ (30)

The maximum value of E. M. F. e_1 is given by equation (28),

$$f = e_1^2 + u^2\, c^2 - e_0^2 \pm 2\, s\, c\, e_1 = 0$$

by the condition,

$$\frac{d\, e_1}{d\, c} = - \frac{\frac{d f}{d c}}{\frac{d f}{d e_1}} = 0 \text{ as, } u^2\, c \pm s\, e_1 = 0.$$

hence,

$$c = e_0 \frac{s}{r\, u}, \quad e_1 = \mp\, e_0 \frac{u}{r} \quad (31)$$

The maximum value of current c is given by equation (28) by

$$\frac{d\, c}{d\, e_1} = 0, \text{ as}$$

$$c = \frac{e_0}{r} \qquad e_1 = \mp\, e_0 \frac{s}{r}. \quad (32)$$

If as abscissæ e_1, and as ordinates $u\, c$ are chosen, the axes of these ellipses pass through the points of maximum power given by equation (22).

It is obvious thus, that in the curves of synchronous motors running light, published by Mordey and others, the two sides of the V-shaped curves are not straight lines, as usually assumed, but arcs of ellipses, the one of concave, the other of convex curvature.

These two ellipses are shown in Fig. 3, and divide the whole space into six parts, the two parts A and A[1], whose areas contain the quartic curves (19) (20) of synchronous motor, the two parts B and B[1], whose areas contain the quartic curves of generator,

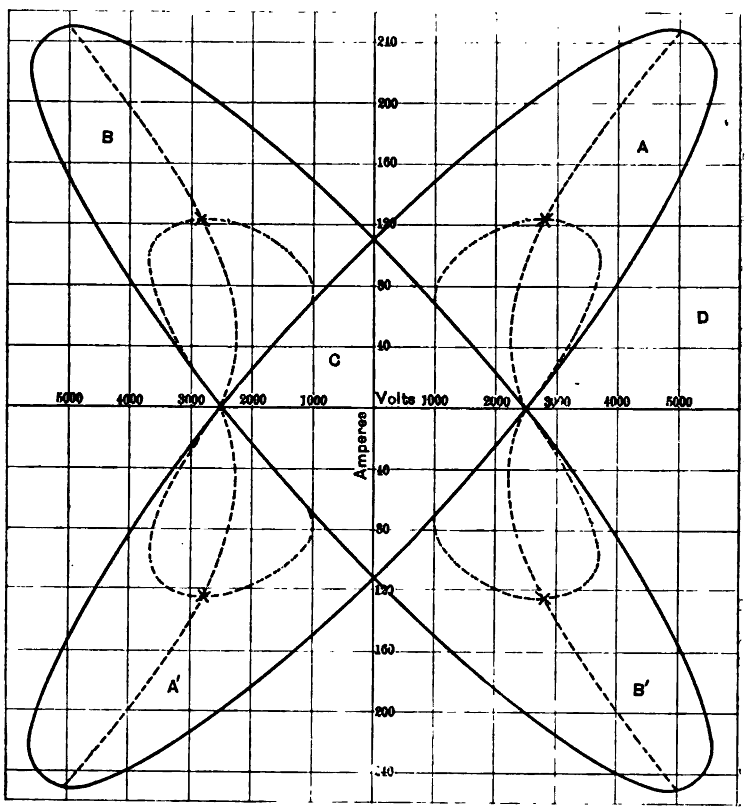


FIG. 3.

and the interior space C and exterior space D, whose points do not represent any actual condition of the alternator circuit, but make the dependence e_1, c imaginary.

A and A[1], and the same B and B[1], are identical conditions of the alternator circuit, differing merely by a simultaneous reversal of current and E. M. F., that is, differing by the time of a half period.

Each of the spaces A and B contains one point of equation (22), representing the condition of maximum output of generator, viz., synchronous motor.

C. Minimum Current at Given Power.

The condition of minimum current, c, at given power, p, is determined by the absence of a phase displacement at the impressed E. M. F. e_0,

$$(e_0, c) = 0.$$

This gives from diagram Fig. 2,

$$e_1^2 = e_0^2 + c^2 u^2 - 2 c e_0 \frac{r}{u} \qquad (33)$$

or, transposed,

$$e_1 = \sqrt{(e_0 - c r)^2 + c^2 s^2} \qquad (34)$$

This quadratic curve passes through the point of zero current and zero power,

$$c = 0, \quad e_1 = e_0$$

through the point of maximum power (22),

$$c = \frac{e_0}{2 r}, \quad e_1 = \frac{e_0 u}{2 r}$$

and through the point of maximum current and zero power,

$$c = \frac{e_0}{r}, \quad e_1 = \frac{e_0 s}{r} \qquad (35)$$

and divides each of the quartic curves or power characteristics into two sections, one with leading, the other with lagging current, which sections are separated by the two points of curve (34), the one corresponding to minimum, the other to maximum current.

It is interesting to note, that at the latter point the current can be many times larger than the current which would pass through the motor while at rest, which latter current is,

$$c = \frac{e_0}{u} \qquad (36)$$

while at no load, the current can reach the maximum value,

$$c = \frac{e_0}{r} \qquad (35)$$

the same value as would exist in a non-inductive circuit of the same resistance.

The minimum value at C. E. M. F. e_1, at which coincidence of phase, $(e_0, c) = 0$, can still be reached, is determined from equation (34) by,

$$\frac{d\,e_1}{d\,c} = 0$$

as,

$$c = e_0 \frac{r}{u^2} \quad e_1 = e_0 \frac{s}{u} \qquad 37)$$

The curve of no displacement, or of minimum current, is shown in Figs. 3 and 4 in dotted lines.[1]

D. Maximum Displacement of Phase.

$$(e_0, c) = \text{maximum}.$$

At a given power p the input is,

$$p_0 = p + c^2 r = e_0\, c \cos (e_0, c) \qquad (38)$$

hence,

$$\cos (e_0, c) = \frac{p + c^2 r}{e_0\, c} \qquad (39)$$

At a given power p, this value, as function of the current c, is a maximum when

$$\frac{d}{d\,c}\left(\frac{p + c^2 r}{e_0\, c}\right) = 0$$

this gives,

$$p = c^2 r \qquad (40)$$

or,

$$c = \sqrt{\frac{p}{r}} \qquad (41)$$

That is, the displacement of phase, lead or lag, is a maximum, when the power of the motor equals the power consumed by the resistance, that is, at the electrical efficiency of 50 per cent.

Substituting (40) in equation (7) gives, after squaring and transposing, the Quartic Equation of Maximum Displacement,

$$(e_0^2 - e_1^2)^2 + c^4 u^2 (u^2 + 8 r^2) + 2 c^2 e_1^2 (5 r^2 - u^2) - 2 c^2 e_0^2 (u^2 + 3 r^2) = 0 \qquad (42)$$

1. It is interesting to note, that the equation (34) is similar to the value, $e_1 = \sqrt{(e_0 - c\, r)^2 - c^2 s^2}$, which represents the output transmitted over an inductive line of impedance $u = \sqrt{r^2 + s^2}$ into a non-inductive circuit.

Equation (34) is identical with the equation giving the maximum voltage e_1, at current c, which can be produced by shunting the receiving circuit with a condenser, that is, the condition of "complete resonance" of the line $u = \sqrt{r^2 + s^2}$ with current c. Hence, referring to equation (35), $e_1 = e_0 \frac{s}{r}$ is the maximum resonance voltage of the line, reached when closed by a condenser of reactance, $- s$.

The curve of maximum displacement is shown in dash-dotted lines in Fig. 3 and 4. It passes through the point of zero current —as singular or nodal point—and through the point of maximum power, where the maximum displacement is zero, and it intersects the curve of zero displacement.

E. Constant Counter E. M. F.

At constant C. E. M. F., $e_1 =$ constant,

If, $$e_1 < e_0 \sqrt{1 - \frac{r^2 x^2}{u^4}},$$

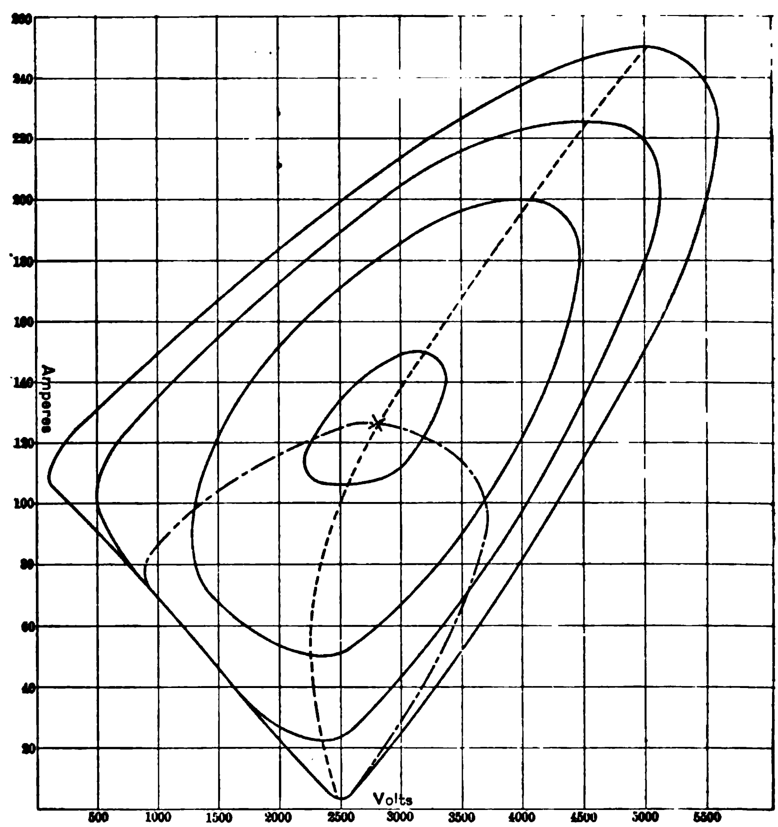


Fig. 4.

the current at no load is not a minimum, and is lagging. With increasing load, the lag decreases, reaches a minimum, and then increases again, until the motor falls out of step, without ever coming into coincidence of phase.

If,

$$e_0 \sqrt{1 - \frac{r^2 s^2}{u^4}} < e_1 < e_0,$$

the current is lagging at no load; with increasing load the lag decreases, the current comes into coincidence of phase with e_0, then becomes leading, reaches a maximum lead; then the lead decreases again, the current comes again into coincidence of phase, and becomes lagging, until the motor falls out of step.

If $e_0 < e_1$, the current is leading at no load, and the lead first increases, reaches a maximum, then decreases, and whether the current ever comes into coincidence of phase, and then becomes lagging, or whether the motor falls out of step while the current is still leading, depends, whether the C. E. M. F. at the point of maximum output is $> e_0$ or $< e_0$.

F. Numerical Instance.

Figs. 3 and 4 shows the characteristics of a 100 K.W. motor, supplied from a 2500-volt generator over a distance of 5 miles, the line consisting of two wires, No. 2 B. and S. G., 18 inches apart.

In this case we have,

$$\left.\begin{array}{rl} e_0 = & 2500 \text{ volts constant at generator terminals.} \\ r = & 10 \text{ ohms, including line and motor.} \\ s = & 20 \text{ ohms, including line and motor.} \\ \text{hence, } u = & 22.36 \text{ ohms.} \end{array}\right\} \quad (43)$$

Substituting these values, we get,

$$2{,}500^2 - e_1^2 - 500\, c^2 - 20\, p = 40 \sqrt{c^2 e_1^2 - p^2} \qquad (7)$$

$$\{e_1^2 + 500\, c^2 - 31.25 \times 10^6 + 100\, p\}^2$$
$$+ \{2\, e_1^2 - 1{,}000\, c^4\}^2 = 7.8125 \times 10^{15} - 5 + 10^9\, p. \qquad (17)$$

$$e_1 = 5590 \qquad (19)$$
$$\sqrt{\tfrac{1}{2}\{(1 - 3.2 \times 10^{-6} p) + (.894 \cos\varphi + .447 \sin\varphi) \sqrt{1 - 6.4 \times 10^{-6} p}\}}.$$

$$c = 559 \qquad (20)$$
$$\sqrt{\tfrac{1}{2}\{(1 - 3.2 \times 10^{-6} p) + (894 \cos\varphi - .447 \sin\varphi \sqrt{1 - 6.4 \times 10^{-6} p}\}}.$$

Maximum output,

$$p = 156.25 \text{ K.W.} \qquad (21)$$

at,

$$\left.\begin{array}{l} e_1 = 2795 \text{ volts} \\ c = 125 \text{ amperes} \end{array}\right\} \qquad (22)$$

Running light,

$$\left.\begin{array}{l} e_1^2 + 500\, c^2 - 6.25 \times 10^4 \mp 40\, c\, e_1 = 0 \\ e_1 = 20\, c \pm \sqrt{6.25 \times 10^4 - 100\, c^2} \end{array}\right\} \qquad (28)$$

At the minimum value of C. E. M. F. $e_1 = 0$, is. $c = 112$ (29)
At the minimum value of current, $c = 0$, is, $e_1 = 2500$ (30)
At the maximum value of C.E.M.F. $e_1 = 5590$ is, $c = 223.5$ (31)
At the maximum value of current, $c = 250$, is, $e_1 = 5000$ (32)
Curve of zero displacement of phase,

$$e_1 = 10 \sqrt{(250 - c)^2 + 4 c^2} \tag{34}$$
$$= 10 \sqrt{6.25 \times 10^4 - 500 c + 5 c^2}$$

Minimum C. E. M. F. point of this curve,

$$c = 50 \quad e_1 = 2240 \tag{35}$$

Curve of maximum displacement of phase,

$$p = 10 c^2 \tag{40}$$
$$(6.25 \times 10^6 - e_1^2)^2 + .65 \times 10^8 c^4 - 10^{10} c^2 = 0 \tag{42}$$

Fig. 3 gives the two ellipses of zero power, in drawn lines, with the curves of zero displacement in dotted, the curves of maximum displacement in dash-dotted lines, and the points of maximum power as crosses.

Fig. 4 gives the Motor Power Characteristics, for,

$p = 10$ K. W.
$p = 50$ K. W.
$p = 100$ K. W.
$p = 150$ K. W.
$p = 156.25$ K. W.

together with the curves of zero displacement, and of maximum displacement.

G. Discussion of Results.

The characteristic curves of the synchronous motor, as shown in Fig. 4, have been observed by me frequently, with their essential features, the V-shaped curve of no load, with the point rounded off and the two legs slightly curved, the one concave, the other convex; the increased rounding off and contraction of the curves with increasing load; and the gradual shifting of the point of minimum current with increasing load, first towards lower, then towards higher values of C. E. M. F. e_1.

The upper parts of the curves however I have never been able to observe experimentally, and consider it as probable, that they correspond to a condition of synchronous motor running, which is unstable. The experimental observations usually extend about over that part of the curves of Fig. 4, which is reproduced in Fig. 5, and in trying to extend the curves further to either side, the motor is thrown out of synchronism.

It must be understood, however, that these power characteristics of the synchronous motor in Fig. 4 can be considered as approximations only, since a number of assumptions are made, which are not, or only partly fulfilled in practice. The foremost of these are:

1. It is assumed that e_1 can be varied unrestrictedly; while in reality the possible increase of e_1 is limited by magnetic saturation. Thus in Fig. 4, at an impressed E. M. F. $e_0 = 2{,}500$ volts, e_1 rises up to 5,590 volts, which may or may not be beyond that which can be produced by the motor, but certainly is beyond that which can be constantly given by the motor.

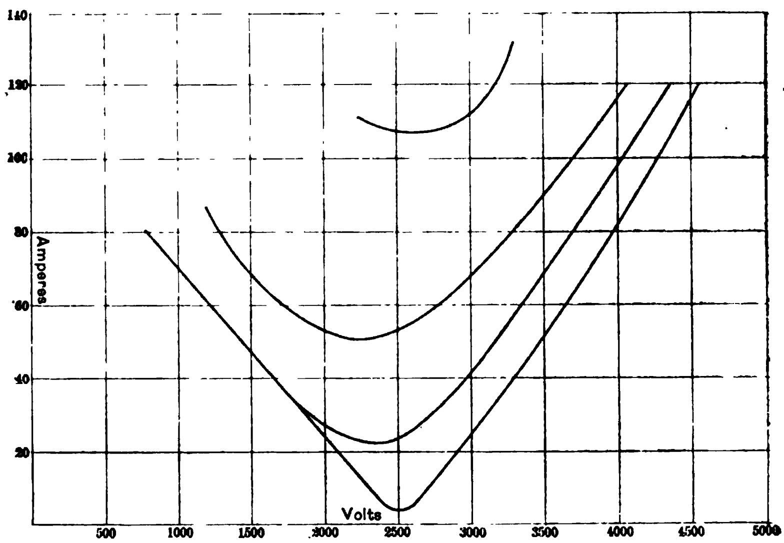


FIG. 5.

2. The reactance s is assumed as constant. While the reactance of the line is practically constant, that of the motor is not, but varies more or less with the saturation, decreasing for higher values. This decrease of s increases the current c corresponding to higher values of e_1, and thereby bends the curves upwards at a lower value of e_1 than represented in Fig. 4.

It must be understood that the motor reactance is not a simple quantity, but represents the combined effect of self-induction, that is, the E. M. F. induced in the armature conductor by the current flowing therein, and armature reaction, that is, the variation of the C. E. M. F. of the motor by the change of the resultant field, due to the superposition of the M. M. F. of the armature current upon the field excitation.

3. These curves in Fig. 4 represent the conditions of constant electric power of the motor, thus including the mechanical and the magnetic friction (core loss). While the mechanical friction can be considered as approximately constant, the magnetic friction is not, but increases with the magnetic induction, that is with e_1, and the same holds for the power consumed for field excitation.

Hence the useful mechanical output of the motor will on the same curve $p =$ const. be larger at points of lower C. E. M. F. e_1, than at points of higher e_1, and if the curves are plotted for constant useful mechanical output, the whole system of curves will be shifted somewhat towards lower values of e_1, hence the points of maximum output of the motor correspond to a lower E. M. F. also.

It is obvious that the true mechanical power characteristics of the synchronous motor can be determined only in the case of the particular conditions of the installation under consideration.

DISCUSSION IN NEW YORK.

MR. KENNELLY:—This subject is so very interesting and it is capable of such very beautiful expression, that if Mr. Steinmetz will pardon me for making room on the blackboard, I think it might interest some of those who have followed Mr. Steinmetz's able exposition of it to see the same results and the same treatment presented under a different point of view.

The fundamental relations of the continuous current motor are two, namely an E. M. F. relation and an activity relation. If

E be the impressed E. M. F. at armature terminals (volts)
e " counter E. M. F. in the armature (volts)
R " resistance of the armature (ohms)
I " current through the armature (amperes)
P " activity absorbed electro-dynamically by the armature (watts).

Then the drop in the armature resistance must be $(E - e)$ volts, and this divided by the resistance of the armature must give the current in the motor by Ohm's law, so that

$$\frac{E - e}{R} = I. \qquad (1)$$

The product of the current and the C. E. M. F. of the motor must be the activity absorbed by the armature and expended in mechanical output and in overcoming frictions of every kind, so that

$$e\,I = P. \qquad (2)$$

The C. E. M. F. e depends upon the speed and excitation. For a given excitation the speed of the armature automatically adjusts itself until the right amount I of current passes through, to enable $e\,I$ just to absorb the power needed for output plus frictions.

In the synchronous alternating current motor we have the same equations, employing the same terms, but with their meaning slightly extended to suit the changed condition. The speed of the armature cannot now change, assuming the generator to run at a constant speed, and the C. E. M. F. of the armature depends entirely on the excitation. If the C. E. M. F. was restricted to being in phase with the impressed E. M. F., it is clear that with equations (1) and (2) could only be satisfied by giving just the right excitation, and that the relations would be unstable, for any change in the load would upset the relation until a new and correct field excitation had been given to make e properly limit the entering current. But if, as is found to be the case, the C. E. M. F. e can differ in phase from E, a new means of automatic adjustment is provided. The resistance R of the armature becomes, in the case of an alternating current circuit, an impedance J (ohms). Also, when the current and the C. E. M. F. are out of step, the activity P is not their simple numerical product, but their co-directed product, *i.e.*, the product of one and the resolved part of the other in its direction, viz.:

$$e\,I \cos a = P,$$

a being the angle of phase difference. The fundamental equations of a synchronous motor are, therefore,

$$\frac{E - \varepsilon}{J} = I \qquad (3)$$

$$e\,I \cos a = P. \qquad (4)$$

In equation (3) E ε J and I are all vector quantities, while in equation (4) all the quantities are numerics only, e and I being the number of volts and amperes in ε and I respectively, without regard to direction. The consequences of these relations are most clearly manifested graphically.

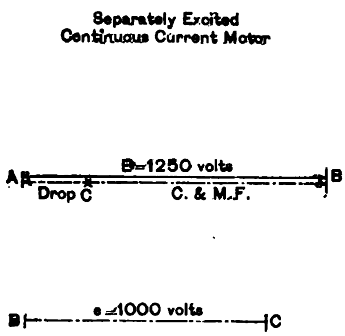


FIG. 1.

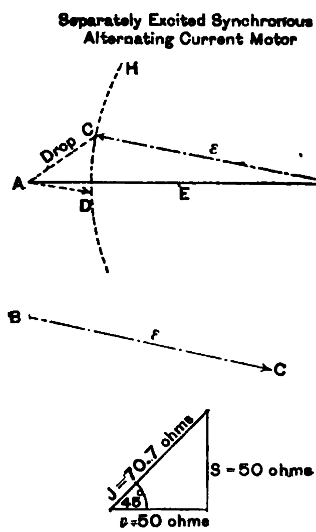


FIG. 2.

If we take a 1,250 volt continuous current motor of 50 ohms armature resistance, and let it run under a load such that its mechanical output is 4 KW., then the losses of activity in friction, hysteresis and eddies (by measurement or computation) might be 5 KW., making the power absorption $P = 5$ K. W. and the current in the armature 5 amperes. The drop in the armature will be 250 volts, and the C. E. M. F. 1,000 volts.

If the line A B in Fig. 1 represents 1,250 volts to scale, and the line B C beneath 1,000 volts, then $E - e$ will be A C, or 250 volts, and this divided by the resistance of the armature 50 ohms gives 5 amperes the current in the motor.

Taking now the corresponding case of an alternating synchronous motor, A B (Fig. 2) represents the E. M. F. ε of 1,250 volts at terminals. The C. E. M. F. ε will not usually be in phase with this, but may have an inclined direction, such as B C, beneath, of 1,000 volts magnitude, this C. E. M. F. will, neglecting the secondary effects of armature reaction, remain constant at all loads when the excitation of the field magnets is constant. $E - \varepsilon$ will therefore be A B — B C or A C, Fig. 2, shown as 354 volts, and this will be the drop in the armature, instead of 250 volts in the continuous current case. To find the current, we divide A C by the impedance of the armature. If the armature resistance $r = 50$ ohms, and its reactance $s =$ say 50 ohms, the impedance will be $J = 70.7 \angle 45°$ ohms, as shown in Fig. 2. If we divide this into A C, we not only obtain the numerical quotient $\frac{353.5}{70.7} = 5$ amperes, but we lay off this quotient along A D inclined at the impedance angle 45° from A C, in accordance with the rules of vector division. In this case A B happens to be in phase with or parallel to B C the C. E. M. F., so that their co-directed product is their simple numerical product $5 \times 1000 = 5$ K.W. the activity mechanically absorbed by the armature. Usually, however, they will not be parallel, but will include some angle α less than 90°. The phase angle between ε and E will therefore be such that the vector drop A C, which must connect A with some point on the circle H C K of radius ε, when divided by the vector impedance J, gives a current whose co-directed product with ε gives the activity which the motor requires at its imposed torque.

The separately excited continuous current motor adjusts its current by its speed and C. E. M. F. The separately excited synchronous motor cannot alter its speed or C. E. M. F., but adjusts its current by its phase of C. E. M. F. By varying the magnet excitation and, therefore, the C. E. M. F. of the armature, the phase angle of the current for a constant load can be widely altered. It can be made to lag behind, to run before, or to keep step with the impressed E. M. F.

DR. PUPIN:—I would like to add one or two remarks to what I have already said, merely as a comment to Mr. Steinmetz's remarks. The differences between Mr. Steinmetz's method and that of Lieutenant Reber are essential if I understand Mr. Steinmetz correctly. The practical results to which they lead will, of course, be the same. They can be stated as follows:—that Mr. Steinmetz defines his quantities by calling them certain names. He calls a thing "impedance" and defines it by saying that it is the product of an electromotive force multiplied by a current. The reactance is then defined by the relation $u^2 = r^2 + s^2$ where r is the resistance, s the reactance and u the impedance. The electromotive forces, and the currents are given by the amplitudes. The back electromotive force is the E. M. F. of the motor. These definitions are non-committal and therefore Mr. Steinmetz's results will be true even if hysteresis, Foucault current, and magnetic leakage are present. But if they are to hold true in the presence of these, then the quantities which they define are only experimentally measurable quantities, and cannot be calculated by a general formula; as we can calculate, for instance, the coefficient of self-induction of a coil surrounding a closed magnetic circuit in terms of the number of turns, and the reluctance of the magnetic circuit, when the modifying effects due to the Foucault currents and magnetic leakage are neglected.

Of course to a practical engineer who wishes to consult the results only, Mr. Steinmetz's results are just as valuable as any other. But to a designer or to an inventor, or to an investigator who wishes to go beyond the actual results, and inquire into the causes of armature reactions, magnetic leakages, variations of permeability, etc., the theory which speaks in terms of those quantities as defined in Lieut. Reber's paper seems to me to be much preferable. Besides the fundamental theory of a new apparatus like the motor should be written in terms of the fundamental quantities, that is resistance, co-efficient of self-induction, and capacity, as ideally defined. It is a theory of an important practice, and it is intended to be the foundation on which the members of this INSTITUTE are going to build, and in the course of this building up it will be time enough to take care of hysteresis, Foucault currents and magnetic leakage. If you look at the question from the fundamental standpoint, you must take an ideal view of it, and therefore you must treat the fundamental quantities like resistance, self-induction, and capacity in an ideal sense. The theory so constructed, as I said before, will serve two purposes. It will express relations of the fundamental quantities in an ideal, and therefore perfectly definite sense; and secondly, it will give you a hint why in a given case practical results differ from the purely ideal relations. It will give you hints which may lead to discovery of new properties of the material of which the machine is constructed. It will assist you in investigating the properties of the various parts of electric and magnetic circuits—the properties which you did not take into consideration in the ideal theory, but the existence of which will in all probability bring about practical results differing from the ideal theory. I think that this is a very important thing to consider in theoretical discussions.

There is another reason why, in my opinion, Lieut. Reber could not adopt Mr. Steinmetz's method. If he had adopted this method, I do not see how he could have considered the quantity K to which he attaches so much importance: namely, the relation between the reactance and the resistance; and how could he, for instance, arrive at the result that it is a very good thing to introduce a resistance or capacity in a motor if you want to vary its torque. So that I do not agree with Mr. Kennelly or with Mr. Steinmetz that because one theory *seems* to be formally simpler than another, that it is more desirable. One theory may be good for one man, the other theory is good for another man, and that theory which is to serve as the fundamental theory, say, of an induction motor, must take into consideration, not the mean value of experimentally determinable quantities, like impedance, counter-electromotive force, etc., but the fundamental quantities, the ideal resistance. the self-induction, and the capacity, and that too the values of these quantities for ideal conditions; for it is under these conditions only, that we can calculate beforehand the numerical values of these quantities.

MR. STEINMETZ:—I think Prof. Pupin misunderstood, due to the shortness of what I said. These equations of the induction motor, which I wrote here, giving the torque and the power, did not contain the impedance, but contained the resistance and the reactance. The quantity, s, is nothing else than $= 2 \pi N L$. As long as the frequency is the same, the reactance remains the same, and in the impedance of the armature I used resistance and reactance separately, and in the latter introduced the slip so as not to get a variable quantity, but reduced to reactance at standstill.

Thus all the quantities used by me are reduced to the fundamental constants, resistance, inductance, capacity—which is absent in an induction motor—and coefficient of hysteresis. The armature resistance is contained explicitly in the equation, and it was from these very equations that I investigated its effect. Therefore, the exceptions made by Dr. Pupin do not apply to my theory.

DR. PUPIN:—But you do not specify that you are neglecting leakage and hysteresis; as long as you do not do that, so long will s be not the reactance that can be expressed in number of turns, length, cross-section, and permeability of the magnetic circuit, etc., by the well-known rules, but will be a quantity that can be determined by actual experimental test only.

MR. STEINMETZ:—I do not think that is so. When I want to build a 100-horse power induction motor, if I should build it first and then determine by actual test its reactance, etc., I do not think that such a way of designing would do for a long time. All these quantities are determined by calculations beforehand, and when I said that I build the induction motor so as to give a starting torque 50 per cent. in excess of running torque, this is not done by running the motor, and experimentally determining what resistance gives the desired torque, but the resistance is calculated before the motor is built, and the test of the finished motor merely checks the calculation.

DR. PUPIN:—If you take these values, you ought to say in the beginning of the paper that you are neglecting hysteresis and leakage.

MR. STEINMETZ:—No. Hysteresis is not neglected.

DR. PUPIN:—No man in the world can calculate the coefficient of self-induction of a varying magnetic circuit in which the permeability and the distribution of magnetic induction is continually varying.

MR. STEINMETZ:—The permeance does not vary. The iron in an induction motor necessarily is worked at a low magnetic induction, where its permeability is so high, that the reluctance of the magnetic circuit is practically all the reluctance of the air-gap. I can show you such motor magnetization curves, where I had the same induction motor running at various magnetic inductions, that is, at various impressed electromotive forces. The curve of current, that is, of M. M. F., as function of the E. M. F., that is the magnetic induction, is practically a straight line, which extended, passes through the origin. That is, the magnetic reluctance is practically constant.

DR. PUPIN:—If that is so, and I think it is so, as you say, then hysteresis and the variation of the magnetic permeability and the variation of the magnetic leakage with the load does not amount to quite as much as you led me to infer in your discussion of Mr. Reber's paper.

MR. STEINMETZ:—No. That is a different thing. The hysteresis may vary, and so may vary the magnetic stray field, and the useful flux. But the reactance has nothing to do with hysteresis. It is entirely independent. The magnetic leakage means the magnetic flux passing between armature and field, between primary and secondary circuits. If you take an ordinary induction motor and have no resistance in the armature, a highly efficient induction motor, then at standstill more than half the flux passes between the primary and secondary, and the product of the two L's is very greatly different from M^2. It is nearly four times as large.

Coming now to the theory of the synchronous motor, the quantities introduced in the equations are: the impressed E. M. F., the counter E. M F., and the impedance. The counter E. M. F. is the E. M. F. induced in the armature conductors by their rotation through the magnetic field, thus is derived from the magnetic characteristic of the machine, which can be determined experimentally or calculated as known. The impedance is composed of the resistance, r, and the reactance, s.

The only quantity which is variable and of complex nature is the reactance of the motor proper, which includes the self-induction and the armature reaction. Thus this quantity has been specially discussed by me in the last chapter.

I have tried to reduce all the quantities entering the equations to their elementary constants, which can be determined experimentally, and which can be calculated beforehand, because a theory of any machine is very nice and of high scientific interest, but to be useful, the *first* condition is that you can determine all the quantities which enter into the theory beforehand by calculation, the *second* condition is, that you can easily determine these quantities by experiment to check the correctness of your calculation, and the *third* condition is that you can easily recognize the limits of the practical application of your theory.

[Adjourned.]

[DISCUSSION IN CHICAGO, OCT. 17TH.]

MR. M. A. EDSON:—In discussing a paper of this kind we cannot, of course, go into the mathematical details and blackboard work, as it becomes very uninteresting to those not directly interested, and is very much in the nature of pure mathematics. But still you can look at it in another light: that it has some practical value in special design and should form a useful adjunct to our all but too small data on the subject of synchronous motors, and we will assume the theory to be correct until proven otherwise by practice. The conditions under which such motors are generally constructed are such, that the application of the best theory on paper as to the rough, practical problems of every day construction, do not always produce the best ultimate results; in other words, the motor will rarely match the theory. There are, of course, other points in this paper which I have not gone into very fully and looked over; but when we turn to the last note, there appears a little paragraph which seems to explain the whole thing. It says: "It is "obvious that the true mechanical power characteristics of the "synchronous motor can be determined only in the case of the "peculiar conditions of the installation under consideration." I think that is about all the discussion I have upon that particular subject, and I hope there are others here, and believe there are, who will take it up in more mathematical form and enlighten us more on the principle embodied in the theory.

WILLYOUNG ON MEASURING INSTRUMENTS.

[See p. 476.]

THE CHAIRMAN:—There were two papers read at the Philadelphia meeting of the Institute, which are to be taken up this evening for discussion.

All the members, of course, have seen these papers. One is by Mr. Elmer G. Willyoung on "Standardizing Electrical Measuring Instruments," and the other by Prof. R. B. Owens and C. A. Skinner on "Test of a Closed-Coil Arc Dynamo." I will ask Mr. J. G. Wray, of the Chicago Telephone Company, to open the discussion on Mr. Willyoung's paper. We will have to assume in this case also that the paper has been read.

MR. J. G. WRAY:—I notice that there is already a very good discussion of this paper by Mr. R. O. Heinrich. In some things I will agree with him, but in others I will differ. He speaks about the use of this instrument for laboratory practice, and says, "where time is usually a minor consideration, the potentiometer "would be commendable; but when it comes to accuracies as "high as one-tenth of one per cent., I should always take the "silver voltameter and a standard resistance as a last resort for "a check on the standard cell."

Mr. Heinrich, in his discussion, seems to favor the use of the voltameter in standardizing measuring instruments. My experience with the silver voltameter has demonstrated the fact that this method is altogether too delicate for the average station electrician. In order to get the best results, or even results of moderate accuracy, some considerable experience with the method is necessary. The trouble seems to be in securing the right current density. You may take the electrodes of such size as to attain the right current density, but still there is a chance for trouble. The electrodes may not be parallel, or the electrolyte may vary in density, thus causing an increase in current density and a granular deposit. With great care exercised during the experiment, and with precision and accuracy in all measurements, very good results may be attained; but in the hands of any but a laboratory expert, results will be sadly inconsistent. In regard to the potentiometer itself I am surprised that such extreme accuracy is claimed, an accuracy which is phenomenal and hardly consistent with the variation found in a Clark cell. The sliding contact in connection with the lower resistances, would, as Mr. Heinrich has shown, affect the accuracy of measurements. The machine is too complicated, has too many levers and switches. What is needed in a station is a direct reading instrument of fair accuracy.

MR. S. A. RHODES:—The main point in the potentiometer, I think, is that it is a standard instrument, placed in a portable shape, comparing in this respect with standards of resistance. The potentiometer could only be used as a standard instrument, for the reason pointed out by Mr. Heinrich, that the potentiometer method being a zero method would require a constant current for comparison, in order to obtain an accurate balance. However, many users of portable voltmeters would, I think, be greatly relieved at times, to be able to compare their instruments readily with some standard not liable to change, due to decrease in strength of the permanent magnet, or change in tension of spring. As it is now, such voltmeters are returned to the manufacturers for calibration, and after some delay and expense, received back from them, still liable to the suspicion that they were severely handled in transit and thereby still liable to error. If a bridge and standard cell are at hand, the user of the portable voltmeter can, after a somewhat tedious series of adjustments and calculations, obtain a calibration of his voltmeter. It is this series of adjustments and calculations which are performed by the potentiometer directly, and the potentiometer would, thereby, be valued in proportion as its labor saving qualities would be appreciated.

PROF. W. M. STINE:—The idea of arranging the potentiometer, which is essentially a laboratory instrument, so as to place it on the market for commercial testing, is not a new one. When the probable sources of error are considered together with the delicacy and complexity of the method, the attempt seems rather rash. Though this is not the first attempt, yet so far, I believe, the others have been demonstrated to be partial failures.

The potentiometer method is admirably adapted for the comparison of low E. M. F's., such as are produced by standard and other cells. However, when the standard cell is employed as the unit of E. M. F., and this is multiplied in various ways to offset voltage varying from 100 to 300 volts, an error which in itself may be inappreciable becomes a factor which cannot be overlooked. I question if a standard cell in ordinary commercial testing can be relied upon for any length of time.

I think the experience of the London Board of Trade is of value in this connection. I understand that they employ fall of potential methods for the evaluation of E. M. F's. in preference to the standard cell. A case of coils wound to carry about .25 amperes, and the total resistance divided into 50 coils of equal resistance, and provided with suitable contacts, would constitute a less expensive and more reliable piece of apparatus. If the coils were of manganin wire and immersed in oil, the temperature error could be kept very low. A Weston mil-ammeter could be employed to measure the current. If the apparatus was properly constructed the source of error would lie principally in the reading of the current. But since the error is not multiplied, the method may be relied upon to a small fraction of a per cent. throughout the range. Since the error in this case depends upon the ammeter, the question arises, why not employ a voltmeter of standard make at once? In the first place the milammeter is more reliable and accurate, and further it is comparatively easy to check its readings by the use of a voltameter.

My experience, however, with Weston instruments inclines me to believe a Weston voltmeter used only for standardizing purposes, will in general use, prove as accurate as a potentiometer. Recently, I had occasion to compare eight Weston voltmeters at 110 volts, and found that the actual variation did not exceed $\frac{1}{10}$ per cent.

I question whether Mr. Willyoung's attempt to make his apparatus complete and compact by including the galvanometer is wise. I do not know if the galvanometer is sufficiently sensitive for this class of work. The galvanometer being inclosed in the case cannot readily be gotten at for adjustment, and from its construction will doubtless readily get out of order.

I have the pleasure of showing to the members of the INSTITUTE, this evening, a potentiometer made by Nalder Bros. and Co., of London. As a piece of apparatus it is not complicated, and seems about as well adapted to general use as it is possible to construct a potentiometer. In this instrument the resistance is so proportioned that there is a drop of one volt over each 200 ohms. A standard cell is opposed to the drop over a coil whose resistance is 200 times in ohms the E. M. F. of the battery. A galvanometer included in the battery circuit indicates when the two E. M. F's. are exactly opposed.

The diagram very clearly indicates the resistance relations of the apparatus and its method of use. A suitable galvanometer is connected to binding posts 1 and 2. The source of the main E. M. F., either lighting or storage battery mains, is connected to posts 4 and 5, while the voltmeter to be calibrated is connected to 3 and 6. These connections are suitably indicated on the ebonite top of the case. It will be noticed that the instrument contains two standard cells, which may be opposed through the galvanometer for the purpose of checking. One of these cells, by a spring attachment to the contact is always used in calibration, while the second is reserved for checking the first one. This suffices to indicate when the working cell becomes unreliable. When the 20,000 ohm coil is short circuited by inserting the plug, the instrument will check to 111 volts by tenths. This coil compensates for a drop of 100 volts, so that removing the plug, the range extends over 211 volts.

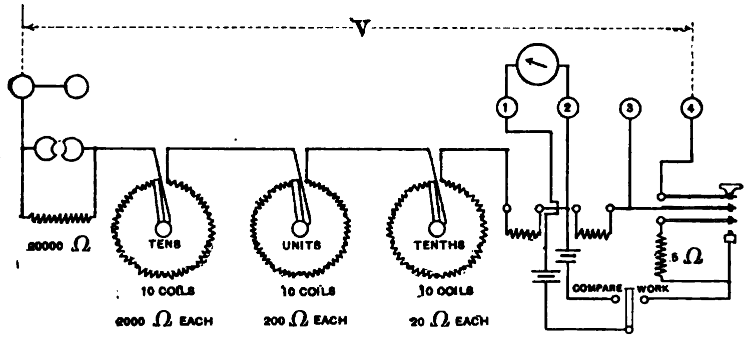


DIAGRAM OF NALDER BROS. POTENTIOMETER.

An objection has been raised to the use of such apparatus on incandescent mains. I find that a dynamo run on a light and constant load answers very well for furnishing E. M. F. for such testing, and can be varied so as to give a sufficient number of points for calibration of voltmeters.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

New York, November 21st, 1894.

The 91st meeting of the INSTITUTE was held this date at 12 West Thirty-first street, and was called to order at 8.15 P. M. by President Houston:

The Secretary read the minutes of the meeting of October 17th, 1894.

THE PRESIDENT:—Gentlemen, you have heard the minutes of the last stated meeting. What action will you take on them? If the Chair hears no objection he will consider them as being approved. They are approved. The Secretary will now read the names of associate members elected at the Council meeting to-day, and also of those transferred to full membership.

The Secretary read the following list:

Name.	Address.	Endorsed by
APPLETON, JOSEPH	Electrical Engineer, The Electric Storage Battery Co.; residence, 706 South Washington Square, Philadelphia, Pa.	T. C. Martin. Jos. Wetzler. Geo. M. Phelps.
ARCHER, GEO. F.	Student in Electrical Engineering, Columbia College; residence, Garden City, L. I.	F. B. Crocker. Max Osterberg. W. H. Freedman.
ASHLEY, FRANK M.	Master Mechanic, Ashley Engineering Works, Hawthorne, N. J., 136 Liberty St., N. Y.	Townsend Wolcott. Joseph Sachs. W. A. Rosenbaum.
BLIZARD, CHARLES	General Sales Agent, Electric Storage Battery Co., 45 B'way; residence, 76 Madison Ave., N. Y. City.	Townsend Wolcott. E. T. Birdsall. F. Reckenzaun.
DAVENPORT, C. G.	Expert and Agent, General Electric Co., 44 Broad St., N. Y. City.	J. R. Lovejoy. C. D. Haskins. J. H. Vail.
HADAWAY, W. S., Jr.	Electrician, Central Electric Heating Co., 26 Cortlandt St., N. Y. City.	Joseph Wetzler. Ralph W. Pope. T. C. Martin.
HEDENBERG, WM. L.	Draughtsman, W. R. Fleming & Co.; residence, 83 Clinton Place, N. Y. City.	C. H. Davis. F. S. Holmes. B. P. Flint.

Name.	Address.	Endorsed by
MUNNS, CHAS. K.	Electrician, Strowger Autom. Tel. Exchange, Chicago, Ill.; residence, 1002 W. Monroe Street, Chicago, Ill.	Ludwig Gutmann. C. L. Brown. Fred. De Land.
RIDLEY, A. E. BROOKE	Agent, Electrical Engineer, Siemens and Halske Electric Co., 508 California St., San Francisco, Cal.	Geo. P. Low. F. C. Colville. Sidney Sprout.
ROBINSON, FRANCIS G.	Foreman, Bushwick and Union Ave. Depots, Brooklyn Heights R.R. Co.; residence, 156 Macon St., Brooklyn, N. Y.	F. B. Crocker. M. I. Pupin. W. H. Freedman.
VARNEY, WILLIAM WESLEY	Attorney at Law, Electrical Expert, 118 East Lexington St.; residence, 1001 Harlem Ave., Baltimore, Md.	J. F. Morrison. Chas R. Cross. W. J. Humphrey.

Total, 11.

TRANSFERRED FROM ASSOCIATE TO FULL MEMBERSHIP.

Approved by Board of Examiners, October 5, 1894.

SANDS, H. S.	Consulting and Contracting Electrical Engineer, Wheeling, W. Va.
ROSS, NORMAN N.	District Engineer, Canadian General Electric Co., Winnipeg, Man.
CHENEY, W. C.	Superintendent and Electrical Engineer, General Electric Co., Portland, Or.
BOWMAN, FRED. A.	Superintendent New Glasgow Electric Co., New Glasgow, N. S.
TISCHENDOERFER, F. W.	Electrical Engineer, Schückert & Co., Nuremberg, Germany.

Total, 5.

THE PRESIDENT:—We will now have the first paper announced for the evening. It is by Professor George D. Shepardson, and is entitled "Suggestions for an Index of Engineering Literature." In the absence of Prof. Shepardson, Mr. T. C. Martin has kindly agreed to read the paper.

Mr. Martin gave an abstract of the following paper:

A paper presented at the Ninty-first Meeting of the American Institute of Electrical Engineers, New York, President Houston in the Chair, and Chicago, Mr. Leland L. Summers in the Chair, November 21st, 1894.

SUGGESTIONS FOR AN INDEX OF ENGINEERING LITERATURE.

BY PROF. GEORGE D. SHEPARDSON.

It is said that at one time all existing knowledge could be obtained by one man. Indeed there is a general belief that Dante, who lived from 1265 to 1321 A. D., was the happy possessor of all that was known in civilized countries. One occasionally meets, even at the present day, individuals whose manner indicates the possession of such universal knowledge, but their claims lack official certification. It is impossible to know everything relating to even one of the many branches of learning at the present day, although the sciences are so finely subdivided.

Not many years ago it was practicable for persons of moderate income to take all the papers and magazines treating of engineering and allied sciences. To-day the number of periodicals is so great that one could hardly read all that is in them, or even digest all the memoirs bearing upon his own field of work, unless he devoted to it the major part of his time. He would be so busy learning what others are doing, that he would have no time to do anything original himself.

Nevertheless, in undertaking an investigation on any subject, whether for engineering, manufacturing or purely scientific purposes, one of the first things one should do is to look up the subject thoroughly in order to learn what others have done and are doing. One thereby avoids spending energy on problems that others have already solved, or working along lines that others have proved to be fruitless. He obtains hints as to the most promising directions in which to work, and the best methods for working.

In some lines of engineering much has been crystallized, and standard text-books and manuals have been prepared, containing the essence of the best that has been written on the subject. The authors of many of the earlier manuals gave little credit to others, and left the reader with the impression that the book contained the sum of all knowledge. The abstracts in books are necessarily colored by the individual views of the editor. It is preferable to refer back to the original memoirs, especially if one wishes to obtain details of the investigations. More recently the commendable custom of continental writers is being adopted in scientific books and papers published in this country, so that this difficulty is being removed to a considerable extent. As all lines of engineering and science are developing at a rapid rate, the books are necessarily behind the times, and the live worker must read the current periodicals in order to know what is being done by others.

A common method of learning the history and present state of knowledge on a given subject, is to obtain access to the files of periodicals in a library, and search the indexes of the separate volumes for articles upon the subject under investigation. Some journals of long standing have occasionally published indexes of complete files covering a number of years. Searching the various indexes volume by volume is tedious and difficult. One is apt to overlook the very papers for which he is searching, when hunting cursorily through index after index. One usually works under the disadvantage of having only incomplete files or those of only a few periodicals. After the most careful search through the material available, he still has the feeling that some other periodical not immediately accessible may contain exactly the information he seeks.

To save the time of busy people and to call attention to a mass of material that would otherwise be available to comparatively few, attempts have been made at different times to compile indexes of literature. Perhaps the greatest of these is the "Royal Society Catalogue of Scientific Papers," consisting of eight quarto volumes of about 1000 pages each. This was compiled under the auspices of the Royal Society of England and was printed by the English government at public expense. It gives alphabetical lists of titles and authors of scientific papers published in every civilized country between the years 1800 and 1873, but excludes technical and professional papers.

Next to this stands Poole's Index of Current Literature, published in this country by private enterprise. The first volume, compiled with the cooperation of 52 libraries, indexes 6200 volumes which were published during the period from 1803 to 1882, and contains about 1000 references to articles on electrical subjects that appeared in popular and scientific journals. The second volume, to which 97 libraries contributed, covers the period from 1882 to 1887 and contains about 600 references to articles on electrical subjects. The third volume, compiled with the cooperation of 122 libraries, covers the period from 1887 to 1892 and gives about 500 references to electrical papers. The volume for 1893 gives about 100 such references. In Poole's Index there is some attempt at classification of electrical papers and there are a few cross references.

The Descriptive Index of Current Engineering Literature, compiled from the monthly summaries in the *Journal of the Association of Engineering Societies*, gives an excellent resume of engineering literature between 1884 and 1891. This is devoted principally to civil engineering, but gives about 30 pages with 700 references to electrical papers. Most of the references indicate the character of the papers. The cross references are excellent. The monthly index in the *Journal of the Association of Engineering Societies*, is commendable in that it prints the indexes on only one side of the leaf so that they may be cut and re-arranged according to the ideas of the individual. The *Journal* for each December contains a summary of the monthly indexes of the year.

The American Institute of Electrical Engineers published in its Transactions for about three years, 1886 to 1888, an excellent index of current electrical literature compiled gratuitously by a gentleman too modest to have his name appear. Volume v of the Transactions contains about 5,100 references, volume vi about 5,800, and the first few numbers of volume vii contain about 1,500 references. Some of these are accompanied by descriptive notes.

The first volume of Galloupe's Engineering Index, compiled and published by private enterprise, gives about 28 pages with 1,500 references to articles on electrical subjects appearing in the years 1883 to 1887. The second volume, 1888 to 1892, gives 61 pages with about 3,300 references, titles and authors being given.

An increasingly excellent index appears in the *Engineering Magazine*, the figures for which are not at hand. Recently *Electric Power* has launched out into this field. The digest of foreign literature in the *Electrical World* is commendable, but confessedly incomplete. Extended indexes are published at intervals by the French and Germans. A universal index, published weekly at Leipzig, aims to gives tables of contents of a number of journals arranged in the alphabetical order of their places of publication. Doubtless there are other indexes not known to the present writer.

A commendable custom of *La Lumière Électrique*, that might well be imitated by other periodicals, was to publish at intervals complete lists of articles that had previously appeared in the paper bearing upon particular subjects.

To the mind of the writer, the most ideal index yet attempted was that published at first in connection with *Electrical Engineering* and later as a too short-lived publication by itself under the name of *Electrical Literature*. The printing of the indexes on separate sheets, perforated and tied together, each sheet being devoted to a single subject, combines the advantage of flexible classification of subjects with ease of arranging the sheets from month to month so as to make the bibliography of each subject consecutive and up to date. Having the sheets loose and perforated for binding with brass clips or cords, allows each individual to supplement the printed sheet by his own notes or additional references. The publisher is also able to add sheets descriptive of later or earlier memoirs without impairing the integrity of previous work. It should be and doubtless is a matter of sincere regret by the electrical and mechanical engineering professions that this publication was forced to suspend on account of lack of financial support.

Since the available printed indexes are imperfect, many individual students feel the need of keeping private indexes. In some centers, such as universities and large cities, personal friends sometimes cooperate in maintaining a common index. Yet even in such a case one cannot escape the feeling that much of his labor is needless, since some one else may be doing the same thing better. He feels also that it is not as useful as it might be, because only he and his immediate friends have the benefit of his index which might be of use to many if it could be made more available.

Occasionally one favors his fellows by publishing his bibliography on some special subject. An excellent example is "Titles of Literature Concerning the Fixation of Free Nitrogen by Plants," contained in Bulletin No. IX of Minnesota Botanical Studies, which gives references to over 600 papers, compiled by Professor D. T. MacDougal, of the University of Minnesota. Another valuable, but less extensive index was published in the *Electrical World*, vol. xii, p. 6, July 1, 1893, by J. Stanford Brown, giving about 375 references to papers on "Accumulators in Practical Work."

Those who have free use of the published indexes, find it desirable to supplement them by manuscript notes for several reasons. Many of the memoirs deal with more subjects than the one indicated by the title. Discussions of matters of interest are often found buried in papers having little other value, and bearing other titles. In many cases the titles given by the authors are misleading and do not indicate the true nature of the memoir. The published indexes cover only periodical literature and give little or no attention to the books which often contain valuable matter that has not found its way into the journals. In many cases books contain reprints or abstracts of papers, the originals of which are inaccessible while the books containing them may be easily procured. Some of the published indexes give only bare references to the articles, while the investigator finds it desirable to obtain abstracts or outlines of papers when looking up any subject. He must therefore supplement the index with comments of his own. Some of the monthly summaries are poorly classified and it is almost as laborious to hunt up the bibliography of a subject from these as from the original periodicals. Even those that are well classified, are published in sections and it is necessary to look through the index month by month to complete a search.

In view of the present state of the case, as imperfectly outlined above, what seems to be the best method of securing the publication of an index that shall avoid the failings of those already existent? One of the first considerations is, that the compilation of an index which will be an improvement upon all existing ones, must be the result of the cooperation of a large number of people, as was Poole's Index to which over 122 libraries contributed. It will require the expenditure of a considerable amount of money and will probably not be self-supporting. Such work could probably be done best under the auspices of some society or societies, as was the Royal Society Catalogue. Since the index might well include the subjects of interest to electrical, mechanical, hydraulic, civil and perhaps mining engineers, it seems reasonable that the various national engineering societies might combine forces to maintain the publication of an index that would be of lasting value to the engineering profession of the entire world. Since the headquarters of four of the national societies are in the same city, two of them being in the same building, such cooperation would not seem difficult. A joint committee might be appointed which would receive the confidence and the cooperation of all. They could draw upon a large constituency for cooperation and subscriptions. The committee might outline the plan for the work, and prepare preliminary lists of subjects, which might be published and submitted to the members for suggestions. The committee could then call upon different members for contributions from their private index files. The committee should have authority to edit the contributions thus received. Doubtless a number of publishing houses would contribute complete files of their publications for such indexing. Indeed some of them have already made such offers to a private enterprise for such purpose. Incidentally the material thus received would make a valuable addition to the libraries which are being gathered at the headquarters of the societies. Such a committee could certainly compile an index that would be far in advance of the imperfect ones now published. The vast amount of duplicate labor now expended by the papers and by individuals could then be turned to more efficient work.

In conclusion the writer begs to add some suggestions as to the style of such an index. It should include subjects relating to electrical, mechanical, hydraulic, civil and perhaps mining engineering. It should index books as well as periodicals. The references should be descriptive so far as practicable. Each subject should have a page by itself. Digests and simple references might be on separate sheets. References and sheets on each subject might be numbered consecutively for convenience in adding later notes from other files or from later publications of the same articles. Abbreviations or other symbols should indicate whether the reference is to the original paper or to a re-print, whether the paper is in full or abstract, whether accompanied by discussion and by plates or illustrations. The separate sheets should be perforated for re-arranging and re-binding and should be printed on one side only. It might be preferable to print the whole index on cards similar to those used in the card catalogues of libraries.

In this way it would be practicable to begin publication at an early date and to issue parts at frequent intervals. The indexes to current publications could be issued without delay, and those of completed files or historical works could appear as the work proceeded. Publishing the work in sections would allow each engineer or student to purchase or subscribe for only such subjects as were of importance or interest to him. Indeed the work, if carried out properly, would result in so massive a catalogue that few outside of libraries or other public institutions could afford to obtain the entire work.

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After this paper was prepared, the writer learned, in looking over some periodicals which accumulated during the summer, that the Royal Society recognizes the defects of its admirable catalogue and is taking steps to revise and bring it down to date with the cooperation of learned societies in various parts of the world. (*London Electrician*, xxxiii, 268, July 6, 1894.)

University of Minnesota,
November 1, 1894.

Discussion.

Mr. Martin:—There is an index of electrical literature which is not mentioned by Prof. Shepardson. I refer to the publication of which I have brought a volume this evening, so that the members may look at it. I dare say it is familiar to a great many, certainly to the President. It is the *Fortschritte der Elektrotechnik.* This is a most masterly index of electrical literature; but you will see at once of how little value it is for purposes of current work when I tell you that this, which is the last volume bound up to date, is for 1891, and we are now on the verge of 1895. An index that is four years behind the date is hardly likely to be of much use to American engineers, to say the least. It may be of use in Europe, but certainly not here, in the way we like to use indexes. The whole field of an industry, as in electric railroads or alternating currents or electric heating or electrolytic work, may often change within that period, and very materially; and the whole literature you may want to look up in the space of four years is wanting. But, so far as it goes, this is certainly by long odds and altogether the best thing that I know. I have had one or two dabs at indexes myself, and I think I can appreciate a good thing of this kind when I see it. I look at this publication with admiration and awe. A work of this kind, it seems to me, is best done under the auspices and with the assistance of such a society as ours. It is best accomplished when the work is divided up, so that the work as divided, is handled by men who are experts in the several fields. We have had a good many efforts in this direction, and one or two very creditable ones are being made in this country. The *Electrical World*, as you know, has an admirable digest of foreign literature, and Mr. Fred DeLand, of Chicago, has made a good attempt in the direction of literature generally; and *Electric Power*, in this city, has done some excellent work. But, while those attempts are open to commendation, I think we all appreciate the fact that the efforts, while good, fall short of that which an index must be, and must accomplish, to be of absolute and final value as an index. It is a subject that members can well give their attention to. I have myself tried once or twice to bring it within the purview and scope of the Institute's work, and I still believe firmly that it is part of the work that the Institute, in the long run, will have to take a hand in carrying out, whether by itself or in association with other engineering bodies.

The President:—We should be pleased to hear from any member either concerning the paper itself, which we presume you have all read, or any matter referring to the subject of the paper—"The Indexing of Engineering Literature."

Mr. J. Stanford Brown (*communicated*):—Prof. Shepardson's paper touches on a subject of growing importance, and one which, it would seem, might be wisely taken up, as he suggests, co-operatively by the great engineering societies having their headquarters in New York; and I trust the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS will appoint a committee to at least investigate and report.

There is one very important, and, in some respects, the most important, index published in the electrical field which is not mentioned in the paper to-night, namely, the *Fortschritte der Elektrotechnik*, a quarterly begun in 1887 and published in Berlin by Julius Springer. The collaborators are L. Orth, M. Seg-ffert, D. H. Borns, Dr. M. Dolivo-Dobrowolsky, Dr. C. Heim, Dr. Kiliani, G. Grawinkel, Dr. E. Pirani and Dr. A. Koepsel.

The volume for 1887 consists of 840 large 8vo pages with 40 pages of appendix, which includes 4,493 articles, with digests of all the important ones.

Vol. I.	has	4,493 articles.
Vol. II.	has	4,672 articles.
Vol. III.	has	5,213 articles.
Vol. IV.	has	6,000 articles.
Vol, V.	has	5,987 articles.
Or..........		26,365 articles in 5 years.

The rest are not at hand to enable figures to be given. Besides, each of these articles is reprinted, more or less abbreviated, in anywhere from one to twenty periodicals, all of which are noted, with the length of such abstract, so that one can choose how fully he will look any one of them up. Each volume also presents an author's index which, at times, is invaluable.

It would seem, however, more feasible to make use of existing material and work, than to start all over and duplicate, which the professor himself mentions. The drawbacks to the usefulness of the *Fortschritte* are, first, that it is in German, and, second, that it is always a year behind time. Now, why not try and co-operate with these gentlemen, aid them, and get the work published contemporaneously in, say, New York, London, Paris and Berlin, in the monthly journals of the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS, the British Institution of Electrical Engineers and the corresponding French and German societies? This would make the work available to us all. It is a matter of regret that I am not present so as to give some experience in the detail of such work.

For a list of 66 different makes of arc lamps, including three or four references each, see *Electrical Enterprise*, vol. ii., No. 18, p. 359, 31 O '91.

And on 128 makers of primary cells see *Electrical Enterprise*, vol. ii., No. 20, p. 403, 14 N, '91, and vol. ii., No. 22. p. 445, 28 N, '91, both compiled by the writer. These were refused publication in the New York electrical papers at that time. The AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS has now under consideration the publication of several hundred references on "electrical power distribution" collected by the writer and offered by him to the INSTITUTE. I have a number of other electrical lists that I would give to any electrical journal that will promise to publish them at once. These lists have all been made in the course of professional work, and carried only so far as the matter in hand demanded. On some future occasion the art of taking and filing professional notes might, with advantage, be discussed by the INSTITUTE; meantime those interested can refer to "Scientific Note Making" in the New York *Electrical Engineer*, xii., No. 167, p. 56, 15th July, 1891.

At present I am compiling references on the "Steam Engine." I have entered to date some 230 books, giving author, title, publisher, size, binding, date, edition, number of illustrations and price; library catalogue numbers for Columbia College, Brooklyn Library, R. R. Branch, Y. M. C. A., American Society of Mechanical Engineers, etc.—in short, of all important libraries in and around New York City. And also references to articles, including, to date, some 1,200 entries. Should any one be willing to let me consult their lists, I will gladly exchange courtesies.

PROF. W. E. GOLDSBOROUGH (*communicated*):—After consulting with several members of the INSTITUTE I have decided to make the following suggestion, in view of the paper to be presented by Prof. Shepardson on the 21st instant.

Would it not be well for the INSTITUTE to undertake the publication of a card catalogue of current engineering literature, including in its scope not only the leading articles in the engineering journals and notices of the papers presented before the various engineering and scientific societies, but also references, in a digested form, of all new books, publications or other engineering matter of importance? The bundles of cards might be mailed at the end of each month.

In view of the demand of the profession for some ready means of keeping in touch with the rapid advances at present being made in all branches of engineering, which make it almost impossible for one to even spend the time to cull out from the whole mass of matter that which is of interest without attempting to read it, it would seem that such a publication, by responsible parties, would meet with very general approval and support, on the part of scientific men.

An addition of four or five dollars to the present amount of the annual dues would surely cover the expense attached to such a venture, supported as it would be, in the outset, by our whole membership of several hundred; and a thorough canvass of our membership would, I believe, disclose a decided willingness, on the part of a large majority, to endorse such a project.

The journals and books necessary would be furnished by the publishers without doubt, on account of the advertising feature of the catalogue.

MR. MAX OSTERBERG (*communicated*):—Having given the question of indexing a good deal of thought, I wish to make use of this opportune moment to point out a few of the features that present themselves before starting that work.

1. *Index vs. Synopsis.*—The writer believes that a synopsis is preferable to an index, as the title of papers alone does not always imply what the article contains, as Prof. Shepardson has pointed out in his paper.

2. *Weekly or Monthly vs. Annual Synopsis.*—A journal appearing weekly or monthly can easily give a short synopsis of the previous week or month, and, for the benefit of those who wish to have the reports of a whole year, it is but a small matter comparatively to compile, at the end of the year, all the single synopses which have appeared during that time.

3. *Cross-References of Articles.*—It is often an extremely hard question to decide under what heading an article is to be put, and, furthermore, not infrequently a matter of opinion; hence, every article which leaves any room for doubt ought to be indexed under more than one heading, the accompanying synopsis, of course, being only necessary at one place.

4. *Original Articles vs. Trade Notes.*—Trade notes are practically nothing but advertisements; however, there are some trade notes which are of some interest as well as of actual value, so that the compilers are often in doubt as to the advisability of indexing the same. A greater difficulty, however, lies in the fact that many technical journals print trade notes without mentioning that fact, which leaves the compiler in a most embarrassing position.

5. *Giving First Source or All Sources.*—Is it advisable to mention only the first paper which contains an original article or should all papers be referred to which give the article in abstract or in full? The writer is decidedly in favor of the latter method, for two reasons: (*a*) Many papers are issued ahead of their dating; (*b*) the investigator gets the opportunity to look up such papers as are the most convenient for him.

6. *Should the length of the article be given, and, if so, should words or columns be enumerated?* An index should certainly have some indication as to the length of the article; and, the forms and types of the different papers not being alike, the number of words should be given. In a synopsis this is not necessary, as the gist of it is given.

7. *Object of a Synopsis or Index.*—There are three classes of people who make use of them: (*a*) The scientists who do original work and make original researches; they are certainly in the minority, and usually sufficiently posted on their particular line of research to make their own investigations. It seems hardly fair to make societies pay towards an absolutely complete index of every subject, with so many cross-references as conceivable, for the benefit of the few. (*b*) The practical engineer who is

interested in some specialties can look them up in the weekly or monthly digests or synopses, which some papers publish regularly for their special benefit. (*c*) The teachers who have to prepare lectures on many different subjects, and who often find it necessary to investigate certain questions complete. They cannot use a simple synopsis, for, besides the gist of the article, they want to get at the individuality of the writers, especially if they are men of reputation. In this case an index prepared by a scientific society, as was done some years ago by the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS, seems to answer admirably.

MR. CARL HERING (*communicated*):—It seems hardly necessary to endorse the views expressed regarding the importance of such an index, as the very great and growing need for it is recognized only too well by all engineers and scientists who desire to keep abreast of the times. I agree with Prof. Shepardson in most of what he says, but would like to emphasize some of the points and add a few further suggestions.

A mere index, giving only the title of the paper, the author, and the reference to the publication, though of considerable value, is generally conceded to be of far less value than one giving in addition a few lines describing the general nature and scope of the article, as for instance, whether it is of a mathematical nature, or based on experiments, or a mere description of an apparatus, etc.; the importance of this additional information cannot be emphasized too much. Owing to the inaccessibility of many of the publications, I would recommend giving somewhat more than this whenever possible, by adding briefly the conclusions, final data or results, as the case may be, for in many articles certain valuable conclusions are arrived at, which will be of great use to many, while the lengthy process by which they are reached, though of course of great importance also, may be of special interest only to comparatively few persons, and these few will most likely want to see the original in any case, whether it has been abstracted or not. While it would be of value to include very full abstracts of all articles, yet if an index of this sort is attempted on too great a scale it will be more likely to fail, and perhaps lose some of its value by being too bulky; some limit must be fixed, and I think the one just pointed out will in general be the most practical.

Any abstract, no matter how competent may be the one who makes it, necessarily contains some personal element, even though it may be slight; no man is so proficient in all branches that he can abstract all articles equally well, and to him the relative importance of different articles and results may be quite different to what it would be to some one else. It must not be forgotten that there are persons interested in different branches, and they should all be entitled to consideration. To many electricians the peculiar freak of a bolt of lightning for instance, may be a matter of no special interest, or perhaps of no interest whatsoever, yet to the one who is studying the subject of lightning arresters, lightning rods or the action of lightning, the particular article may be of the greatest value. In general, though not without notable exceptions, the author of an article may be supposed to know more about the particular subject matter of the article than the average reader, and it therefore seems to me that the proposition made in England some time ago, is an excellent one; it is to have the author of every paper make a brief abstract himself. This I would suggest should be limited to giving briefly the object, general conclusions, results or final data, and perhaps also very briefly, the method of reasoning by which they are reached. It cannot be emphasized too strongly that the *final results* or *conclusions* should be stated in such an abstract, provided they can be condensed sufficiently.

An index loses very much of its value if not complete, but it is a difficult matter to decide what constitutes completeness. There are published weekly, perhaps a thousand or more separate articles and notes on electrical matters, of which perhaps nine hundred and fifty are of no general or permanent interest whatsoever, being either an article or note of local or passing interest only, such as a note that a certain city is about to consider the erection of a plant,—or perhaps they are little paragraphs technically termed fillers, whose only value is to fill up a column, thereby permitting an important article to start at the top of a page. There is probably no doubt that an index should not be burdened with such matters. On the other hand, to limit an index to the leading articles only, or to the table of contents, is to reduce its value very greatly, as there are many other paragraphs which often contain very valuable and useful matter in the form of abstracts, results, notes, criticisms, suggestions, etc. A systematic index therefore should include all matters likely to be of permanent interest and value, its scope should be clearly defined at the outset, and the ground to be covered should then be covered completely.

PROF. F. B. CROCKER:—The subject of this communication, while not interesting, is nevertheless of great importance to many of us, and particularly to those who are forced to keep up with, or refer back to the literature of electricity. A great many persons are fortunate enough not to be in that position; they carry their knowledge in their heads, and they learn for the most part by actual experience. Nevertheless, electrical literature has got to be followed and referred to by a large proportion of electrical engineers, and each man must either have it done for him, or do it himself. On general principles, it would seem to be far better and a great economy in time, if one or more persons devoted their attention to classifying electrical literature for the benefit of thousands of their fellow engineers. Nevertheless, I think each one of us has a peculiar experience or position which requires him to index and classify electrical literature, to a certain extent, from his own personal point of view; and where these two methods come together, and how much one can take the place of the other is a matter that, I think, will have to be decided in starting an index. In other words, we should not attempt to make an index which would cover every specialist's case. I think that attempt has been made in the past, and for that reason the success has not been very great. I think a general index giving the more important articles, irrespective of their length—and the importance of an article is by no means measured by its length—would dispose of the great bulk of the work of each individual; and any personally interesting article a man would have time to handle according to his own convenience. I think that the great trouble with indexes heretofore has been that they attempted to go too far, and, consequently, have failed entirely. I think the same in regard to individual efforts in this direction; a man starts out with a very elaborate system, which he keeps up for one week or two weeks, or a few months, if he is very persevering, and then he gives it up entirely and does not even take the wrapper off from his electrical papers for the next few months. Now, that is not the way to get along. The only way is to start out with a scheme which is feasible, and which you can continue to carry out indefinitely without taxing your perseverance too greatly. In my own case, I have started out and kept up a scheme which is fairly satisfactory, and it consists in classifying the whole subject so far as I am interested in it—which is, unfortunately, a large part of electrical engineering—into about 200 subdivisions. Each of these classes is represented by one or more large envelopes of stout manila paper, into which I put the various references, etc.; and, if possible, I take the article itself, which may be a pamphlet, photograph or a clipping obtained by cutting up an extra copy of the journal. I make a collection of these as I get them, and then classify them when I have an opportunity; and even that takes as much time as I can possibly give to it. Any more elaborate system than that, I would not be able to pursue at all. That system I have been able to carry out for several years; but I am beginning to think now that I shall have to devise a still easier one. Now, if one person could do this work for 5,000 others the economy in labor would be enormous, and would save hours and hours of work for each individual, provided it could be carried out; and I believe that it can be, and then each man could simply make notes of some particularly interesting article in his particular line; and I think an index which did not attempt to go too far, and simply took care of the bulk of the subject, would be sufficient. And I am not quite sure that it would not be better than one that attempted to cover the entire subject; and, probably, it would be kept up, whereas one that attempted to cover the entire subject would not be. I think the keeping up of the index is a very important point, because one that lasts for a year or two is little better than no index at all. Another trouble with indexes is that they are not kept up to date. That book which I have seen passed about the room—*Fortschritte der Elektrotechnik*—is very full and very satisfactory as an index; but, unfortunately, it is very far behind the times. If I am not mistaken, it is two years behind.

MR. MARTIN:—The last complete volume was for 1891. There are separate parts that are later.

PROF. CROCKER:—Then, apparently, it is about three years behind the times. The plan of it, and the way it has been carried out, seem to be very satisfactory. But it is too far behind, and the abstracts are not altogether satisfactory. They go fully into some particular points, and do not pay enough attention to the general idea of the article. The abstract, I think, is almost an essential feature of any very valuable index, because, from the simple title, even accompanied by the author's name, we cannot tell whether the article is one that we are looking for, or one that we care nothing for; and the briefest abstract of a few words would give us a clue that would save us from looking up nine out of ten references. Those are the principal points that have occurred to me. I, for one, heartily favor some system by which a labor-saving index scheme may be gotten up, so that every one will not have to make an index for himself, which seems to be a very crude and unnecessarily laborious scheme; but that is really the present one.

MR. EDWARD CALDWELL:—The subject of Prof. Shepardson's paper is one of great importance, and one in which I have taken a personal interest for several years. My first attempt at indexing was made some years ago, when I was compelled, in making a report upon a certain engineering subject, to search through hundreds of indexes to trace its literature. In doing this I found so much information on other subjects that I thought might be of value at another time, that I undertook to make an index that would cover the electrical engineering literature of one or two important sets of society publications. The first set indexed—and the last one at that time—was the *Proceedings of the Royal Society* of London, extending from 1800 to 1880, some 45 volumes. From these I got about 300 titles of papers from well-known investigators, among them Joule, Faraday, Maxwell, Siemens, Matthiessen and many others. They are a remarkable series of papers, and the index to them has proved to be a very serviceable one. Among later attempts was a card index covering nearly the entire set of the *Electrical Engineer*, of New York, and some volumes of the *Electrical Review*, of London. My last and somewhat more ambitious work in this line was the making of a card catalogue of the papers on electricity and magnetism, the titles to which are given in the "Royal Society Catalogue of Scientific Papers," the entire set of nine or ten volumes containing about 8,000 such titles arranged alphabetically by authors. These titles are in all languages—Russian, Italian, Spanish, German, French and other continental tongues—and it is not always safe to select the subject from the titles as given in this magnificent catalogue. But, of course, an examination of the original paper for this purpose was impossible. In this way I have indexed three volumes of the set, making a subject catalogue on the "dictionary" plan. A sample entry is the following:—

Gases, Electro-chemical Polarity of. (W. R. Grove). Phil. Trans., 1852, pp. 87–102; Ann. de Chimie, 1858, 37: 376–85; Phil. Mag., 1852, 4: 498–515; Pogg. Ann., 1854, 93: 417–31, 582–94; Roy. Soc. Proc., 1852, 6: 168–9.

I have here a number of the cards taken from the set, which may be examined by those interested. They give, as you will see, the subject, the author, the periodical, the date of publication, the volume and the page or pages.

What has been already said by others on this subject relates principally to indexes to current literature. While I think such an index would be of great value, there is, it seems to me, a more urgent need for a satisfactory guide to the literature that has already found its way into libraries, and is there more or less successfully buried. The need of such an index for this class of periodical literature was well expressed by Lord Rayleigh at the meeting of the British Association in Montreal about ten years ago. In his presidential address[1] he said:

"By a fiction as remarkable as any to be found in law, what has "once been published, even though it be in the Russian language, is spoken of as 'known'; and it is often forgotten that "the rediscovery in the library, may be a more difficult and uncertain process than the first discovery in the laboratory."

In considering what ought to be the character or "construction" of such an index, we ought to keep clearly in mind the objects for which it is made. Using a modified form of the classification given by Mr. Chas. A. Cutter, formerly librarian of the Boston Athenæum, in his "Rules for a Dictionary Catalogue," these objects may be stated to be:—

1. To enable a person to find an article, whether in book, periodical or pamphlet form, of which either

(*a*) The author } is known.
(*b*) The subject }

2. To show what technical literature, whether in book, periodical or pamphlet form, has

(*c*) By a given author.
(*d*) On a given subject.

3. To assist in the choice of an article, whether in book or periodical,

(*e*) As to its length.
(*f*) As to its date of publication.
(*g*) As to its character.

1. See the British Association Report for 1884, page 20.

To fulfil these objects we must have:—

1. An author index for (*a*) and (*c*).

2. A subject index for (*b*) and (*d*), giving in the reference the length (*e*) and the date (*f*) and, in proper annotations, (*g*).

The following might be taken as specimen entries in such subject and author indexes:—

Booster, Use of, on Street Railway Circuits. (J. H. Vail and S. H. Wynkoop). St. Ry. Journal, 1894, 10: 700–702.

Full treatment, with diagrams and formulæ. Figures as to quantity of copper used.

Vail, J. H., and S. H. Wynkoop. Booster, Use of, on Street Railway Circuits.
Wynkoop, S. H. See Vail, J. H., and S. H. Wynkoop.

The title of an article ought never to be taken as the index entry, unless after examination it should also prove to be its subject as well. Articles should be read, if necessary, to ascertain full information as to their subject matter, and if two or more subjects are treated of, there should be a corresponding number of index entries. If an article on a single subject might be looked for under two headings, it should be put under the one where it is most likely to be looked for *first*, and a reference to it inserted under other heads.

The annotations should not be, in any sense, abstracts or digests of the articles themselves. This, it seems to me, is not the purpose of an index, which should be simply to guide a searcher to the literature of the subject and leave him there to make his own abstracts after deciding what part of it suits his purpose. I think that Mr. Hering has hit the nail very squarely on the head, when he says that no matter how conscientious a man may be in making abstracts of scientific articles, the personal element will appear to such an extent, that the abstract or digest, especially when too much condensed, may be useless or misleading to others. He may give undue prominence to some subjects and slight others. The index should not be in any degree a substitute for the original publications. This is the legitimate field of the newspaper and technical journal. Besides this, an index to existing periodical electrical literature, which would undertake to include abstracts of articles, would pass beyond all reasonable bounds in its bulk, and its preparation and publication would be a physical and financial impossibility. Prof. Shepardson has in mind an index to cover the whole range of engineering literature, but we may consider the single branch of electricity and its applications. Anyone who has examined the periodical literature of electricity since the time of Volta and Galvani knows how vast is the amount of recorded information that would be of great value if its existence were only made known, and the records rendered accessible. It is very difficult to make any estimate, but I should say that an index that would go back to the beginning of the century and cover the literature of electricity and its applications, would include, if it came up to the present time, certainly not less than 50,000 titles. It requires no very complicated mathematics to calculate the number of volumes that 50,000 titles with abstracts would make, and the actual labor involved in making satisfactory abstracts or digests of these articles. Mr. Carl Hering, who personally prepares the weekly digest of current electrical literature, which appears in *The Electrical World*, told me, if I remember rightly, that he examines each week about 40 periodicals, or 160 in a month. This occupies the greater part of his time, and he is able to prepare perhaps 40 abstracts, some of them very brief, each week. A man who would give all his time to this work would probably have some difficulty in making 2,500 abstracts in a year.

The partial republication of these original articles in the form of abstracts ought not to be seriously considered in connection with an index to them. Such an undertaking would be a very commendable one as a private and entirely separate enterprise. The importance of going to original sources for information has been referred to by Dr. E. L. Nichols in his recent "Laboratory Manual of Physics." In speaking of the matters to be considered in the training of an advanced student, he says that the student ought to "turn more freely than ever before to original sources for his information, learning to regard all compendiums and treatises as secondary."

Instead of abstracts, the ideal index should contain annotations, which would give, if I may use the expression, the physical and mental dimensions of the article. These might state, for instance, whether the article was a compilation or a record of original investigation or of actual experience; whether it was given in full or in abstract; for what classes of readers it was suited; its noteworthy excellencies, defects or errors; its value compared with other articles on the same subject; whether it treated of a debated question, and if so outlining the author's position.

A good specimen of notes of this character is the following, taken from a little pamphlet by Prof. F. B. Crocker, entitled "Electricity: a selection from its literature":

Crosby, Oscar T., and Bell, Louis: *The Electric Railway in Theory and Practice.* Ed. II., 416 pp., ill. O. N.Y., 1893. Johnston. $2.50.

The most complete and authoritative treatise on the construction and operation of electric railways. The scientific and practical facts involved are clearly stated and explained. For the general reader, the practical and practical advanced student, and the engineer, whether electrical or not.

This pamphlet came into my hands through the kindness of Mr. George Iles, its publisher, who is greatly interested in this subject, and who has done much excellent work in agitating it among the librarians of the country.[1]

With Prof. Shepardson I believe that such an index would be

1 See a paper on "The Evolution of Literature," read by George Iles before the American Library Association, at Lakewood, N. J., May 19, 1892.

improved by making it to include the literature of books and pamphlets, although there is not so much need for this, as for one that will make accessible the buried treasures of periodicals and society publications. It ought not, however, to be issued in parts as he suggests, but should be furnished in bound volumes of suitable size, leaving the subscriber nothing to do in the way of arrangement or classification. I think this is the point, the neglect of which has been the cause of failure in many otherwise well conducted plans for indexes to current literature. The subscriber should have what he pays for, a finished index—not a substitute for the technical papers, but a supplement to them—which does not require the pasting of titles, the assortment of printed pages into their places, or the rearrangement of printed matter of any kind.

The ideal scheme for an index to current literature in my opinion is that proposed by Prof. Parks, who had charge of the "model library" exhibit in the Government Building at the World's Fair last year. In this the annotated titles published each week would include all that had appeared since the beginning of the quarter, so that each issue would include all that had been previously published, together with the new titles for one week. This relieves the subscriber of all scrap-book labor in the preparation of his index, and it seems to me that this is an all-important point. Unfortunately the plan is an expensive one, and Prof. Parks was obliged to give it up because of lack of financial support.[1]

In regard to classification methods, each man has his own[2] and no two can ever agree upon a common plan that possesses any degree of merit. I have tried all sorts of classification schemes, and have given them all up. I am a thorough believer in the "dictionary" plan, especially for general use, following very closely the style of Poole's "Index to Periodical Literature,"[3] which was the result of an extended discussion among the leading authorities on library cataloguing and indexing.[4]

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1. See Prof. Parks' contributed discussion on page 818.

2. For a very elaborate scheme, see a paper on "A General Engineering Classification and Index," by Wm. L. Chase, in vol. xiv. of the *Transactions* of the American Society of Mechanical Engineers.

3. For the rules under which Poole's Index was compiled, see the *Library Journal*, vol. i, p. 286; and the *Proceedings* of the Conference of Librarians, at London, October, 1877, p. 199.

4. Those who wish to follow up this discussion should consult: "The Plan of the New Poole's Index: A Library Symposium," *Library Journal*, vol. iii., p. 141; "The Index Symposium and Its Moral," by W. F. Poole; *Library Journal*, vol. iii., p. 178; "Some Points in Indexing," by W. I. Fletcher; *Library Journal*, vol. iv., p. 243; and "Classification in Dictionary Catalogues, *Library Journal*, vol. iv., p. 226. For a history of catalogue making, and a criticism of the different kinds of catalogues, see an article on "Library Catalogues" in a Special Report on public libraries, issued at Washington in 1876 by the Bureau of Education. Part II of this Special Report contains Mr. C. A. Cutter's "Rules for a Dictionary Catalogue," which has also been published separately in revised form, and may be had upon application to the department.

No member of this society who gives the subject careful consideration will dispute the statement that such an index is necessary. The only questions that come up for consideration are, "Can it be made?" and "What, if any, is the best plan?" These are questions, it seems to me, for the INSTITUTE to decide. I think it is an undertaking that comes well within the scope of the INSTITUTE work as an association for the advancement of electricity as a science. There is no other body of electrical engineers in the country that is progressive enough to do it, or that has the numerical strength of this organization. I agree with Prof. Shepardson that a committee ought to be appointed by the council to canvass the subject thoroughly, and report upon the advisability of undertaking the work, and, if it were thought best, along what lines to proceed. A set of questions, something like the following, which were prepared for this very purpose nearly a year ago, might be submitted to those whose opinions were desired:—

1. Is there, in your opinion, any need of an index to the periodical literature of electricity ?

2. What period should it cover ?

3. Should other than English and American publications be indexed ?

4. Should the contents of books be indexed ?

5. Should it be an author or subject index, or both ?

6. Can such a work, in your opinion, be satisfactorily done by twenty or thirty individual compilers working under a prearranged set of rules, the work of all to be passed upon, rearranged and combined by a committee of one or more supervising editors ?

7. What would you suggest as the best plan of organization for a staff of compilers ?

8. To what extent is periodical electrical literature represented in your library?

9. Would you personally be willing to undertake any part of such a work ?

If favorably reported upon, this committee should be given authority to select compilers, who would work under fixed rules, and, of course, without compensation; assign the work to each, and appoint a supervising editor, who would have authority to pass upon, rearrange and edit the entire work.

I have here for inspection a number of indexes which are more or less familiar, including Poole's, Galloupe's, Prof. J. B. Johnson's, the Boston Public Library list, the Ronalds' catalogue, Prof. Crocker's little pamphlet, and some others to which I have referred. The Boston Public Library list, aside from a number of typographical errors, is an excellent one, containing as it does, references to all papers on electricity and magnetism in the *Transactions of the Royal Society* of London from 1665 to 1892, a period of 227 years, covered by about 190 volumes. I have here also an annotated catalogue of some 60 lists of books and indexes to articles on electricity and magnetism—in other words, guides to the literature of these subjects. This list includes all of those mentioned by Prof. Shepardson and many others. The list is too long for me to read at this time, but it may be examined by those interested.

In conclusion, I would simply add that I think such an index as we have been discussing should not be made for the scientist, or the engineer, or the editor, or the student alone, but should be planned on lines broad enough to suit the needs of all these, as well as other classes of possible users.

THE SECRETARY:—As the index formerly published by the INSTITUTE has been referred to, it may be of interest, at least to some of the more recently elected members to know why the work was undertaken and why it was suspended. The matter was brought up in Council about seven years ago, by Mr. Mailloux, and the proposition at that time was that it should be done by the Secretary. I had never done any indexing of the kind and that was one reason why I did not undertake it. I have been very glad since that I did not assume the responsibility. However, we had a member, who kindly undertook the duty and it was carried on for some time, until I think it became too much of a burden for him, and it was dropped. I do not know whether at that time I had heard of the experience of two Philadelphia editors. Perhaps Prof. Houston may know something of it. They were equal partners in a daily paper, and one of them had secured the advance sheets of a serial novel which he proposed to publish in daily installments. The editors did not agree upon the question as to whether their readers wanted this kind of mental pabulum, and after it had gone on a week or so, they had a little tiff as to the expense, and whether the people wanted it, and finally they compromised, the editor who objected to it saying, "Let us drop it out, and if we have a complaint from any of the subscribers that we have not finished the story, why I will submit to your idea and we will go on and publish the story to its conclusion." So they actually dropped it, and they waited a week or two and not hearing a word from anybody, they concluded there was no general interest in the subject. I felt that way about our index. At that time, of course, we had only three or four hundred members and perhaps the need of an index was not appreciated at that time as much as it is to-day. I might add incidentally also, that the proposition was made that the index be printed on one side of the leaf only, so that members who desired could cut it up for their card catalogues. Of course that would add considerably to the expense when we came to print 500 copies, and I cogitated over it a little, and suggested another plan: that if any member wished to cut it up for a card catalogue, we would supply him with an extra copy for that purpose. I thought that would be cheaper than to run a whole edition printed on one side. We had three applications for an extra copy to cut up, and those were furnished to the members who asked for them. I mention this because the printing on one side is suggested by Prof. Shepardson, but it is proposed to apply a little salve in the shape of advertisements, on the other side that would perhaps pay the extra cost. But what I was going to say was, that taking this into consideration, and also the experience of the Philadelphia editors, I thought I would wait and see if anybody complained because we did not continue the index. I believe no inquiries were made. Possibly the members thought it was all right, and that we had good reasons for dropping it. Later this subject has been brought up, and my advice has been asked by one of the editors who was referred to to-night, who inquired what I thought of such a publication as a commercial venture. I told him that my experience was that there were a few people who wanted an index of this kind, and wanted it very badly. But I didn't think they wanted it sufficiently to pay the expense of getting it up. That is to say, there was a limited demand and perhaps that limited number who wanted it would be willing to pay a pretty fair price, but, at the same time, they would be willing to pay but a small proportion of what it actually cost to compile and print, and that is one reason why it has not been a successful financial undertaking. There were some other points about it that I brought up at the same time. One was this, that there are a great many engineers, not only in the branch of electrical engineering, but other branches as well, who compile their own indexes and have their own ideas about what they wish a catalogue to be, and, therefore an index compiled by others, unless it was thoroughly complete, might not satisfy their wants. That is to say, each man feels that if he runs the index himself, he would know exactly what he wanted, and he could discard what he did not want, and a man who was very thorough and very particular would not have that confidence in the work of others which would lead him to throw up his own, and take that of somebody else, especially if he doubted its permanence.

There is another point about an index of this kind which is worthy of consideration, and that is that, like some other things, it improves by age. An index that is only a few days old does not amount to anything; but, after it has been kept up for five or ten years, it begins to become of greater value, and, if the man who undertakes it is patient enough, he may arrive at that time in the course of his life when he will meet with his reward. But the difficulty is that few of us are willing to keep on till that time. We get tired, as some of our friends have confessed to-night, with shuffling these cards and going over these papers. Perhaps my early experience in making literary scrapbooks was one of the reasons why I refrained from taking up indexing. Those scrapbooks are something over twenty years old now, and occasionally when I do not feel first rate I get out one of those scrapbooks, and am thankful that I was so industrious in my younger days.

Then, again, an index for the public, as it were, should, of course, be general and cover all branches. It would be necessary, I suppose, to classify it in some way; but, in order to meet the general market, it would have to cover all branches of electrical engineering, at least. I ran across, in the English paper called *Industry*, a few years ago, a reference to the first use of electric welding by Siemens in the Siemens factory in welding the iron wires of an armored cable, and this reference spoke of its coming up in a discussion before the Iron and Steel Institute in 1887. The date was given, and all the particulars about this use of the process; and I thought it was a very interesting fact. So I went on a search for that and found the discussion at the very meeting of the Iron and Steel Institute, but could not find a word about electric welding. Now, a good index, I presume, would have brought that to my attention; but I could not find it.

I do not have as many papers to index as Mr. Hering. I have about 110 in the office each month, most of them weekly. I do not index them, excepting as Mark Twain, I think it is, who indexes such things in his brain and has his head full of imaginary pigeonholes, that he mentally puts them into. I index in this way for the benefit of members who come in and say, "Have you seen anything of such an article?" And I think it over and reply, "Yes, I have seen it," and I run over this assortment of a hundred pigeonholes in my head, to try and find out in which particular paper it was that I read it. Well, sometimes I succeed. It is, at least, some satisfaction to know that I have seen it; but, when a man comes in and says that he has written an article that appeared in such a paper and asks me if I have read it, then I weaken. Because, after reading our TRANSACTIONS through three times each issue, and looking over these papers and finding out what is in them, just glancing at the titles and authors, and reading the regular daily paper, to say nothing of the Sunday edition, why, it leaves me very little time to digest these various articles.

But, coming down to the work of the INSTITUTE, there is one point I desire to call your attention to, which is sometimes overlooked. Associations of this kind exist for doing a certain class of work that is of interest and value to the members, but which will not be undertaken by any person or firm as a commercial venture. This question has come up many times in regard to certain papers that have been published in the TRANSACTIONS, papers that are of value as works of reference and which many of our industrious editors abstract or squeeze out altogether, but which some one *should* publish; and it appears to me that that is one of the missions of a technical society—to do work in a co-operative way which is of benefit, perhaps not to all the members, but to a few of the members; and the few that are not benefited this time may be benefited next month or next year. So that the mere fact that work of this kind is not financially profitable should not debar the INSTITUTE from undertaking it if it is considered of value to the membership.

The question of co-operation with other societies was brought up by Prof. Shepardson, which is worthy of some attention. My experience has been that it would not be worth while to undertake to co-operate in this way. I believe that we should go on with our own work, and keep as closely to the line of electrical engineering as possible. Of course, we must wander over into the line of mechanical engineering and steam engineering, to a certain extent. But, in undertaking it, I think that it might be well to propose a system which would meet with the approbation of the other societies, so that, in case they should undertake something of the same kind, the work might be conducted on the same system, so that any person who is interested in all branches of engineering could combine the three as systematically as possible.

Some of the cases of cross-references and indexing under different subjects brought up by the gentleman who spoke last, reminds me that this classification in English would be a comparatively simple matter compared to the Arabic. In one of my expeditions through the Midway Plaisance last year I was informed by an intelligent Arab who had spent ten years at a university in learning his own vernacular that in the Arabic language there were 1,400 different words for camel and 1,200 for sword, so that the work of indexing with us could hardy be considered burdensome in comparison.

THE PRESIDENT:—In common with all students, I have personally experienced, for the want of a properly arranged index, no little inconvenience in pursuing my studies as to the state of some art in unearthing, as it were, the records of the past. Indeed, the weakness of the human mind is, perhaps, more thoroughly illustrated in the preparation of an index, than in anything else. Take, for example, any serial publication that has been in existence for half a century or so, and has, therefore, passed under the hands of successive editors. Suppose you are compelled to search through the index of such a book. You will find, after having thoroughly mastered the method in which the subject is indexed, and can readily put your finger on the part of the volume where the index is placed, that the editor, as if possessed by the Evil One, suddenly turns a mental somersault and rearranges the entire method of indexing, thus putting you to considerable trouble in studying the method all over again. And then, possibly, another editor comes in and returns to the first plan, with modifications. Now, an index, to be of value, should be uniform throughout. It should all be treated in the same way. The index of a book should always be inserted in the same part of the book, and should treat the subject in the same manner.

I have had considerable experience in the preparation of indexes since I was a lad. I have always believed in the systematic arrangement of information—information placed where you can readily put your finger on it. The *Journal of the Franklin Institute*, a book of the type I referred to a moment ago, has passed through many different hands, and in its indexes exhibits an exasperating want of uniformity of arrangement. Some time ago, however, the *Institute* thought it wise to rearrange all these separate indexes under one general index, and they have been able to carry out this work in a fairly good way.

The average index of the average book is almost worthless. I remember a story which I will repeat, since our Secretary has started the idea of telling stories, of an index in a book under this caption: " Earl of Chatham, his great mind, page 99 " ; and. on turning to page 99, all that was found about the Earl of Chatham's great mind was that he had said, " He had a great mind to pursue this line of policy to the end." (Laughter.)

As regards the method of arranging indexes, I thoroughly agree as to the importance of a full classification. But, at the same time, I would urge those who are thinking of making extended indexes, not to fall into the error into which the French have fallen by the too admirable classification adopted for the indexing of their system of patents. I yet have to meet a man of intelligence who thoroughly understands the classification adopted in the copious indexing of the French patents, where he is able to turn at once to the number of the patent, to the page, or even the volume in which the information desired can be found. For this index is essentially polyglot, at least so far as it reflects a number of different minds that have been instrumental in getting it up. Not only is it classified, but sub-classified; and the sub-classification is carried to such an extent that, as already mentioned, I never yet have met anyone who actually could tell me from the index, to what volume he should go to find the patent he is looking for. This, I take it, will be one of the difficulties with any plan of making an index, not only of periodical or current literature, but also of the literature of the past, which I agree with the gentleman who has spoken, in regarding as even more important than current literature. Such a work is not one that any single individual should undertake. It is essentially a work of co-operation. But if this index is to be of any value at all, then one mind should edit all that a number of different collaborators prepare; and unless that one mind is a vigorous, broad mind, the index will be probably as bad as the index of the French patents I have alluded to.

Take another work of the same character, say the synopsis of the British patents. You all know how essential it is in patent suits, in the event of a patent containing such a full, accurate and clear description as would enable one skilled in the art pertaining thereto to make and use the same, to prove that it does not constitute an act of invention anyhow, since the matter claimed to be disclosed, was clearly anticipated by a prior publication. You want to find whether such a thing was previously published. You go to the synopsis of the British patents. By an exceedingly stupid way they have in England of giving the patent to the attorney and not to the inventor, if you do not happen to know, as you hardly can be expected to know, the attorney who took out the British patent,—you will find great difficulty. You will find the names of the attorneys all indexed—Lake, for example. Lake appears to have been a very wonderful inventor, to a person who does not know this system.

In almost all the indexes that I have ever looked at there is a lack of uniformity. In an author's index, the author is too often indexed under his last name. That might be an admirable plan if he happens to represent the great and only individual of that name. But suppose you look in an index of this character for a comparatively general name say Thomson—plain simple Thomson, unadorned by a "p." You cannot tell whether the omission of the "p" is not a mistake in spelling, and whether, therefore, you should not look under Thompson. My old colleague's name is, in some regions, almost as frequently spelled with the "p" as without it. Such an index is comparatively useless, if there is any possibility of several persons of the same name being confounded.

Another difficulty which may come up, after you have determined what shall be the head under which an article is to be placed, is the name of the publication in which it appears. Now this appears to be a very simple matter, but unfortunately different publications may have similar names; or a publication may continue for a little while under a given name, and then get into the hands of a new editor who thinks to distinguish himself, not only by upsetting all the preceding editor's work in the line of indexes. but also by changing or slightly changing the name, and this will continue, perhaps for five or six years, until another editor will come in, and restore the old name. Or, you may get the right name in the index, but the volume number is given wrong. Now, by a very pernicious system, in the publication of a book every now and then, a new editor or a new management calmly changes the volume number, so that you have to remember, when speaking of volume 1, you do not mean the original volume 1, but the second volume 1. I have known periodical literature to go on until there were three or four volume 1's,. Such things require to be very carefully considered in the preparation of an index.

I do not at all agree with the remarks that have been made, expressing doubt as to whether or not the reading public want a good index. I think there can be no question about this. The question is not whether a man wants an index in scientific literature; he must have one. It is true that we cannot judge of the value of an index simply by the number of people who want it; for, it is unfortunately true for the weal of the world that comparatively few people can want, or have put themselves in a position that they are able to want to know all that has been written on a certain subject. But I contend that a carefully prepared index put in the hands of such people that not only do want it, but are able to want it, will be of great value to the scientific progress of the world. For such an index means that good efforts shall not be misspent in doing work over again that has been better done before.

Therefore, I trust that the society will seriously consider the advisability of preparing an index of electrical literature. What we want is an electrical index. Our legal friends understand thoroughly the value of an index in legal lore. To-day a man can readily obtain, or he can send his clerk to obtain, information which will give him an insight into almost all the decisions that have been rendered on almost any topic, in one or another of the admirably prepared indexes of different cases that have been argued in different parts of the world. Our medical friends are much better off than we are, though they are not, perhaps, as far advanced as the legal profession. It seems to me that in the exceedingly active field of electrical science, where ideas grow with almost mushroom-like rapidity, that an immense benefit to the profession would be afforded by the preparation of a careful digest of all that has ever been written in electricity. It is a tremendous work, I know. I believe, as I said before, that it ought to be a work undertaken by experts in the different branches of the science. But in order to give the index the uniformity that will enable the best good to be readily derived from it, the work prepared by the many collaborators should be prepared for publication by a level-headed editor with power to render everything uniform.

I have jotted down, during the discussion, some of the points that I think should characterize such an index. Briefly, they are as follows:

1. Such an index should be based on a carefully considered general classification, under which the subject matter to be indexed is arranged. This classification ought not to be too prolix.

2. A strict adherence to this classification should be maintained throughout the entire index.

Just here a difficulty will probably arise. Hindsight is invariably better than foresight. No matter how carefully the classification is arranged, before long it will become evident that it can be improved; but if the index is to be worth anything, the desire to improve by changing the classification or arrangement, must be resisted.

3. As nearly as possible the same order of presentation of all

matter to be indexed should be followed throughout. This can be done only by putting the work of the collaborators in the hands of an editor-in-chief.

4. As long a time as possible should be included in the same index. This I regard as very important. Although, almost impossible fully to attain, it should be attained as nearly as possible. An index covering a quarter of a year is better than one only covering two weeks or two months; one covering half a year is better than one covering only one quarter, and three-quarters of a year is still better. But an index to be of the greatest value in 1895, should be an alphabetical index of everything that has ever been written up to 1895. If we have separate volumes, it takes too much of one's time to read through the separate indexes.

5. There should as nearly as possible be an avoidance of unduly magnifying the importance of one subject over another. This can be avoided only by an editor-in-chief.

6. Precise information should be given as to the exact publication in which the work is to be found. Some indexes make this too brief; or use contractions which unfortunately may apply to several different purposes; or, if they give the name of the publication in full, do not state whether it is the first vol. 1, or series 2, vol. 1, or series 3, vol. 1, or series 4 or 5, vol. 1, and you do not find what you expect to find, as Mr. Pope discovered when he tried to do it.

I think this covers all I wish to say, except that I hope our Secretary will resume the indexing of our publications. I have had within the last few months considerable difficulty in finding a paper that I wanted to find, and should have readily been able to find if we had even an ordinary index. I trust, gentlemen, that you will seriously consider the advisability of the society undertaking a work of this kind. We can do a good work for electrical science if we undertake it

THE SECRETARY:—While Mr. Caldwell was speaking, I figured out as nearly as I could from the information he gave us, the number of volumes required to contain the 50,000 titles he referred to. He suggested there should be no synopsis, but simply a sufficient guide that the searcher might find the article. Twenty titles on a page would be 2,500 pages, and 500 pages to the volume would require only five volumes to contain those 50,000 titles. Of course, if one page contained but ten titles, it would be double the number, making ten volumes.

MR. MARTIN:—Our old index contained about 5,000 titles in a year and did not run anything like that.

[COMMUNICATIONS RECEIVED AFTER ADJOURNMENT.]

PROF. WM. S. ALDRICH:—(*communicated.*) To index will require a recognized classification of engineering literature; this appears to be the greatest difficulty in the way of any project for a general engineering index. The classification and the index founded upon it, require to be varied to suit the conditions of their use. Thus, the Dewey Decimal System, while admirably adapted to the indexing of a general library along Mr. Dewey's line of decimal sub-divisions and a somewhat forced technical classification, fails entirely to satisfy even the lay reader pursuing any special line of research in technical literature. The decimal system is entirely satisfactory for any engineering classification and future indexing that may be founded upon it. But, it may be arranged along lines which are as rigid and mechanical as the alphabetical system which it seeks to replace; or, it may be developed in a perfectly natural and therefore logical order, and carried to any desired extent,—two requisites of a classification which it is impossible for the alphabetical system to realize. These features of the decimal system have recommended it almost exclusively for individual use in classifying private collections of books, journals, periodicals, clippings, technical data, memoranda, etc. Each individual appears to classify and index his branch of technical work according to his own ideas and particular requirements. It is this capacity for infinite detail which may exclude the logically-developed decimal classification from ever becoming universally adopted. The general public want some external, mechanical classification, easily remembered; or, better, not requiring to be remembered at all.

The valuable references given by Prof. Shepardson to the many periodicals containing a more or less satisfactory index of current technical literature are, without exception, founded upon the alphabetical system, with its cumbersome cross-references, if any; and, a very great probability that the title is altogether unknown. One knows very well what one wants to find, when he wants to find it; but, he would need no index if he remembered the titles of all the articles, papers, books, etc, he may have had brought before him in a longer or shorter professional career. Some exceedingly valuable technical papers have very misleading alphabetical titles; add to this the weariness to the flesh which Prof. Shepardson points out as the natural concomitant of "searching the various indexes volume by volume," and it is no wonder that busy professional men so often misdirect their efforts, through not knowing just what others have done along some special line of work. What he wants is a "subject-matter index," not an index of titles or authors, as required by a general library. He wants an up-to-date index of subject matter of a special line of technical work,—an index of the information obtainable on a given subject, irrespective of titles and authors.

Engineering knowledge must develop before it can be classified. Yet, while it is developing it should be arranged in an orderly manner and properly classified at once. We wish it so brought to our doors, whether as periodical or book publications; so that herein lies another difficulty of a general adoption of any engineering classification,—it cannot well anticipate additions to our knowledge. What shall be done with the ever increasing volume of the unclassifiable? One has but to watch the yearly growth of any well-known technical index, to see the list of "miscellaneous" steadily lengthen out, till a new heading for the ensuing year absorbs some of that which was formerly "miscellaneous." This method of extension does not promote research into the literature of a technical subject whenever it becomes necessary. However, at the present day, enough has already been accomplished to enable one to anticipate the most probable lines of future development, at least in his own specialty,—if not to conform this development to existing lines.

While we are waiting for some concerted action by the engineering societies regarding the adoption of a general engineering classification, I think we should all be glad to see some enterprising publisher adopt Prof. Shepardson's suggestions as to publishing monthly, or oftener, a separate index sheet (or card of standard library size), containing, with reference to any one article, all of the information which he has suggested. If these were ruled, or otherwise divided like the standard cards of library lists, with blank spaces in the upper right and left hand corner, it would be possible for individuals to number and arrange them in card catalogue cases according to their own classifications. I think it is the general experience of all those who have done anything at all in this line, that they can more readily handle their own system of classification than that of another. What they want is the subject-matter in printed shape to handle. The index of the *Journal of the Association of Engineering Societies*, though having all of the desirable qualities noted by Prof. Shepardson, is still not in shape for putting all articles of one kind together without cutting and pasting, *ad libitum*. Each and every article should come to the subscriber, completely indexed by itself. He can then do what he wants to with it. As has been noted in the papers, this admits of all the past being similarly indexed, sold and filed by individuals or societies.

There is a suggestion I should like to add to those given by Prof. Shepardson, were it not that I fear that it is farther from being realized than any he has noted. It is to encourage the hearty co-operation of publishers of all engineering journals and periodicals, to establish the precedent of sending out to their subscribers, with each issue, a card catalogue of the table of contents. To get all these of a standard size will be the difficulty; that granted, the card contents should possess all of the special features noted in the paper; to which might be added a general or serial filing number, for the particular periodical issuing the same, and liberal blank space in the upper right and left hand corners for the individual's classification and filing number.

PROF. C. WELLMAN PARKS:—(*communicated.*) Circumstances prevented me from attending the meeting of the INSTITUTE which was held this week, and I regret it very much for I take a great interest in everything which is said that has a tendency to advance the time when one will be able to keep up with the current literature on any subject, and yet have time to do some good, either as an instructor or as a practicing engineer. Until somebody provides a reliable index which can be used easily. one must devote his time either to reading or to practicing, and neither occupation is wholly satisfactory. While teaching in the Troy Polytechnic, I felt so strongly the need of a proper index, that I was led to give much attention to this subject, and believe that under proper conditions such an index can be prepared and issued. I will not say that it can be made self-supporting, but I believe that it can, for it should have a clientage of at least fifteen thousand users. Its success depends to a great extent upon the subscription price as well as upon its form. Its subscription price depends upon cost of production and upon advertising. A very serious item in cost of production is cost of composition. Recently invented machinery which will probably be ready for the market within a short time will probably reduce the cost of composition so much that the index can be sold at a low price, if too much is not paid for indexing and editorial work. Indexing need not cost much if one is willing to use the unpaid services of co-laborers, but such a system means delay in issuing the index.

I believe that the index should be issued almost as soon as the periodicals indexed, and that each week's issue should index all of the articles published during the current quarter. At the end of six. nine, and twelve months, single volumes should be made to index every article of current year up to the date of publication. This method makes it unnecessary to consult more than two lists for all of the articles of the current year. Such a method would be expensive if one were obliged to pay for recomposition, but it would be necessary to pay for new matter only. Again, if regular type were used, the cost of the type would be very great.

Considerable attention given to book classification has convinced me that no system yet published is good for all libraries, and I am sure that no one of them is as convenient as the ordinary alphabetical list in which books and articles appear under title, subject, and author. But the latter system is more expensive to print, for it is more bulky. I believe its greater convenience makes it really cheaper, so would advise its use.

The proper place for the preparation of such an index is either New York or Chicago. The former because it is the gateway through which all foreign publications reach America, it is the publication center of America, and it is the center of a very large number of scientists and engineers who are near enough to be good co-operators. The special advantage of Chicago is the greater ease with which money can be secured for projects which promise either fame or pecuniary return. It is also much nearer the center of population of the country.

My first impression was that this work should be done by one of our great universities, but a reason given against this seems sufficient. Now, I think that it should be done by an association of all the scientific and engineering societies, universities and scientific schools, and periodicals near whatever place is chosen for the work. The plan of the *Cosmopolitan Magazine* should be followed, that is, an office in the city, and a factory in the neighboring country.

[Adjourned.]

Discussion in Chicago, Nov. 21st, 1894.

[Mr. L. L. Summers in the Chair.]

A meeting of the western members of the Institute was held in the Physics Lecture Room of Armour Institute. In the absence of the Local Honorary Secretary, Mr. B. J. Arnold, the meeting was called to order by Professor Stine, about thirty members and visitors being in attendance. Upon motion, Mr. L. L. Summers was appointed Chairman. The paper of the evening, "Indexing Electrical Literature," by Professor Shepardson was read in detail by Mr. Fred. DeLand, at the author's request.

Mr. Fred. DeLand:—Prof. Shepardson's paper has very clearly set forth the pressing need of a comprehensive index to the engineering literature of the past as well as the present, to the end that valuable time now spent in vain searching may be saved, and the duplication of unnecessary and expensive work avoided; has outlined the spasmodic efforts put forth from time to time; and has shown how easily a grand work may be accomplished by systematic co-operation. Thus it remains for us to discuss how all interested parties can best act as a unit in accomplishing the desired end.

How can a proper synopsis of all important articles in current publications be most easily obtained? I believe that a concerted effort should be made to have appear in each issue of every technical publication, a brief synopsis of the important articles arranged after the plan shown on the sheets handed to you. At first thought this may seem impracticable, but it will only require a trial in a few issues to demonstrate the value of it both as a time saver, and a seller of the periodical. How greatly it would simplify the work of indexing must be obvious to all. I have occasionally followed this plan in the publication of my magazine, and found that it proved very useful to readers, especially to news-stand buyers, and of some profit to the publisher through increased sales. Every writer for the technical press should be encouraged to send with his manuscript a brief summary of its contents, not exceeding forty words. This the publisher should have arranged something after the following example, and should place it in that portion of the paper that may be scissored without destroying its value for binding:

The Motion of the Ether near the Earth. By Oliver Lodge. Proceedings Royal Institution, Great Britain, No. 86, 1893. Believes the ether does not move; if moving matter disturbs ether in its neighborhood at all, it does so by some minute action, comparable in amount perhaps to gravitation, and possibly by means of the same property as that to which gravitation is due. Not by anything that can fairly be likened to ethereal viscosity. 16p., 10 i.

I propose to follow this plan in coming numbers of *Electrical Engineering*, using a four inch column, bold-face brevier type for title, and nonpareil for author's name, name of publication, and summary of contents, as above. By adopting this size of column it will enable card index owners to clip and paste on cards, or the slips may be pasted in scrap-books suitably divided, though the card index will probably be found far the most satisfactory. If a majority of publishers of technical journals will only agree to follow a similar plan, it will establish a nucleus for a magnificent index, and one that will prove invaluable to theoretical and practical engineers in the very near future.

PROF. W. M. STINE:—I have read the paper with considerable interest, and possibly can connect it to some extent with a matter in which I was interested some time ago. Mr. Caldwell, who has removed to New York, and myself, some months since discussed with each other the feasibility of the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS taking up the subject of indexing current periodical literature. It is almost needless to discuss the demand for this, and it seems but a question of ways and means. We came to no definite conclusion further than that I believe Mr. Caldwell sent out some 200 letters in connection with it. He left for New York soon after, and I have lost track of the results of his efforts.

I feel very grateful to the author for bringing the matter to a focus. I have had quite a little experience in this connection in the conducting of a private index, in fact I can scarcely remember a time when the necessity for indexing was not forced upon me. I recognized very early that to become a good student, I must have command of not only the current periodicals, but also of crystallized literature. I made a study of a few forms of indexes that were on the market—blank forms rather—and from them in the manner suggested, I constructed one which I have followed for a number of years with considerable success as far as my own efforts were concerned. Mr. Caldwell informed me that his experience was somewhat similar, that he had even gone further and had employed the services of a stenographer, and had made a brave attempt to form an extensive index. He, however, was forced to give it up, and it is not too much to say that others have been brought to the same conclusion.

It is a matter of considerable interest to me to know what action will result from this paper. The paper before us is a timely one. It brings to a focus a general feeling that some means be taken at once, to enable the student or the busy engineer to compass the whole field of thought and investigation on his subject and to be permanently master of the matter by being able to refer to it when occasion demands. The prevailing tendency of our time is toward highest efficiencies in all lines. The steam engine within its limits, electrical devices and machinery, manufacturing processes one and all, are receiving the deepest thought, to make them approximate as closely as possible to the ideal efficiency of 100 per cent. But, strange as it may seem, very little attention has been paid to the human mind and working hours at one's disposal, to enable it to work under conditions of maximum efficiency. It may be that human talent and time are so inexhaustible that from their great abundance, the need of economy, except in cases of individuals is not so apparent. But sporadic attempts in this direction are without doubt giving place to well directed effort, and in no case is this better illustrated than in the consideration of our subject. The enormous activity of thought and investigation has produced a mass of matter utterly beyond the power of any one to compass, even in narrow lines. Like a great business concern, the electrical work must call a halt, and take an inventory to prevent needless waste, and the constant transfer of the same article from debit to credit pages. The relation of cause and effect, and the workings of the human mind are so similar, that the investigator or writer, if he works without reference to what has already been done, will develop the subject, and find on subsequent search that he has done just what others have done before him. It is one of the most singular facts in the action of the human mind that the development of a subject is consecutive and to a great extent independent of the worker. Though so much has already been accomplished, it is but a tithe of what still remains, and it is to be earnestly desired that means be at hand to prevent waste of time by doing what has already been accomplished. But all this is apparent and well understood. There is no need to enter into further detail. It is an absolute necessity to provide some suitable means for utilizing the great mass of facts and wealth of thought on electrical subjects. The question is only one of ways and means to this end.

As the paper intimates, some of us have made attempts at individual references. Some years since I elaborated a system of indexing of the books and periodicals in my possession. I made it a rule not to read either book or periodical which was not worth indexing, but the task is so herculean that it is fast becoming impossible to perform. I feel more and more that the labor spent upon it is unproductive. Not that the index in itself does not repay me, but realizing I am only doing what others are engaged upon, I believe that the subject is one for co-operative rather than individual effort. On the other hand, where I have failed to index, the time spent in searching for the desired article has been annoying and practically wasted.

Our libraries, with their array of bound volumes of periodicals, memoirs, and treatises are practically sealed to all but a favored few, who are so situated that they may take the time necessary to familiarize themselves with their contents.

A universal language would certainly prove a vast boon to the world in saving time and effort. I look upon an index of past and current thought as one of the grandest forms of universal language that can be placed in the hands of the scientific man.

An index to be valuable must be evolved from the expressed needs and opinions of the many. This paper will prove most valuable as a means for bringing out various opinions, and lead to giving precise detail to the varying needs of widely different classes of workers, even should it not directly result in realization.

Realizing the importance of a full expression of opinion on this subject I have endeavored to formulate my opinion of what an index should be, in order to place it alongside of that of others engaged in different lines.

I cannot agree with Professor Shepardson in his opinion that both the periodical and literature index be combined in one. This would render the matter unwieldy and eventually defeat the project. I believe that the two should be kept separate and distinct. It still remains to be proven that sufficient co-operation can be secured to render an index possible. Better attempt only one phase at a time until the proper experience is gained, and the public educated up to its support. By all means let the periodical index be the first attempted. Periodical literature is always more or less chaotic. Books may be considered as crystallizations of this literature, and amount in volume to a small fraction of it. The student is still able to handle his books readily, though it may be impossible for him to deal with current writings. If all electrical literature appeared only in the strictly electrical publications, the difficulty of keeping abreast with it would not be so great. But the subject is receiving such widespread attention, that electrical articles of value appear in all classes of periodicals. To render the index then of the greatest value, the entire field of periodical publication must be constantly gone over. This is the scope of the work.

What form should this index take? An index by mere title as has here been pointed out is of little value. The title and author being given, this should be supplemented by as few words of description as possible. The indexer often pays too little attention to the intelligence of his readers, and is needlessly explanatory. In most cases the mere names of subjects treated will prove sufficient. The number of pages of words which the article contains is scarcely worth the trouble and space which it requires. I would in no case advocate abstracts unless they are absolutely necessary. It is needless to seek by an abstract to give the information which the article itself should convey. In order to index an article properly it must be carefully read and the subject matter noted. The subject matter so far as possible in mere mention can then be incorporated with its title, and also used for cross-reference. The feature of cross-references should be as carefully worked out as that of the leading subjects of the articles themselves.

But we must also recognize that in an index we may load the camel until the breaking of its back. Because the subject of electricity has associated with it nearly every other science, and avenue of human activity, many people imagine that an index of electrical literature should embrace references to all other subjects directly or remotely affecting it. A line must be drawn or an electrical index will become impossible. Let steam engines, die-presses, etc., be cared for in their own appropriate indexes so as to keep the subjects within workable limits. I see no necessity for catering to the demands of partially associated interests.

I also desire to offer some suggestions on matters of detail. Economy of space and time dictates that the matter of the index be as concise as possible. Many current indexes use abbreviations for the names of periodicals which necessitates a key. The name in full and date, though desirable, is by no means essential. I have come to regard a numeral notation as best adapted to this work. Let each periodical be assigned a specific number. Then the index should conclude with the periodical number in heavy bold-faced type; the date in italics; the page in numerals of the face of type employed in the index. For bound volumes the numerical key might be repeated on every even hundredth page, and if issued in parts, the key should be included in each. The index should be issued monthly or quarterly and be also obtainable in yearly volumes.

The same considerations apply to the means for publishing the index. The AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS is preeminently a suitable center to undertake the task. A suitably selected committee could adopt a plan and divide the indexing into appropriate headings. It seems that if the work be done at all it must be done by individuals voluntarily, whose work shall be co-ordinated by a central editing committee. I have reason to believe that all publishers will contribute to the enterprise to the extent of contributing at least one copy of their respective periodicals.

The volunteers should be carefully selected by this committee, and one or more subjects assigned to each, with special reference to qualifications for the work. In order to prevent confusion, each person should be assigned one or more periodicals, or, perhaps better, each one be expected to thoroughly index his respective periodicals in accordance with the subdivisions adopted by the committee. I feel certain that the financial portion of the undertaking will receive ready support and be well provided for, once a definite plan is adopted.

MR. L. L. SUMMERS:—There can be little doubt the need of a suitable index is felt, yet experience would seem to indicate that owing to lack of support, as an independent enterprise it can not be made successful financially. Whether this is due to the limited number who have libraries or periodical files extensive enough to warrant subscription to an index, or whether most of the attempts thus far made have been in the nature of a compromise, and have not had support for that reason, it is difficult to decide. It would seem that a compromise of some form must be agreed upon, as an index broad enough in scope to suit perhaps the majority will not be able to contain abstracts of articles which others seem to desire. To my mind it is absolutely necessary, that in addition to a proper title and the authors name, some idea of the length of an article, its scope, relative importance, or general conclusions be given. This should be very brief and would not necessarily be a digest of the article. If the author's name alone is accepted as a guarantee of merit, much that is valuable from the pens of comparatively unknown writers may be buried through lack of this protective shield, and an appreciation of the value of an article can only be formed by referring to it. That there is a useless waste of time in the individual efforts which have been made, is readily apparent, and it would seem that an investigation on the part of the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS might accomplish much, not only in ascertaining the best plan to be followed, but in securing co-operation and in avoiding the necessity for a duplication of individual efforts.

REPORT OF COMMITTEE ON UNITS AND STANDARDS.

[Submitted to Council December 19, 1894, and ordered to be printed in the TRANSACTIONS.]

December 4, 1894.

To the President and Council of the American Institute of Electrical Engineers.

Gentlemen:—Your Committee on "Units and Standards" begs to submit the appended printed copy of an act defining and establishing units of electrical measure in the United States.

The bill was introduced at the second session the Fifty-third Congress, numbered "H. R. 6500," by the Hon. Charles W. Stone, a Representative of Pennsylvania, who, aided by Prof. T. C. Mendenhall, took an active interest in its progress and passage. On the 30th of March, 1894, the bill was referred to the Committee on "Coinage, Weights and Measures" and ordered to be printed. On the 15th of May, 1894, it was referred to the calendar of the House of Representatives and ordered to be printed, together with the accompanying report, hereto appended. On the 29th of June, 1894, the bill passed the Senate with minor amendments. On the 30th of June the House of Representatives concurred, and on the 12th of July the bill received the approval of the President.

Yours respectfully,

WILLIAM E. GEYER,
GEO. A. HAMILTON,
W. D. WEAVER,
F. B. CROCKER,
A. E. KENNELLY, Chairman.
} Committee.

[PUBLIC—No. 105.]

An Act to define and establish the units of electrical measure.

Be it enacted by the Senate and House of Representatives of the United States of America in Congress assembled, That from and after the passage of this Act the legal units of electrical measure in the United States shall be as follows:

First. The unit of resistance shall be what is known as the international ohm, which is substantially equal to one thousand million units of resistance of the centimeter-gram-second system of electro-magnetic units, and is represented by the resistance offered to an unvarying electric current by a column of mercury at the temperature of melting ice fourteen and four thousand five hundred and twenty-one ten-thousandths grams in mass, of a constant cross-sectional area, and of the length of one hundred and six and three tenths centimeters.

Second. The unit of current shall be what is known as the international ampere, which is one-tenth of the unit of current of the centimeter-gram-second system of electro-magnetic units, and is the practical equivalent of the unvarying current, which, when passed through a solution of nitrate of silver in water in accordance with standard specifications, deposits silver at the rate of one thousand one hundred and eighteen millionths of a gram per second.

Third. The unit of electro-motive force shall be what is known as the international volt, which is the electro-motive force that, steadily applied to a conductor whose resistance is one international ohm, will produce a current of an international ampere, and is practically equivalent to one thousand fourteen hundred and thirty-fourths of the electro-motive force between the poles or electrodes of the voltaic cell known as Clark's cell, at a temperature of fifteen degrees centigrade, and prepared in the manner described in the standard specifications.

Fourth. The unit of quantity shall be what is known as the international coulomb, which is the quantity of electricity transferred by a current of one international ampere in one second.

Fifth. The unit of capacity shall be what is known as the international farad, which is the capacity of a condenser charged to a potential of one intertermational volt by one international coulomb of electricity.

Sixth. The unit of work shall be the Joule, which is equal to ten million units of work in the centimeter-gram-second system, and which is practically equivalent to the energy expended in one second by an international ampere in an international ohm.

Seventh. The unit of power shall be the Watt, which is equal to ten million units of power in the centimeter-gram-second system, and which is practically equivalent to the work done at the rate of one Joule per second.

Eighth. The unit of induction shall be the Henry, which is the induction in a circuit when the electro-motive force induced in this circuit is one international volt while the inducing current varies at the rate of one ampere per second.

Sec. 2. That it shall be the duty of the National Academy of Sciences to prescribe and publish, as soon as possible after the passage of this Act, such specifications of details as shall be necessary for the practical application of the definitions of the ampere and volt hereinbefore given, and such specifications shall be the standard specifications herein mentioned.

Approved, July 12, 1894.

53d Congress, 2d Session. | HOUSE OF REPRESETATIVES. | Report No. 901.

UNITS OF ELECTRICAL MEASURE.

May 15, 1894.—Referred to the House Calendar and ordered to be printed.

Mr. Charles W. Stone, from the Committee on Coinage, Weights and Measures, submitted the following

REPORT:

[To accompany H. R. 6500.]

The Committee on Coinage, Weights and Measures, to whom was referred the bill (H. R. 6500) to define and establish the units of electrical measure, respectfully report:

More than $100,000,000 (and probably several times that amount) are invested in the appliances for the production and use of electricity and electrical machinery in the United States, and yet there are at the present time no legally defined units or standards for the measurement of this enormous product. Although contracts, involving the expenditure of large sums of money are being made every day for supplying electricity to be utilized as power, light or otherwise, and for furnishing electrical machinery, there are no properly authorized standards by means of which either the seller or the buyer can be protected or to enable the courts to determine definitely when the contract has been fulfilled. This is not owing to the non-existence of suitable units or standards, for in no other department of engineering are the methods of precise measurement so satisfactory as in this.

Units of electrical measure which have been used in scientific investigations for more than a decade have come into use in the practical application of electricity and have been accepted as necessary and sufficient in all countries. A formal international agreement upon the names and precise definitions of these units was necessary before it was desirable to fix them by legislative enactment. This agreement has now been reached after several preliminary conferences, and is to be found in the official report of the Chamber of delegates of the International Congress of Electricians, held at Chicago in August, 1893. The Chamber consisted of delegates officially representing all the leading nations of the world—the United States, Great Britain, France and Germany being represented by five delegates each, and other nations by a smaller number.

At the end of the conference of the delegates, which extended through a week, it was unanimously resolved "that the several governments represented by the delegates of this International Congress of Electricians be, and they are hereby, recommended to formally adopt as legal units of electrical measure the following:"—the names and definitions of the eight units being then given as in the bill herewith reported.

The names of these units are in all cases derived from those of distinguished electricians. No name of a living man has been used, and those who have contributed most to the development of the science have been selected, some attention being also given to their distribution among the several great contributing nations. Nearly all of these names have been long in use and all have become fixed in the literature of the subject. The three most frequently used are the ohm, ampere and volt, being the units of resistance, current strength and electro-motive force or pressure, respectively. The *ohm* is named in honor of a distinguished German, born in the latter part of the last century; the *ampere* is named for the Frenchman of that name, who so greatly enriched the science of electricity in the early part of the present century; the *volt* is named for Volta, the Italian contemporary of Galvani.

Following these are the *coulomb*, named for a French electrician of the last century; the *farad*, in honor of the celebrated Faraday; the *joule*, in honor of another English scientist, James Prescott Joule; the *watt*, for James Watt, the inventor of the steam engine, and also eminent in electrical investigation, and, finally, the *henry*, in which a place is provided in this splendid galaxy of physicists for our own Joseph Henry, whose electrical researches well deserve to rank with those of the most famous of any age.

These units form a complete and accurate system of electrical measurements, and their legalization by act of Congress is earnestly desired by all electricians and those interested in the manufacture of electrical machinery or the use of electricity. It is imperatively demanded as a means of protection for the rapidly increasing number of consumers of electricity, and it is also urged by those engaged in the production.

It is also important to the Government, which is itself a large and constantly increasing consumer of electricity and electrical machinery, that authoritative units of measure be adopted. This has been fully recognized by the superintendent of standard weights and measures and by the Secretary of the Treasury, and the units of measurement defined in this bill have already been formally approved by them and adopted for Government use.

This departmental action should receive legislative approval, and, beyond that, the necessities of commercial use, as well as the convenience and accuracy of scientific investigation, require the establishment of authoritative units of electrical measure.

The system embraced in this bill, having received the unanimous approval of the representatives of all the nations participating in the recent international Congress of Electricians held in Chicago, it is believed will be adopted by the other nations of Europe and America and become a uniform and international system.

As the congress formulating this system was held in the United States and presided over by one of her most distinguished scientists, it will be a matter of pride for this country to lead in the formal and legal adoption of this system, and your committee recommended the prompt passage of this bill.

Two or three clerical errors should, however, be corrected, and the committee recommended that it be amended by striking out the words "ten and nine-tenths" in the sixth line, and inserting "one thousand million," and in the fortieth line strike out the words "ten and seven-tenths,' and in the forty-fifth line strike out the same words, and each instance insert in lieu of these words stricken out the words "ten million."

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

NEW YORK, December 19th, 1894.

The 92d meeting was held this date at 12 West 31st Street, and was called to order at 8 P. M. by President Houston.

The Secretary read the minutes of the last meeting, which on motion were approved.

The Secretary read the following list of associate members elected and transferred at the Council meeting, held in the afternoon.

Name	Address	Endorsed by
BLACKALL, F. S.	N.Y. City Representative, Crocker-Wheeler Elec. Co., 126 Liberty St., N. Y. City.	Jos. Wetzler. Ralph W. Pope. T. C. Martin.
DERYCKERF, G.	Professor in Electrical Engineering, Gand University, residence, Boulevard Frere Orban 18, Gand Belgium.	A. G. Inrig. G. Sacco Albanese. R. W. Pope.
DEY, HARRY F.	342 Tenth Street, Brooklyn, N. Y.	H. Ward Leonard. Jos. Wetzler. Geo. W. Tuttle.
GRAHAM, GEORGE WALLACE	Secretary, Interior Telephone Co., 203 Broadway, N. Y.; residence, 163 Hicks Street, Brooklyn, N. Y.	Ralph W. Pope. T. C. Martin. Geo. M. Phelps.
LANE, VANCE	Manager and Supt. Construction, Nebraska Telephone Co., Omaha, Neb.	Thos. D. Lockwood. T. B. Doolittle. Wm. S. Ford.
LARDNER, HENRY ACKLEY	Instructor in Electrical Engineering, State College, Pennsylvania.	J. P. Jackson. D. C. Jackson, J. G. White.
MOSES, PERCIVAL ROBERT	Student of Electrical Engineering, Columbia College, N.Y.; residence, 46 West 97th Street, New York.	F. B. Crocker. Geo. F. Sever. W. H. Freedman.
PRATT, CHARLES A.	Electrical Engineer, The Independent Elec. Co., 39th Street and Stewart Ave., Chicago.	Elmer A. Sperry. Chas. N. Black. E. P. Roberts.
PRIVAT, LOUIS	Electrician, Cicero Water, Gas and Electric Light Co., Oak Park, Ill.	Albert Scheible. Fred DeLanr. Geo. Cutted.

SHEDD, JOHN C.	Professor of Physics and Applied Electricity, Marietta College; residence, 512 Fourth Street, Marietta, Ohio.	Ernest Merritt. Frederick Bedell. Harris J. Ryan.
VANDERSLICE, G. HAMILTON	Westinghouse Electric and Mfg. Co., P. O. Box 911, Pittsburg, Pa.	L. B. Stillwell. Chas. F. Scott. Alex. J. Wurts.

Total, 11.

TRANSFERRED FROM ASSOCIATE TO FULL MEMBERSHIP.

Approved by Board of Examiners, October 5, 1894.

REIST, HENRY G.	Designing Engineer. General Electric Co., Schenectady, N. Y.
TRAFFORD, E. W.	Electrical Engineer, Richmond Railway and Electric Co., 104 N. 7th St., Richmond, Va.
JACKSON, HENRY	Telegraph Superintendent and Engineer, The Lancashire & Yorkshire Railway Co., Horwich, Bolton-le-Moors, Lancashire, England.
ROUQUETTE, WILLIAM FREDERICK BLAKEWAY,	Proprietor Rouquette & Co., 47 Dey Street, N. Y.
UHLENHAUT, FRITZ, JR.	Electrical Engineer, Philadelphia Traction Co., Philadelphia, Pa.

Total, 5,

Report of Meeting of Board of Examiners, December 17th, 1894.

Present—Messrs. G. A. Hamilton, *Chairman;* C. O. Mailloux and E. P. Thompson; R. W. Pope, Secretary, present *ex officio.*

	Approved.	Disapproved.	Laid over for further consideration.	Total Considered.
Applications for Transfer Reconsidered.	11		2	13
New Applications for Transfer Considered.				
Totals.	11		2	13

THE PRESIDENT:—There being no other business, the next matter as announced, will be a paper by Mr. Ludwig Gutmann of Chicago, entitled, "The Production of Rotary Magnetic Fields by a Single Alternating Current." As the author is not here, Dr. Pupin has kindly agreed to present the paper.

A paper presented at the Ninety-first Meeting of the American Institute of Electrical Engineers, New York, President Houston in the Chair, and Chicago, Mr. Leland L. Summers in the Chair, December 19th, 1894.

ON THE PRODUCTION OF ROTARY MAGNETIC FIELDS BY A SINGLE ALTERNATING CURRENT.

BY LUDWIG GUTMANN.

Since the first announcements of Prof. Ferraris and Mr. Tesla, that rotation can be accomplished by employing two or more alternating currents of displaced phase, the question has been raised by many an engineer as to the possibility of producing a rotary magnetic field by the use of a single circuit, carrying the ordinary alternating current, instead of requiring three or more wires or several independent circuits. So far it has been necessary to construct a special generator armature, the windings of which were rotated in a field of constant polarity, to produce two or more currents lagging in phase behind each other. To use the currents generated for establishing a progressive or rotary field, it was necessary to carry as many wires to the place of consumption as currents were employed differing in phase. Six years have passed, and quite rapid progress has been made in various directions. I wish now, to bring to your notice, a method of generating a rotary magnetic field by a single alternating current or its field, without condensers or choking coils. This is accomplished by a device brought out by me some three years ago. Its fundamental principle is hardly as yet appreciated; however, the World's Fair exhibits as well as literature of late, show that the device has attracted attention, and that several engineers have approached the principle involved.

I wish to refer to my motor and especially to its armature described in 1891.[1] Figures 1 and 2 show the device. The

1 *Electrical Engineer*, vol. xii., Aug. 26, 1891, p. 230.

former is principally used in the American patents, while the latter is given in the English patent of the same year. The novelty of the armature construction, lies in a winding containing closed sub-circuits; and another point is, that the closed circuits do not coincide in number with the poles of the field magnet.

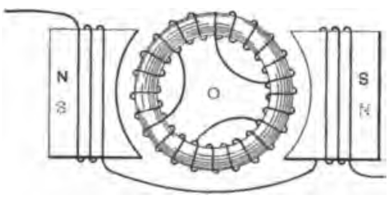


FIG. 1.

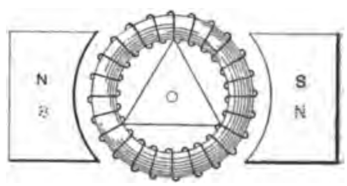


FIG. 2.

Before going into the action of this device, I will mention two simple experiments which are essential in order to understand the operation of this rotary magnetic field system.

If we have a closed coil moving in an alternating magnetic field, or better, between the poles of *a field magnet* as illustrated in Figs. 3 and 3*a*, energized by alternating currents; then the coil A, will be repelled from the pole, owing to the secondary field which it establishes, so long as it cuts, or is threaded by the lines of force of the field. The consequence is, that it will rotate, and place itself between the north and south pole, Fig. 4, where it reaches a *stable magnetic equilibrium*, as I

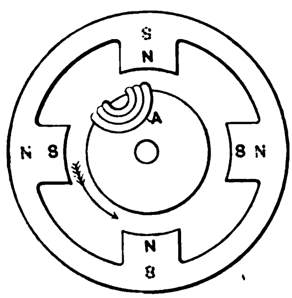


FIG. 3.

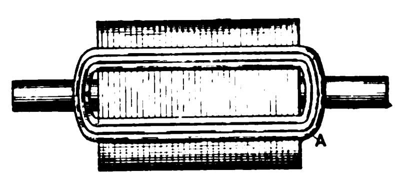


FIG. 3a.

like to term it, because in this position it acts like a pendulum properly suspended (Fig. 5). Any motion imparted causes the pendulum to make a few oscillations and come to rest, and similarly any attempt made to move the coil into an energized field will cause it to be repelled, and after a few oscillations it will remain at rest between the poles. It is repelled in either direction, because as soon as it approaches a strong magnetic flux, currents are induced in its windings and create an opposing field. The same coil can be in unstable magnetic equilibrium (Fig. 6), if it stands just in front of a pole, in which position it would act like a pendulum which is balanced with the weight above the point of support (Fig. 7). Both are in unstable equilibrium; the least motion to the left or right imparted to either, will cause them to accelerate in the direction of the impulse, and to assume a position of stability (Figs. 4 and 5). In this position, Fig. 4, the coil is therefore currentless. *This stable position, is the natural position of synchronous motors in operation.*

If such a synchronous motor is held behind the phase when rotating under load, a heavier current is induced in the spools

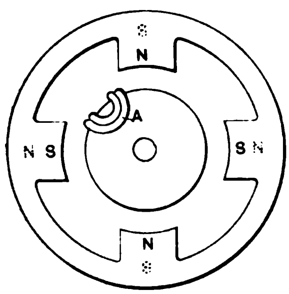


FIG. 4.

FIG. 5.

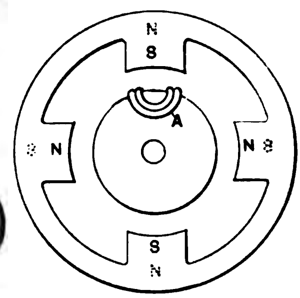


FIG. 6.

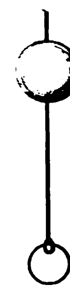


FIG. 7.

which accelerate the armature and keep it in step. If, however, the motor runs in harmony, and suddenly a great deal of the load is taken off, then the armature has a tendency to go more quickly, but in this case opposing currents are generated in the windings which retard the speed of the armature and keep it back, to remain in step, or in stability.

Let us look into the device before us, and consider first its quality as a motor. Returning to Figs. 1 or 2, preferably the latter, which is simpler and easier to explain, we find that if we send into the field magnet coil an alternating current, the armature will turn through a certain angle and stop. This motion depends upon the position of the three closed coils with respect to the two poles. This same device can itself be self-starting if we make the poles far enough apart and the energizing current sufficiently strong, so that at the initial speed the armature is repelled at a certain rate. This initial speed changes with the number of poles, closed circuits and alternations. Let us take the first case and examine it. If we try to rotate the *armature* (as I shall call it hereafter), in a field of alternating polarity, we induce currents in the closed circuits, which establish poles,

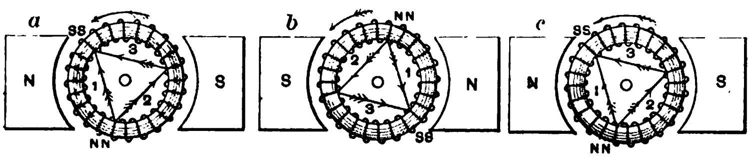


FIG. 8.

but these poles are alternating in nature, and owing to the motion imparted, and owing further to the permanent connections in the winding, the poles move or travel, with the latter. This accounts for the fact that if the field poles are close together the armature consumes power during its rotation through the alternating field: it acts as a dynamo and does not start from a state of rest. If we impart by mechanical means an impulse to the armature, currents are generated whose sum has a variable and principally negative value. The armature makes a few revolutions and places itself in a stable magnetic equilibrium, viz., in such a position, symmetrical if possible, as not to generate any currents, and hence not to induce any reactionary[1] poles in the core. If, however, we impart to this armature a certain initial speed, which I shall call *the critical speed*, then it will continue to rotate, with a tendency to reach a certain fixed higher speed. This initial impulse may be given by mechanical or electrical means. I shall not state the results as yet, but prefer

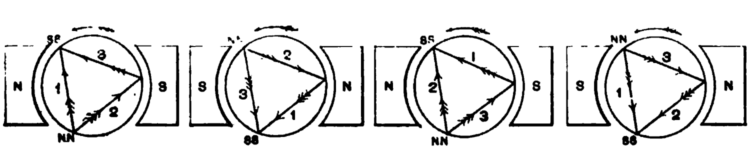


FIG. 9.

to explore the action first, so that the same may be before us in various diagrams, and enable us to make any deduction desirable. First, let us assume that the armature rotates syn-

1. It is well to call the armature poles due to the armature currents, reactionary poles. The other armature poles are induced directly by the field poles, and contribute nothing to the torque.

chronously: It is evident that in order to have an armature with three closed-circuited coils, interconnected as shown, rotate in one direction, we must have currents induced whose sum has a positive value, viz., two of the coils must do work, in the same sense, while the third coil is either without a current, or has a smaller current in opposite direction, so that the value still remains positive. For true synchronism a closed-circuited coil reaches within a half-period the opposite pole, and consequently our diagram for one revolution would look as follows: (See Fig. 8).

The three diagrams, *a*, *b*, *c*, show the flow of currents and resultant poles. The closed-circuited coil 2, diagram *a*, is at this moment generating opposing currents to the others; coil, 1, is passing just in front of a pole. All three induced currents combined are developing poles as indicated, and rotating the armature

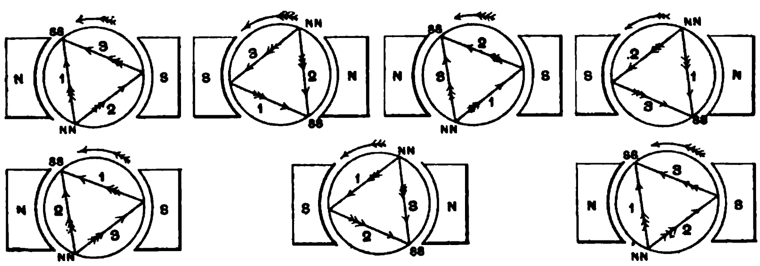


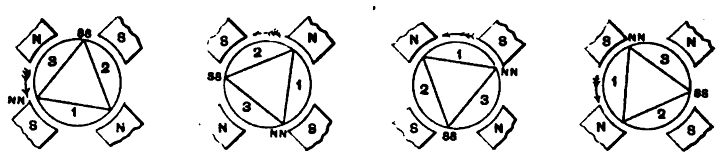
FIG. 10.

in counter-clockwise direction. The rotation being synchronous, diagram *b* shows the position of the armature when the polarity in the field has reversed; diagram *c* gives the position of the coils after the polarity has changed again. It will be noticed that the original position, as shown at *a*, has been reached again, and that the cycle will repeat itself. Without explaining these diagrams at this point, I wish to bring before you several other conditions, all of which will assist in reaching definite conclusions. Looking at the armature we see that it has a symmetrical winding, and as long as these closed circuits do not coincide with the number of poles of the field magnet (contrary to all synchronous motors built heretofore), other working positions are possible. The operation will take place as long as the sum of the armature currents have a positive value, since as far as the rotation is concerned it is indifferent which of the coils are doing the work. Let us assume that the armature does not run at a synchronous rate, bnt at another fixed speed which may have a certain commensurable relation to the periods of the exciting field current, which speed I shall call the *harmonic speed.* Instead of showing the diagram with windings as in the first set, I have simplified them. In the following sets of diagrams, Fig. 9, I assume that the armature, on account of the load or other reasons, cannot reach synchronism, and that at each field reversal the position of the armature with respect to the field is the same, only the coils have changed places as indicated. You will notice that in this set of diagrams the armature, for each half-period, makes one-third of a revolution only, namely, coils 1, 3 and 2 exchange positions with the change of polarity. Let us step at once to another speed (Fig. 10), which you will no doubt admit as possible; viz ;—let us assume that coils 1, 2 and 3 cannot arrive at synchronous speed, and that coil 2 reaches the position which 1 ought to occupy, it will be seen that owing to the symmetrical position, it is quite indifferent which of these coils arrives at a given position required for harmonic operation. This will be clear to you by referring to the third set of diagrams, Fig. 10.

We are now enabled to compare three sets of diagrams, Figs. 8, 9 and 10. The first set shows for this new type of synchronous motor that at synchronous speed the pole positions in the armature are practically stationary relatively to the coils and core, and rotate with it. The second set shows that during a complete revolution of the armature, the field has reversed three times, that is, an odd number of times, hence, when the armature returns to its original position, after a complete revolution, it finds the field magnetization of reversed polarity. The magnetic field of the armature rotates in the opposite sense, one and a-half times per revolution. In the third set, the armature makes but one-sixth of a revolution per half-period, and the magnetic field of the armature in a complete revolution rotates three times in the opposite direction. This statement may be startling, and my argument, so far, may not be entirely convincing, as on close inspection it will be found that the armature poles have to do considerable jumping. I will now explain the performance of the rotary magnetic field of this armature in a four-pole field, where the changes occur far more gradually, leaving no doubt whatever as to the assertion made. Fig. 11 shows diagrams illustrating synchronous speed. By the aid of these four diagrams we can more easily follow the pole positions in the armature core, and find them stationary in it. Inspecting now Fig. 12, in which the armature speed is reduced, we are able to obtain a clear picture of the main subject under consideration.

In this set of five diagrams we see five positions, which coil 1, for instance, occupies before it has made one-third of a revolution. Nevertheless, if we now look at the changing pole positions we can readily detect that they are progressively shifting, and we also see that they have made one whole revolution and that in opposite direction to the mechanical rotation, or in other words, the magnetic field would travel three times around the armature core during one revolution. In all armature diagrams described, and to be described, the resultant of the magneto-motive forces of the armature currents is given only. To complete the picture showing every one of the component magneto-motive forces act-

FIG. 11.



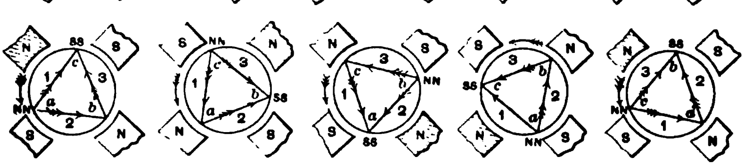


FIG. 12.

ing, it becomes desirable to plot a diagram of force (Fig. 13). These components act like three variable concurrent forces including an angle of 120 degrees with each other. These forces we may represent as equal, for the sake of convenience, as it does not influence the idea which we wish to illustrate. However, those two forces which are acting in the same direction are indicated by heavier lines.

Plotting the direction of the forces in order, as, for instance, in the last named set of diagrams (Fig. 12), where the coils act in the order 1, 2, against 3; 3, 1, against 2, and 2, 3, against 1, we obtain Fig. 13 for one cycle. The three figures marked *a*, *b*, and *c* indicate the direction in which the three forces are acting in the three positions of one cycle or revolution.

By drawing the resultant diagram of force from the predominating forces of each position, we obtain the accompanying figure (Fig. 14). These forces form a triangle, and the rotation resulting, may be seen from the direction of arrows on the circumference of the surrounding circle. However, the reactive forces should be considered and represented separately to obtain

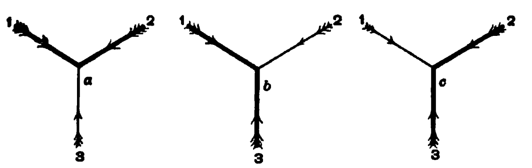


FIG. 13.

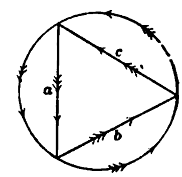


FIG. 14.

the true picture, and we have to add them to find the total force applied, and the manner in which all the forces are acting. We, therefore, repeat the resultant diagram (Fig. 14), and add the opposing forces at their proper places. This diagram (Fig. 15) shows that the reactive forces, shown by dotted arrows, also have a common center, and that as regards time of maximum effect, they succeed each other in the direction of rotation of the armature, while they are acting in the opposite direction of the main force. If we now plot a diagram of these reactive forces alone, we obtain the smaller triangle shown (Fig. 15) inside the larger. It will be noticed that rotation is in the opposite direction; or, in other words, that in reality we have, with this disposition, to deal with two rotary magnetic fields in opposite direction; the useful work done will depend on the difference of the angular velocity of these two rotating fields and their relative intensity.

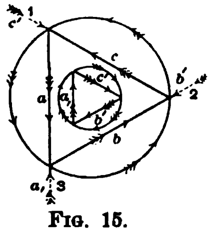


FIG. 15.

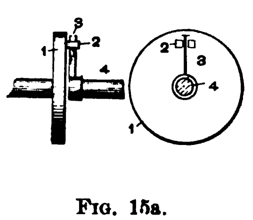


FIG. 15a.

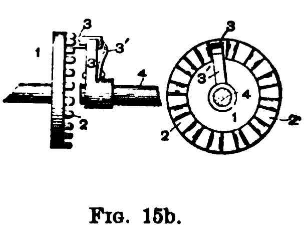


FIG. 15b.

Looking now at the device as a generator (Fig. 12), we can account for the rotary magnetic field because we generate polyphased currents in the coils 1, 2 and 3. They operate together in the following order: 1, 2; 3, 1; 2, 3; 1, 2, etc. If rotated, as shown, the coils will distribute three-phased currents over the armature, and this will be understood when examining into the flow of currents of the closed-circuiting conductors which are here provided with arrows. At this particular harmonic speed, it will be further noticed that the simple alternating field of force has the identical qualities as a polyphase energizing field, as the poles in front of the closed-circuited coil seem to be traveling (for the armature) in clockwise direction. This is indicated by the shading of the pole of same sign in front of the coil most powerfully acted upon in the five positions, representing one revolution for the field. The currents displaced in phase, cause the armature to rotate harmoniously with the alternations, and prevent the armature from being easily thrown out of step.

Returning once more to the earlier diagrams, Figs. 9 and 10, we see that below synchronism there are two other speeds which are entitled to be called synchronous, because a certain fixed relation exists between the periods of field excitation and armature rotation.

It will be evident that the greater the number of closed circuits, the greater is the number of harmonic or synchronous speeds in which the armature can operate. If the armature contains ten closed circuits, and rotates in a two-pole field, it will be able to generate five currents, lagging 72 degrees behind one another in phase, and would have at least five speeds which are synchronous, and the armature owing to its peculiar construction, can operate at a slower speed than the synchronous speed as interpreted heretofore.

Looking at the device from a general standpoint, it will be clear that this particular armature construction embodies all the weak points of a synchronous, and all the strong points of the polyphase motor. The weak points are:

1. that the machine will not start from state of rest in the form shown, because in Figs. 1, 2, and 12, the coils act on one another differentially and have a variable and negative value; they are in magnetic stability when at rest, hence, no rotation can result.

2. The device has a small starting torque when rotated, owing to the differential action of the coils, which cause, with a small number of closed circuits strongly oscillating or jumping poles, until a harmonic speed is reached.

3. The device when in rotation, is not reversible by simply changing circuit connections.

The strong points are:

First, that the motor develops polyphased currents in its own windings.

Second, that therefore it is more difficult to pull it out of step as it has the capacity to stand, what I would term, magnetic slippage.

Let us assume that a certain coil, which should reach a given point to run harmonically, remains behind owing to a slower motion of the armature; in this event the coil ahead has to perform the function at the expense of a heavier current. The loss of revolutions due to this action may be comparable to the slipping of a belt over a pulley. However, there is also a limit to the slipping. The armature should have some harmonic speed with respect to the alternations. Should it fall below the lowest harmonic speed, then it will come to a standstill just like any other overworked synchronous machine.

Synchronism in a motor means nothing more nor less than a magnetic flexible clutch, which may be represented mechanically as follows:

The old fashioned non-starting synchronous machine having equal armature coils and field poles, can be compared with the arrangement shown in Fig. 15*a*, giving side view and elevation. The disk 1 is provided with two projections; 2, imparts rotation to shaft 4 by means of the flexible arm, or spring 3, which enables the shaft 4 to be slightly behind or ahead of disk 1. However, if the work put on shaft 4 is in excess of the strength of spring 3, then it will break, and shaft 4 will speedily come to a standstill.

In contrast to this older form of synchronous motors stands the new one, which forms the subject of this paper and the machines operated by bi or polyphased currents. The mechanism for showing magnetic coupling for these latter types may be represented by Fig. 15*b*. In this case, 1 represents a crown-wheel the teeth marked 2; into these teeth meshes a catch 3 in a flexible manner, which catch is connected rigidly to shaft 4. Within the range of normal work, catch 3 would remain fixed between the two given teeth, but if the shaft 4 is overloaded, the flexibility of catch 3 will cause it to yield, and pull out of its position, drop in between some other two teeth, and continue to operate but at a slower speed. The greater the excess of work, the greater will be the slippage, and the more teeth will be jumped by the catch 3 in every revolution of disk 1. For the synchronous motor herein described, the overloading to the extent that it cannot reach the lowest harmonic speed, would be equivalent to the breaking of spring 3′.

It will be remembered that Prof. Ferraris always, and Tesla sometimes obtained a rotary magnetic field and rotation by influencing one coil carrying a primary current, by another at right angles thereto carrying secondary currents. Here we have two phases from a single source, Fig. 16. To obtain a multiple of phases we may take two or three iron cores A, and surround them with coils B, like the links of a chain, Fig. 17, so that the secondary coil of transformer 1, is the primary of transformer 2, and so on; or we may design a special core for a transformer, and create a rotary magnetic field by applying to one coil a primary current from the line, and to another coil a secondary current from a transformer, whereupon by the rotary field obtained,

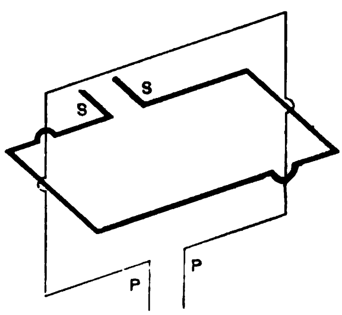


FIG. 16.

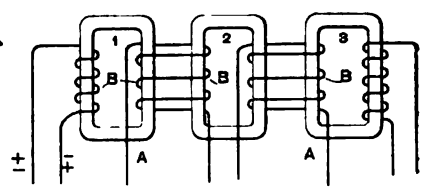


FIG. 17.

polyphased currents may be taken through other coils on the transformer. These methods have, however. one drawback, and that is that the phase will change more or less in lag with difference in load, while for many purposes it is most essential that for efficient working, the phase should remain at the same constant angle. This desideratum has been accomplished by the device under consideration, and any suitable number of currents lagging in phase may be obtained from a single alternating current.

To obtain a rotary magnetic field in or by a stationary body, at least two electric or electromagnetic forces of definite phase relations, acting at an angle to one another and changing at periods harmoniously, are essential requirements.

If, however, we allow the body, or part of the same, to be movable, we may substitute mechanical rotation primarily as an adjuster of the periodicity and phase of one of the two forces;

the fundamental relation between the two forces must in this case also be the same as in the other; namely, they must work in harmony, and the periodicity and phase of one force determines the periodicity and phase of the other. If the second force is a current, it must be one of the same period or some harmonic of that period; if it is rotation, it has also to be synchronous or harmonic. We can now clearly understand why, when rotating the armature, Figs. 1 and 2, in an ordinary alternating current field, we have no system of polyphased currents and no rotary field, simply because the armature rotation does not stand in any relationship whatever to the alternations; but as soon as the speed does so, there are generated polyphased currents lagging in phase at a fixed angle, and a rotary magnetic field.

There is a difference whether the armature is influenced by a single alternating current, or by several. causing themselves a rotating energizing field. Nevertheless, in either condition the armature is in magnetic stability, and this state is the reason that it remains stationary in the first case, and that it rotates in the second. The reason that the motor starts in the second case is evidently because the inducing magnetic field is itself rotating in the stationary field magnet core. As said before, it is considered necessary for the proper operation of the armature for giving maximum torque, that if placed in an alternating current field of force, the armature rotation should have a definite relation to the alternations. Up to the publication of this device some three years ago, synchronous motors had as many armature coils as field coils, and hence to obtain the value of the synchronous speed, all that was necessary was to divide the alternations of the armature current n by the number of field poles p of the magnet employed. But if the number of armature coils are not the same as the field poles, then the speed may vary from this synchronous speed, and a modification is thought to be necessary. We would have to find the smallest common factor between field poles p and armature coils p^1 with which we have to divide the number of alternations to obtain the value of the lowest synchronous speed while the condition of slippage may allow for the lowest harmonic, the value $\frac{n}{p\,p^1}$.

It will now be clear that motors can be designed having any desired variation of speed, and have defined synchronous speeds or approach the indifferent relation. I wish it understood, that I do not intend to treat the subject in all its details, and I am especially reluctant on the ground of having carried on but few experiments.

The few diagrams presented do not disclose many conditions of those possible. They show only one or two facts of conditions which frequently are of a complex nature. Evidently, the rotary magnetic fields in the device under consideration may have altogether different speeds from those shown in the diagrams; also, that the magnetic field may rotate with and ahead of the armature, and lastly, the speed of rotation of the rotary field of the armature may be synchronous, and opposite in direction to its rotation.

The device has been given in form of a diagram, and as it has been described as not self-starting, I wish now to state that it be-

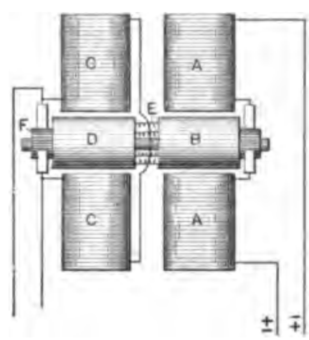


FIG. 18.

comes self-starting as a series, a shunt or an induction motor in a field of single phase or polyphased alternating currents. Fig. 18 represents one of the earliest forms which have since been simplified to a considerable extent. The part A B, represents here a simple series motor whose armature B may be any drum or ring type, open or closed coil winding; the part C D, may be similarly constructed. Both armature windings B and D are connected electrically by 4 wires E at points of equal distance, which number of conductors E, may be reduced or increased, depending on the number of phase currents desired. Now send an alternating current into the series motor, part A B[1], then the armature will start from state of rest, and will send current impulses into the D part winding, which becomes the more regular the more the armature

1. Preferably so that no reaction takes place from part D.

has approached synchronous speed, where bi-phase currents will be established therein; these react on the field magnet core O, and for the purpose of maintaining the armature in this synchronous speed, the commutator F is applied to rectify the polyphase currents; which continuous current may be used to energize the magnet O.

The fundamental principle disclosed, opens to us a very wide field for work. Not only are we enabled to construct motors almost as cheap and simple as continuous current motors, but there is in this principle involved the germ for the ideal distribution of power, inasmuch as we are enabled to collect the polyphased currents generated, or commutate them immediately at will.

Finally I wish to call your attention to the fact, that the last figure is in reality a motor-generator, and it is therefore evident that the electrical energy supplied need not all to be transformed into mechanical power, but that a portion may be utilized in one of the modified forms named above.

Discussion in New York.

Dr. M. I. Pupin:—The author states in the introduction that his paper contains a discussion of a method of generating a rotary magnetic field by a single alternating current without condensers or choking coils, and its application to the construction of single phase alternating current induction motors.

Figs. 1 and 2 show diagrammatically the principle of the devices. An armature core, uniformly wound with wire and the winding divided into a number of closed sections, preferably equal to each other, but in such a way that the number of these sections is not equal to the number of poles of the exciting field. The field is an alternating field. Figs. 4 and 6 illustrate what the author calls the *stable* and the *unstable* positions of magnetic equilibrium of a coil in an alternating field. In the first place a slight displacement of the armature would cause it to oscillate about the line of symmetry between the two poles. In the second case such a displacement would cause the armature to flop over and rotate toward the position of stable equilibrium. An armature winding as that in Fig 2 has also a position of stable magnetic equilibrium about which it will oscillate if slightly displaced. So for instance the position represented in Fig. 2 is an unstable position. A mechanical impulse will start the armature to rotate toward the position of stable equilibrium, and during its journey toward that position it will be also acted upon by the repulsive force between the alternating field and the armature current induced by that field. The armature, therefore, reaches the position of stable equilibrium with a certain amount of kinetic energy which is equal to the energy of the mechanical impulse imparted to it, and the work done upon it by the repulsive force. With this stored up energy, the armature is enabled to continue its journey beyond the position of stable magnetic equilibrium although from this point on, it has to move *against* the electromagnetic forces, at the expense of its kinetic energy. How far it will continue this journey will depend entirely on the kinetic energy with which it started from the unstable position, how much work was done upon it by the variable field during its journey from the unstable to the stable position, and at what rate it has to expend its kinetic energy after passing this position.

Three cases must be carefully distinguished:

1st. The armature arrives at the position of stable magnetic equilibrium with less kinetic energy than the amount required to carry it beyond this position and up to the next position of unstable magnetic equilibrium. In this case it will oscillate about the stable position and be finally reduced to rest.

2d. The armature arrives at the position of stable magnetic equilibrium with enough kinetic energy to carry it beyond this position and up to the next position of unstable magnetic equilibrium, but unfortunately it arrives there with less kinetic energy than it had in parting from the preceding unstable position. In this case it will generally continue to rotate several times around, and be finally reduced to rest.

3d. The third possible case is the most important one and forms the groundwork on which Mr. Gutmann proposes to raise a new system of electrical distribution. It is this: Suppose the armature receives from some external source a certain initial speed, and is then left to take care of itself. Suppose again that this speed is such that the kinetic energy with which the armature passes through a position of stable magnetic equilibrium is considerably more than necessary to carry it to the next position of unstable equilibrium, so that the armature reaches this position with more kinetic energy than it had when it left the last position of unstable equilibrium. It is evident that in this case the armature will not only continue to rotate, but that it will rotate with continuously increasing angular velocity. This initial speed the author calls the *critical speed*.

The question arises now, how long will this continual increase in angular velocity go on? Evidently until synchronism is reached, that is if the armature does no appreciable external work during its rotation. In a bipolar machine with three armature coils, the synchronous rotation is illustrated in diagrams Fig. 8. During one reversal of the field the armature has moved through 180°, so that the triangle 1, 2, 3, in B has its sides parallel to the triangle 1, 2, 3 in A. During the next reversal the armature has reached its initial position again, as given in C. But if the armature cannot reach synchronism on account of the external load, or on account of too large frictional resistances, then the best it can do is to rotate at some other lower but constant speed, and the question arises, is any other speed possible? The answer is in the affirmative, for if you look at Fig. 9 you will see that if the speed of the armature under consideration is such that it will carry the armature through one-third of the circumference during one reversal of the field, that the armature can go on rotating at this constant speed. This speed Mr. Gutmann calls *harmonic* speed. Another harmonic speed is illustrated in Fig. 10. Here the armature passes through one-sixth of the circumference during one reversal of the field, so that during three reversals of the field the armature has gone through one-half revolution. The possible speeds of this armature are, therefore, 1, or 2, or 3 revolutions for the time interval corresponding to three complete periods of the exciting field. We can now generalize as follows:

An armature containing n coils will have n *harmonic speeds*, that is to say, it can rotate 1, 2, 3, . . . n times during the time interval corresponding to n complete periods of the exciting current. But it must be remembered that for a given value of the impressed electromotive force and a given mechanical force acting on the shaft of the armature there is, according to the Principle of Conservation of Energy, only one speed at which the armature can rotate uniformly, and if that speed is below the lowest harmonic speed, the armature will stop. We can say, therefore, that the critical speed is equal to the lowest harmonic speed. If the lowest harmonic speed should be too high for practical working, we can evidently reduce it to any practical limit by using a multipolar field, as an inspection of Figs. 11 and 12 will show.

There is another way in which the forces acting between the field and the armature can be pointed out, and that is, by pointing out the reactions between the exciting alternating field and the magnetic field produced by the induced currents in the armature. It does not make the subject any clearer, and I shall consider it in so far only as necessary to bring out a rather interesting fact. Consider the armature in Fig. 2. Suppose we had one closed coil only, covering $1/n$th part of the armature, and suppose, also, that the field is constant. If the armature is rotated, an alternating current is generated in it, whose frequency is equal to the frequency of rotation of the armature. If, however, while the armature is rotating uniformly, the field alternates uniformly also, then two alternating currents are generated in the coil, one whose frequency is equal to the sum of the frequencies of the armature and field, and the other whose frequency is equal to the difference of these two frequencies. The current of lower frequency has a smaller amplitude than the one of the higher. Its amplitude is zero when the field and the armature are in synchronism, and then the frequency of the other is equal to twice the frequency of the field. If now we cover the whole armature with n equal short-circuited coils by simply connecting, as Mr. Gutmann does in Fig. 2, n successive equidistant points to each other, we shall then have n pairs of alternating currents differing from each other in phase by $1/n$th of the period. They will, therefore, give, theoretically, two rotary magnetic fields. One rotating with a frequency equal to the sum of the frequencies of the armature rotation and the frequency of the alternating field, and the other rotating with a frequency equal to the difference of these two frequencies. At synchronism the slower rotary field vanishes, and the speedier, rotates with twice the angular velocity of the armature. Mr. Gutmann evidently neglects the slowly rotating field, and he may be right. But, however that may be, it is certainly plain that a rotary magnetic field is established by Mr. Gutmann's device, and that we could obtain by a suitable arrangement polyphase currents from such armatures. It is also plain that such a motor could be employed as a motor generator. These appear to me to be the novel features of Mr. Gutmann's proposition. The proposition is certainly interesting. Let us hope that Mr. Gutmann will soon succeed in proving its practical value.

THE PRESIDENT:—Gentlemen, you have heard Dr. Pupin's able exposition of this paper. It is now open for discussion.

Mr. Wolcott:—I would like to ask one or two questions. On page 841 it says the motor allows magnetic slippage; in other words, it works like an ordinary induction motor. In an ordinary induction motor, in synchronism, the torque is zero, or at all events, it is minimum. It is the same here. Put on a load and get the armature running at a little slower speed than synchronism, the polyphase principle begins—the current induced in the armature makes the pull stronger. Well, if I understand the thing, that is also true of the harmonic speeds. Therefore, starting from the synchronous speed, dropping down to the first harmonic speed, there must be a point where, after there is a certain amount of slip, it drops suddenly to the harmonic speed, because if the harmonic speed works in that respect like a synchronous speed, there must be a tendency either side of it to approach it. That is, if you are going a little faster than the harmonic speed, the tendency is to pull down, and as you go slower again the tendency would be to bring it up again. Is that correct?

Dr. Pupin:—I don't think so.

Mr. Wolcott:—Would the torque go on increasing infinitely?

Dr. Pupin:—No, because as the armature increases its angular velocity the torque varies; it may diminish. The torque must have a certain value, otherwise it is not able to overcome the mechanical pull on the shaft.

Mr. Wolcott:—That is very true. But, if I understand this motor, it has two separate components. That is, it works like the ordinary synchronous motor, and then, in addition, it works like the polyphase motor—induction motor.

Dr. Pupin:—Yes, that is right.

Mr. Wolcott:—Considering the portion of the torque which is due to the synchronous action, the instant you get out of synchronism, as with an ordinary synchronous motor, it stops.

Dr. Pupin:—Not quite so.

Mr. Wolcott:—Now with this one, as soon as you get out of absolute synchronism, then you depend on the induced currents in the armature.

Dr. Pupin:—The pull between the armature and field, changes rather suddenly; that is, it is a sort of harmonically varying function.

Mr. Wolcott:—As the speed lags a little, the torque increases, does it not?

Dr. Pupin:—Not necessarily.

Mr. Wolcott:—It does not do the work of an induction motor then? If it does not do that it seems to me that it is no good.

Dr. Pupin:—It has a fair torque at definite speeds only.

Mr. Wolcott:—The author says it allows similar magnetic slippage.

Dr. Pupin:—Magnetic slippage from the synchronous speed to the next harmonic speed, and then to the next and next, and so on. It does not allow a gradual slippage.

MR. WOLCOTT:—I understand the author to say that it works like a polyphase motor.

DR. PUPIN:—In the sense that it has more than one speed at which it can work. It can work at two or three speeds, practically any number of speeds, if you increase the number of sections in the armature.

MR. KENNELLY:—If I understand the paper correctly, there are certain speeds called harmonic speeds at which the torque is a maximum. In the polyphase motor at certain speeds the torque is zero. So that this action is different altogether from the action of a polyphase motor.

DR. PUPIN:—That is my idea of it.

MR. WOLCOTT:—On page 841 he says: "Second, that therefore "it is more difficult to pull it out of step, as it has the capacity to "stand what I would term magnetic slippage." If that means anything it means that if you pull it a little out of step the torque increases. If the torque does not increase, I do not see how it is any more difficult to pull out of step than the ordinary synchronous motor.

DR. PUPIN:—Because, if you pull it out of one step you get it into another.

MR. WOLCOTT:—That may be what the author means, but if it is, I think that is a very poor foundation for his claim.

DR. PUPIN:—It has several chances; whereas the ordinary synchonous motor has only one.

DR. SAMUEL SHELDON:—I wish to say a word in reference to the currents generated in the different coils upon the ring of the armature. Suppose the armature to rotate n times in a second in a constant field of unit strength. At any instant of time t, the electromotive force generated in a single coil would be proportional to the sine $2 \pi n t$. If the armature remain at rest and the bipolar field should alternate harmonically n' times per second, the electromotive force generated would be proportional to sine $2 \pi n' t$. If now the armature should rotate and, at the same time, the field should alternate, the electromotive force produced would be proportional to the sine of $2 \pi n t$ times the sine of $2 \pi n' t$. Suppose the armature to rotate at that harmonic speed which makes the ratio of n to n' as 2 to 3. The graphic combination of two sine curves of these frequencies yields a third curve, which represents the time change of electromotive force generated in the coil. In this case there is a succession of alternations at two different frequencies. One complete alternation at a certain frequency is followed by another complete alternation at twice that frequency. I do not know what mechanical action would result from this.

DR. PUPIN:—I suspected the difficulty which Dr. Sheldon mentions. The two currents are of unequal amplitude. The one of lower frequency has a smaller amplitude than the one of higher frequency, and Mr. Gutmann seems to rely for the total action upon the quicker field. Whereas, really, when you have two alternating currents of unequal frequencies, and they are magnetizing iron, then although the current of lower frequency may have a smaller amplitude, it may produce a stronger magnetic effect than the other current which has a higher frequency and larger amplitude and weaken considerably its magnetic effect. That may give Mr. Gutmann some difficulty. I must confess, however, that I have not examined the proposition thoroughly enough to speak with much definiteness.

THE PRESIDENT:—Is there any other discussion of this paper? It may interest the members of the INSTITUTE to know that Council this afternoon took an action which resulted in the appointment of a committee consisting of Prof. Crocker as Chairman, Prof. Kennelly, Mr. Caldwell, Mr. Stine and Prof. Shepardson to discuss and formulate a plan to determine whether or not it is advisable for the INSTITUTE to undertake the indexing of electrical literature. This grew out of the discussion which you remember we had at our last meeting here. I do not know whether the INSTITUTE would care to discuss the matter prior to the work of the committee. This committee is charged with the duty of determining whether such a thing is desirable, and second if it be desirable, to formulate a plan according to which it can best be carried out.

[Adjourned.]

DISCUSSION IN CHICAGO.

A meeting of the Western members of the INSTITUTE was held in Armour Institute on December 19. The meeting was called to order by Mr. Arnold, about thirty-five members and visitors being present. Mr. L. L. Summers was appointed Chairman.

Mr. Gutmann's paper was read in detail by the author.

MR. CARL K. MACFADDEN:—I have had the good fortune to witness a number of experiments that have been made on this system, and I would say that in this new plan, as is stated in the last clause of this paper, it will find a wide field of application. As far as I know there is no method of obtaining polyphased currents in any manner which will ensure a definite relation between the phases of the currents. The method here shown for getting a direct current from a single phase alternating current through a single machine is undoubtedly of great value. One of the drawbacks to the distribution of alternating current for power is the necessity for having more than two wires, and having special generators for furnishing such a current. If our simple single-phase apparatus could be used, and a current obtained which might then be converted into polyphase or direct currents, I think that we would find it very desirable.

MR. L. L. SUMMERS:—In the rotary transformer it is evident that as the motor has several speeds, the dynamo end, if used to develop polyphase currents, would be liable to furnish currents of variable frequency under variable load. The question of the efficiency of the motor, which is a most important one, would seem to depend upon the magnitude of the opposing forces which are represented in triangular figures. I should like to ask Mr. Gutmann what the efficiency would probably be in practice.

MR. GUTMANN:—In a motor it is desirable to have several speeds or many speeds changing gradually. This characteristic is however not essential or even desirable in a rotary transformer, especially if polyphased currents are to be distributed. We would select an armature construction which would have the tendency of retaining a fixed speed, and which is not easily thrown out of step, so that the frequency of the polyphased currents would remain constant.

The efficiency of the motor depends entirely on the construction of the armature, and also on the field. This should not be judged by the diagram, which is simply a convenient mode of illustrating the idea. It is of course understood that in any motor the internal waste of energy should be reduced to a minimum, and in the present case in proper machines the reactive force can be made very small.

MR. MACFADDEN:—One of the most interesting feature of this system to me, is the method of obtaining polyphased currents of any number of phases from a single phase alternating current, a single armature and a single winding. From a second winding a direct current might be obtained. Mr. Gutmann has suggested that the whole thing can be done in one armature, which I understand would not be a difficult matter to design. The principle involved is one that I think is applicable to almost all classes of alternating work, and I see many advantages in it from my way of looking at it that are not possessed by any polyphased system.

MR. SUMMERS:—In a rotary transformer for obtaining direct currents there would also seem to be drawbacks, for if an increased load were put upon the secondary, it would of course load the motor, and if this load is greater than the motor will carry at synchronous speed it would immediately drop to an harmonic speed, and if no system of compensating for the decreased speed is used, the motor would of course remain at this speed without difficulty as the power required would be less, and the torque at an harmonic speed would naturally be greater than at a synchronous speed, providing the power developed is the same. This decrease of speed of course causes a decreased output from the secondary. But suppose a method of compensating for this decrease be obtained, the load would not then be diminished by the decreased speed, and unless the motor is capable of delivering an increased power at the lower speeds, it would behave like an overloaded synchronous motor. What advantage would there be, Mr. Gutmann, in using this transformer say in place of a self-starting single phase machine connected with a direct current dynamo?

MR. GUTMANN:—A system of compensation may be adopted, as for instance, the placing of the field magnet c in series with the line, which would by its increased strength on increased load superimpose an E. M. F. on the armature conductors, compensating for the decreased speed. As regards a synchronous motor driving a direct current machine of any size, it will be clear that a third machine must be used for starting the synchronous motor, or at least some complicated devices, such as condensers, reactive coils or the like. This complicates a system considerably, and I think a self-contained device will at all times be preferred. This advantage will be appreciated for instance, if we wish to supply continuous currents for long distance railway transmissions, overcoming the distance by high potential alternating currents. The sub-stations would each contain but a single rotary transformer, while with the former disposition, a starting device, a synchronous motor and a continuous current generator, or three separate machines, would be required to replace the motor generator.

THE CHAIRMAN:—We have heard an interesting paper this evening, dealing with a subject of great practical importance. I think we all regret that Mr. Gutmann has been unable, or rather unprepared, to furnish data which would enable a comparison to be made with the present methods in vogue, both as to practicability and efficiency.

[Adjourned.]

[COMMUNICATIONS RECEIVED AFTER ADJOURNMENT.]

PROF. ELIHU THOMSON:—(*communicated*): In the winter of 1887-88, I made an alternating current motor for single phase currents having a pair of single phase energizing coils surrounding the revolving iron mass, upon which latter three coils were wound as is the ordinary three-coil winding of a Thomson-Houston arc dynamo. This machine with its three coils closed, was without starting torque, and was therefore provided with a commutator which short-circuited the coils while in proper position successively. This gave the desired starting torque. It was found that the coils after starting could be short-circuited and the commutator dispensed with. So far as I am aware this machine differed in no essential particular from Mr. Gutmann's, Figs. 1 and 2 of which disposition he evidently believed himself the originator.

He refers to such machines as "having a small starting torque *when rotated.*"

Surely a torque exhibited only after rotation is given, is not a *starting* torque, though it may be an accelerating torque, provided there is little friction and no load.

It is not unusual for single phase induction motors to exhibit such accelerating torque, and I have made many of them in which the relations of windings were very different from those referred to by Mr. Gutmann which gave similar results as the author points out. Such machines will not, of course, start from a state of rest unless some modification of the structure or its mode of operation be made with the purpose of securing starting torque. Some of such methods give starting torque quite comparable with the torque exhibited, while running under full load.

It has been, and is the practice of the General Electric Company to construct both polyphase and single phase induction motors with a three-coil winding on the induced or revolving part, kept on closed circuit through a resistance at the start, and on short-circuit at speed. Mr. Gutmann claims as a point of novelty in his described apparatus, "that the closed circuits do not coincide in number with the poles of the field magnet." My original machine, which, by the way, has been described in publications and patents, had two poles and three closed armature coils. This has not been novel since 1888.

The apparatus which Mr. Gutmann alludes to in connection with his Fig. 18, seems complicated. Involving, as it does, two commutators, one of them directly in the alternating current supply circuit, it would hardly appear to present any advantages over the motor generators, now well-known in the art, which receive alternating currents of single or polyphased character and commute them into continuous currents by the inevitable single commutator.

Mr. Ludwig Gutmann (*communicated*):—Noticing that the discussion in Chicago is incomplete and that in New York calls for a reply containing similar statements, I have condensed the two in order to answer the questions of several members in the East.

I wish to express at this place my thanks to Professor Pupin for his able exposition of the paper. The differences which, however, exist between a few of his views expressed in the discussion, and my own, are due mainly to his strict adherence to the armature diagrams, Figs. 1 and 2, and this adherence, while correct, is caused no doubt by the omission of certain parts which were left out with a view of shortening the already lengthy paper. I wish to remark further that these armature diagrams were selected on account of the simplicity which they admit in presenting the various actions and results of each winding or coil, while for efficient working a number of other dispositions would suggest themselves.

Mr. Wolcott has grasped the situation. The qualities which are shown around the synchronous speed are repeated at the harmonic speeds, which are positions of torque and not dead points, as Mr. Kennelly has assumed. Near synchronism, or at the harmonic speeds, the armature is in stability, and it takes power to pull it out of step; however, when the speed is changing from a given speed into another, the armature is temporarily in instability until the current has adjusted itself to the new speed.

Both Mr. Wolcott and Dr. Pupin are correct in their assumptions, inasmuch as the motor can either be synchronous or practically so-called non-synchronous. This will be clear if we select, for instance, Professor Ferraris' cylinder armature. (See Fig. 19.) This cylinder, disk or drum has in all positions a symmetrical relation to the energizing field, and the same magnetic resistance to the lines of force of the field contrary to Figs. 1 and 2. Therefore, it may be considered in step at any speed and in any position. This is caused by the continuity of the conductor in the direction of rotation, which allows the greatest possible slippage, owing to the absence of strongly defined poles, and the armature may come to rest in any position in the energized field. Such an armature can rotate at any desired speed which in a given energizing field depends on the load. The greater the load, the smaller the speed, and the greater becomes the amount of energy wasted in the latter by heating. Figs. 1 and 2 show a few interconnected coils, while the Ferraris armature repre-

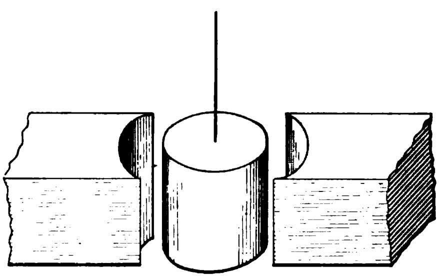


FIG. 19.

sents one of an infinite number of closed coils. The former develops strongly defined poles at given points, and nowhere else, while the cylinder will produce weak poles at any given point of its circumference. The armature, Fig. 2, will, therefore, rotate at certain fixed speeds and not at another, half way between any of the former. Between these two extreme cases belong those types with any other number of coils or circuits which may be designed for other speeds, changing abruptly or gradually. The greater the number of armature conductors or coils the smaller will be the difference of change from one speed to the next.

The remarks of Drs. Pupin and Sheldon regarding the action of the slower field are very pertinent. Certainly this second field may become detrimental in some machines; however, this fact has been recognized at an early stage. To make any motor efficient, reactive forces should be as small as possible, and they can be reduced to a few per cent. of the main force in machines of this type.

In reply to Professor Thomson I wish to say that the communication regarding his motor construction is something new to me. Looking over a number of references relating to the Professor's motors, I have failed to find an armature with closed sub-circuits, which has been used since 1891, especially in polyphase work. Had I known that I would raise a question of priority, I would have carefully avoided it. Should I be mistaken in this matter, and it be found that the devices are identical, which question is an open one, I would only be too glad to repair the error. The criticism regards "starting torque" is correct, accelerating torque is the proper term.

The practice of the General Electric Company is widely known, and while I have read of their polyphase and so-called monocyclic systems, I must confess that I have not heard or read anything in technical journals regarding the single phase motors and their installations, except perhaps the fan motor, the outside of which was described last year. Had the power motors given the desired results, I do not understand the unnecessary expense of the creation of the monocyclic system within the last six months. It may be contended that a motor with a commutator is objectionable, but I believe the necessity of a special generator and an additional conductor is still more objectionable. I hope Professor Thomson will pardon my ignorance of this practice of the General Electric Company as regards their single phase motors, which are no doubt motors operated on the ordinary two-wire circuits, if I understand correctly.

I fully agree with Professor Thomson that Fig. 18 would be for many cases a complicated machine, and I have tried to prevent the raising of this objection by stating that the device has been simplified to a considerable extent. It was not the object to show these devices up to date, but to show that the ordinary single phase alternating current can be split up at any point into any number of phases lagging at a constant angle, which, to my knowledge, has not been done heretofore, and secondly, that a constant current can also be produced by such a machine, also at any point of the ordinary circuit. The state of the art has been, as far as I am aware, to commutate a single alternating current into a pulsatory one by means of a commutator rotated synchronously, or by leading into a motor generator bi or polyphased currents and commutating these into continuous currents. However, neither the method of changing a single alternating current into a continuous current, "now well known in the art," to which Professor Thomson refers, nor the machine accomplishing it, are known to me.

RAIL-BONDING, AND ITS BEARING ON ELECTRO-LYTIC CORROSION.

BY GEORGE P. LOW.

[*A Communication in Discussion of a Paper, by Isaiah H. Farnham, on the Destructive Effect of Electrical Currents on Subterranean Pipes, read April 18th and 25th*, 1894. See p. 191 *ante*.]

Investigations into the characteristics of electrolytic corrosion of underground metal work by the earth return currents of electric railway circuits, and the methods suggested for remedying the troubles therefrom, lead to the conclusion that the problem of electrolytic corrosion and of rail-bonding electric railways may best be considered conjointly, for in the solution of one, may be found that of the other. The conditions of the many electric railway systems I have examined on the Pacific Coast, demonstrate that in such roads electrolytic corrosion is evidenced when the return circuit is in one or more of the several conditions soon to be described.

As explanatory of the terms used, the word "main" refers to the water main, gas main, Edison tube, telephone or telegraph cable, or the other underground metal structure that may be affected. In compound words consisting of two nouns connected by the preposition "to," a noun appearing first indicates that it represents a positive polarity, as in the expression "main to-track-bonding," the bonding is between a positive main and a negative track. Conversely, "track-to-dynamo-bonding" indicates that a track having a positive polarity is bonded to the negative side of the dynamo. All generators are assumed to feed positive to line. By "cross-bonding" is meant the bonding between main and track without reference to the polarity of either.

The several conditions referred to as causing electrolysis, and which describe actual cases, are as follows:

1. When the track conductivity is low.
2. When the track is bonded with various points of mains

without regard to the polarity of the main or track, and is poorly bonded or not bonded with the negative of the dynamo.

3. When mains connect one portion of a track to a distant portion, there being no proper cross-bonding.

4. When the mains show positive potentials at one or more locations, there being no main-to-track or main-to-dynamo bonding at such points.

The preceding present the ordinary conditions conducive to aggravated forms of electrolysis, all of which may be summed up, though less distinctively in the single declaration that *corrosive*

FIG. 1.

electrolysis occurs when a main shows a positive potential at any point, without proper main-to-track or main-to-dynamo bonding.

5. When the track is not bonded to main or dynamo, there being only main-to-dynamo bonding.

This case is significant in that it exhibits an electric railway system which shows unusual considerations for the interests of a water company, to the material detriment of the coal pile of the former.

6. When the track is intimately cross-bonded with mains throughout, without regard to the polarity of either main or

track, there being no track-to-dynamo or main-to-dynamo bonding, and ground plates only being used.

The importance, not to say enormity, of Case 6, is shown in a remarkable example of corrosive electrolysis that was uncovered in the writer's presence. In a certain lighting and power station situated off the line of the railway system, an earth return was used exclusively, in order to avoid the expense of carrying negative feeders out to the track. Ordinary ground plates proving inadequate, a two in. iron pipe with a No. 0000 wire inside, was driven to a depth of about ten feet to the water line under the station, and a second No. 0000 wire was carried out to the river

FIG. 1*a*.

several hundred feet distant and sunk, with some 40 feet of bare wire attached. This being still inadequate, a No. 0000 wire was attached to a six in. water main running into the river, and which was independent from the city mains, when good results were obtained. The method of cross-bonding consisted in tapping ⅝ in. tinned brass plugs into the cast-iron water main at intervals of 1600 feet over the entire system. Four strands of No. 4 wire or two strands of No. 0 wire were used in this cross-bonding. These wires were soldered to the plugs, and driven-bonded into the rails after the usual manner. In making alterations in the power-house last summer it was deemed advisable to lower the height of an engine which rested upon a foundation probably 15 feet deep, the foundation consisting of one part of Portland cement to three parts of clean sand and five parts of broken stone. The engine was bolted down to this by means of six ⅞ in. bolts, each about 4 feet long, each bolt being sulphur centered in a 2 in. iron pipe, 4½ feet long, the pipe being set in the cement, the bolts projecting above. The whole foundation top was flushed with ½ in. of sulphur, upon which the engine was placed. Upon removing the engine and chiseling off 8 in. of concrete it was found that four bolts were in prime condition. The fifth bolt which was out of the pipe center because of having been bent, was corroded on the side nearest the pipe to a depth of approximately ⅛ in., while the iron it had lost was deposited in the form of a sulphide upon the inner side of this pipe. Figs. 1, 1*a*, show the condition of the sixth bolt as it was taken from the concrete. Parts of the interior of the pipe bear the familiar marks of electrolytic pitting, probably occasioned at a time when negative was fed to line. The fact that the bolt was entirely corroded through is evident, as is the further fact that the iron taken from the bolt is deposited upon the inside of the pipe. The photographs clearly show streaks or veins of impurities that were developed in the sulphur which, it would appear, presented the usual conditions favorable to corrosive electrolysis. Had it been possible to reach the lower end of the pipe, marked corrosion on it would have undoubtedly been found. Obviously the principal condition leading to this experience was the fact that the boilers, engines, steam fittings, and even the concrete foundation served as a path for the return current. That the current was making a frantic effort to reach the ground plate was evident, and it would be hardly possible to find a more significant specimen of the insidious character of corrosive electrolysis, which is here proven capable of attacking ordinary inaccessible metal substructure. Having learned of this occurrence, the prudent station manager will see that his machinery shows absolute zero to the negative of his railway generators, or perchance some day his most trusted engine may gyrate through the station, causing ruin that would make a tornado envious.

In addition to the six conditions named, which are taken from actual experience, there yet remains a case that is as distinctive as it is perplexing. Both the corrosive action itself and the local conditions are so unusual that all details will be related. Fig. 2 is a map showing the location of the power-house and the affected water main, which consists of an 8-in. kalomine pipe under a street of a city, that is undoubtedly one of the most trying locations for underground iron work on the face of the globe. This is owing to the fact that the earth is almost saturated with a solution of sulphate of copper that seeps down from enormous copper mines on the hills, through the ground under the city. The avidity with which this solution attacks iron is shown in the fact that the iron pipes used in pumping the cupric sulphate water out of the copper mines seldom lasted a month, until a preventive was found in lining the pipes with wood. Kalomine pipe withstands copper solutions better than cast or wrought-iron, but now an extraordinary trouble has appeared that seems to present a new and unrecorded form of corrosive electrolysis.

The map shown in Fig. 2, together with the explanation ac-

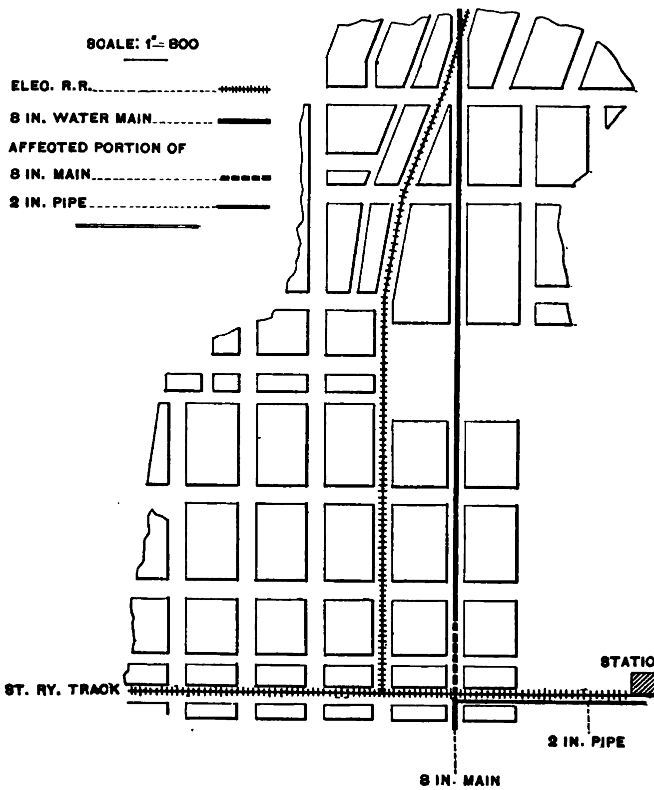


FIG. 2.

companying it, conveys all information necessary concerning the location of tracks, water main, the power-house, etc. The negative of the generators is grounded by two No. 0 wires, one to the track and the other to the 2-in. iron water pipe furnishing water to the station from the affected main. The affected portion of the 8-in. main referred to is about 300 feet in length, as shown on the map, and so serious is the action that its life seldom exceeds eight months. The peculiarity of the corrosion is in the fact that the pitting invariably occurs on the inside, working outward.

Fig. 3 shows a photograph of the interior of a short length of this pipe within 22 in. on which are 18 perforations, all of the character described, and inside the pipe are clearly seen other pittings that have penetrated from within outward to various depths,

FIG. 3.

from the merest fraction of an inch to almost a complete perforation, also the sediment running longitudinally along the center that has been deposited from ordinary causes.

Fig. 3*a* exhibits the same specimen with the sediment on the right hand portion of the pipe scraped off, in order to show more clearly the fact that the pittings occur from the inside.

Fig. 3*b* shows the specimen set up and photographed against the sun, which proved the only way by which a satisfactory

FIG. 3*a*.

photograph of the perforations could be obtained. In one instance the sun being immediately in range, the perforation is greatly exaggerated.

Fig. 3*c* is from an inferior photograph of the reverse side of the plate, which is in reality of a very smooth surface with many perforations.

A great deal of the same pipe is used throughout other por-

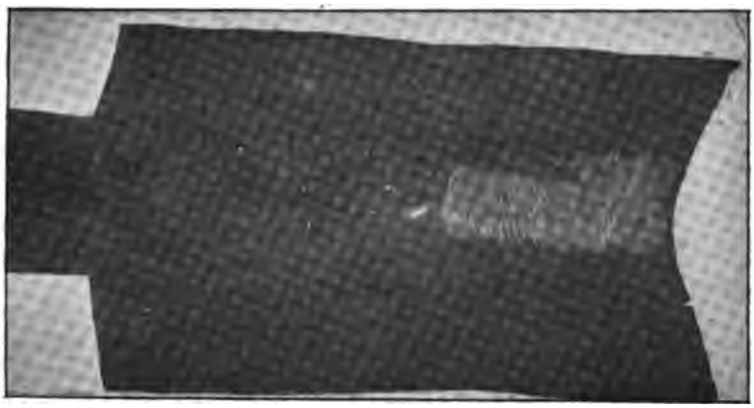


FIG. 3b.

tions of the city, but the internal pitting described occurs only on the 300 foot length defined. The exterior surface presents the usual rusted appearance, and in view of the opinion recently expressed by an eminent authority,[1] that the corrosion of nascent

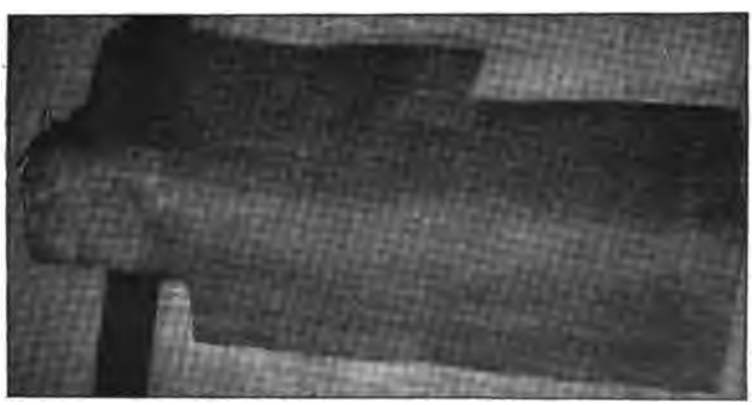


FIG. 3c.

oxygen liberated by the electrolysis of water is not an effect of practical magnitude, it is difficult to assign any satisfactory cause

1. Prof, D. C. Jackson on "The Corrosion of Iron Pipes by the Action of Railway Currents," read before the Western Society of Engineers, July, 1894.

for the action. It has not been possible to obtain an analysis of the water in the pipe, but it is obtained from mountain streams 14 miles away and tastes pure and wholesome. Nor does it appear possible that the soil could affect the main internally. The fact that the trouble occurs at the point nearest the power-house affords circumstantial evidence against electricity, which is almost confirmed by the further fact that internal pitting and perforation is not to be found elsewhere in the city, even with the same pipe, water and other external conditions, excluding only the proximity of the power-house. It is for those having facilities for experiment to point out the action occasioning this internal corrosion and to indicate a cure.

Returning again to the usual forms of electrolytic corrosion, to relieve the main from injury, excepting that due to internal pitting and electrolysis at low conductivity joints in mains, it is advisable that the return current be confined to the track as closely as is possible. The most competent method of doing this in a double-track road, appears to be in an adaptation of the three-wire system, in which one trolley wire forms the positive, the other trolley wire forms the negative, and the rails forming the neutral. In this particular arrangement, which is different from those three-wire applications now in use in different localities, if the rails and tracks are properly bonded it is clear that under normal conditions the earth will be free from the enormous return current that at present seeks its way to the power-house by every possible channel.

Regardless of the carrying out of this eminently practical suggestion, however, electrolytic corrosion of mains from the earth return currents of electric railway circuits. will not occur when the following requirements have been complied with.

I. No cross-bonding at points where the mains show permanently negative potentials.

II. Heavy cross-bonding at points where the mains show permanently positive potentials.

III. Rail bonds of absolute permanence and reliability.

IV. High conductivity in track return circuit.

V. Greatest possible resistance from tracks and mains, at points where the mains show permanently negative potentials.

VI. Generators to permanently feed positive to line and negative to track and main.

VII. Heavy bonding between the negative side of generators to various proper points of mains not heretofore designated.

It is hardly necessary to emphasize the fact that trouble occasioned by corrosive electrolysis is largely due to the inadequate conductivity of the rail return circuit, or, to repeat the oft-told truth that the difficulty attending the use of the track as a return, is at the rail joint. The welding of rails is now believed to have reached the commercially successful stage, and it certainly forms a perfect construction in every sense, but up to the present, it has not come into general use. Whether its slow development has been due to inherent defects or to demands for excessive royalties or other causes, has not been explained. A remedy may be found in another direction, and in looking for it the subject of rail joints must be thoroughly digested, not only electrically but from mechanical standpoints both in steam and street railways.

The method of joining together the abutting ends of rails is known as fishing, a term doubtless originated by Mr. Thos. Tredgold, one of the honored fathers of the profession of civil engineering, who defines fishing as perhaps the simplest and best method of joining beams. Continuing in the same strain Mr. H. Conybere, C. E., says:

"Of rail joints there are two varieties, the suspended and the "insistent fish. The former has to perform two functions, the "latter only one. The suspended joint had first to act as a "girder by giving vertical strength to the joint between the two "rails; the fishes therefore required to be long and deep as pos-"sible, to be fastened by four bolts, and to have the bearing "edges as nearly horizontal, in order to keep the two ends of the "rail in accurate opposition so that the joint might offer no ob-"stacle to the smooth moving of the wheels. The insistent fish "was not required to add to the vertical strength of the joint, "such joint being supported by the joint sleepers or ties, the "only function it had to fulfil was to maintain the extremities "of the continuous rail in accurate opposition. For this pur-"pose a much shorter and shallower fish than would be required "in suspension, answered every purpose."

Another writer says:

"If rails were very deep the fish-joint would leave little to be "desired. When a heavily loaded train is caused to run along a "rail, it will be found to continually deflect under the insistent "strain, and this deflection is transmitted onward in advance of "the wheel, the rail assuming a curve in a vertical plane of "greater or less length. When the wheel approaches a weak "joint on a shallow rail, the curve is rudely broken. The stiff-"ness of the joint principally depends on the section of the rail, "often only a third of the stiffness of the joints is afforded as "compared with the solid rail. Improved fish-joints give 80 "per cent. of the stiffness of the solid rail."

Experiments made with two deep fish-plates having supports two feet between the centers of ties, and carrying a load of 25 tons shows more deflection on the solid rail than on the joint. The bolts were not the least hurt during the test. (Sandberg.)

A certain amount of elasticity is essential to the durability of the rails, which is the reason for the preference given to wooden ties. The compression of new ties under an engine, is found to be from $\frac{1}{4}$ in. to $\frac{3}{8}$ in. sometimes with old ties. Besides the movement in the tie itself, there is also a movement of the tie upon its bed, and these are the sole causes of the deflection of the rail. Considered as a beam to support an insistent weight, a rail of over 60 pounds per yard is abundantly able to stand up under five times the pressure that it will ever be called upon to bear. As regards deflection under a swiftly passing load, Mr. Bidder, President Institution of Civil Engineers, England, says, that if there was no rail in a space of three feet, at 40 miles per hour, the wheels of an engine would not deflect more than $\frac{1}{8}$ in. from gravity, therefore if a rail deflected more than $\frac{1}{8}$ in., no further pressure from the engine could occur. Capt. W. H. Taylor, another authority, expresses the opinion that when the rails are shallow, or are wanting in depth, the blows become serious, but when the section is deep and the ties are sufficiently near together, the deflection of rails under a passing load, is imperceptible. The most instructive expositions of the principles of track construction, are two pamphlets on "Smooth Track" from the pen of Mr. Philip Noonan of Troup, Texas, who is a pioneer in the advocacy of continuous rails and to whose researches is due much that is known concerning wave motion in the rails.

It is evident from the foregoing, that steam roads are designed to operate on a yielding roadbed, which gives from less than $\frac{1}{8}$ in. to $\frac{3}{4}$ in. or more, as the train progresses, the train being continually preceded by a wave of depression. This is a feature that becomes marked in considering the points of divergence between the requirements of steam locomotive railroads, and street railroads. The latter is required to be operated at from 5 to 15 miles per hour; the routes they traverse are occupied jointly by all kinds of vehicles, hence the conditions are that these streets and highways shall be so paved as to support such traffic. This requires the placing of the top of the ties 6, 7, 8 and 10 inches below the surface of the street, and has necessitated the use of much heavier rails than those required on other lines of railway. When it is known that a rail is to be regarded as an iron beam, the stiffness of which is as the cube of its depth, the fact that the key for deriving a solution of the question of how to obtain a smooth line of rail has been reached, can be realized. The supported joint is preferable for street railroads because of the solidity and immovable character of the roadbed; the locomotive roads require the suspended joint so as to be in keeping with the elastic character of their roadbed. The difference between the two roads lies in the magnitude of the wave of depression passing along the rails under the traffic peculiar to each system. In the steam road the wave of depression is clearly defined and considerable; in the street road with its concrete bed and perfected construction if such wave exists at all it is negligible, and owing to the extreme rigidity of the joints, the abutting ends of rails certainly have no appreciable play. This is a point to be borne in mind.

The expansion of rails from increased temperature presents a further point that has long been considered insurmountable, but recent experiments of great engineering value indicate the end of trouble from this cause. Mr. A. J. Moxham,[1] in experimenting upon a roadbed in actual operation, placed 1100 feet of 78-pound track in such a manner that it practically formed a continuous rail. The joints were made with the utmost rigidity, and many thousand temperature and other essential readings were carefully taken periodically day and night during the months that the experiment lasted. In Mr. Moxham's own words:

"The experiment proved absolutely and beyond cavil that the "track is restrained and held by surface friction of the surrounding roadbed. From first to last, from a temperature 22 degrees "below freezing point (or 10 degrees) to a temperature of 89 "degrees above freezing point (or 121 degrees) there was absolutely no movement of the track out of place. Even at the "ends this was true; proving that not only will the roadbed hold "the track as a complete structure, but that it will do it consecutively. Once bedded, it will hold a rail 10 feet or 30 feet as "well as one 110 feet. On this point there is no room for "error."

A further experiment in this direction was made by Mr. Wason of Cleveland, who, in order to get as nearly as possible a continuous rail, put down 1000 feet of track, riveting the joints with red hot bolts put in by boiler makers, and placing the ends together as closely as possible, six rivets to each joints. It was just as straight after ten months as when first put down, the joints being absolutely imperceptible.

It is now clear that the deep rails being imbedded in a solid roadway, enables the engineer to defy the forces of expansion in the rails; he therefore prepares the ends so that they closely abut, and then double bolts, rivets or welds the joints so as to make a practically continuous rail. In the case of a steel rail buried in the earth, the necessary expansion on account of increase of temperature consists of a minute enlargement of the sectional area. thus following the direction of least resistance. A fruitful source of trouble on street railroads, has been the space unnecessarily left between the rail ends for expansion, as practiced hitherto by steam roads. The wheels of electric motor cars and even cable cars soon pound these places sufficiently to cause considerable deflection. Another point which will assist in maintaining perfect steel lines for street railway traffic is to adopt the four-wheeled truck system, eight wheels under each car, the motor to be attached to the two axles nearest the center of the car. By this means, the forward wheels, being idlers, will carry forward the minute wave of depression to be further depressed by the driving wheel carrying the greatest weight. This will tend to preserve the perfect line.

In view of the numerous references at hand, one can not ques-

1. Proceedings 11th Annual Convention of the American Street Railway Association, Cleveland, 1892.

tion the fact that recent practices have made it possible to obviate two details of track construction that have hitherto been considered insurmountable in rendering rails electrically continuous, namely, extreme rigidity at rail joints and perfect control of the

FIG. 4.

longitudinal expansion of rails. The art of street railway building having advanced sufficiently to enable the construction of mechanically perfect rail joints, the problem of using the rails exclusively for the return circuit may now be attacked without fear of failure from the difficulties attending either bonding or corrosive electrolysis. To comprehend the importance of the return circuit problem, it is instructive to carry out a line of reasoning that has appeared in a communication published in the *Street Railway Review*. "In steel rails" the article states, "it is usual "to say that every 10 lbs. weight per yard means one square in. "of cross-section. Four rails of 70 lbs. per yard = 28 square "inches in section = a steel bar 4 in. x 7 in. = a copper bar 1 "in. x 5 in."

The conductivity of a copper bar having an area of 5 square inches is equal to that of 38 No. 000 B. & S. wires. If, therefore, the four 70-lb. rails of a double-track system were electrically continuous and carried current at a density equivalent to 1000

FIG. 5.

amperes per square inch of copper, they would carry 5000 amperes a distance of one mile with a loss of but 88 volts, or less than 15 per cent. if the initial E.M.F. is 600 volts, or 16 per cent. if it is 550 volts, or 17.6 per cent. if it is 500 volts. This is a loss in potential of less than 1.8 volts or $\frac{36}{100}$ of one per cent. per 100 amperes per mile. The attainment of these results by the electric railways of the country would mean an annual saving of many thousands of dollars.

The communication referred to concludes by defining the following conditions as affording the best return circuit.

"1. Intrinsic resistance low enough to need no help from the "earth.

"2. Utilization of rails as return conductors.

"3. Rail bonds of heaviest practicable size.

"4. Rail bonds of shortest possible length.

"5. Rail bonds protected against corrosion.

"6. An underground main or trunk return from the power-"house to the track, and there connected with each line of rails,

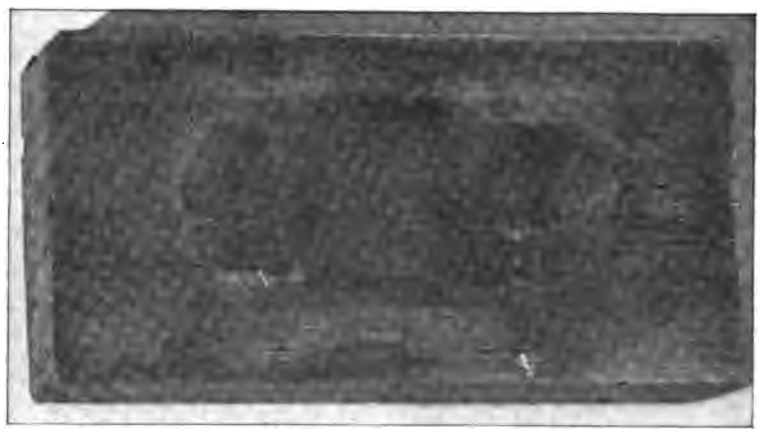


FIG. 6.

"and low enough resistance to carry the maximum current with "but slight drop in potential."

It will be conceded that these conditions are thoroughly comprehensive and that their correctness is more apparent than is a means for their accomplishment, but to Mr. Frederick T. Newberry, a well known civil engineer of the Pacific Coast is due the credit of devising a method of bonding that so completely satisfies every possible condition in a thoroughly practical way, that only in extraordinarily large systems will the trunk return be necessary. This method is termed "direct bonding," and in it the bond consists of merely a copper dowel that is shrunken-fit into holes drilled into the abutting rail ends. The bond is formed by reaming out holes in the ends of the rails to be coupled one in the head and one or more in the bottom flange, as shown in Figs. 4 and 5. For a 70-pound rail the preferred diameter of the holes is $\frac{5}{16}$ in. These holes are to be bored just before placing the rail, which is then heated by a gasoline torch, and a round bolt or dowel of pure soft copper, two inches long and slightly larger than $\frac{5}{16}$ in. in diameter is driven into one rail. Then the abutting rail is forced upon the other end of the dowel, the faces of the rails being brought as closely together as possible. The fish-plates are then placed and double-bolted, riveted or welded together. The carrying capacity of these two dowels would be equal to one-half that of the rail, or equal to a bar of copper $\frac{1}{2}$ in. wide by $1\frac{1}{2}$ in. thick, which is amply sufficient to carry the return current of any road now in operation in this country. If it were found advisable to double this capacity, it would be done with three dowels, one of $\frac{7}{8}$ in. in diameter in the head, and two of $\frac{5}{8}$ in. in diameter in the bottom flange. The junction of the copper and steel in the interior of the rail is electrically perfect, and remains so as long as the rail continues in place. When the work is well done, no particle of copper will be exposed to any action from air, water or other agencies, and being completely enveloped in the rail will be perfectly protected from fracture or harm of any kind.

The connecting surface of this bond is seven times its area, thereby affording the rail a carrying capacity at the point of bonding equal to that of the bond itself, and with rails 30 feet long, 352 joints per mile, 70 pounds of copper will be consumed in bonding a single-track mile. Experience proves that no great heating of the rail is required to effect an immovable grip on the copper dowel, which is polished and placed in the freshly bored hole in the rail end that has been heated at so low a temperature that there is not the slightest oxidation. Additional rigidity is secured by rendering the dowel ends slightly conical, so that they are upset in being driven home. A sectional view of the dowel bond is shown in Fig. 6, consisting of two cubes of steel, each $1\frac{1}{2}$ in. on the side, into which a $\frac{5}{8}$ in. copper dowel has been shrunken-fit. It does not appear that a more perfect union could have been made even by welding. According to Crosby and Bell, the cost of single No. 0 bonding is $600 per mile, viz., $400 for material and $200 for labor per mile of single track. The cost per mile for boring and setting direct bonding having a carrying capacity of 600 amperes is estimated at $150. Adding a royalty of 50 cents per joint, makes a total cost of $326 per mile of single track, while with $\frac{23}{32}$ in. dowels having a carrying capacity of 800 amperes, the total cost is estimated to be $400 per mile. Among the advantages of direct bonding will be enumerated:

1st. The practical elimination of the factor of length in bond wires.

2d. The use of bonds of maximum cross-section.

3d. Most perfect means available for effecting electrical contact between bonds and rails, hence

4th. Highest possible conductivity in bonding, and

5th. Absolute permanence.

6th. Perfect protection against water, acids, alkalies, gas, atmospheric or other corrosive influences.

7th. Minimum consumption of copper in bond wires (approximately 70 lbs. per mile at 15 cents per lb., $10.50).

8th. Carrying capacity limited only by that of the rail.

9th. No cross track bonding.

10th. No supplementary bonding.

11th. No rivets, solder, paint, screws, bolts or channel pins.

12th. No electrolysis.

13th. Practical equivalent of welded rails from almost every standpoint.

14th. Easily attached to finished systems at a

15th. Cost not exceeding that of present methods of bonding, and at a

16th. Cost far less than that of welded rails.

A marked peculiarity in the operation of electric roads consists in the fact that for the same results in foot-pounds, electric roads require more than twice the boiler and engine duty that is necessary on steam roads. A brochure recently issued by a leading electric company states that although a ton weight may be moved on steam roads by an expenditure of force equal to a pull of eight pounds, which is the usual estimate, yet electric roads require for the same duty a steam and electric plant capable of exacting an effort of 30 pounds, this despite the fact that the line of track of a well-built electric road is far superior to that of steam roads in the true, even and immobile character of the rail surface. A perfect system of rail-bonding will effect an increase in efficiency of an electric road by various degrees of from 5 per cent. to 20 per cent., and the writer knows of at least one instance wherein perfect bonding would increase the efficiency of that particular system by not less than 25 per cent. It must be conceded, therefore, that a very material part of this extraordinary loss will be obviated by perfect rail-bonding.

A prevailing practice that might in generosity be termed an inconsistency, is that of bonding a wire to the web of a rail and then rating the carrying capacity of the bond as that of the wire. This fact suggests a criticism. A No. 0 B. & S. wire, for instance, has a circumference of 1.021 in., while the web of a rail is generally .375 in. in thickness. This multiplied by 1.021 and divided by 5.63, gives the carrying capacity of the rail surface exposed to the bond wire as the equivalent of a copper wire having an area of .068 square inches, or about 82 per cent. of the conductivity of the No. 0 bond. Under these generally prevailing conditions, the carrying capacity of such bonding is overestimated by about 20 per cent. To enable the copper bond to take a volume of current up to its full capacity, its surface contact with the rail must be roughly six times the area of the wire. It is obvious therefore, that the web of the rail is not the proper place to attach bond wires.

To illustrate: the *Street Railway Journal* for January, 1894, contains this statement regarding the bonding of rails on the lines of the Electric Traction Company of Philadelphia:

"A bare copper No. 00 wire is used for electrically connecting "the rails. This is laced in and out of the adjoining ends out-"side of the angle bars (fish-plates) three times, so that the carry-"ing capacity of the bonds at each joint is three times that of a "single No. 00 wire, and is equal to that of a 90-pound girder "rail of the Pennsylvania Steel Company."

The fact is then, that the rails are triple lace bonded through the web with No. 00 wire. The bond wires alone, have a carrying capacity of 652 amperes at 122° F., but as they (being laced) make contact with the rails only by means of two holes in the web, the actual surface of steel exposed to the copper wire is but .859 square inches, whereas it should be at least 1.765 square inches to obtain the full carrying capacity of the three No. 00 wires used. From this it is evident that the bond will carry but one-half the current claimed, but after theory and practice have compromised the case, it may be found that the duty actually performed will approach the designed duty to within 20 per cent. or 25 per cent. This will reduce the advisable carrying capacity of the rail bond to less than 500 amperes. Again the statement quoted, claims that the carrying capacity of the bonding is equal to that of a 90-pound rail. The carrying capacity of a 90-pound rail is approximately 1500 amperes, against which 500 ampere bonding appears small, particularly in a system proposing to consume 10,000 horse-power. As scores of smaller enterprises anxiously await the lead of the great companies and follow implicitly in the same path, too much caution in publishing such statements as that quoted can not be exercised.

In conclusion, the art of rail-bonding now appears to have been perfected, and the damage that has been caused by corrosive electrolysis may be attributed to defective bonding, for without doubt, proper main-to-track, rail-to-rail, and track-to-dynamo bonding will cure the ill almost without exception. Should the street railway companies delay in correcting the evil it is probable that municipal authorities will take action for the protection of citizens in their vested interests, in which event the conditions now existing may be further and seriously complicated by ill-advised municipal exactions. The problem of eliminating electrolytic corrosion is, in brief, simply one of judicious bonding.

San Franciscö, Oct. 12, 1894.

OBITUARY.

Franz Schulze-Berge, Ph. D., [*Associate Member, Februrary 7th*, 1890, *Member, April 21st*, 1891,] was born January 2d, 1856, in Ober-Cassel, Germany. After graduating from the Gymnasium at Bonn, he studied mathematics and physics in the universities of Heidelberg, and Strasburg, and then worked, principally under the guidance of Professor Helmholtz in Berlin. In 1880 he took the degree of Ph.D. in Berlin, for which degree Professors Kerchoff and Helmholtz were his principal examiners. Under the guidance of Professor Helmholtz, Dr. Schulze-Berge conducted, after methods of his own, experimental researches concerning the contact potential difference between metals and gases. The theme of his dissertation was of especial importance scientifically, since in physical investigations this potential difference is nearly always present as a disturbing element, as all metals are, generally speaking, in contact with gases.

His investigations involved the greatest nicety of manipulation and his methods evinced great originality. It was for his thesis on this investigation that he received his degree of Doctor of Philosophy.

Almost simultaneously with his work, the results of a similar investigation by another party were published, which disagreed with those obtained by Schulze-Berge. The latter at once prepared another paper in which he proved the correctness of his own conclusions and the error of those reached by the other author.

After graduating in 1880 he passed an examination for Ober-Lehrer and occupied this position at the Louisenstaedtisches Gymnasium at Berlin, and that of Gymnasial-Lehrer at the Gymnasium at Charlottenburg.

He also, during this period became a member of the "Berliner Physikalische Gesellschaft" and as such had charge of the official annual reports on different branches of electrical science, which were published in the *Fortschritte der Physik* edited by the society, and at the same time continued electrical work in the laboratory of Professor Helmholtz. It was at this time that he was a co-worker with the late lamented Heinrich Hertz. He next turned his attention to the investigation of the electric conductivity of so-called non-conductors in very thin films (of $\frac{1}{10}$ of a millimeter or less). The results of these investigations and others are published in *Weidmann's Annalen*, vols. xii. and xv. and in the *Sitzungsberichte der Berlin Physikalischen Gesselschaft*, 1880.

In 1887 Dr. Schulze-Berge came to this country and became assistant to Mr. Thos. A. Edison. Perhaps the most notable of his work with Edison was the solution of the problem of duplicating phonographic records which was intrusted to his care. The method employed of producing a conducting film upon the wax cylinder, of such a character as not to sensibly obscure or modify the finest harmonics, was the result of long study and experiment and evinced the originality of the man. It was an entire success.

Leaving the employ of Mr. Edison, he retired to Brooklyn, where for the past year or two of his life he was engaged in his own private laboratory in physical investigations of various kinds, the results of some of which will doubtless soon be published. One of the results of his recent work has been a rotary mercury vacuum pump, an earlier form of which he described in a paper before the International Electrical Congress at Chicago in 1893. This has been still further improved upon, and adapted to the exhaustion of lamps, but the new form has not yet been described.

Dr. Schulze-Berge had long been suffering from an organic disease, the exact nature of which was not known until after his death. He died suddenly in Brooklyn on March 21, 1894, of diffuse nephritis, in the 39th year of his age.

Personally he was of a very retiring disposition and a man of such extreme modesty, that his nature recoiled from notoriety of any kind. For this reason he was less widely known than he was entitled to be through his natural gifts and his contributions to science. He rarely contributed to the technical press except to give the results of some important investigation. He broke away from this rule which he had made for himself, however, shortly before his death, the occasion being the contribution to one of the electrical journals of a biographical sketch of his friend Heinrich Hertz. This was his last appearance in print.

Though he did not count a very wide circle of personal friends, his manner was so kind, so gentle, and so evidently sincere, that none came within his influence but to love him, to admire him, to respect him, both as a man and a scientist.

ALEXANDER HENRY BAUER, [*Associate Member, February 7th,* 1890, *Member, April 21st,* 1891,] who since 1882 has been actively engaged in practical secondary battery applications for light and power purposes, died in New York City, January 15th, 1895, of Bright's disease. He passed away quietly and suddenly, while apparently in full possession of his reasoning faculties. Mr. Bauer who was born in Baltimore, August 9th, 1846, had spent his working life in electrical pursuits, due possibly to hereditary traits, his father, William Henry Bauer, having been in service on the original Baltimore and Washington line of Prof. Morse, and was also a friend and admirer of Prof. Henry. "Aleck," as he was familiarly called by his many old-time friends, entered the service of the Baltimore and Ohio Railroad Company in 1859, as a messenger boy. In that capacity he learned telegraphy, and at the beginning of the civil war, was sufficiently advanced to be assigned to regular duty at the Annapolis Junction office. Up to the year 1864 he was actively engaged as a telegraph operator, in the government as well as railroad employ, in various West Virginia offices, and afterwards entered the service of the United States Telegraph Company at Philadelphia. He subsequently returned to Baltimore, where he was employed by the Bankers and Brokers Telegraph Company, and latterly by the Western Union, where he was appointed manager of the Commercial News Department, which included the "ticker" service. Upon the completion of the lines of the Mutual Union Telegraph Company, he was appointed manager of the Baltimore office, but after the amalgamation of that company with the Western Union, he gave up telegraphy which, in his opinion, no longer offered a field for advancement.

The importation from Europe of 60 cells of the Faure secondary battery in 1882, offered him an opportunity to enter upon what proved to be his future branch in the rapidly growing field of electric lighting. In a paper on "Secondary Batteries for Light and Power," before the INSTITUTE[1], presented May 16th, 1886, Mr. Bauer gave a very interesting history of three years practical experience, based upon the introduction of the Faure battery in electric lighting, street railway and telegraph service. This paper was of especial value at the date when it was written, as very little information upon the subject treated, was accessible. He continued in this line of work with the Electric Storage Company, of Baltimore, and the Electrical Accumulator Company, of New York, until 1888, during which period he became more generally known in the rapidly growing ranks of the elec-

1. TRANSACTIONS, vol. iii, p. 129.

trical fraternity. The introduction of the secondary battery for the lighting of railroad trains led to his appointment, on February 18th, 1888, as electrical engineer of the Pullman Palace Car Company, a position which he held at the time of his death. At the Chicago meeting of the INSTITUTE, June 6th, 7th and 8th, 1892, Mr. Bauer read a paper[1] on "Railway Train Lighting," containing a description of the system used in the Pullman service, together with valuable data as to the cost of lighting, made up from actual records. These two papers by Mr. Bauer are characteristic of the man, and showed how thoroughly he had mastered all the details of the actual merits of the secondary battery for the service with which he had been most prominently identified.

In thus giving to the public through the INSTITUTE, the benefit of his investigations extending over a decade, Mr. Bauer performed a service which is no doubt appreciated by all whose duties have led them in similar directions. He also made many improvements in train lighting, some of which were patented. Recently his headquarters were at Jersey City, where he was enabled to personally supervise the apparatus on such through trains to the south and west as were provided with electric lighting plants. Mr. Bauer had a wide circle of friends who were warmly attached to him, all of whom were deeply shocked by his death, not being aware of the fatal character of his illness. He leaves a widow and one son, William F. Bauer, who following the example of his father and grandfather, is also engaged in electrical pursuits, and who was elected an associate member of the INSTITUTE, April 15th, 1890.

RUDOLF EICKEMEYER, [*Associate Member, September 20th,* 1893, *Member, April 18th,* 1894,] was born in Altenburg, Palatinate, Bavaria, October 18th, 1831, and was educated at the Polytechnic Institute, Darmstadt, Hesse. In 1850 he came to the United States and was first employed on the Erie Railway, and shortly after in the Buffalo Steam Engine Works. On the first of September, 1854, he established himself in business at Yonkers, N. Y., first as a repairer of tools, and subsequently as an inventor and manufacturer of hat machinery. Mr. George Osterheld, who came from Europe with him, was his partner in this business. Before interesting himself in electrical work in

1. TRANSACTIONS, vol. ix, p. 445.

1878, he had taken out about 150 patents. He continued his electrical experimenting and investigating, at first in the line of telephony, and in 1882 invented the type of dynamo which has since been known by his name. He began its manufacture for commercial use in 1886.

Owing to its compact form and high efficiency, this ironclad dynamo has been found especially adapted for railway train lighting. The motor proved equally desirable for street railway work, and its plan of winding was used in the Edison system. Mr. Eickemeyer devoted his attention especially to the magnetic qualities of his dynamos, and in the course of his investigations in 1887, he devised the differential magnetometer which proved of great service in determining the relative merits of different kinds of iron. Working alone as he did, and deriving his electrical knowledge from books, he naturally felt somewhat diffident as to the merits of his inventions. It was not until he had been assured by Mr. Stephen D. Field, that his electrical machinery was of a superior quality, that he decided to place it on the market. Even as late as 1893, when his inventions were well-known and appreciated, he was loth to admit that his work was worthy of consideration. "I am only a maker of hat machinery" were the words he used when asked about his electrical career. In the city of Yonkers, where he had built up a prosperous business, and among his friends who knew him best, his public spirit and genuine worth were highly respected and appreciated. Mr. Eickemeyer died at Washington, D. C., on January 23rd, 1895.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

CATALOGUE OF MEMBERS.

MARCH 1ST, 1895.

HONORARY MEMBERS.

Name.	Address.	Date of Membership.
KELVIN, *Lord*,	*LL.D., F.R.S.S.L.* and *E.* The University, Glasgow, Scotland,	H.M. May 17, 1892
PREECE, WM. H. *F.R.S.*	Electrician, General Post Office, London, Eng. Residence, Gothic Lodge, Wimbledon.	H.M. Oct. 21, 1884

Total, 2.

MEMBERS.

ABBOTT, ARTHUR V.	Chief Engineer, Chicago Telephone Co., 203 Washington St., Chicago, Ill.	A Oct. 21, 1890 M Jan. 16, 1895
ACHESON, EDW. G.	Electrician and Electrical Engineer, Carborundum Co., Monongahela City, Pa.	A Jan. 3, 1888 M May 1, 1888
ADAMS, ALTON D.	Electrical Engineer, 620 Atlantic Ave., Boston, Mass.	A April 18, 1893 M Jan. 17, 1894
AHEARN, T.	Ahearn & Soper, Electrical Supplies, Ottawa, Ont.	A July 12, 1887 M Sept. 6, 1887
ALBRIGHT, H. FLEETWOOD	Electrical Engineer, Western Electric Co., New York; residence, 5304 Lancaster Ave., Philadelphia, Pa.	A Sept. 27, 1892 M June 20, 1894
ALMON, G. H.	Electrical Engineer and Contractor, 136 Liberty St., New York City, and 620 Atlantic Ave., Boston, Mass.	A Sept. 20, 1893 M Mar. 21, 1894
ANTHONY, PROF. W. A.	(*Past President.*) (*Vice-President.*) Consulting Electrician, Temple Court, New York, N. Y.	A Dec. 9, 1884 M Jan. 6, 1885
ARNOLD, BION J.	Consulting Electrical Engineer, 574 The Rookery and 4128 Prairie Ave., Chicago, Ill.	A Oct. 25, 1892 M Nov. 15, 1893

Name.	Address.	Date of Membership.
AYER, JAMES I.	James I. Ayer & Co., Consulting Engineers, Security Building St. Louis Mo.; Manager, Marks-Ayer Electric Co., 73 Watts St., New York City.	A May 19, 1891 M April 19, 1892
AYRES, BROWN	Professor of Physics and Electrical Engineering, Tulane University, New Orleans, La.	A Dec. 16, 1891 M Mar. 15, 1892
BAILLARD, E. V.	Manufacturer of Electrical Instruments and Fine Machinery, 106 Liberty St., New York City.	A Dec. 3, 1889 M Jan. 16, 1895
BARBERIE, E. T.	Electrician, 159 W. 66th St., New York City.	A Jan. 19, 1892 M Oct. 25, 1892
BATCHELOR, CHAS.	Electrical Engineer, 33 West 25th St., New York City.	A June 8, 1887 M July 12, 1887
BATES, J. H.	Assistant Engineer and Draughtsman, with C. J. Bates & Co., 126 Liberty St., New York City, and 321 Hudson St., Hoboken, N. J.	A Sept. 6, 1887 M Oct. 1, 1889
BATES, MAURICE E.	Electrician National Electric Mfg. Co., Eau Claire, Wis.	A Aug. 6, 1889 M Oct. 1, 1889
BAYLIS, ROBERT NELSON	Electrical Engineer, 81 Fulton St., New York City.	A Oct. 1, 1889 M May 17, 1892
BELL, PROF. A. GRAHAM	(*Past President.*) 1331 Conn. Ave., Washington, D. C., and Baddeck, N. S.	A April 15, 1884 M Oct. 21, 1884
BELL, DR. LOUIS	Electrical Engineer, General Electric Co., Monadnock Bldg., Chicago, Ill.	A May 20, 1890 M June 18, 1890
BENJAMIN, PARK	Electrical Expert and Engineer, 203 Broadway, N. Y. City.	A Dec. 16, 1891 M Feb. 16, 1892
BERNARD, EDGAR G.	Electrical Engineer, President, E. G. Bernard & Co., 43 4th St., Troy, N. Y.	A. Jan. 5, 1886 M July 12, 1887
BILLBERG, C. O. C.	Electrical Engineer, 3200 Arch St., Philadelphia, Pa.	A Mar. 21, 1894 M Feb. 27, 1895
BINNEY, HAROLD	Patent Solicitor and Expert, Potter Building, 38 Park Row, New York City.	A Sept. 16, 1890 M Dec. 16, 1890
BIRDSALL, E. T. *M. E.*	Consulting Electrical Engineer, 18 Broadway; Residence, 56 West 38th St., New York.	A June 8, 1887 M Nov. 1, 1887
BISHOP, JAMES DRAPER	Electrical Engineer of the Safety Insulated Wire and Cable Co., 234 W. 29th St., New York City.	A Dec. 16, 1891 M Oct. 25, 1892
BLAKE, FRANCIS	Auburndale, Mass.	A Sept. 3, 1889 M Oct. 1, 1889
BLODGETT, GEO. W.	Electrical Engineer, B. & A. R. R. and Consulting Electrician, Boston, Mass.	A July 12, 1887 M Sept. 6, 1887
BOSCH, ADAM	Sup't Fire Alarm Telegraph, Newark, N. J.	A April 15, 1884 M Jan. 6, 1885
BOSSON, FREDERICK N.	Consulting Mining and Electrical Engineer; Electrician, Calumet and Hecla Mining Co., Calumet, Mich.	A May 17, 1892 M Feb. 21, 1893

Name.	Address.	Date of Membership.
BOURNE, FRANK	Electrical Engineer, 1322 Havemeyer Building, New York City.	A April 21, 1891 M Nov. 15, 1892
BOWMAN, FRED. A.	Supt., New Glasgow Electric Co., New Glasgow, Nova Scotia.	A May 19, 1891 M Nov. 21, 1894
BOYNTON, EDWARD C.	Electrician of the Lovell Mfg. Co., Ltd., 156 W. 8th St., Erie, Pa.	A Aug. 6, 1889 M Nov. 24, 1891
BRADLEY, CHAS. S.	(*Manager.*) Electrical Engineer, 1919 Seventh Ave., New York City.	A May 24, 1887 M Dec. 6, 1887
BRENNER, WILLIAM H.	Electrical Engineer, 39 Cortlandt St., New York City.	A Sept. 20, 1893 M Mar. 21, 1894
BROADNAX, FRANCIS	Engineer, Safety Insulated Wire and Cable Co., 50 Broadway, New York City; residence, 47 Christopher St., Montclair, N. J.	A Jan. 17, 1894 M Jan. 16, 1895
BROOKS, MORGAN	President and Manager, The Electrical Engineering Co., 249 Second Ave., South; residence, 2950 Park Ave., Minneapolis, Minn.	A May 20, 1890 M June 17, 1890
BROWN, ALFRED S.	Electrical Engineer, Western Union Telegraph Co., 195 Broadway, P. O. Box 856, New York City.	A Mar. 18, 1890 M Feb. 21, 1893
BROWN, J. STANFORD, *E. E.*,	Consulting and Constructing Electrical Engineer; residence, Park Hill, Yonkers, N. Y.	A Sept. 6, 1887 M Nov. 1, 1887
BRUSH, CHAS. F.	Electrical Engineer, 453 The Arcade, Cleveland, O.	A April 15, 1884 M Oct. 21, 1884
BURLEIGH, CHAS. B.	Supt. Isolated Dept. General Electric Co., 620 Atlantic Ave., Boston, Mass.	A April 21, 1891 M Feb. 16, 1892
BYLLESBY, HENRY M.	Northwest General Electric Co., 403 Sibley St., St. Paul, Minn.	A Sept. 7, 1888 M Oct. 2, 1888
CAHOON, JAS. B.	Electrical Engineer, Manager, Expert Dept., General Electric Co., 226 Union St., Schenectady, N. Y.	A June 17, 1890 M May 19, 1891
CARROLL, LEIGH	New Orleans, La.	A Oct. 1, 1889 M Nov. 12, 1889
CHAMBERLAIN, J. C.	Electrical Engineer, Morris Heights; residence, 135 East 18th St., New York City.	A Dec. 6, 1887 M Jan. 3, 1888
CHANDLER, PROFESSOR CHARLES F.	Columbia College, 41 East 49th St., New York City.	A Jan. 20, 1891 M June 7, 1892.
CHENEY, W. C.	Superintendent and Electrical Engineer, Portland General Electric Co, Portland; residence, Oregon City, Ore.	A Sept. 22, 1891 M Nov. 21, 1894
CHURCHILL, ARTHUR	Engineering Dept., Schenectady Works, General Elec. Co., 5 So. Church St., Schenectady, N. Y.	A April 15, 1890 M Jan. 17, 1893
CLARK, ERNEST P.	Electrical Engineer, Clark Electric Co, 192 Broadway, New York City.	A Jan. 8, 1887 M Nov. 1, 1887

Name.	Address.	Date of Membership.
CLARKE, CHAS. L.	Electrical Engineer and Patent Expert, 55 Liberty St., New York City.	A April 15, 1884 M Jan. 6, 1885
COLBY, EDWARD A.	Consulting Engineer, Lock Box 313, Newark, N. J.	A April 2, 1889 M May 7, 1889
CONDICT, G. HERBERT	Electrical Engineer, 5328 Green St., Germantown, Pa.	A July 12, 1887 M Sept. 6, 1887
COTHREN, WM. H.	The Cincinnati Edison Electric Co., 164 W. 8th St., Cincinnati, O.	A Aug. 6, 1889 M Oct. 1, 1889
COWLES, ALFRED H.	Technical Adviser to the Cowles Smelting and Aluminum Co., 656 Prospect St., Cleveland, O.	A Mar. 5, 1886 M May 7, 1889
CRAIG, J. HALLY	Cumner, Craig & Co., 69 Broad St., Boston, Mass.	A May 16, 1893 M Feb. 27, 1895
CRAIGIN, HENRY A.	Engineer, Westinghouse Electric and Mfg. Co., 120 Broadway, New York City.	A June 6, 1893 M May 15, 1894
CRANDALL, JOSEPH EDWIN	Electrician, C. & P. Telephone Co., 619 Fourteenth St., N. W. Washington, D. C.	A April 18, 1892 M April 18, 1894
CROCKER, FRANCIS BACON [Life Member.]	(*Vice-President*) Professor of Electrical Engineering, School of Mines, Columbia College, New York.	A May 24, 1887 M April 2, 1889
CROSBY, JAMES WELLINGTON	Electrical Engineer, 38 Bedford St., Boston; residence, Wellington, Mass.	A Feb. 21, 1894 M Feb. 27, 1895
CROSS, PROF. CHAS. R.	Thayer Professor of Physics, and Director of the Rogers Laboratory, Mass. Institute of Technology, Boston, Mass.	A April 15, 1884 M Oct. 21, 1884
CURTISS, GEORGE F.	Electrical Engineer, General Electric Co., Schenectady, N. Y.	A April 2, 1889 M Nov. 24, 1891
CUTTER, GEORGE	Dealer in Electrical Supplies, 851 The Rookery, Chicago, Ills.	A June 17, 1890 M May 19, 1891
CUTTRISS, CHAS.	Electrician, The Commercial Cable Co., 1 Broad St., New York.	A Nov. 1, 1887 M Dec. 6, 1887
DAFT, LEO	Consulting Electrical Engineer and Contractor, Los Angeles, Cal.	A Dec. 9, 1884 M Jan. 6, 1885
DANIELL, FRANCIS G.	Electrical Engineer, Fairhaven and Westville R. R. Co., P. O. Box 394, New Haven, Conn.	A Nov. 12, 1889 M June 20, 1894
DANVERS, ALAN	[Address unknown.]	A Nov. 1, 1887 M Sept. 3, 1889
DAVIES, JOHN E.	Professor of Physics, University of Wisconsin, 523 North Carroll St., Madison, Wis.	A Jan. 7, 1890 M Mar. 18, 1890
DAVIS, CHARLES H., *C. E.*	Consulting and Constructing Engineer, 120 Broadway, 576 Lexington Ave., New York City, and 308 Walnut St., Philadelphia, Pa.	A Mar. 18, 1890 M June 17, 1890

Name.	Address.	Date of Membership.
DAVIS, MINOR M.	Ass't Electrician, Postal Telegraph-Cable Co., 253 Broadway, New York City	A April 6, 1886 M May 16, 1893
DELAFIELD, A. FLOYD, *Ph. D.*	Electrical Engineer, Noroton, Conn.	A May 7, 1889 M Oct. 1, 1889
DELANY, PATRICK BERNARD (*Vice President.*)	Inventor, South Orange, N. J.	A April 19, 1884 M Nov. 24, 1891
DICKENSON, SAMUEL S.	Sup't, Commercial Cable Co., Hazel-Hill, Guysborough Co., N. S.	A Mar. 6, 1888 M Oct. 1, 1889
DIEHL, PHILIP	Inventor, Singer Sewing Machine Co., 508 Morris Ave., Elizabeth, N. J.	A April 15, 1884 M Dec. 9, 1884
D'INFREVILLE, GEORGES	Electrical Engineer and Expert, 10 Desbrosses St., New York City.	A Nov. 1, 1887 M Dec. 6, 1887
DION, ALFRED A.	General Supt., The Ottawa Electric Co., 72 Sparks St., Ottawa, Ont.	A Jan. 7, 1890 M Nov. 15, 1893
DOIJER, H.	Consulting Electrical Engineer, 8 Choorstraat, Delft, Holland.	A Jan. 7, 1890 M Mar. 18, 1890
DONNER, WILLIAM H.	241 Sixth Ave., Newark, N. J.	A Nov. 18, 1890 M Dec. 16, 1890
DUDLEY, CHARLES B.	Chemist and Scientific Expert, Penn. R. R. Co., 1219 Twelfth Ave., Altoona, Pa.	A Oct. 1, 1889 M Nov. 12, 1889
DUNBAR, F. W.	425 West 22nd St., New York City.	A Dec. 21, 1892 M May 16, 1893
DUNCAN, DR. LOUIS	Johns Hopkins University, Baltimore, Md.	A July 12, 1887 M Sept. 6, 1887
DUNN, GANO SILLICK	Electrical Engineer, of the Crocker-Wheeler Electric Co., Ampere, E. Orange, N. J.; Residence, 223 Central Park, West, New York City.	A April 21, 1891 M June 20, 1894
DUNSTON, ROBT. EDWARD	The Cortland and Homer Traction Co., Cortland, N. Y.	A Oct. 27, 1891 M Feb. 16, 1892
DYER, R. N.	Patent Attorney, 36 Wall St., New York City.	A July 12, 1887 M Sept. 6, 1887
EDISON, THOMAS A.	Mechanician and Inventor, Orange, N. J.	A April 15, 1884 M Oct. 21, 1884
EGGER, ERNST	Electrical Engineer care of B. Egger & Co, X., Simmeringstr, 187, Vienna, Austria.	A Feb. 21, 1893 M Mar. 21, 1894
EMERY, CHARLES EDWARD	Consulting Engineer, 915 Bennett Building, cor. Fulton and Nassau Sts., New York City.	A June 26, 1891 M April 19, 1892
EMMET, W. L. R.	Electrical Engineer, General Electric Co., Schenectady, N. Y.	A June 6, 1893 M Jan. 17, 1894
EUSTIS, HERBERT H.	President and Electrician, Eastern Electric Cable Co., 61 Hampshire St., Boston, Mass.	A April 15, 1890 M Nov. 24, 1891

Name.	Address.	Date of Membership.
EVEREST, AUGUSTINE R.	Electrical Engineer, Thomson Electric Welding Co., Lynn, Mass.	A May 19, 1891 M Dec. 20, 1893
FARNHAM, ISAIAH H.	Electrical Engineer, N. E. Telephone & Telegraph Co., 125 Milk St., Boston, Mass.	A June 8, 1887 M July 12, 1887
FESSENDEN, REGINALD A.	Professor of Electrical Engineering, Western University of Pennsylvania, Allegheny, Pa.	A Oct. 21, 1890 M Dec. 16, 1890
FIELD, C. J., *M. E.*	Consulting and Constructing Engineer, Havemeyer Building, 26 Cortlandt St., New York City.	A June 8, 1887 M Nov. 1, 1887
FIELD, STEPHEN D.	Electrical Engineer, Stockbridge, Mass.	A April 15, 1884 M Oct. 21, 1884
FITZMAURICE, JAMES S.	Chief Engineer, The Electric Light Branch, 210 George St, Sydney, N. S. W.	A Sept. 20, 1893 M Mar. 21, 1894
FLEMING, WILFRID H.	70 W. 3d St., Bayonne City, N. J.	A Dec. 6, 1887 M Jan. 3, 1888
FOSTER, HORATIO A.	Electrical Engineer, Editor, *Electric Power*, 36 Cortlandt St.; residence, 569 Park Ave., New York City.	A June 8, 1887 M Sept. 6, 1887
FREEMAN, DR. FRANK L.	Attorney-at-Law, Solicitor of Patents, Electrical Expert, 931 F St., Washington, D. C.	A May 7, 1889 M Sept. 3, 1889
GALE, HORACE B.	Consulting Electrical and Mechanical Engineer, 40 California St., San Francisco, Cal.	A Nov. 15, 1892 M May 16, 1893
GEYER, DR. WM. E.	Stevens Institute of Technology, Hoboken, N. J.	A June 5, 1888 M Sept. 7, 1888
GIFFORD, CLARENCE E.	Electrical Engineer, Supt. Jamestown Electric Light and Power Co., Jamestown, N. Y.	A May 16, 1893 M Feb. 21, 1894
GRAY, DR. ELISHA	Electrician and Inventor, Highland Park, Ill.	A Feb. 16, 1892 M May 17, 1892
GREENE, S. DANA	Assistant General Manager, General Electric Co., Schenectady, N. Y.	A Sept. 20, 1893 M April 18, 1894
GRISCOM, WM. W., *M.A.*	Electrical Engineer, 224 Chestnut St., Philadelphia; residence, Haverford, Pa.	A June 5, 1888 M Mar. 18, 1890
GUTMANN, LUDWIG	Electrical Engineer, care of *Street Railway Review*, 269 Dearborn St., Chicago, Ill.	A Sept. 14, 1888 M Mar. 21, 1893
HALL, CLAYTON C.	Civil Engineer, 810 Park Ave., Baltimore, Md.	A April 15, 1884 M Oct. 21, 1884
HALL, JOHN L.	Manager, Western Union Telegraph Co., 300 Market St., Wilmington, Del.	A Sept. 22, 1891 M Dec. 20, 1893

Name.	Address.	Date of Membership.
HAMBLET, JAMES	(*Vice-President.*) Manager Time Service, W. U. Tel. Co., 195 Broadway, P. O. Box 856, New York City; residence, 20 Sidney Place, Brooklyn, N. Y.	A Nov. 1, 1887 M Dec. 6, 1887
HAMILTON, GEO. A.	Electrician, Western Electric Co., 22 Thames, cor. Greenwich St., New York; residence, 532 Morris Ave., Elizabeth, N. J.	A April 15, 1884 M Oct. 21, 1884
HAMMER, WILLIAM J.	(*Manager.*) Consulting and Supervising Electrical Engineer, 1305 Havemeyer Building, 26 Cortlandt St., New York City; residence, Elmora, N. J.	A June 8, 1887 M July 12, 1887
HASKINS, CARYL D.	Electrical Engineer, General Electric Co., 620 Atlantic Ave., Boston, Mass.	A Mar. 18, 1890 M June 20, 1894
HASKINS, CHARLES H.	Electrician, The Marie Antoinette, 66th St. and Boulevard, New York City.	A April 15, 1884 M Oct. 21, 1884
HASKINS, CLARK CARYL	City Electric Light Inspector, 582 West Congress St., Chicago, Ill.	A Sept. 20, 1893 M Mar. 21, 1894
HASSON, W. F. C.	Firm of Hasson & Hunt, Consulting and Supervising Mechanical and Electrical Engineers, 310 Pine St., Telephone 5650, San Francisco, Cal.	A Mar. 18, 1890 M May 15, 1894
HAYES, HAMMOND V.	Electrician, American Bell Telephone Co., 42 Farnsworth St., So. Boston Mass.	A Nov. 12, 1889 M Mar. 18, 1890
HAYES, HARRY E.	Asst. Electrician, American Telegraph and Telephone Co., 153 Cedar St., New York City.	A April 18, 1893 M Dec. 20, 1893
HAYNES, F. T. J.	Divisional Telegraph Engineer, Great Western Railway, Residence, Belmont Villa, Cheddon Road, Taunton, Eng.	A Dec. 6, 1886 M Jan. 3, 1887
HEINRICH, RICHARD O.	Electrical Engineer, Weston Electrical Instrument Co., 114 William St., Newark, N. J.	A Oct. 1, 1889 M Oct. 25, 1892
HERING, CARL	Consulting Electrical Engineer, 927 Chestnut St., Philadelphia, Pa.	A Jan. 3, 1888 M June 5, 1888
HERING, HERMANN S.	Associate in Electrical Engineering, Johns Hopkins University, Baltimore, Md.	A April 21, 1891 M April 18, 1893
HERRICK, CHARLES H.	Consulting and Constructing Electrical Engineer, 133 Oliver St., Boston; residence, 132 Highland Ave., Winchester, Mass.	A April 21, 1891 M Jan. 17, 1893
HERZOG, DR. F. BENEDICT	President, Herzog Teleseme Co., 30 Broad St., New York City.	A May 24, 1887 M July 12, 1887
HEWITT, CHARLES	(*Manager.*) Assistant Engineer, Philadelphia Traction Co., 920 Spruce St., Philadelphia, Pa.	A Sept. 16, 1890 M May 17, 1892

Name.	Address.	Date of Membership.
HIBBARD, ANGUS S.	(*Manager.*) General Manager, Chicago Telephone Co., 203 Washington St., Chicago, Ill.	A Nov. 24. 1891 M Feb. 16, 1892
HIGGINS, EDWARD E.	Expert in Street Railway Values and Economics, 26 Cortlandt St., New York City.	A June 8, 1887 M July 12, 1887
HIX, E. RANDOLPH	Hix, Hamilton & Co., Electrical Engineers and Contractors, 41 Wall St., New York City.	A Feb. 21, 1894 M Feb. 27, 1895
HOLMES, FRANKLIN S.	Electrical Engineer, 108 Fulton St., New York City; residence 445a Macon St., Brooklyn, N. Y.	A April 21, 1891 M June 20, 1894
HOUSTON, EDWIN J., *Ph.D.* [Life Member.]	(*President.*) Prof. of Physics, Franklin Inst., Firm of Houston & Kennelly, 1105 Betz Bldg.; residence. 1809 Spring Garden St., Philadelphia, Pa.	A April 15, 1884 M Oct. 21, 1884
HOWELL, JOHN W.	Electrician, 761 High St., Newark, N. J.	A July 12, 1887 M June 5, 1888
HOWELL, WILSON S.	General Electric Lamp Works, Harrison, N.J.; residence, 19 Webster Place, Orange, N. J.	A Sept. 3, 1889 M Mar. 18, 1890
HUNTER, RUDOLPH M.	Expert and Counsellor in Patent Causes, 926 Walnut St., Philadelphia, Pa.	A July 13, 1886 M May 17, 1887
HUNTING, FRED S.	Chief Engineer, Fort Wayne Electric Co., 330 West Washington St., Fort Wayne, Ind.	A Nov. 15, 1892 M May 16, 1893
HUTCHINSON, DR. CARY T.	Electrical Engineer, 253 Broadway; residence, 56 West 25th St., New York City.	A Feb. 7, 1890 M Dec. 16, 1890
HYDE, JEROME W.	Springfield, Mass.	A June 8, 1887 M Nov. 1, 1887
INRIG, ALEC GAVAN	Rue St. Gommaire, 23, Antwerp, Belgium.	A Jan. 19, 1892 M May 17, 1892
IVES, EDWARD B.	Chief Engineer, Electric Traction Co. of Philadelphia, 421 Chestnut St., Philadelphia, Pa.	A April 2, 1889 M May 15, 1894
JACKSON, DUGALD C.	Professor of Electrical Engineering, University of Wisconsin, Madison, Wis.	A May 3, 1887 M June 17, 1890
JACKSON, FRANCIS E.	Aylsworth & Jackson, Incandescent Filament Manufacturers, 128 Essex Ave., Orange; residence, 61 South Grove St., East Orange, N. J.	A Jan. 3, 1888 M June 17, 1890
JACKSON, HENRY	Telegraph Supt. and Engineer, The Lancashire & Yorkshire Railway Co., Horwich, Bolton-le-Moors, Lancashire, England.	A Mar. 21, 1894 M Dec. 19, 1894
JACKSON, J. P.	Professor of Electrical Engineering, Penn. State College, State College, Pa.	A Sept. 27, 1892 M Jan. 17, 1894

Name.	Address.	Date of Membership.
JANNUS, FRANKLAND	Attorney-at-Law, Solicitor of Patents, 928-30 F. St., N. W. Washington, D. C.	A Nov. 12, 1889 M Mar. 18, 1890
JENKS, W. J.	Patent Department. General Electric Co., 44 Broad Street, Box 3067, New York City.	A June 8, 1887 M Nov. 1, 1887
JOHNSTON, A. LANGSTAFF	Consulting Engineer, Hestonville, Mantua and Fairmount Passenger R.R. Co, 4300 Lancaster Ave., Philadelphia, Pa.	A April 21, 1891 M April 18, 1894
JONES, FRANCIS W. [Life Member.]	Assistant Gen'l-Manager and Electrician, Postal Telegraph-Cable Co., 253 Broadway, New York City.	A April 15, 1884 M Oct. 21, 1884
KEITH, DR. NATHANIEL S.	Sandycroft Foundry, E. W. Co., Hawarden, near Chester, Eng.	A June 6, 1893 M Jan. 17, 1894
KINSMAN, FRANK E.	Electrical Engineer and President Kinsman Block Signal Co., 143 Liberty St., New York City and Plainfield, N. J.	A Sept 27, 1892 M May 16, 1893
KNOWLES, E. R.	Electrical Engineer, The Schuyler Electric Company, Middletown, Conn.	A June 8, 1887 M July 12, 1887
KNUDSON, A. A.	Electrical Engineer, 106 Fulton St., New York City.	A Dec. 6, 1887 M Jan. 3, 1888
LANGE, PHILIP A.	Superintendent Westinghouse Electric and Manufacturing Co., East Pittsburg, Pa.	A Mar. 6, 1888 M June 5, 1888
LANGTON, JOHN	Electrical Engineer, Canada Life Building, Toronto, Ont.	A Mar. 6, 1888 M June 5, 1888
LATTIG, J. W.	Electrical Engineer, Supt. of Telegraph and Electrical Apparatus, Lehigh Valley R. R. Co., So. Bethlehem, Pa.; residence, 335 Broad St., West Bethlehem, Pa.	A June 8, 1887 M July 12, 1887
LAWSON, A. J.	Electrical Engineer, The County of London and Brush Provincial Electric Lighting Co., Ltd., 49 Queen Victoria St., London, Eng.	A Mar. 18, 8910 M June 17, 1890
LEMP, HERMANN, JR.	Electrician, Thomson Electric Welding Co., Lynn, Mass.	A April 2, 1889 M Feb. 21, 1893
LEONARD, H. WARD	(*Vice President.*) Electrical Engineer, 44 Broad St.; residence, 38 West 35th St., New York City.	A July 12, 1887 M Sept. 6, 1887
LEONARD, M. B.	Electrical Engineer, and Supt. of Telegraph, Chesapeake & Ohio R'y. Co., Richmond, Va.	A Nov. 6, 1886 M May 1, 1888
LIEB, JOHN W., JR.	Electrical Engineer, Edison Electric Ill. Co.; Residence, 166 West 97th St., New York City.	A Sept. 6, 1887 M Nov. 1, 1887
LLOYD, ROBERT MCA.	Electrician, 27 Pine St.; residence, 1 West 39th St., New York City.	A Oct. 21, 1890 M Nov. 15, 1893

Name.	Address.	Date of Membership.
LOCKWOOD, THOMAS D., *F. I. Inst.* [Life Member.]	Electrical Engineer, and Advisory Electrician, P.O. Drawer 2, Boston, Mass.	A April 15, 1884 M Oct. 21, 1884
LORRAIN, JAMES GRIEVE	Norfolk House, Norfolk St., London, W. C., England.	A May 16, 1891 M May 15, 1894
LOVEJOY, J. R.	General Manager, Supply Dept., General Electric Co., Schenectady, N. Y.	A April 21, 1891 M Feb. 21, 1894
MACFARLANE, ALEXANDER, *D. Sc., LL.D.*	84 Heustis St., Ithaca, N. Y.	A Jan. 19, 1892 M May 17, 1892
MAILLOUX, C. O.	Consulting Electrical Engineer, 45 William St., Telephone 3457A Cortlandt, New York City.	A April 15, 1884 M Oct. 21, 1884
MANSFIELD, ARTHUR NEWHALL	Assistant Electrician, American Telephone and Telegraph Co., 153 Cedar St., New York City.	A Dec. 20, 1893 M June 20, 1894
MARKS, LOUIS B., *M. M. E.*	Engineer-in-Chief, Marks-Ayer Electric Co., 73 Watts St.; residence, 51 East 67th St., New York City.	A May 20, 1890 M Jan. 16, 1895
MARKS, WILLIAM DENNIS, *Ph.B. C. E.*	Edison Electric Light Co., Philadelphia, Pa.	A Feb. 7, 1888 M May 1. 1888
MARSHALL, J. T.	Metuchen, N. J.	A Oct. 1, 1889 M Nov. 12, 1889
MARVIN, HARRY N.	Secretary and Expert, Marvin Electric Drill Co., 208 South Geddes St., Syracuse, N. Y.	A April 19, 1892 M Jan. 17, 1893
MAVER, WILLIAM, JR.	Electrical Expert and Consulting Electrical Engineer, 31 Nassau St., New York City.	A July 12, 1887 M April 21, 1891
MAYER, GEORGE M.	Enterprise Block, 5th Floor, 79 Fifth Ave., Chicago, Ill.	A Dec. 16, 1890 M June 20, 1894
MAYNARD, GEO. C.	Electrical Engineer, 800 H. St., N. W., Washington, D. C.	A April 15, 1884 M Dec. 9, 1888
MCCAY, H. KENT	Electrical Engineer and Contractor, 106 E. German St., Baltimore, Md.	A Sept. 16, 1890 M May 19, 1891
MCCLUER, C. E.	Superintendent, First District, So. Bell Telephone and Telegraph Co., P. O. Box 32, Richmond, Va.	A Mar. 21, 1893 M Jan. 17, 1894
METCALFE, GEORGE R.	Firm of Metcalfe & Moeller, Electrical Engineers, 343 Ninth Ave.; residence, 404 West 22d St., New York City.	A April 19, 1892 M Nov. 15, 1892
MILLIS, JOHN	Captain of Engineers U. S. A., The Lighthouse Board, Washington, D. C.	A July 7, 1884 M Mar. 3, 1885
MILLS, FRANK P.	Superintendent Cleveland Iron Mining Co., Ishpeming, Mich.	A Jan. 6, 1885 M Mar. 3, 1885
MOLERA, E. J.	Civil Engineer, 606 Clay St., San Francisco, Cal.	A Jan. 16, 1892 M June 7, 1892
MOORE, D. MCFARLAN	Inventor, Moore Electrical Co., 321 Harrison St., Harrison, N. J.	A Dec. 20, 1893 M June 20, 1894

Name.	Address.	Date of Membership.
MORROW, JOHN THOMAS	Supt. Electrolytic Plant, Boston and Montana Consolidated Copper and Silver Mining Co., Great Falls, Mont.	A Dec. 21, 1892 M April 18, 1894
NICHOLS, DR. EDWARD L.	Professor of Physics, Cornell University, Ithaca, N. Y.	A Oct. 4, 1887 M Dec. 6, 1887
NOLL, AUGUSTUS	Firm of Noll & MacLean, Contracting Electrical Engineers, 8 East 17th St., New York City.	A Sept. 27, 1892 M April 18, 1893
PAINE, F. B. H.	Westinghouse Electric and Mfg. Co., 328 Exchange Building, Boston, Mass.	A Dec. 16, 1890 M Nov. 25, 1891
PAINE, SIDNEY B.	General Electric Co., 180 Summer St., Boston, Mass.	A June 8, 1887 M Nov. 1, 1887
PARKHURST, CHARLES D.	1st Lieut. 4th Artillery, U. S. Army, Fortress Monroe, Va.	A Dec. 21, 1892 M Nov. 15, 1893
PARKS, C. WELLMAN	1825 Fifth Ave., Troy, N. Y.	A July 12, 1887 M May 1, 1888
PARSHALL, H. F.	Electrical Engineer, British Thomson-Houston, Ltd., 38 Parliament St., Westminster, London, Eng.	A Sept. 7, 1888 M Mar. 18, 1890
PATTISON, FRANK A.	Firm of Pattison Bros, Consulting and Constructing Electrical Engineers, 136 Liberty St., New York City.	A Sept. 22, 1891 M Dec. 16, 1891
PEARSON, F. S.	Engineer, Room 841, 621 Broadway, New York City.	A Oct. 25, 1892 M Feb. 21, 1893
PERRINE, FREDERIC A. C., *D. Sc.*	Professor of Electrical Engineering. Leland Stanford, Jr., University, Palo Alto, Cal.	A Sept. 16, 1890 M Dec. 16, 1890
PERRY, NELSON W., *E. M.*	Editor, *Electricity*, 6 Park Place, N. Y. City; residence, 18 Sidney Place, Brooklyn, N. Y.	A May 17, 1892 M Mar. 21, 1893
PHELPS, GEO. M.	(*Treasurer*). President, *Electrical Engineer*, 203 Broadway, New York.	A April 15, 1884 M Oct. 21, 1884
PICKERNELL, F. A.	Chief Engineer. Amer. Tel. & Tel. Co., 153 Cedar St., New York City.	A Feb. 7, 1890 M Mar. 18, 1890
PIERCE, RICHARD H.	Pierce & Richardson, Electrical Engineers, 1409 and 1410 Manhattan Bldg., Chicago; residence. 5434 Monroe Ave., Hyde Park, Ill.	A April 18, 1893 M Dec. 20, 1893
PIKE, CLAYTON W., *B.S.*	Electrical Engineer, Falkenau Engineering Co., 1210 Betz Building, Philadelphia, Pa.	A Dec. 16, 1891 M Oct. 25, 1892
POPE, FRANKLIN LEONARD	(*Past President*). Consulting Electrical Engineer and Expert, 39 Cortlandt St., N. Y.; residence, Great Barrington, Mass.	A April 15, 1884 M Oct. 21, 1884
PORTER, J. F.	Manager, Alton Electric Street Railroad Co., Alton, Ill.	A Sept. 6, 1887 M Nov. 1, 1887

Name.	Address.	Date of Membership.
PRATT, ROBERT J.	Electrician, Treas. and Mgr. Electric Mfg. Co. and Gas Engine Co., Greenbush, N. Y.	A July 12, 1887 M Sept. 6, 1887
RAE, FRANK B.	Electrical Engineer, Home Bank Building, Detroit, Mich.	A April 15, 1884 M Oct. 25, 1892
RAYMOND, CHAS. W.	[Address unknown.]	A June 8, 1887 M May 17, 1887
RECKENZAUN, FREDERICK.	Electrical Engineer, Box 225, 58 Demott St., West Hoboken, N. J.; 43 Wall St., New York City.	A Mar. 6, 1888 M June 5, 1888
REIST, HENRY G.	Designing Engineer, General Electric Co., 5 South Church St., Schenectady, N. Y.	A June 17, 1890 M Dec, 19, 1894
RICE, E. WILBUR, JR.	Technical Director, The General Electric Co., Schenectady, N. Y.	A Dec. 6, 1887 M Jan. 3, 1888
RIES, ELIAS E.	Electrical Engineer and Inventor, Prest. Ries Electric Specialty Co., Baltimore and Eutaw Sts.; residence, 430 South Broadway, Baltimore. Md.	A July 12, 1887 M Sept. 6, 1887
ROBB, WM. LISPENARD	Professor of Physics, Trinity College, Hartford, Conn.	A Dec. 16, 1891 M Mar. 15, 1892
ROBERTS, E. P.	E. P. Roberts & Co., Electrical and Mechanical Engineers, Brainard Block, Telephone 2656, Cleveland, O.	A Jan. 6, 1885 M Feb. 3, 1885
RODGERS, HOWARD S.	Electrical Engineer, Eddy Electric Mfg. Co., Windsor, Conn.	A Sept. 27, 1892 M May 16, 1893
ROHRER, ALBERT L.	Electrical Engineer, with General Electric Co., Schenectady, N. Y.	A Nov. 1, 1887 M May 1, 1888
ROSS, NORMAN	Assistant Works Engineer, Canadian General Electric Co., Peterboro, Ont.	A Sept 20, 1893 M Nov. 21, 1894
ROSS, ROBERT A.	Electrical Engineer, Royal Electric Co., 94 Queen St., Montreal, P. Q.	A Sept. 27, 1892 M April 18, 1893
ROUQUETTE, WILLIAM F. B.	Proprietor, Rouquette & Co., 47 Dey St., New York City.	A Mar. 21, 1894 M Dec. 19, 1894
SALOMONS, *Sir* DAVID LIONEL, *Bart.* *M. A.*, [Life Member	Engineer and Barrister, Broomhill, Tunbridge Wells, Kent, and 49 Grosvenor St., London, W. England.	A Feb. 7, 1888 M May 1, 1888
SANDS H. S.	Consulting and Constructing Electrical Engineer, Peabody Building, Wheeling, W. Va.	A Feb. 21, 1893 M Nov. 21. 1894
SARGENT, W. D.	General Manager, N. Y. & N. J. Tel. Co., 16 Smith St., Brooklyn, N. Y.	A April 15, 1884 M Feb. 21, 1894
SCOTT, CHARLES F.	Chief Electrician, Westinghouse Electric and Mfg. Co., Pittsburg, Pa.	A April 19, 1892 M Jan. 17, 1893
SHALLENBERGER, O. B.	Consulting Electrician, Westinghouse Electric and Mfg. Co., of Pittsburg; Rochester, Pa.	A Sept. 7, 1888 M Dec. 4, 1888

Name.	Address.	Date of Membership.
SHAW, EDWIN C.	Manager, Akron General Electric Co., Akron, O.	A May 17, 1892 M Feb. 27, 1895
SHEA, DANIEL W.	Professor of Physics, University of Illinois, 403 W. Hill St., Champaign, Ill.	A Dec. 20, 1893 M June 20, 1894
SHELDON, SAMUEL, *A. M., Ph.D.*	Professor of Physics and Electrical Engineering, Polytechnic Institute, 170 State St., Brooklyn, N. Y.	A Dec. 16, 1890 M Oct. 27, 1891
SHEPARD, WM. E.	Steinway Railway Co., Long Island City, N. Y.	A Feb. 7, 1890 M Mar. 18, 1890
SLATER, HENRY B.	Vice-President and Manager, The Canon City Electric Light and Power Co., Canon City, Col.	A April 15, 1884 M Dec. 9, 1884
SMITH, FRANK STUART	Supt. Lamp Factory, Westinghouse Electric & Mfg. Co., Pittsburg, Pa.	A Sept. 27, 1892 M April 18, 1893
SMITH, JESSE M.	Consulting Electrical Engineer and Expert in Patent Causes, 36 Moffatt Block, Detroit, Mich.	A April 15, 1884 M June 26, 1891
SMITH, T. CARPENTER	Mechanical and Electrical Engineer, 212 Drexel Building, Philadelphia, Pa.	A Oct. 27, 1891 M Dec. 16, 1891
SPAULDING, HOLLON C.	Engineer, Siemens & Halske Electric Co., 31 Milk St., Boston, Mass.	A April 21, 1891 M June 20, 1894
SPERRY, ELMER A.	Electrical Engineer, Sperry Electric Railway Co., Mason and Belden Sts., Cleveland, O.	A April 19, 1892 M Feb. 21, 1893
STANDFORD, WILLIAM	Asst. Supt. Telegraphs, Colonial Govt., Cape Town, Cape of Good Hope, Africa.	A Oct. 4, 1887 M Dec. 6, 1887
STEARNS, CHARLES K.	Electrical Engineer, Newton Centre, Mass.	A Aug. 6, 1889 M May 16, 1893
STEBBINS, THEODORE	Superintendent Railway Construction, General Electric Co., Schenectady, N. Y.	A July 9, 1889 M June 17, 1890
STEINMETZ, CHARLES P.	(*Manager,*) Electrician, General Electric Co., Schenectady, N. Y.	A Mar. 18, 1890 M April 21, 1891
STEVENS, W. LE CONTE	Professor of Physics, Rensselaer Polytechnic Institute, Troy, N. Y.	A Dec. 20, 1893 M June 20, 1894
STIERINGER, LUTHER	Electrical Expert, Morris Building, 68 Broad St.; residence, 1873 Lexington Ave., New York.	A June 8, 1887 M Nov. 1, 1887
STILLWELL, LEWIS B.	Electrical Engineer, Westinghouse Electric and M'f'g Co., Pittsburg, Pa.	A April 19, 1892 M Nov. 15, 1892
TAINTOR, GILES	Division Sup't. Western Division New England Telephone and Telegraph Co., Springfield, Mass.	A June 26, 1891 M Dec. 16, 1891
TALTAVALL, THOS. R.	Editor, *Electric Age*, World Building, New York City.	A Jan. 20, 1891 M Oct. 27, 1891

Name.	Address.	Date of Membership.
TERRY, CHARLES A.	Lawyer, Westinghouse Electric and Mfg. Co., 120 Broadway, New York City.	A April 5, 1887 M May 17, 1887
THOMAS, BENJAMIN F., *Ph. D.*	Professor of Physics, Ohio State University, Columbus, O.	A June 7, 1892 M Nov. 15, 1892
THOMSON, PROF. ELIHU	(*Past President*). Electrician, General Electric, and Thomson Electric Welding Companies, Lynn, Mass.	A April 15, 1884 M April 21, 1891
THOMPSON, EDWARD P.	Consulting Electrician and Patent Attorney in Electrical Cases, 5 Beekman St., New York City.	A April 15, 1884 M Dec. 3, 1889
THURNAUER, ERNST	Manager, Thomson-Houston International Elec. Co., 27 Rue de Londres, Paris, France.	A Oct. 14, 1887 M Dec. 6, 1887
TISCHENDOERFER, F. W.	Electrical Engineer, Schückert & Co., Nuremberg, Germany.	A April 19, 1892 M Nov. 21, 1894
TRAFFORD, EDWARD W.	Electrical Engineer, Richmond Railway and Electric Co., 104 N. 7th St., Richmond, Va.	A Feb. 21, 1894 M Dec. 19, 1894
TURNER, WILLIAM S.	President, Woodbridge & Turner Engineering Co., 47 Times Building, New York City.	A Dec. 7, 1886 M Oct. 2, 1888
UEBELACKER, CHAS. F.	Electrical Engineer, 88 Avenue C., Bayonne, N. J.	A Feb. 7, 1890 M Nov. 15, 1893
UHLENHAUT, FRITZ, JR.	Philadelphia Traction Co., 4101 Haverford St., Philadelphia, Pa.	A May 7, 1889 M Dec. 19, 1894
UPTON, FRANCIS R.	General Manager, Edison Lamp Works, Harrison, N. J.; residence, 107 Day St., Orange, N. J.	A May 17, 1887 M Mar. 15, 1892
VAIL, J. H.	President and Chief Engineer, Electrical and Mechanical Engineering Co., 39 Cortlandt St., New York City.	A June 8, 1887 M Nov. 1, 1887
VANSIZE, WILLIAM B.	(*Manager.*) Solicitor of Patents and Expert, 253 Broadway, New York City.	A April 15, 1884 M Oct. 21, 1884
VAN TRUMP, C. REGINALD	Engineer and Manager, Wilmington City Electric Co., Wilmington, Del.	A Feb. 5, 1886 M Feb. 21, 1894
WADDELL, MONTGOMERY	Engineer, The Waddell-Entz Electric Co., Bridgeport, Conn.	A Feb. 7, 1888 M May 1, 1888
WAIT, HENRY H.	Assistant Electrical Engineer, Western Electric Co., 4919 Madison Ave., Chicago, Ill.	A Sept. 20, 1893 M June 20, 1894
WALDO, DR. LEONARD	Electrical Engineer, Secretary, The Waldo Foundry, 57 Coleman St., Bridgeport, Conn.	A June 5, 1888 M Dec. 4, 1888
WALKER, SYDNEY F.	Electrical Engineer, 195 Severn Road, Cardiff, Wales.	A June 2, 1885 M May 17, 1887
WARNER, ERNEST P.	Electrical Engineer, Western Electric Co., 227 So. Clinton St., Chicago, Ill.	A Sept. 20, 1893 M June 20, 1894

Name.	Address.	Date of Membership.
WATERMAN, F. N.	Electrical Engineer, Westinghouse Electric and Mfg. Co., 120 Broadway, New York City.	A Feb. 21, 1893 M June 20, 1894
WEAVER, W. D.	(*Manager.*) 7 West 26th St., New York City.	A May 17, 1887 M May 17, 1887
WEBB, HERBERT LAWS	(*Manager.*) 18 Cortlandt St.; residence, 126 East 19th St., New York City.	A Oct. 21, 1890 M Dec. 16, 1890
WEEKS, EDWIN R.	Rooms 301 to 303, National Bank of Kansas City Building, Kansas City, Mo.	A Sept. 6, 1887 M Nov. 1, 1887
WELLER, HARRY W.	Electrical Engineer, The Woodlands, Jamaica, N. Y.	A Oct. 21, 1890 M Nov. 24, 1891
WELLS, DOUGLAS	Hurstfield, The Avenue, Gipsy Hill, London, Eng.	A June 7, 1892 M Jan. 17, 1893
WESTON, EDWARD	(*Past President.*) Vice-President, Weston Electrical Instrument Co, 120 William St., and 645 High St., Newark, N. J.	A April 15, 1884 M Oct. 21, 1884
WETZLER, JOSEPH	Editor *The Electrical Engineer*, 203 Broadway, New York City.	A April 15, 1884 M Dec. 9, 1884
WHARTON, CHAS. J.	82 Bond St., London, Eng,	A Jan. 3, 1888 M May 1, 1888
WHEELER, SCHUYLER S., [Life Member.]	*Sc.D.* President, Crocker-Wheeler Electric Co., 39 Cortlandt St., and Ampere, N. J.; residence, 34 Gramercy Park, New York City.	A June 2, 1885 M Sept. 1, 1885
WIENER, ALFRED E.	Electrical and Mechanical Engineer; residence, 208 Liberty St., Schenectady, N. Y.	A May 16, 1893 M May 15, 1894
WILKES, GILBERT	Consulting Electrical Engineer, 149 Griswold St., Detroit, Mich.	A Jan. 7, 1890 M Mar. 18, 1890
WILLYOUNG, ELMER G.	Secretary and Treasurer, Falkenau Engineering Co., Ltd., 1210 Betz Building, Philadelphia.	A Nov. 24, 1891 M Dec. 20, 1893
WILSON, CHARLES H.	Monadnock Building, Chicago, Ill.	A Nov. 24, 1891 M Feb. 16, 1892
WILSON, FREMONT	Electrician, 108 Fulton St., and 2153 Seventh Ave., New York City.	A Mar. 6, 1888 M June 5, 1888
WILSON, HARRY C.	Supt. of P. O. Telegraph with the Government, Kingston, Jamaica, West Indies.	A Jan. 19, 1891 M June 7, 1892
WINCHESTER, A. E.	Consulting Engineer and Designer of Electric Systems, South Norwalk, Conn.	A June 8, 1887 M Nov. 1, 1887
WIRT, CHARLES	(*Manager.*) Consulting Engineer, 5104 Newhall St., Germantown, Pa.	A Sept. 8, 1888 M June 20, 1894
WOLCOTT, TOWNSEND	Electrician, 1002 Bennett Building, New York City.	A Mar. 6, 1888 M Dec. 16, 1890
WOLVERTON, B. C.	Electrician, N. Y. & Pa. Telephone and Telegraph Co., Elmira, N. Y.	A Mar. 18, 1890 M Feb. 21, 1895

Name.	Address.	Date of Membership.
WOODBRIDGE, J. L.	Secretary and Treasurer, Woodbridge & Turner Engineering Co., 47 Times Building, New York City.	A June 8, 1887 M Nov. 1, 1887
WRIGHT, PETER	Inspector of Electrical Works, United Gas Improvement Co., 807 Drexel Bldg, Philadelphia, Pa.	A May 16, 1889 M Jan. 16, 1895
WURTS, ALEXANDER JAY	Electrical Expert, Westinghouse Electric & Mfg. Co., Pittsburg, Pa.	A April 19, 1892 M Nov. 15, 1892
YOUNG, C. GRIFFITH	Electrical Engineer, White-Crosby Co., 706 Equitable Building., Baltimore, Md.	A Jan. 3, 1889 M April 21, 1891

Members, - - - - 273.

ASSOCIATE MEMBERS.

Name.	Address.	Date of Election.
ADAMS, COMFORT A., JR.	Instructor in Electrical Engineering, Harvard University, 13 Farrar St., Cambridge, Mass.	Jan. 17, 1894
AGNEW, CORNELIUS R.	Electrical Engineer, Kinsman Block System Co., 23 West 39th St., New York City.	Mar. 21, 1894
ALBANESE, G. SACCO	Electrical Expert, Compagnie Francaise Thomson-Houston, 27 Rue de Londres, Paris, France	Sept. 20, 1893
ALBERT, HENRY	Key West, Fla.	Feb. 21, 1893
ALDEN, JAMES S.	Assistant Manager, with L. H. Alden, 486 River Drive, Passaic, N. J.	May 19, 1891
ALDRICH, WILLIAM S.	Professor of Mechanical Engineering and Director Mechanical Arts, West Virginia University, P. O. Box 256, Morgantown, W. Va.	Mar. 15, 1892
ALEXANDER, HARRY	Electrical Engineer, General Manager and Vice Prest. Alexander-Chamberlain Electric Co., 126 Liberty, and 348 W. 145th St., New York City.	April 21, 1891
ALEXANDER, P. H.	Manager, Lighting Dept., Electric Selector and Signal Co., 45 Broadway, New York City.	Dec. 16, 1890
ANDERSON, HENRY S.	General Manager and Electrician, United Electric Light Co., Springfield, Mass.	Jan. 16, 1895
ANDERSON, W. E.	Professor of Physics and Electrical Engineering, Pacific Building, Washington, D. C.	Sept. 20, 1893
ANDREWS, WM. S.	General Electric Co., Schenectady, N. Y.	Mar. 5, 1889
ANSON, FRANKLIN ROBERT	Manager, Salem Consolidated Street Railway Co., Salem, Ore.	Feb. 27, 1895
ANTHONY, WATSON G.	Electrician, 32½ Webster St., Newark, N. J.	Feb. 24, 1891
ARCHBOLD, WM. K.	Westinghouse Electric and Mfg. Co. 328 Exchange Bldg., Boston, Mass.	June 20, 1894
ARCHER, GEO. F.	Student in Electrical Engineering, Columbia College; Residence, Garden City, L. I.	Nov. 21, 1894
ARMSTRONG, CHAS. G.	Electrical Expert, 1400 Auditorium Tower, Chicago, Ill.	Sept. 27, 1892
ARNOLD, CRAIG R.	Electrician and Treasurer, Arnold Electric Co., Chester and Sharon Hill, Pa.	Nov. 15, 1892
ASHLEY, FRANK M.	Master Mechanic, Ashley Engineering Works, Hawthorne, N. J., 136 Liberty St., N. Y.	Nov. 21, 1894
ATWOOD, GEORGE F.	Mechanic and Lawrence Sts., Newark, N. J.	Sept. 16, 1890

Name.	Address.	Date of Election.
AUERBACHER, LOUIS J.	Secretary and General Manager, Automatic Electrical Specialty Co., 136 Liberty St., New York City.	Sept. 20, 1893
BABCOCK, CLIFFORD D.	Rome, Kanawha Co., W. Va.	Feb. 21, 1894
BADT, LIEUT. FRANCIS B.	Electrical Engineer, Siemens & Halske Electric Co. of America, 1215 Monadnock Block, 6506 Lafayette Ave., (Englewood), Chicago, Ill.	April 19, 1892
BARBOUR, FRED FISKE	Manager, Power and Mining Department, Pacific District, General Electric Co., 15 First St., San Francisco, Cal.	May 16, 1893
BARNARD, JOHN H.	Vice-President and General Manager, Wilmington Street Railway Co., Wilmington, N. C.	June 26, 1891
BARNES, EDWARD A.	Electrical Expert, Fort Wayne Electric Co., Fort Wayne, Ind.	Sept. 20, 1893
BARSTOW, WILLIAM S.	General Supt., Edison Electric Illuminating Co., 360 Pearl St., Brooklyn, N. Y.	Feb. 21, 1894
BARTH-BARTOSHEVITCH, A.	Mechanical and Electrical Engineer, [Address unknown.]	May 16, 1893
BARTLETT, EDWARD E.	Member Firm Bartlett & Co., 23 Rose St., New York City.	June 6, 1893
BARTON, ENOS M.	President Western Electric Co., 227 South Clinton St., Chicago, Ill.	July 12, 1887
BARRETT, JOHN A.	Elektron Mfg. Co., 126 Liberty Street, New York City.	June 8, 1887
BATES, FREDERICK C.	Electrical Engineer, Union Elektricitäts Gesellschaft, Berlin, Germany.	Jan. 20, 1891
BAUER, W. F.	Electrician, 313 High St., Newark, N. J.	April 15, 1890
BEATTIE, JOHN, JR.	Manager and Superintendent, The Beattie Battery, Zinc and Electric Co., Fall River, Mass.	Sept. 6, 1887
BEDELL, DR. FREDERICK,	Assistant Professor in Physics, Cornell University, Ithaca, N. Y.	April 21, 1891
BENNETT, EDWIN H., JR.	Electrician and Engineer, Diehl & Co., Elizabethport, N. J., and 17 E. 33d St., Bayonne, N. J.	June 20, 1894
BENNETT, JOHN C.	Electrician, General Electric Co., Box 3067, 44 Broad St., New York City.	Mar. 18, 1890
BENTLEY, MERTON H.	Chicago Telephone Co.; residence, 221 Scoville Ave., Oak Park, Ill.	Oct. 18, 1893
BERG, ERNST JULIUS	Engineer, General Electric Co.; residence, 53 Washington Ave., Schenectady, N. Y.	Sept. 19, 1894
BERGHOLTZ, HERMAN	Secretary and Treasurer, Ithaca Street Railway Co., Ithaca, N. Y.	April 2, 1889
BERLINER, EMILE	Inventor, Columbia Road, between Fourteenth and Fifteenth Sts., Washington, D. C.	April 15, 1884

Name.	Address.	Date of Election.
BERRESFORD, ARTHUR W.,	*B. S.*, *M. E.* Electrician, Brooklyn City R. R. Co., 197 Van Buren St., Brooklyn, N. Y.	May 15, 1894
BERTHOLD, VICTOR M.	Patent Department, American Bell Telephone Co., 125 Milk St., Boston; residence, 16 Upton St., Cambridgeport, Mass.	May 17, 1892
BEST, A. T.	Electrical Engineer, St. Augustine, Florida.	Feb. 21, 1894
BETHELL, U. N.	Acting General Manager, The Metropolitan Telephone and Telegraph Co., 18 Cortlandt St., N. Y. City.	Jan. 17, 1894
BIJUR, JOSEPH	Student in Electrical Engineering, Columbia College, residence, 41 West 53d St., New York City.	May 15, 1894
BLACK, CHAS. N.	Brush Electric Co., Belden St., Cleveland, O.	Feb. 7, 1890
BLADES, HARRY H.	General Superintendent, The Detroit Motor Co., 1343-55 Cass Ave., Detroit, Mich.	April 19, 1892
BLAKE, HENRY W.	Editor, *Street Railway Journal*, 26 Cortlandt St., New York City.	Nov. 13, 1888
BLAKE, THEODORE W.	Electrician, National India Rubber Co., Bristol, R. I.	Sept. 20, 1893
BLANCHARD, CHARLES M.	1209 Betz Building, Philadelphia, Pa.; residence, 5113 Pulaski Ave., Germantown, Pa.	Sept. 19, 1894
BLISS, DONALD M.	Electrician, Holtzer-Cabot Electric Co., 1 Davis Court, Washington St., Brookline, Mass.	Feb. 7, 1890
BLISS, GEORGE S.	Electrical Engineer, Central District and Printing Telegraph Co., Telephone Bldg., Pittsburg, Pa.	June 20, 1894
BLISS, WM. J. A.	820 Connecticut Ave., Washington, D. C.	Jan. 20, 1891
BLISS, WILLIAM L., *B. S.*,	*M. M. E.* Electrical Engineer, Riker Electric Motor Co.; residence, 24 Irving Place, Brooklyn, N. Y.	Mar. 21, 1894
BLIZARD, CHARLES	Sales Agent, Electric Storage Battery Co., 66 Broadway; residence, Passaic, N. J.	Nov. 21, 1894
BLOOD, JOHN B.	Assistant Engineer, Railway Dept., General Electric Co., Schenectady, N. Y.	June 20, 1894
BLOOD, W. HENRY JR.	The Franklin Electric Co., 535 Delaware St., Kansas City, Mo.	April 2, 1889
BOARDMAN, HARRY B.	Wisconsin Telephone Co., 1530 Grand Ave., Milwaukee, Wis.	Sept. 20, 1893
BOGART, A. LIVINGSTON	Electrical and Patent Expert, 22 Union Square, New York City.	July 10, 1888
BOGGS, LEMUEL STEARNS	Lafayette, Ind.	Sept. 20, 1893
BOGUE, CHARLES J.	Manufacturer and Dealer in Electrical Supplies, 206 Centre St., N.Y. City.	Dec. 3, 1889

Name.	Address.	Date of Election.
Bohm, Ludwig K., *Ph.D.*,	Consulting Electrical and Chemical Expert, 117 Nassau St., N. Y. City.	Nov. 15, 1892
Boileau, William E.	Superintendent and Electrician, Brush Electric Light & Power Co., Columbus, Ga.	Sept. 19, 1894
Bottomley, Harry	Electrical Engineer, Supt., Marlboro Electric Co., Marlboro, Mass.	April 2, 1889
Boughan, Edward L.	Supply Agent, American Telephone and Telegraph Co., 153 Cedar St., New York City.	Dec. 21, 1892
Brackett, Prof. Cyrus F.	Princeton, N. J.	April 15, 1889
Braddell, Alfred E.	Electrical Inspector, Underwriters' Association, Middle Department, 316 Walnut St., Philadelphia, Pa.	Sept. 1, 1890
Brady, E. D. A.	Consulting and Constructing Engineer, Lock P. O. Box 132, Waterbury, Conn.	Sept. 19, 1894
Brady, Frank W.	Electrical Engineer, Wellsburg, W. Va.	June 20, 1894
Brady, Paul T.	Manager, Central N. Y. Agency, Westinghouse Electric and Mfg. Co., Syracuse, N. Y.	July 12, 1887
Bragg, Charles A.	Manager Phila. Agency, Westinghouse Electric and Mfg. Co., 302 Girard Building, Philadelphia, Pa.	Sept. 20, 1893
Breithaupt, E. Carl	Electrical Engineer, Berlin, Ont.	June 6, 1893
Brixey, W. R.	Proprietor and Manufacturer, Day's Kerite Wire and Cables, 203 Broadway, New York City.	Sept. 20, 1893
Broich, Joseph	Superintendent and Electrician, with F. Pearce, New York City; residence, 448 8th Ave., Brooklyn, N. Y.	Jan. 17, 1894
Brophy, William	17 Egleston St., Jamaica Plain, Mass.	Mar. 5, 1889
Brown, Alex. S.	Electrical Engineer, 397 St. Nicholas Ave., New York City.	Jan. 7, 1890
Brown, Edward D.	District Inspector, American Telephone and Telegraph Co., 18 Cortlandt St , New York City; residence, 75 Hicks St., Brooklyn, N. Y.	Sept. 19, 1894
Bryant, Waldo C.	Manager and Treasurer, The Bryant Electric Co., Bridgeport, Conn.	May 16, 1893
Bubert, J. F.	Supervising and Contracting Electrical Engineer, 620 Atlantic Ave., Boston, Mass.	June 7, 1892
Buck, Harold W.	Student in Electrical Engineering, Columbia College: residence, 14 East 45th St., New York City	Jan. 16, 1895
Buckingham, Chas. L.	Patent Attorney, Western Union Telegraph Co., 195 Broadway, P. O. Box 856, New York City.	April 15, 1884
Bunce, Theodore D.	The Storage Battery Supply Co., 239 E. 27th St., New York City.	May 20, 1890

Name.	Address.	Date of Election.
BURKE, JAMES	Firm of Herrick & Burke, 203 Broadway, New York City.	May 16, 1893
BURNETT, DOUGLAS, *B.S.*	Edison Illuminating Co., Inspection Dept., 55 Duane St., New York City; residence, 42 Livingston St., Brooklyn, N. Y.	Feb. 21, 1893
BURTON, GEO. D.	Electrician and President, Electrical Forging Co., 194 Washington St., Boston, Mass.	April 21, 1891
BURTON, WILLIAM C.	With White-Crosby Co., Equitable Bldg. Baltimore, Md.	Sept. 20, 1893
BUTLER, WILLIAM C.	President, The Puget Sound Reduction Co., Everett, Washington.	Mar. 21, 1893
BUYS, ALBERT	Electrical Engineer, Rutherford, N. J.	Feb. 7, 1890
CABOT, JOHN ALFRED	City Electrician, 123 W. 8th St., Cincinnati, O.	May 16, 1893
CALDWELL, EDWARD	*Street Railway Journal*, Havemeyer Bldg., New York City.	Jan. 20, 1891
CALDWELL, FORDYCE S.	Proprietor Western Electric Construction Co., 503 Delaware St., Kansas City, Mo.; residence, 151 Henry St., Brooklyn, N. Y.	Sept. 22, 1891
CALDWELL, FRANCIS C.	Assistant Professor of Electrical Engineering, Ohio State University, Columbus, O.	June 20, 1894
CALLENDER, ROMAINE	Electrician, Decker Building, New York City.	Sept. 27, 1892
CANFIELD, MILTON C.	Electrical Engineer, 18 Clinton St., Cleveland, O.	Feb. 21, 1893
CAPUCCIO, MARIO	Electrical Engineer, Piazza Statuto 15, Torino, Italy.	Dec. 20, 1893
CARICHOFF, E. R.	Electrical Engineer. Sprague Electric Elevator Co., Postal Tel. Bldg., 253 Broadway, New York City.	Mar. 21, 1894
CARSON, DAVID I.	Secy. and Gen. Supt., The Southern Bell Telephone and Telegraph Co., 18 Cortlandt St., New York City.	Dec. 21, 1892
CARTWRIGHT, FRED'K G.	Electrical Engineer and Agent, Fort Wayne Electric Co., 41 and 43 Stevenson St., San Francisco, Cal.	Sept. 22, 1891
CARTY, J. J.	(*Manager*.) Engineer, Metropolitan Telephone and Telegraph Co., 18 Cortlandt St., New York City.	April 15, 1890
CARUS-WILSON, CHARLES A.	Professor of Electrical Engineering. McGill University, Montreal, P. Q.	April 18, 1894
CASE, WILLARD E.	6 Fort St., Auburn, N. Y.	Feb. 7, 1888
CASPER, LOUIS	Electricial Engineer and Contractor, 307 New Ridge Bldg., Kansas City, Mo.	April 21, 1891
CHADBOURNE, HENRY R., JR.	Electrical Engineer, Troy City Ry. Co., Troy, N. Y.	May 15, 1894

Name.	Address.	Date of Election.
CHAMBERLAIN, F. H.	Electrician, Metropolitan R. R. Co., 2411 P St., N. W. Washington, D. C.	June 17, 1890
CHASE, HARVEY STUART	Mechanical and Electrical Engineer, 136 Liberty St., New York City.	Sept. 19, 1894
CHENEY, FREDERICK A.	Secretary, Treasurer, and General Manager, The Elmira Illuminating Co., Elmira, N. Y.	Oct. 1, 1889
CHERMONT, ANTONIO LEITE	Engineer, Box 252, Para, U. S. Brazil.	Mar. 18, 1890
CHESNEY, C. C.	Electrician, Stanley Laboratory, Pittsfield, Mass.	June 20, 1894
CHILDS, ARTHUR EDWARDS, *B. Sc. M.E E.E.*	Electrical Engineer. Westinghouse Elec. and Mfg. Co., 302 Girard Bldg., Philadelphia, Pa.	June 20, 1894
CHILDS, SUMNER W.	Columbus Central Railway Co., Columbus, O.	May 15, 1894
CHILDS, WALTER H.	Brattleboro, Vt.	Sept. 6, 1887
CHINNOCK, C. E.	137 Sixth Ave., Brooklyn, N. Y.	April 15, 1884
CHUBBUCK, H. EUGENE	Electrical Engineer, Pueblo, Col.	Dec. 4, 1888
CLAFLIN, ADAMS D.	President, Perkins Lamp Co., and of Claflin & Kimball, (Inc.), Electrical Contractors, Room 53, 110 Boylston St., Boston; residence, Newton Centre, Mass.	June 7, 1892
CLARK, LEROY, JR.	Electrician, Safety Insulated Wire and Cable Co., 229 West 28th St., residence, 350 West 30th St., New York City.	May 15, 1894
CLEMENT, LEWIS M.	1013 Central Ave., Oakland, Cal.	April 21, 1891
CLEVELAND, WM. B.	[Address unknown.]	April 15, 1884
CLOUGH, ALBERT L.	Box 114, Manchester, N. H.	Feb. 21, 1894
COBB, JOHN S.	[Address unknown.]	June 17, 1890
COFFIN, CHAS. A.	General Electric Co., 620 Atlantic Ave., Boston, Mass.	Dec. 6, 1887
COGSWELL, A. R.	Electrician and Superintendent, Halifax Illuminating and Motor Co., Ltd., 34 Bishop St., Halifax, N. S.	April 21, 1891
COHO, HERBERT B.	H. B. Coho & Co., Electrical Engineers, 203 Broadway, New York City.	Mar. 21, 1894
COLGATE, GEO. L.	Electrical Engineer, 136 Liberty St., New York City.	June 17, 1890
COLLEY, BENJAMIN W.	First Ass't. Superintendent, The Commercial Cable Co., Hazel Hill, N. S.	Oct. 21, 1890
COLVILLE, FRANK C.	Electrician and Inventor, 1503 Seventh Ave., Oakland, Cal.	May 19, 1891
COLVIN, FRANK R.	President, Interior Telephone Co., 203 Broadway, New York City.	April 18, 1894

Name.	Address.	Date of Election.
COMPTON, ALFRED G.	Professor of Applied Mathematics, College of the City of New York, 17 Lexington Ave., New York City.	Nov. 1, 1887
COMSTOCK, LOUIS K.	Monadnock Building. Chicago, Ill.	Dec. 20, 1893
COOLIDGE, CHARLES A.	Electrical Engineer, Superintendent, Northern Improvement Co., 591 Hood St., Portland, Ore.	April 19, 1892
CORBY, FRED. B.	Consulting Engineer, 114 Chandler St., Boston, Mass.	Dec. 20, 1893
CORNELL, CHAS. L.	Electrical Engineer, Hamilton, O.	Feb. 7 1890
CORSON, WILLIAM R. C.	Assistant Electrical Engineer, The Eddy Electric Mfg. Co., Windsor, Conn.	Jan. 17, 1893
CORY, CLARENCE L.	Professor of Electrical Engineering, University of California, Berkeley, Cal.	April 19, 1892
COX, EDMUND V.	Student in Electrical Engineering. Columbia College; residence, 50 East 31st St., New York City.	Jan. 16, 1895
CRANDALL, CHESTER D.	Assistant Treasurer, Western Electric Co., 227 South Clinton St., Residence, 4438 Ellis Ave. Chicago, Ill.	Sept. 27, 1892
CRANE, W. F. D.	Manager Electrical Department H. W. Johns Manufacturing Co., 87 Maiden Lane, New York City; residence, 24 Halstead Pl., East Orange, N. J.	Feb. 7, 1888
CREAGHEAD, THOMAS J.	President and General Manager, Creaghead Engineering Co., 296 Plum St., Cincinnati, O.	Sept. 20, 1893
CREHORE, ALBERT C., *Ph.D.*	Assistant Professor of Physics, Dartmouth College, Hanover, N. H.	Dec. 21, 1892
CREWS, J. W.	Manager, Southern Bell Telephone and Telegraph Co, Telephone Exchange, Norfolk, Va.	Sept. 19, 1894
CRIGGAL, JOHN E.	Electrician, 138 Bloomfield Ave., Newark, N. J.	June 20, 1894
CROSBY, OSCAR T.	White-Crosby Co., 29 Broadway, New York City.	Mar. 18, 1890
CROXTON, A. L.	Electrical Engineer, Standard Electric Co., 7118 Drexel Ave., Chicago, Ill.	June 20, 1894
CUMNER, ARTHUR B.	Senior Member, firm of Cumner, Craig & Co., 69 Broad St., Boston, Mass.	Feb. 27, 1895
CUNTZ, JOHANNES H.	Assistant to President Henry Morton, Stevens Institute of Technology, 325 Hudson St, Hoboken, N. J.	Mar. 5, 1889
CURTIS, CHAS. G.	President, Curtis Electric Mfg. Co., Box 412, New York City,	April 15, 1884
CUSHING, F. W.	Electrical Engineer, 1106 The Rookery, Chicago, Ill.	Nov. 24, 1891

Name.	Address.	Date of Election.
CUSHING, HARRY COOKE, JR.	Electrical Inspector, Boston Board of Fire Underwriters, 55 Kilby St.; Residence, 259 Beacon St., Boston, Mass.	Sept. 19, 1894
CUSHMAN, HOLBROOK	Instructor in Physics, Columbia College, 337 West 22d St., New York City.	June 5, 1888
DACUNHA, MANOEL IGNACIO	Manager of the Electrical Section, Emprera Industrial Gram-Para, Para, U. S. of Brazil.	May 16, 1893
DAME, FRANK L.	Engineer, General Electric Co., Portland, Ore.	June 26, 1891
DANA, R. K.	Agent, Washburn and Moen. Mfg. Co., 16 Cliff St., New York City.	April 15, 1884
DARLINGTON, FREDERIC W.	Consulting Electrical and Mechanical Engineer, 503 Girard Building, Philadelphia, Pa.	Sept. 19, 1894
DAVENPORT, C. G.	Expert and Agent, General Electric Co., 44 Broad St., New York City.	Nov. 21, 1891
DAVENPORT, GEORGE W.	61 Ames Bldg., Boston, Mass.	June 4, 1889
DAVIDSON, EDW. C.	Patent Lawyer, Room 179 Times Bldg., New York City.	Feb. 7, 1890
DAVIS, DELAMORE L.	Superintendent, Salem Electric Light and Power Co., 299 Lincoln Ave., Salem, O.	April 2, 1889
DAVIS, JOSEPH P.	Engineer, American Bell Telephone Co., 113 W. 38th St., New York City.	April 15, 1884
DECKER, DELBERT H.	Solicitor of Patents, with H. C. Townsend, 5 Beekman St., New York City.	Sept. 20, 1893
DEKHOTINSKY, CAPT. ACHILLES,	Late Chief Electrician and Torpedo Officer, Imperial Russian Navy, Marlboro, Mass.	Oct. 27, 1891
DELANCEY, DARRAGH	Manager of Kodak Park Works, Eastman Kodak Co., Rochester, N. Y.	Sept. 19, 1894
DELAND, FRED	Publisher, *Electrical Engineering*, 436 The Rookery, Chicago, Ill.	Feb. 16, 1892
DENISON, SYLVESTER P.	143 Centre St., New York City; residence, Belleville, N. J.	Jan. 16, 1895
DENTON, JAMES E.	Professor of Experimental Mechanics, Stevens Institute of Technology, Hoboken, N. J.	July 12, 1887
DESMOND, JERE. A.	Electrical Engineer, Kingston, N. Y.	Jan. 19, 1892
DEY, HARRY E.	342 Tenth St., Brooklyn, N. Y.	Dec. 19, 1894
DICKERSON, E. N.	Attorney-at-Law, 64 E. 34th St., New York City.	April 15, 1884
DOANE, S. EVERETT	68 Park Pl., Newark, N. J.	Aug. 6, 1889
DOBBIE, ROBERT S.	Electrical Engineer, 43 Wellington Park, Clifton, Bristol, Eng.	Feb. 5, 1889
DODGE, PROF. OMENZO G.,	U. S. Navy, Navy Dep't, Washington, D. C.	Sept. 20, 1893

Name.	Address.	Date of Election.
DOMMERQUE, FRANZ J.	Chief Draughtsman, Chicago Telephone Co.; residence, 71 Potomac Ave., Chicago, Ill.	Oct. 17, 1894
DOOLITTLE, CLARENCE E.	Manager and Electrician, Roaring Fork Electric Light and Power Co., Aspen, Colo.	May 15, 1894
DOOLITTLE, THOMAS B.	Engineering Department, American Bell Telephone Co., 125 Milk St., Boston, Mass.	May 16, 1893
DOREMUS, CHARLES A.	*M.D. Ph.D.* 59 W. 51st St., New York City.	July 7, 1884
DORR, FRANK H.	Electrical Engineer, General Electric Co., Monadnock Building, Chicago, Ill.	May 15, 1894
DOW, ALEX	Engineer, Public Lighting Commission, Detroit, Mich.	Sept. 20, 1893
DRESSLER, CHARLES E.	17 Lexington Ave., New York City.	Dec 16, 1890
DRYSDALE, WILLIAM A.	Consulting Electrical Engineer, Hale Building, Philadelphia, Pa.	Sept. 19, 1894
DUNCAN, THOMAS	Electrician, Laboratory Fort Wayne Electric Corporation, 407 Broadway, Fort Wayne, Ind.	Oct. 17, 1894
DUNN, KINGSLEY G.	Electrician, Care of *Pacific Electrician*, San Francisco, Cal.	Oct. 17, 1894
DURANT, EDWARD	Electrician, Gas Engine Plants for Electric Light and Heat, 39 Cortlandt St.; residence, 115 East 26th St., New York City.	Nov. 15, 1892
DURANT, GEO. F.	Vice-Pres't Bell Telephone Co., of Mo., 511 No. 4th St., St. Louis, Mo.	April 15, 1884
DYER, FRANCIS MARON	Associate Engineer with Chas. L. Eidlitz, 10 West 23d St; residence, 355 Lenox Ave., New York City.	Sept. 19, 1894
EDDY, H. C.	Salesman, Western Electric Co., Room 22. 170 La Salle St., Chicago, Ill.	June 20, 1894
EDEN, MORTON EDWARD	Electrical Inspector, Western District the Underwriters' Association of the Middle Department, Philadelphia, Pa.; residence, 83 Fourth Ave., Pittsburg, Pa.	Sept. 19, 1894
EDWARDS, JAMES P.	Electrical Engineer, 1569 Walton Way, Augusta, Ga.	April 19, 1892
EGLIN, WM. C. L.	Chief of Electrical Department, Edison Electric Light Co., 909 Walnut St.; residence, 4230 Chester Ave., Philadelphia, Pa.	Sept. 19, 1893
EIDLITZ, CHAS. L.	10 West 23d St.; residence, 1125 Madison Ave, New York City.	Sept. 19, 1894
EKSTROM, AXEL	Electrical Engineer, General Electric Co.; residence, 92 Hamilton Ave., Lynn, Mass.	June 17, 1890

Name.	Address.	Date of Election.
ELEY, HARRIS H.	Electrical Workshop Supt. W. C. & S. W. Telephone Co., 88 Colston St., Bristol, Eng.	Jan. 7, 1890
ELLICOTT, EDWARD B.	Superintendent of Construction, Western Electric Co., 4438 Ellis Ave., Chicago, Ill.	Sept. 19, 1894
ELMER, WILLIAM, JR.	Firm of Elmer & Hall, Consulting and Contracting Electrical Engineers, 25-26 Forst-Richey Building, Trenton, N. J.	Mar. 18, 1890
ELY, WM. GROSVENOR, JR.	239 5th St., Niagara Falls, N. Y.	Mar. 21, 1893
EMMET, HERMAN L. R.	Publisher and Printer, 36 Cortlandt St., New York City.	April 15, 1884
ENTZ, JUSTUS B.	Electrical Engineer, Accumulatoren Fabrik, Hagen, Westphalia, Germany.	Jan. 7, 1890
ERICKSON, F. WM.	Edison Electric Illuminating Co., 3 Head Place, Boston, Mass.	Sep. 19, 1894
ESSICK, SAMUEL V.	Electrician, The Essick Printing Tel. Co., Yonkers, N. Y.	May 19, 1891
ETHERIDGE, CHAS. LOCKE	Chicago Telephone Co.; residence, 4714 Kenwood Ave., Chicago, Ill.	Oct. 17, 1894
ETHERIDGE, E. L.	Electrician, with Equipment Dep't, Navy Yard; residence, 66 No. Oxford St., Brooklyn, N. Y.	Dec. 20, 1893
EYRE, M. K.	Assistant to Manager of Lamp Sales, General Electric Co., Harrison, N. J.	Oct. 17, 1894
FARNSWORTH, ARTHUR J.	Chief Engineer, Larchmont Electric Co., Mamaroneck, N. Y.	Jan. 16, 1895
FAY, THOMAS J.	"C. & C." Electric Co., 143 Liberty St., New York City.	June 26, 1891
FIELDING, FRANK E. [Life Member.]	Chemist and Assayer, Virginia City, Nev.	Sept. 6, 1887
FISCHER, GUSTAVE J.	Engineer for Tramway Construction, Public Works Department, Sydney, N. S. W.	Jan. 20, 1891
FISH, WALTER C.	General Electric Co., Lynn, Mass.	June 26, 1891
FISHER, HENRY W.	Electrician and Director of Elec. and Chem. Laboratories; The Standard Underground Cable Co., Pittsburg, Pa.	Jan. 16, 1895
FISKE, HENRY G.	Electrician, 45 E. 22d St., New York City.	Nov. 12, 1889
FISKE, J. P. B.	Electrical Engineer, General Electric Co., Lynn, Mass.	June 17, 1890
FLACK, J. DAY	252 West 85th St., New York City.	Dec. 6, 1887
FLAGG, STANLEY G., JR.	Stanley G. Flagg & Co., 19th St., and Penna. Ave., Philadelphia, Pa.	April 18, 1893
FLANAGAN, THOMAS FRANCIS	Supt. and Electrician, Portsmouth Gas Light Co., Portsmouth, N. H	Jan. 17, 1894
FLATHER, JOHN J.	Professor of Mechanical Engineering, Purdue University, Lafayette, Ind.	April 19, 1892

Name.	Address.	Date of Election.
FLEGEL, GEO. C.	East Chicago, Ind.	Sept. 20, 1893
FLEMING, RICHARD	Electrician, Navy Yard, N. Y.; residence, Jamaica, N. Y.	Oct. 18, 1893
FLESCH, CHARLES	Electrical Engineer, Allgemeine Elektricitats-Gesellschaft, 22 Schiffbauerdamm, Berlin, N. W. Germany.	Sept. 27, 1892
FLINT, BERTRAM P.	Electrical and Mechanical Engineer, with Chas. H. Davis, 120 Broadway, New York City.	Jan. 17, 1894
FLOOD, J. F.	Sup't Steubenville Street Railway Co., Steubenville, O.	Mar. 18, 1890
FLOY, HENRY	Westinghouse Electric and Mfg. Co., Pittsburg, Pa.	May 17, 1892
FOOTE, ALLEN R.	Special Agent, Electrical Industries, U. S. Census, Takoma Park, D. C.	April 21, 1891
FOOTE, CHARLES W.	Consulting Electrical Engineer, Pomona, Cal.	Sept. 22, 1891
FOOTE, THOS. H.	Consulting Electrical Engineer, 215 West 125th St., New York City.	April 21, 1891
FORBES, FRANCIS	Lawyer, 32 Nassau St., New York City.	Sept. 16, 1890
FORBES, GEORGE	Electrical Engineer, 34 Great George St., London, Eng.	Feb. 21, 1894
FORD, WM. S.	Assistant to Chief Engineer, The American Bell Telephone Co., Room 73, 125 Milk St., Boston, Mass.	June 7, 1892
FRANCISCO, M. J.	President and General Manager, Rutland Electric Light Co., Rutland, Vt.	June 17, 1890
FRANTZEN, ARTHUR	Frantzen & Bennett, Electrical Contractors, 84 La Salle St., Chicago, Ill.	Feb. 21, 1894
FREEDMAN, WILLIAM H.	Tutor in Electrical Engineering, School of Mines, Columbia College; residence, 120 W. 125th St., New York City.	Mar. 18, 1890
FRENCH, PROF. THOMAS, JR. *Ph.D.*	Avondale, Cincinnati, O.	Sept. 20, 1893
FREY, CHARLES P.	Electrician, The E. S. Greeley & Co., 5 and 7 Dey St., New York City.	June 6, 1893
FRIDENBERG, HENRY LESLIE, *M. E.*	Stanley Mfg. Co., (Meter Dept.,) Pittsfield, Mass.	Jan. 16, 1895
FROST, FRANCIS R.	Westinghouse Electric and Mfg. Co., 427 South Ave., Wilkinsburg, Pa.	Dec. 20, 1893
FRYE, HENRY W.	Clayton, New Mexico	May 16, 1893
FULLER, FRANK G.	Salesman, with W. R. Brixey, 203 Broadway, New York City.	Oct. 19, 1894
FULLER, LEVI K.	Vice-President, Estey Organ Co., Brattleboro, Vt.	Mar. 5, 1889
GALLAHER, EDWARD B.	Electrical Engineer, 253 Broadway, room 910; residence, 1190 Madison, Ave., New York City.	Jan. 19, 1895
GALLETLY, J. FRED.	Electrician, Swift & Co., Chicago, Ill.	Mar. 21, 1894

Name.	Address.	Date of Election.
GARDANIER, GEORGE W.	Electrician, Western Union Telegraph Co., 195 Broadway, New York City.	April 18, 1893
GERRY, EDWARD M.	Western Electrical Instrument Co., 114 William St.; residence, 181 Washington St., Newark, N. J.	Feb. 21, 1894
GERRY, JAMES H.	Superintendent, The Self-Winding Clock Co., 163 Grand Ave., Brooklyn, N. Y.	April 18, 1894
GERRY, M. H., JR.	Engineer, St. Paul Office, General Electric Co., 3333 Cedar Ave., Minneapolis, Minn.	April 18, 1893
GERSON, LOUIS JAY	President and Electrical Engineer, The Gerson Electrical Co., 809 Sansom St., Philadelphia, Pa.	Sept. 19, 1894
GESSEAUME, CHARLES	Draughtsman, 78 Sheffield St., Newark, N. J.	Feb. 21, 1894
GILES, WALTER A.	Goubert Mfg. Co., 29 Cortlandt St., New York City.	Nov. 1, 1887
GILLILAND, E. T.	Pelham Manor, N. Y.	April 15, 1884
GLADING, FRANK W.	1 DeWitt Ave., Ithaca, N. Y.	May 15, 1894
GLADSTONE, JAMES WM.	Manager, Edison Mfg. Co., 110 East 23d St.; residence, West Orange, N. J.	April 18, 1894
GOLDMARK, CHAS. J.	Electrical Engineer, 49 Liberty St., and 473 Park Ave., New York City.	June 5, 1888
GOLDSBOROUGH, WINDER E., *M.E.*	Associate Professor of Electrical Engineering, Purdue University, Lafayette, Ind.	Mar. 21, 1893
GORTON, CHARLES	Civil Engineer, Belmont, N. Y.	Nov. 12, 1889
GORDON, REGINALD	Tutor in Physics, Columbia College, residence, 339 Lexington Ave., New York City.	Feb. 24, 1891
GOSSLER, PHILIP G.	Electrical Engineer, United Electric Light and Power Co., 108 Fulton St., New York City.	June 20, 1894
GRAHAM, GEORGE WALLACE	Secretary, Interior Telephone Co., 203 Broadway, New York; residence, 163 Hicks St., Brooklyn, N. Y.	Dec. 19, 1894
GRAY, W. N.	Electrical Engineer, 200 Neave Building, Cincinnati, O.	Oct. 1, 1889
GRIFFIN, CAPT. EUGENE	First Vice-President, General Electric Co., Schenectady, N. Y.; residence, 323 State St, Albany, N. Y.	Feb. 7, 1890
GROSS, S. ROSS	Electrician, Tennessee Coal, Iron and R.R. Co., Ensley, Ala.	May 17, 1892
GROWER, GEORGE G.	Electrician and Chemist, Ansonia Brass and Copper Co., Ansonia, Conn.	Mar. 18, 1890
GUY, GEORGE HELI	Secretary, The New York Electrical Society, 534 Temple Court, New York City.	May 16, 1893

Name.	Address.	Date of Election.
HADAWAY, W. S., Jr.	Electrician, Central Electric Heating Co., 26 Cortlandt St., New York City.	Nov. 21, 1894
HADLEY, ARTHUR L.	Assistant Electrician to Chief Electrician and Gen'l Supt., Fort Wayne Electric Corporation, 149 Griffith St., Fort Wayne, Ind.	Oct. 17, 1894
HADLEY, WARREN, B.	30 Cortlandt St., New York City.	June 26, 1891
HALL, EDWARD J.	Vice-President and General Manager, American Telephone and Telegraph Co., 18 Cortlandt St , New York City.	April 18, 1893
HALL, EDWIN H.	Assistant Professor of Physics, Harvard College, Gorham St., Cambridge, Mass.	Sept. 3, 1889
HALL, WILLIAM P.	President, The Hall Signal Co., Vice-President The Johnson Railroad Signal Co., 80 Broadway, New York City.	Sept. 16, 1890
HALSEY, WILLIAM B.	Electrician and Horologist, 246 Elton St., Brooklyn, N. Y.	Mar. 18, 1890
HAMMATT, CLARENCE S.	Supt., Jacksonville Electric Light Co., Jacksonville, Fla.	Sept. 20, 1893
HANCOCK, L. M.	With Western Electric Co., 227 South Clinton St., Chicago, Ill.	May 19, 1891
HANDLEY, ARTHUR	[Address unknown.]	Dec. 16, 1890
HARDING, H. McL.	253 Broadway, New York City.	May 24, 1887
HARRINGTON, WALTER E.	Electric Railway Engineer, 307 Market St., Camden, N. J.	Mar. 17, 1891
HARRIS, GEORGE H.	Superintendent, Electric Car Shops, Birmingham, Ala.	June 20, 1894
HARRISON, HAROLD	New York Representative, Slater Engine Co., Montclair, N. J.	Feb. 21, 1894
HART, FRANCIS R.	President and General Manager, Cartagena-Magdalena Railway Co., care of Old Colony Trust Co., 1 Court St., Boston, Mass.	April 21, 1891
HARTMAN, HERBERT T.	Works Engineer, Peterborough Factory, Canadian General Electric Co., Peterborough, Ont.	Mar. 21, 1893
HATZEL, J. C.	Electrical Engineer and Contractor, 114 Fifth Ave., New York City.	Sept. 3, 1889
HAVILAND, FOSTER L.	163 St. Nicholas Ave., New York City.	May 15, 1894
HEATH, HARRY E.	Chief Draughtsman, Eddy Electric Mfg. Co., Box 189, Windsor, Conn.	Mar. 21. 1893
HEALY, LOUIS W.	The Westinghouse Electric and Mfg. Co., Pittsburg, Pa.	June 26, 1891
HEDENBERG, WM. L.	Firm of Hedenberg & Kinsey, Consulting and Constructing Engineers, 108 Fulton St.; residence, 83 Clinton Place, New York City.	Nov. 21, 1894

Name.	Address.	Date of Election.
HENSHAW, FREDERICK V.	Electrical Engineer, The "C. & C." Electric Co., Garwood; P. O. Westfield, N. J.	Feb. 5, 1889
HEWITT, WILLIAM R.	Superintendent, Fire Alarm and Police Telegraph, 9 Brenham Place, San Francisco, Cal.	May 15, 1894
HEWLETT, EDWARD M.	Electrical Engineer, Railway Dept. General Electric Co., Schenectady, N. Y.	May 19, 1891
HIGGINS, EUGENE	Electrical Engineer, Springport, Mich.	April 19, 1892
HILL, GEORGE, *C.E.*	Consulting Engineer, 44 Broadway, New York City.	April 19, 1892
HOBART, HENRY M.	Engineer, General Electric Co., Schenectady, N. Y.	April 18, 1894
HOCHHAUSEN, WILLIAM	Electrician, 74 Hanson Pl., Brooklyn, N. Y.	April 15, 1884
HOLBERTON, GEORGE C.	Electrical Engineer, General Electric Co., 15 First St., San Francisco, Cal.	May 15, 1894
HOLCOMB, EUGENE R.	[Address unknown.]	June 17, 1890
HOLLERITH, HERMAN	Hollerith Electric Tabulating System, 1054 31st St., Washington, D. C.	Sept. 19, 1894
HOLT, MARMADUKE BURRELL,	Mining and Electrical Engineer, Silverton, Col.	April 15, 1890
HOOD, RALPH O.	Electrical Engineer, 15 Federal St., Boston, Mass.	April 18, 1894
HOWSON, HUBERT	Patent Lawyer, 38 Park Row, New York City.	June 8, 1887
HUBBARD, WILLIAM C.	Engineering Department, Marks-Ayer Electric Co., 73 Watt St., New York City; residence, 109 West 5th St., Plainfield, N. J.	April 18, 1894
HUBLEY, G. WILBUR	Electrical Engineer, Louisville Electric Light Co.; residence, Kenton Club, Louisville, Ky.	Sept. 19, 1894
HUBRECHT, DR. H. F. R.	Director, Nederlandsche Bell Telephone Co., Amsterdam, Holland.	Oct. 4, 1887
HUDSON, JOHN E.	President, The American Bell Telephone Co., 125 Milk St., Boston, Mass.	Dec. 20, 1893
HUMPHREYS, C. J. R.	Manager, Lawrence Gas Co., and Edison Electrical Ill. Co., Lawrence, Mass.	Sept. 6, 1887
HUMPHREYS, PROF. WM. J.	Washington College, Chestertown, Md.	April 18, 1893
HUNT, ARTHUR L.	Electrician, Utica State Hospital, Utica, N. Y.	Sept. 19, 1894
HUTCHINSON, FREDERICK L.	Electrical Engineer with Westinghouse Electric and Mfg. Co., East Pittsburg, Pa.	June 20, 1894
IDELL, FRANK E.	Havemeyer Building, 26 Cortlandt St., New York City.	July 12, 1887

Name.	Address.	Date of Election.
IHLDER, JOHN D.	Electrical Engineer, Otis Electric Co., Yonkers, N. Y.	Oct. 2, 1888
INGOLD, EUGENE	Consulting Engineer and Expert, 1669 Second Ave., Pittsburg, Pa.	April 18, 1894
INSULL, SAMUEL	President, Chicago Edison Co., 513 Rookery, Chicago, Ill.	Dec. 7, 1886
IWADARE, KUNIHIKO	Electrician, Osaka Electric Light Co., Osaka, Japan.	Sept. 20, 1893
IZARD, E. M.	Electrical Engineer, Room 1409, 315 Dearborn St., Chicago, Ill.	Mar. 5, 1889
JAEGER, CHARLES L.	Inventor, Maywood, N. J.	Dec. 20, 1893
JOHNSON, J. N.	Care Palmetto Milling and Com. Co., Columbia, S. C.	Sept. 20, 1893
JOHNSTON, W. J.	*The Electrical World*, 253 Broadway. New York City.	April 15, 1884
JONES, F. R.	Professor of Machine Design, University of Wisconsin, Madison, Wis.	May 20, 1890
JONES, ARTHUR W.	Care of H. H. Kingsbury, Sydney, N. S. W.	Oct. 17, 1894
JUDSON, WM. PIERSON	U. S. Civil Engineer, Oswego, N. Y.	June 8, 1887
KAMMEYER, CARL E.	Western Manager the *Electrical Engineer*, 1439 Monadnock Block, Chicago, Ill	Sept. 19, 1894
KEEFER, EDWIN S.	Supt. of Electric Light Construction, Western Electric Co., 22 Thames St., New York City; residence, Elizabeth, N. J.	April 18, 1894
KEILHOLTZ, P. O.	U. S. Electric Power and Light Co., Holliday and Centre Sts., Baltimore, Md.	Mar. 21, 1893
KELLER, CHAS. L.	Chicago Telephone Co.; Residence, 5940 East End Ave., Chicago, Ill.	Oct. 17, 1894
KELLER, E. E.	Vice-Prest. and General Manager, Westinghouse Machine Co, 224 Murtland Ave., Pittsburg, Pa.	Sept. 20, 1893
KELLER, EDWIN R., *M.E.*	Mechanical and Electrical Engineer, Falkenau Engineering Co., Ltd., 1210 Betz Building, 4823 Springfield Ave., Philadelphia, Pa.	Mar. 21, 1894
KELLOGG, JAMES W., *M.E.*	General Electric Co., Lighting Dept., Schenectady, N. Y.	June 26, 1891
KENNELLY, A. E.	(*Manager.*) Electrician, Firm of Houston & Kennelly, 1105–1106 Betz Bldg., Philadelphia, Pa.	May 1, 1888
KIMBALL, DR. ALONZO S.	Professor of Physics, and Electrical Engineering, Worcester Polytechnic Institute, Worcester, Mass.	Sept. 3, 1889
KINNEY, HARRY A.	Manager, Acme Storage Battery Co., St. Louis, Mo.	Mar. 18, 1890
KIRKEGAARD, GEORG	Electrical Experimental Shop, 329 Union St., Brooklyn, N. Y.	Sept. 20, 1893

Name.	Address.	Date of Election.
KIRKLAND, JOHN W.	Electrical Engineer, General Electric Co., Schenectady, N. Y.	Mar. 21, 1894
KNOX, FRANK H.	White-Crosby Co., 2116 N. Charles St., Baltimore. Md.	June 20, 1894
KNOX, JAMES MASON	Student in Electrical Engineering, Columbia College, School of Mines; Residence, 32 West 129th St., New York City.	Jan. 17, 1894
KREIDLER, W. A.	Editor and Publisher, *Western Electrician*, 6 Lakeside Building, Chicago, Ill.	Oct. 4, 1887
LAND, FRANK	Globe Hotel, Syracuse, N. Y.	Sept. 22, 1891
LANE, VANCE	Manager and Superintendent Construction, Nebraska Telephone Co., Omaha, Neb.	Dec. 19, 1894
LANPHEAR, BURTON S.	Fellow and Graduate Student in Electrical Engineering, Cornell University; residence, 106 Union Ave., Ithaca, N. Y.	Jan. 16, 1895
LANMAN, WILLIAM H.	Patent Dept., General Electric Co., 44 Broad St., New York City.	June 6, 1893
LARDNER, HENRY ACKLEY	Instructor in Electrical Engineering, State College, Penn.	Dec. 19 1894
LARNED, SHERWOOD J.	Electrical Engineer, Chicago Telephone Co., 203 Washington St., Chicago, Ill.	Oct. 17, 1894
LA ROCHE, FRED. A.	President and Manager, La Roche Electric Works, American and Diamond Sts., Residence, 2235 N. 16th St., Philadelphia, Pa.	Sept. 19, 1893
LAWTON, W. C.	Roselle, N. J.	June 6, 1893
LECONTE, JOSEPH NISBET	Instructor in Electrical Engineering, State University, Berkeley, Cal.	Feb. 27, 1895
LEDOUX, A. R., *M. S.*, *Ph.D.*,	9 Cliff St., New York City.	Dec. 7, 1886
LEE, JOHN C.	Chemist and Electrician, American Bell Telephone Co., Mountfort St., Longwood, Brookline, Mass.	Mar. 18, 1890
LENZ, CHARLES OTTO	Electrical Engineer, Room 510, Industrial Trust Bldg., Providence, R. I.	Mar. 15, 1892
LESLIE, EDWARD A.	Vice-President and Manager, Manhattan Electric Light Co., Ltd., New York City; residence, 343 Hancock St., Brooklyn, N. Y.	Jan. 16, 1895
LESTER, WILLIAM B.	Western Union Telegraph Co., 195 Broadway; residence, 346 Lenox Ave., New York City.	Jan. 16, 1895
LEVIS, MINFORD	Superintendent, and Electrical Engineer, Novelty Electric Co., 54 North 4th St., Philadelphia, Pa.	Feb. 21, 1893
LEVY, ARTHUR B.	Assistant Engineer, Arc Light Dept., General Electric Co., 810 Lexington Ave., New York City.	Jan. 20, 1891
LEWIS, HENRY FREDERICK WILLIAM,	Redlands, 48 Sydenham Road, Croydon, Surrey, England.	Mar. 5, 1889

Name.	Address.	Date of Election.
LIEBIG, GUSTAV A., JR.	Electrical Testing Bureau, Johns Hopkins University, Baltimore, Md.	Mar. 6, 1888
LILLEY, L. G.	Electrical Inspector, Underwriters' Association of Cincinnati, Wyoming, O.	June 20, 1894
LIGHTHIPE, JAMES A.	District Engineer, General Electric Co., 15 First St., San Francisco, Cal.	Feb. 21, 1894
LINDNER, CHAS. T.	Martin & Lindner, Electrical Engineers, Luning Building, San Francisco, Cal., residence Berkeley, Cal.	Dec. 20, 1893
LLOYD, HERBERT	General Manager, Electrical Engineer and Chemist, The Electric Storage Battery Co., Drexel Bldg., Philadelphia, Pa.	June 20, 1894
LOWENHERZ, HERMAN	Mechanical Engineer, Met. Tel. and Tel. Co., 18 Cortlandt St., New York City; residence, 311 Hudson St., Hoboken.	Feb. 27, 1895
LOOMIS, OSBORN P.	Electrical Engineer, Bound Brook, N. J.	Sept. 16, 1890
LOW, GEORGE P.	Consulting Electrical Engineer, Electrical Inspector, Pacific Insurance Union, 303 California St., San Francisco, Cal.	Jan. 17, 1893
LOZIER, ROBERT T. E.	Electrical Engineer, Warren & Lozier, 465 Greenwich St., New York City.	May 20, 1890
LUFKIN, HARVEY L.	Crocker-Wheeler Electric Co., 39 Cortlandt St., New York City.	June 17, 1890
LUNDELL, ROBERT	Electrical Engineer, Interior Conduit and Insulation Co., 44 Broad St., New York; residence, 47 Brevoort Pl., Brooklyn, N. Y.	Feb. 7, 1890
LUQUER, THATCHER, T. P	Bedford, N. Y.	June 26, 1891
LYMAN, CHESTER WOLCOTT	Manager, Herkimer Paper Co., Herkimer, N. Y.	Sept. 19, 1894
LYMAN, JAMES [Life Member.]	Student in Electrical Engineering at Cornell University, 39 Eddy St., Ithaca, N. Y.; residence, Middlefield, Conn.	Sept. 19, 1894
MACCULLOCH, ROBERT C.	Manager, Jos. Lough Electric Co., 503 Fifth Ave.; residence, 407 Lexington Ave., New York City.	Feb. 27, 1895
MACFADDEN, CARL K.	Nutting Electric Mfg. Co., 128 So. Clinton St., Chicago, Ill.	Sept. 27, 1892
MACKIE, C. P.	Manager, Electric Selector and Signal Co., 45 Broadway, New York City.	Mar. 21, 1893
MACMULLAN, ROBERT HEATH	Lafayette, Ind.	Sept. 22, 1891
MACQUESTEN, W. D.	Electrical Engineer and Contractor, Room 25, 15 Cortlandt St., New York City.	April 15, 1890
MADDEN, OSCAR E.	41 and 43 Wall St., New York City.	April 15, 1884
MAGEE, LOUIS J.	Electrical Engineer, Director, der Union Elektricitats Gesellschaft, Corneliusstr 1., Berlin, W. Germany.	April 2, 1889

Name.	Address.	Date of Election.
MAGENIS, JAMES P.	Editor. The *Adams Freeman*, Adams, Mass.	Sept. 27, 1892
MALCOLM, PHILIP S.	Electrical Engineer and Contractor, 131 Sixth St., Portland, Ore.	Mar. 18, 1890
MALIA, JAMES P.	Electrician, Armour & Co., 5314 Union Ave., Chicago, Ill.	June 20, 1894
MANN, FRANCIS P.	Maison Breguet, 19 Rue Didot, Paris, France.	June 6, 1893
MANSFIELD, GEO. W.	Electrical Engineer, Melrose Highlands, Mass.	June 2, 1885
MARTIN, A. J.	Complete Electric Construction Co., 121 Liberty St., New York City.	Mar. 15, 1892
MARTIN, FRANK	Electrical Engineer, Madison Square Garden Company, New York City.	Oct. 21, 1890
MARTIN, J.	Electrician, 16 Oak St., Newark, N. J., Master Electrician, Equipment Dept., New York Navy Yard.	Oct. 21, 1890
MARTIN, T. COMMERFORD	(*Past-President.*) Editor, *The Electrical Engineer*, 203 Broadway, New York City.	April 15, 1884
MASON, JAMES H.	Electrical Expert, 10 Fifth Ave., Brooklyn, N. Y.	May 19, 1891
MATTHEWS, CHARLES P.	Instructor in Physics, Cornell University, 211 E. State St., Ithaca, N. Y.	May 16, 1893
MAURO, PHILIP	Counsellor at-Law in Patent Causes (Pollock & Mauro), 620 F. St., Washington, D. C.	Dec. 21, 1892
MAYER, MAXWELL M.	Mfgr. of Dynamos and Motors, 411 107th St., E. R.; residence 242 East 114th St., New York City.	Feb. 27, 1895
MAYRHOFER, JOS. CARL	Electrical Engineer, 165 W. 82d St., New York City.	June 20, 1894
MCBRIDE, JAMES	Superintendent, N. Y. & Boston Dye Wood Co., 146 Kent St., Brooklyn, N. Y.	Sept. 27, 1892
MCCARTHY, LAWRENCE A.	Western Union Telegraph Co., New York City, 1053 Bedford Ave., Brooklyn, N. Y.	Jan. 19, 1892
MCCLURG, W. A.	Manager, Electrical Dept., Plainfield Gas and Electric Light Co., 207 Madison Ave., Plainfield, N. J.	Dec. 20, 1893
MCCROSKY, JAMES W.	Electrical Engineer, Westinghouse Electric and Mfg. Co., Pittsburg, Pa.	Dec. 20, 1893
MCCROSSAN, J. A.	Manager and Electrician, Citizens' Telephone and Electric Co., Rat Portage, Ont.	Oct. 18, 1893
MCELROY, JAMES F.	Mechanical Supt., The Consolidated Car Heating Co., 131 Lake Ave., Albany, N. Y.	Nov. 15, 1892
MCKAY, C. R.	Consulting Engineer, 140 South Main St., Salt Lake City, Utah.	Dec. 20, 1893

Name.	Address.	Date of Election.
McKibbin, George N.	Reed & McKibbin, General Street Railway Contractors, 80 Broadway, New York City.	June 8, 1887
McKinstry, J. P.	General Manager, Cleveland Telephone Co., 316 Seneca St., Cleveland, O.	April 15, 1884
McKissick, A. F.	Professor of Electrical Engineering, The A. & M. College of Ala., Auburn, Ala.	Feb. 16, 1892
McRae, Austin Lee	Professor of Physics, University of Texas, Austin, Texas.	May 17, 1892
Medina, Frank P.	Electrician, Pacific Postal Telegraph Co., 534 Market St., San Francisco, Cal.	Sept. 19, 1894
Mercer, Andrew G.	Electrician, Waterloo Electric Co., Waterloo, N. Y.	Sept. 3, 1889
Meredith, Wynn	Electrical Engineer, Hasson & Hunt, 310 Pine St., San Francisco, Cal.	Jan. 17, 1894
Merrill, E. A.	Electrical Engineer, Pierce & Miller Engineering Co., 42 Cortlandt St., New York City.	Sept. 20, 1893
Merritt, Ernest	Assistant Professor in Physics, Cornell University, Ithaca, N. Y.	Sept. 16, 1890
Meyer, Julius	Consulting Engineer, 44 Broad St., New York City.	Oct. 25, 1892
Miller, Joseph A.	Civil and Consulting Engineer, 25 Butler Exchange, Providence, R. I.	Dec. 9, 1884
Miller, Wm. C., *M. S.*	Electrical Engineer, 3 South Hawk St., Albany, N. Y.	Oct. 21, 1890
Miner, Willard M.	Electrician and Inventor, 339 East Second St., Plainfield, N. J.	July 12, 1887
Mitchell, John Murray	Lawyer, Box 3712, 35 Wall St., New York City.	June 2, 1885
Mitchell, Sidney Z.	Manager, Oregon, Washington and Idaho Agency, General Electric Co., Fleischner Building, Portland, Ore.	Nov. 12, 1889
Mix, Edgar W.	Electrician, with Thomson-Houston International Electric Co., 27 Rue de Loudres, Paris, France.	Sept. 3, 1889
Monell, Joseph T.	Consulting Electrical Engineer, 236 W 22d St., New York City.	Oct. 27, 1891
Moore, John J.	[Address unknown.]	Nov. 12, 1889
Mordey, Wm. Morris	Electrician, Brush Electrical Engineering Co., 34 Montserrat Road, Putney, London, Eng.	Sept. 22, 1891
Morehouse, H. H.	General Manager and Electrician, Alumbrado Electrico de Quezaltenango, Apartado, No. 44, Quezaltenango, Guatemala, C. A.	Feb. 21, 1894
Morrison, J. Frank	15 South St., Baltimore, Md.	April 15, 1884

Name.	Address.	Date of Election.
MORSE, GEORGE H.	910 Wright Block, Minneapolis; residence, Excelsior, Minn.	May 15, 1894
MORSS, EVERETT	Vice-President, Simplex Electric Co., 303 Marlboro St., Boston, Mass	Sept. 22, 1891
MORTON, HENRY, *Ph.D.*	President of Stevens Institute of Technology, Hoboken, N. J.	May 24, 1887
MOSES, DR. OTTO A.	Electrician, 1037 Fifth Ave., New York City.	May 17, 1887
MOSES, PERCIVAL ROBERT	Student of Electrical Engineering, Columbia College; residence, 46 West 97th St., New York City.	Dec. 19, 1894
MOSSCROP, WM. A., *M.E*	Electrical Engineer, 29 Clifton Place, Brooklyn, N. Y.	May 7, 1889
MOTT, S. D.	Electrical Engineer and Inventor, Passaic, N. J.	Sept. 20, 1893
MOTTRAM, WILLIAM T. M.	Electrical Engineer, Room 416, Trust Building, Dallas, Tex.	Mar. 21, 1893
MUNNS, CHAS. K.	Electrician, Strowger Autom. Tel. Exchange; residence, 1002 W. Monroe St., Chicago, Ill.	Nov. 21, 1894
MUSTIN, HERBERT S.	Electrician, care of American Mfg. and Engineering Co., 143 Liberty St, New York City.	Dec. 20, 1893
MYERS, GEO. FRANCIS	Inventive Engineer, Penn Building, Pittsburg, Pa.	June 17, 1890
MYERS, L. E.	Secretary and Treasurer, Electrical Installation Co., 917 Monadnock Building Chicago, Ill.	Sept. 19, 1894
NEILER, SAMUEL G.	1409 Manhattan Building, Chicago, Ill.	April 18, 1894
NEWELL, ARTHUR J.	Electrical Engineer, R. T. Oakes & Co., 366 High St., Holyoke, Mass	Mar. 18, 1890
NICHOLSON, WALTER W.	General Supt. Central N. Y. Telephone and Telegraph Co., 73 Howard Ave., Utica, N. Y.	May 15, 1894
NORTON, ELBERT F.	Chief Inspector, City Electrical Inspection, 15 City Hall, Chicago, Ill.	Dec. 20, 1893
NOXON, C. PER LEE	Contracting Electrical Engineer, 628 Mission St., San Francisco, Cal.	Oct. 17, 1894
NUNN, RICHARD J., *M.D.*	Physician, 119½ York St., Savannah, Ga.	July 12, 1887
NUTTING, SAMUEL E.	Electrician and Supt., Nutting Electric Mfg. Co., 241 South Scoville Ave., Oak Park, Ill.	Sept. 20, 1893
NYHAN, J. T.	Superintendent and Electrician, Macon and Indian Spring Electric Railway, Macon, Ga.	Feb. 27, 1895
OCKERSHAUSEN, H. A.	Electrical Engineer, 65 Madison Ave., Jersey City, N. J.	Sept. 6, 1887
O'CONNELL, J. J.	Telephone Engineer, Chicago Telephone Co, Residence, 76 Eugene St., Chicago, Ill.	Oct. 17, 1894

Name.	Address.	Date of Election.
O'DEA, MICHAEL TORPEY	Professor of Applied Electricity. University of Notre Dame, Notre Dame, Ind.	June 8, 1887
OLAN, THEODOR, J. W.	Civil and Electrical Engineer, 69 West 49th St., New York City.	May 16, 1893
OLIVETTI, CAMILLO	Ingegnere Industriale, Ivrea, Italy.	Oct. 17, 1894
OSBORNE, LOYALL ALLEN	President, Osborne Switch Co., 523 Prudential Building, Newark, N. J.	Oct. 18, 1893
OSTERBERG, MAX, *E.E.*	Associate Editor *Electric Power*, and Post Graduate Student of Mathematical Physics, Columbia College; residence, 232 East 62nd St., New York City,	Jan. 17, 1894
OTTEN, DR. JAN D.	Engineer, Union Elektricitats Gesellschaft, Kleinbeeren-strasse 21, Berlin, S. W. Germany.	Nov. 18, 1890
OUDIN, MAURICE	Electrical Engineer, General Electric Co., Schenectady, N. Y.	June 20, 1894
OWENS, R. B.	Professor of Electrical Engineering, University of Nebraska, Lincoln, Neb.	June 17, 1890
PAGE, A. D.	Assistant Manager, General Electric Co. Lamp Works, Harrison, N. J.	Jan. 19, 1892
PARCELLE, ALBERT L.	Electrician and Inventor, 157 Washington St., Boston, Mass.	Dec. 16, 1891
PARKER, HERSCHEL C.	Tutor in Physics, Columbia College, 21 Fort Green Pl., Brooklyn, N. Y.	April 19, 1892
PARMLY C. HOWARD, *S.M.*, *E.E.*	College of the City of New York, 17 Lexington Ave.; residence, 344 W. 29th St., New York City.	Feb. 21, 1893
PARSELL, HENRY V., JR.	31 E. 21st St., New York City.	Nov. 12, 1889
PAUL, CHAS. M.	Electrician, 172 Remsen St., Brooklyn, N. Y.	May 7, 1889
PECK, EDWARD F.	General Supt. Citizens Electric Illuminating Co., cor. Rockwell Pl. and DeKalb Ave., Brooklyn, N. Y.	May 20, 1890
PECK, SAMUEL C.	Electrician, Apartado 403, City of Mexico, Mexico.	Sept. 6, 1887
PEDERSEN, FREDERICK MALLING	Assistant Electrical Engineer, Crocker-Wheeler Electric Co., Ampere, E. Orange, N. J.; residence, 327 W. 34th St., New York City.	Sept. 20, 1893
PEIRCE, WM. H.	Assistant Manager, Baltimore Smelting and Rolling Co., Keyser Bldg, German and Calvert Sts., Baltimore, Md.	Sept. 7, 1888
PERKINS, FRANK C.	Electrical Engineer and Contractor, 774 Prospect Ave., Buffalo, N. Y.	Oct. 21, 1890
PEROT, L. KNOWLES	General Manager, Eastern Engineering and Construction Co., 308 Walnut St., Philadelphia, Pa.	Mar. 15, 1892
PETTY, WALTER M.	Superintendent Fire Alarm Telegraph, Rutherford, N. J.	May 16, 1893

Name.	Address.	Date of Election.
PFUND, RICHARD	With Western Union Telegraph Co., 195 Broadway, New York City.	April 18, 1893
PHILBRICK, B. W.	Electrician, in charge of Electrical Plant, Hon. Levi P. Morton, Rhinecliff, N. Y.	May 15, 1894
PHILLIPS, EUGENE F.	Manufacturer Insulated Electric Wire, Providence, R. I.	July 13, 1889
PHILLIPS, LEO A.	Westinghouse Electric and Mfg. Co., East Pittsburg, Pa.	Mar. 21, 1894
PLUMB, CHARLES	Proprietor and Electrician, The Chas. Plumb Electrical Works, 89 Erie St., Buffalo, N. Y.	June 20, 1894
POOLE, CECIL P.	Care *Electrical World*, 253 Broadway, New York City.	Jan. 3, 1888
POPE, RALPH WAINWRIGHT	Secretary to the American Institute of Electrical Engineers, 26 Cortlandt St., (Telephone, 2199 Cortlandt), New York City; residence, 570 Cherry St., Elizabeth, N. J.	June 2, 1885
POTTS, CHAS. EDWIN, *B. S., E. E.*	Engineer, 1356 Dean St., Brooklyn, N. Y.	Feb. 21, 1894
POTTER, HENRY NOEL	Electrician, Steglitzer Strasse, 10 parterre, Berlin W., Germany.	Sept. 19, 1894
POWELL, WILLIAM H.	Electrical Engineering Dep't, Lehigh University, South Bethlehem, Pa.	June 17, 1890
PRATT, CHARLES A.	Electrical Engineer, The Independent Electric Co., 39th St. and Stewart Ave., Chicago, Ill.	Dec. 19, 1894
PRICE, CHAS. W.	Editor the *Electrical Review*, 13 Park Row, New York City; residence, 223 Garfield Place, Brooklyn, N. Y.	Sept 19, 1894
PRINCE, J. LLOYD	868 Flatbush Ave., (Flatbush Station), Brooklyn, N. Y.	Feb. 27, 1895
PRIVAT, LOUIS	Electrician, Cicero Water, Gas and Electric Light Co., Oak Park, Ill.	Dec. 19, 1894
PROCTOR, THOS. L.	General Manager, Riker Electric Motor Co., Brooklyn; residence, Newtown, L. I., N. Y.	April 18, 1894
PUFFER, WM. L.	Assistant Professor of Electrical Engineering, Mass. Institute of Technology, Boston, Mass.	Dec. 20, 1893
PUPIN, DR. MICHAEL I.	(*Manager.*) Adjunct Professor in Mechanics, Columbia College, New York City, 46 W. 72d St., New York.	Mar. 18, 1890
RANDALL, JOHN E.	Columbia Incandescent Lamp Co., 1912 Olive St., St. Louis, Mo.	May 7, 1889
RANDOLPH, L. S.	Professor of Mechanical Engineering, Blacksburg, Va.	Feb. 21, 1893
RAY, WILLIAM D.	Superintendent Everett Railway and Electric Co., Everett, Washington.	Sept. 27, 1892
READ, ROBERT H.	Firm of Pope, Read & Rogers, 39 Cortlandt St., New York City.	Jan. 19, 1892

Name.	Address.	Date of Election.
REBER, SAMUEL	Lieut. Signal Corps, U. S. Army, Fort Riley, Kan.	Sept. 20, 1893
REDMAN, GEO. A.	General Supt., Electric Dept., Brush Elec. Light Co., and Rochester Gas and Elec. Co., Rochester, N. Y.	Feb. 27, 1895
REED, CHAS. J.	Electrician, 609 Norris St., Philadelphia, Pa.	Mar. 5, 1889
REED, HARRY D.	Electrician, Bishop Gutta Percha Co., 420 East 25th St., New York City; residence, 88 North 9th St., Newark, N. J.	Sept. 19, 1894
REED, HENRY A.	Secretary and Manager, Bishop Gutta-Percha Co., 422 East 25th St., New York City; residence, 88 North 9th St., Newark, N. J.	June 4, 1889
REID, THORBURN	Electrical Engineer, General Electric Co., Schenectady, N. Y.	Oct. 21, 1890
REILLY, JOHN C.	General Supt., N. Y. & N. J. Tel. Co., 16 Smith St., Brooklyn, N. Y.	April 15, 1884
REINMANN, A. L.	[Address unknown.]	June 8, 1887
RENNARD, JOHN CLIFFORD, *A. B. E. E.*	Assistant to Electrical Engineer, Met. Telephone and Tel. Co.; residence, 302 W. 73d St., New York City.	Jan. 16, 1895
REQUIER, A. MARCEL	Electrical Engineer, Westinghouse Electric and Manufacturing Co., Pittsburg, Pa.	Dec. 20, 1893
RHODES, S. ARTHUR	Electrician, Chief Testing Department, Chicago Telephone Co., Chicago, Ill.; residence, 429 North Pine Ave., Austin, Ill.	Oct. 17, 1894
RICHARDSON, ALBERT E.	Lecturer in Science, 21 Knight's Park, Kingston-on-Thames, England.	Feb. 21, 1894
RICHARDSON, ROBERT E.	Electrical Engineer, Pierce & Richardson, 1409 Manhattan Building; residence, 3827 Forest Ave , Chicago, Ill.	Sept. 19, 1894
RICKER, CHARLES W.	Expert Electrical Engineer, 109 White Bldg., Buffalo, N. Y.	May 15, 1894
RIDLEY, A. E. BROOKE	Agent, Electrical Engineer, Siemens & Halske Electric Co., 508 California St., San Francisco, Cal.	Nov. 21, 1894
RIKER, ANDREW L. [Life Member.]	Electrical Engineer, The Riker Electric Motor Co., 45 York St., Brooklyn; residence, 737 Madison Ave., New York City.	Nov. 1, 1887
RITTENHOUSE, CHAS. T.	Post-Graduate Student, Columbia College, 247 W. 138th St , New York City.	Feb. 21, 1894
ROBB, RUSSELL	With Stone & Webster, 4 Post Office Sq., Boston, Mass	Oct. 18, 1893
ROBERSON, OLIVER R.	Electrician, Western Union Telegraph Co., 195 Broadway, P. O. Box 856, New York City.	Dec. 20, 1893

Name.	Address.	Date of Election.
ROBERTS, WM. H.	15 Harrison St., Cincinnati, O.	Sept. 19, 1894
ROBINSON, ALMON	Draughtsman, Expert in Methods of Gearing, P. O. Box 943, Lewiston, Me.	Sept. 6, 1887
ROBINSON, FRANCIS G.	Foreman, Bushwick and Union Ave. Depots, Brooklyn Heights R. R. Co.; residence, 156 Macon St., Brooklyn, N. Y.	Nov. 21, 1894
RODMAN, SAMUEL, JR.	(Late 1st Lieut., 2nd U. S Artillery), Electrician and Expert in High Explosives. Room 106, Pullman, Bldg., Chicago, Ill.	Sept. 16, 1890
ROEBLING, FERDINAND W.	Manufacturer of Electrical Wires and Cables, Trenton, N. J.	June 8, 1887
ROESSLER, S. W.	Captain, Corps of Engineers U. S. A., 99 Madison St., Memphis, Tenn.	Dec. 3, 1889
ROGERS, EDWARD H.	Patent Lawyer, Firm of Pope, Read & Rogers, New Haven, Conn.	Sept. 22, 1891
ROLLER, JOHN E.	Lieut. U. S. N., in charge of Inspection and Installation, U. S. Navy Yard, New York; residence, 515 Clinton Ave., Brooklyn, N. Y.	Sept. 19, 1894
ROPER, DENNEY W.	Expert Department, General Electric Co., 302 Union St., Schenectady, N. Y.	June 6, 1893
ROSEBRUGH, THOMAS REEVE	Lecturer in Electrical Engineering, School of Practical Science, Toronto, Ont.	June 26, 1891
ROSENBAUM, WM. A.	Electrical Expert and Patent Solicitor, 177 Times Building, New York City.	Jan. 3, 1889
ROSENBERG, E. M.	Engineers' Department, Broadway and 7th Ave. Railroad; residence, 784 Lexington Ave., New York City.	Oct. 21, 1890
ROWLAND, ARTHUR JOHN	Professor of Electrical Engineering, Drexel Institute: residence, 4007 Powelton Ave., Philadelphia, Pa.	Sept. 19, 1894
ROWLAND, HENRY A.	Professor of Physics, Johns Hopkins University, Baltimore, Md.	Mar. 21, 1894
ROYCE, FRED W.	Electrician and Patent Solicitor, 1410 Pennsylvania Ave., Washington, D. C.	April 15, 1884
RUTHERFORD, W. M.	Chief Engineer, Canadian General Electric Co., 65 Front St., W. Toronto, Can.	Sept. 22, 1891
RYAN, HARRIS, J.	(*Manager.*) Professor of Electrical Engineering, Cornell University, Ithaca, N. Y.	Oct. 4, 1887
SACHS, JOSEPH	Devising and Consulting Electrical Engineer, 32 Nassau St., New York City.	Mar. 15, 1892
SACKETT, WARD M.	Assistant Chief Draughtsman, Chicago Telephone Co., residence 3249 Groveland Ave., Chicago, Ill.	Oct. 17, 1894.

Name.	Address.	Date of Election.
SAGE, HENRY JUDSON	Engineer, Lighting and Power Dept., Western Electric Co., 227 So. Clinton St.; residence, 4467 Oakenwald Ave , Chicago, Ill.	Dec. 20, 1893
SAHULKA, DR. JOHANN	Docent of Electrotechnics, Technische Hochschule, Vienna, Austria	Dec. 20, 1893
SANBORN, FRANCIS N.	Assistant Superintendent, Susquehanna Coal Co., Nanticoke, Pa.	Nov. 24, 1891
SANDERSON, EDWIN N.	New England Manager, Westinghouse Electric and Mfg. Co., 328 Exchange Building, Boston, Mass.: residence, Newton Centre, Mass.	Oct. 17, 1894
SAXELBY, FREDERICK	Electrical Engineer, 288 Summer Ave., Newark, N. J.	June 5, 1888
SCHEFFLER, FRED. A.	Stirling Boiler Co., 126 Liberty St., New York City; residence, Passaic, N. J.	May 16, 1893
SCHEIBLE, ALBERT	Secretary and Assistant, with George Cutter, 486 North Park Ave., Chicago, Ill.	June 20, 1894
SCHLOSSER, FRED. G.	Superintendent of Electric Dept., Laclede Gas Light Co., 1038 Leffingwell Ave., St. Louis, Mo.	Sept. 22, 1891
SCHMID, ALBERT	Superintendent, Westinghouse Electric and Mfg. Co., Pittsburg, Pa.	Oct. 21, 1890
SCHOEN, ALLEN MCGEE	Electrician, South Eastern Tariff Association, Fitten Building, Atlanta, Ga.	Sept. 20, 1889
SCHREITER, HEINR, *C. E.*	Editor, "*Der Techniker*," 11 Chambers St , New York City.	Jan. 17, 1893
SEARING, LEWIS	Shepard & Searing, Mechanical and Electrical Engineers, 842-3 Equitable Building, Denver, Col.	April 3, 1888
SEARLES, A. L.	Engineering Dept., The Marks-Ayer Electric Co., 73 Watt St , New York City and 291 7th Ave., Brooklyn, N. Y.	April 18, 1894
SEE, A. B.	A. B. See Manufacturing Co., 116 Front St.; residence, 107 East 19th St., (Flatbush), Brooklyn, N. Y.	Jan. 17, 1893
SEELY, J. A.	Electrical Engineer and Contractor, 121 Liberty St., New York City.	April 15, 1884
SEITZINGER, HARRY M.	Consulting and Constructing Engineer, 6 Northampton St., Wilkesbarre, Pa.	Sept. 20, 1893
SELDEN, R. L., JR.	Deep River, Conn.	Jan. 17, 1893
SERRELL, LEMUEL WM.	Mechanical and Electrical Engineer, 253 Broadway, New York City.	Nov. 1, 1887
SEVER, GEORGE F.	Instructor in Electrical Engineering, Columbia College, 34 West 32nd St., New York City.	Jan. 17, 1894
SERVA, A. A.	With Fort Wayne Electric Corporation, 29 West Jefferson St., Fort Wayne, Ind	Dec. 20, 1893

Name.	Address.	Date of Election.
SHAIN, CHARLES D.	136 Liberty St., New York City.	June 7, 1892
SHARP, CLAYTON H.	Instructor, Department of Physics, Cornell University, 122 University Ave., Ithaca. N. Y.	May 15, 1894
SHEBLE, FRANKLIN	Sheble & Parton, Ltd., 1022 Arch St., Philadelphia, Pa.	Oct. 21, 1890
SHEDD, JOHN C.	Professor of Physics and Applied Electricity, Marietta College; residence, 512 Fourth St., Marietta, Ohio.	Dec. 19, 1894
SHEEHY, ROBERT J.	Engineer and Inventor, 570 Park Ave., New York City.	April 21, 1891
SHEPARDSON, GEORGE D.	Professor of Electrical Engineering, University of Minnesota, Minneapolis, Minn.	April 21, 1891
SHIELDS. W. J.	Shields & Wood, 829 Arch St., Philadelphia, Pa.	Sept. 19, 1894
SHRADER, WILLIAM	Professor of Electrical Engineering, University of Missouri, Columbia, Mo.	Sept. 20, 1893
SINCLAIR, H. A.	Electrical Engineer, The Tucker Electric Co., 950 Bedford Ave., Brooklyn, N. Y.	June 17, 1890
SISE, CHARLES F.	President, Bell Telephone Co., of Canada, P. O. Box 1918, Montreal, Canada.	June 8, 1887
SLADE, ARTHUR J., *Ph.D.*	Engineer, with George Hill, 44 Broadway; residence, 62 East 66th St., New York City.	Sept. 19, 1894
SLATER, FREDERICK R.	Designing Department, Otis Bros. & Co., 153 Warburton Ave., Yonkers, N. Y.	Oct. 17, 1894
SMITH, CHARLES HENRY	Box 346, Rome, Ga.	Jan. 17, 1894
SMITH, FRANK E.	Chief Electrician, Edison Light and Power Co., 229 Stevenson St., San Francisco, Cal.	Sept 19, 1894
SMITH, FREDERICK H.	Civil Engineer, 216 Equitable Bldg., Baltimore. Md.	Nov. 12, 1889
SMITH, HAROLD BABBITT	Professor of Electrical Engineering, Purdue University, 164 Columbia St., Lafayette, Ind.	Nov. 24, 1891
SMITH, J. BRODIE	Supt. and Electrician. Manchester Electric Light Co., 142 Merrimack St., Manchester, N. H.	Mar. 21, 1894
SMITH, J. ELLIOT	Superintendent Fire Alarm Telegraph, 122 W. 73d St., New York City.	April 15, 1884
SMITH, OBERLIN	President and Mechanical Engineer, Ferracute Machine Co., Lochwold, Bridgeton, N. J.	May 19, 1891
SMITH, T. JARRARD	Manager Electrical Dept., The E. S. Greeley & Co., 7 Dey St., New York City.	April 19, 1892
SOUZA, CARLOS MONTEIRO e	[Address unknown.]	Sept. 6, 1887

Name.	Address.	Date of Election.
SPENCER, THEODORE	With Bell Telephone Co., 406 Market St., Philadelphia, Pa.	Mar. 21, 1893
SPICER, CHAS. W.	1054 31st St., Washington. D. C.	Nov. 12, 1889
SPIKE, CLARENCE J.	Halifax, N. S.	Mar. 18, 1890
SPRAGUE, FRANK J.	(*Past-President.*) 182 West End Ave., Vice-Prest. Sprague Electric Elevator Co.. Firm of Sprague, Duncan & Hutchinson, Postal Telegraph Bldg., 253 Broadway, New York City	May 24, 1887
SPROUT, SIDNEY S.	Electrical Engineer, 303 California St., San Francisco, Cal.	Jan. 17, 1894
SPRUSON, WILFRED J.	Member of the firm of Hepburn & Spruson, Consulting Engineers and Electricians, 169 King St , Sydney, Australia.	Dec. 16, 1890
SQUIER, GEORGE O., *Ph.D.*	1st Lieut., 3d Artillery, Fortress Monroe, Va.	May 19, 1891
STADELMAN, WM. A.	[Address unknown.]	Feb. 7, 1890
STAHL, TH.	Creusot Works, Creusot, France.	Nov. 15, 1892
STANLEY, WILLIAM	Electrician, Pittsfield, Mass.	Dec. 6, 1887
STEARNS, JOEL W., JR.	Treasurer. Mountain Electric Co., Box 1545, Denver, Col.	June 20, 1894
STEPHENS, GEORGE	General Supt., Canadian General Electric Co., Ltd., Peterboro, Ont.	June 20, 1894
STEVENS, J. FRANKLIN	Secretary and Treasurer, La Roche Electric Works, American and Diamond Sts.; residence, 1419 Walnut St., Philadelphia, Pa.	Sept. 19, 1894
STINE, PROF. WILBUR M.	Director Electrical Dept., Armour Institute, Chicago, Ill.	May 15, 1894
STOCKBRIDGE, GEO. H.	Patent Attorney, Room 114, 39 Cortlandt St , New York City.	May 24, 1887
STOCKLY, GEO. W.	32 Liberty St , New York City; residence, Lakewood, N. J.	April 15, 1884
STONE, CHARLES A.	With Firm of Stone & Webster, 4 P. O. Sq., Boston, Mass.	May 19, 1891
STORRS, PROF. H. A.	Professor of Electrical Engineering, University of Vt., Burlington, Vt.	Mar. 21, 1893
STRATTON, MILTON G.	Electrician, U. S. Electric Light Co., 213 14th St., N. W., Washington, D. C.	Sept. 20. 1893
STRAUSS, HERMAN A.	Electrical Engineer, Westinghouse Electric and Mfg. Co.; residence, 157 Larimer Ave., East Liberty, Pittsburg, Pa.	Oct. 17, 1894
STRONG, FREDERICK G.	Box 959, Hartford, Conn.	Oct. 27, 1891
STUMP, CLARENCE E.	General Manager, *Street Railway Gazette*, 26 Cortlandt St., New York City.	May 17, 1887
STURTEVANT, CHARLES L.	Patent Attorney, Atlantic Building, Washington. D. C.	Dec. 20, 1893

Name.	Address.	Date of Election.
SUMMERS, LELAND L.	Electrical Engineer, 436 The Rookery, Chicago, Ill.	Feb. 16, 1892
SVENTORZETZKY, CAPT. LOUDOMIR	Military Engineering Academy, St. Petersburg, Russia.	Sept. 20, 1893
SWENSON, BERNARD VICTOR	Instructor in Electrical Engineering, University of Illinois, Champaign, Ill.	Feb. 27, 1895
SWEET, HENRY N.	Chief of Patent Bureau, Thomson Electric Welding Co., 4 Spruce St., Boston, Mass.	May 20, 1890
SYKES, HENRY H.	Assistant Electrician, American Telephone and Telegraph Co., 75 Hicks St., Brooklyn, N. Y.	Oct. 18, 1893
TABER, ROBERT B.	Gas Engineer, Special Agent General Electric Co., 620 Atlantic Ave., Boston, Mass.	Sept. 16, 1890
TAIT, FRANK M.	Superintendent, Catasauqua Electric Light and Power Co., 731 3d St., Catasauqua, Pa.	Sept. 19, 1894
TAPLEY, WALTER H.	Electrician in Government Printing Office, care of Public Printer, Washington, D. C.	Oct. 25, 1892
TEMPLE, WILLIAM CHASE	Mechanical and Electrical Engineer, Lewis Block, P. O. Box 800, Pittsburg, Pa.	May 3, 1887
TESLA, NIKOLA	Electrical Engineer and Inventor, 35 So. 5th Ave., New York City.	June 5, 1888
THOMPSON, WILLIAM GEO. MACNEILL	Resident Engineer, Sault Ste. Marie Canal, St. Catharines, Ont.	July 12, 1887
TOBEY, WILLIAM BOARDMAN	General Manager, S. K. C. Specialty Co., Pittsfield, Mass	Sept. 16, 1890
TOERRING, C. J., JR.	Electrical Engineer. Marks-Ayer Electric Co., 73 Watt St.; residence. 337 West 59th St., New York City.	April 18, 1894
TOWER, GEORGE A.	Electrical Engineer, The Sherwood Land Co., and The Jefferson Hotel Co., 109 S. First St., Richmond, Va.	May 15, 1894
TOWNSEND, HENRY C.	Attorney and Expert in Electrical Cases, 5 Beekman St., New York City.	July 10, 1888
TREADWELL, AUGUSTUS, JR.	Private Assistant, Polytechnic Institute, 488 3d St., Brooklyn, N. Y.	Feb. 21, 1894
TROTT, A. H. HARDY [Life Member.]	Electrical Expert, Union R. R. Co., Providence, R. I.	Jan. 20, 1891
TUTTLE, GEORGE W.	Storekeeper. Sawyer-Man Electric Co., 510 W. 23d St., New York City.	Mar. 17, 1891
VAIL, THEO. N.	18 Cortlandt St., New York City.	April 15, 1884
VAN BRUNT, WALTER	Manager, Duluth Telephone Co., Duluth, Minn.	Sept. 6, 1887
VAN BUREN, GURDON C.	Electrician and Electrical Contractor, 84 Clinton Ave., Albany, N. Y.	Oct. 25, 1892

Name.	Address.	Date of Election.
VANCE, A. ST. CLAIR	[Address unknown.]	April 2, 1889
VANDEGRIFT, JAMES A.	Westinghouse Electric and Mfg. Co., Robinson St., Allegheny City, Pa.	Nov. 24, 1891
VANDERSLICE, G. HAMILTON	Westinghouse Electric and Mfg. Co., P. O. Box 911, Pittsburg, Pa.	Dec. 19, 1894
VAN VLECK, FRANK	President, Van Vleck Tramway Co., Wells Fargo Bldg., Los Angeles, Cal.	Nov. 16, 1886
VAN WYCK, PHILIP V. R., JR.	981 Madison Ave., New York City.	April 21, 1891
VARLEY, RICHARD, JR.	General Manager, Varley Duplex Magnet Co., 62 Cortlandt St., New York City.	Mar. 18, 1890
VARLEY, THOMAS W.	Electrician. The Okonite Co., Ltd.; residence, 250 Bloomfield Ave., Passaic, N. J.	Sept. 19, 1894
VARNEV, WILLIAM WESLEY	Attorney at Law, Electrical Expert, 118 East Lexington St.: residence, 1001 Harlem Ave., Baltimore, Md.	Nov. 21, 1894
VERLEY, HORACE S. L.	With Dr. Wm. E. Geyer, as Laboratory Assistant; Stevens Institute, Hoboken, N. J.	May 17, 1892
VOIT, DR. ERNST	Professor of Electricity, Technical University, Schwanthalerstrasse, Munchen, Germany.	Mar. 21, 1894
WACKER, GEORGE G.	1340 Vanderbilt Ave., New York City.	Sept. 6, 1887
WALLACE, GEO. S.	Telegraph Office Manager, Chesapeake & Ohio Ry. Co., Box 214, Huntington, W. Va.	Oct. 25, 1892
WALLACE, WILLIAM	(*Vice-President.*) Wire Manufacturer, Ansonia, Conn.	April 15, 1884
WALTER, HENRY E.	3 Princes Mansions, Victoria St., London, Eng.	April 2, 1889
WARDELL, GEORGE P.	103 East Tenth St., New York City.	Nov. 12, 1889
WARDLAW, GEORGE A.	412 East Willow St., Syracuse, N. Y.	Jan. 17, 1894
WARING, RICHARD S.	Standard Underground Cable Co., 61 Westinghouse Bldg., Pittsburg, Pa.	April 15, 1884
WARING, JOHN	Waring Electric Co., Ovid, N. Y.	Dec. 16, 1890
WARNER, CHAS. H.	Consulting Electrical Engineer, 50 Broadway, New York City.	Dec. 20, 1893
WASON, CHAS. W.	Electrical Engineer and Purchasing Agent, Cleveland Electric R. R. Co., 1762 Euclid Ave., Cleveland, O.	May 19, 1891
WASON, LEONARD C.	Vice-Prest., The Aberthaw Construction Co., 12 Pearl St., Boston; residence, 199 Harvard St., Brookline, Mass.	Dec. 20, 1893
WATERHOUSE FRANK G.	Room 6, 302 Asylum St., Hartford, Conn.	Sept. 6, 1887
WATERS, EDWARD G.	Electrical Engineer, Pennsylvania General Electric Co., 425 Wood St., Pittsburg, Pa.	Mar. 18, 1890

Name.	Address.	Date of Election.
WATSON, ROBERT	Patent Attorney, 931 F. St., N. W., Washington, D. C.	Oct. 21, 1890
WATTS, H. FRANKLIN	Electrical Engineer and Contractor, Norristown, Pa.	May 20, 1890
WEAVER, NORMAN R.	Box 87, Selma, Ala.	Oct. 25, 1892
WEBSTER, DR. ARTHUR G.	Assistant Professor of Physics, Clark University, 936 Main St., Worcester, Mass.	Jan. 19, 1892
WEBSTER, EDWIN S.	Firm of Stone & Webster, 4 P. O. Sq., Boston, Mass.	April 21, 1891
WENDLE, GEORGE E.	2433 North 7th St., Philadelphia, Pa.	Feb. 21, 1894
WEST, JULIUS HENRIK	Engineer, Handjery St., 58 Friedenau, Berlin, Germany.	Sept. 20, 1893
WELLES, FRANCIS R.	Manufacturer, 46 Avenue de Breteuil, Paris, France.	Sept. 6, 1887
WHARTON, HUGH M.	Electrical Engineer, Westinghouse Electric and Mfg. Co , Room 102, 800 Broad St., Newark, N. J.	May 15, 1894
WHITE, H. C.	Manager, Phœnix Iron Works Co., 15 Cortlandt St., New York City.	April 15, 1884
WHITE, J. G.	J. G White & Co., Electrical Engineers and Contractors, 29 Broadway, New York City.	April 2, 1889
WHITE, WILL F.	Electrical Engineer, Vice-President, New Omaha T.-H. Electric Light Co., 309 So. 13th St., Omaha, Neb.	Feb. 7, 1890
WHITE-FRASER, GEO.	Care of John Stark & Co., 28 Toronto St., Toronto, Ont.	Sept. 22, 1891
WHITMORE, W. G.	Electrical Engineer, General Electric Co., Edison Building, Box 3067, New York City.	Mar. 18, 1890
WHITNEY, HENRY M. [Life Member.]	81 Milk St., Boston, Mass.	July 12, 1887
WHITTEMORE, CHARLES F.	Superintendent and Electrician, Livgro Lamp Co., Harrison, N. J.	Sept. 20, 1893
WIGHTMAN, MERLE J.	Library Building, Scranton, Pa.	Mar. 5, 1889
WILEY, WALTER S.	Supt. South Omaha Electric Light, Heat and Power Co., South Omaha, Neb.	April 18, 1894
WILEY, WM. H.	Scientific Expert, 53 E. 10th St., New York City.	Feb. 7, 1888
WILLIAMS, ARTHUR S.	[Address unknown.]	Nov. 24, 1891
WILLIAMS, CHARLES JR.	Electrician, 100 Sudbury St., Boston, Mass.	April 15, 1884
WILLIAMSON, G. DEWITT	Dobbs Ferry, N. Y.	April 18, 1893
WINAND, PAUL A. N.	Engineer and Supt., Schleicher, Schumm & Co., 3200 Arch St., Philadelphia, Pa.	June 20, 1894
WINCHESTER, SAMUEL B.	9 Laurel St., Holyoke, Mass.	May 15, 1894

Name.	Address.	Date of Election.
WINSLOW, I. E.	British Thomson-Houston, Ltd., 38 Parliament St., Westminster, London, Eng.	Nov. 12, 1889
WINTRINGHAM, J. P.	Theorist, 36 Pine St, New York City, and 153 Henry St., Brooklyn, N.Y.	May 7, 1889
WIRT, HERBERT C.	Engineer, Supply Department, General Electric Co., Schenectady, N. Y.	June 26, 1891
WOOD, E. J.	Consulting Engineer and Contractor, 243 Broadway, New York City.	July 12, 1887
WOODWARD, FRANCKE L.	Undergraduate in Electrical Engineering, Harvard University, 22 Perkins Hall, Cambridge, Mass.	June 26, 1891
WOOLF, ALBERT E.	Electrician and Inventor, Woolf Electric Disinfecting Co. of N. Y., 66 Broad St.; residence, 864 Lexington Ave., New York City.	Sept. 16, 1890
WORSWICK, A. E.	Electrical Engineer, Mutual Light and Power Co, 26 Commerce St, Montgomery, Ala.	Sept. 20, 1893
WRAY, J. GLEN	Cable Tester, Chicago Telephone Co., 162 Centre St., Chicago, Ill.	Sept 20, 1893
WRIGHT, JOHN D.	[Address unknown.]	Oct. 21, 1890
YARNALL, V. H.	Superintendent of Construction, White-Crosby Co., 29 Broadway, New York City.	May 16, 1893
ZALINSKI, EDMUND L.	Captain of Artillery, U. S. A., (retired), The Century, 7 West 43d St., New York City.	May 17, 1887
ZIMMERMAN, LAURENCE J.	Electrical Engineer and Inventor, 57 Pennsylvania Ave., Brooklyn, N. Y.	Mar. 21, 1893.

Associate Members, - - - 651.

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2

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Barbour, Fred. F., 15 First St.
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Dunn, K. G, care of *Pacific Electrician.*
Gale, Horace B., 40 California St.
Hasson, W. F. C., 310 Pine St.
Hewitt, Wm. R., 9 Brenham Place
Holberton, Geo. C., 15 First St.
Lindner, C. T., 106 Market St.
Lighthipe, Jas A., 15 First St.
Low, George P., 303 California St.
Medina, F. P., 534 Market St.
Meredith, Wynn, 310 Pine St.
Molera, E. J., 606 Clay St.
Noxon, C. P. L., 628 Mission St.
Ridley, A. E. B., 508 California St.

San Francisco.—Continued.

Smith, F. E., 229 Stevenson St.
Sprout, Sidney, 303 California St.

17

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Aspen.—Doolittle, Clarence E.

Canon City.—Slater, H. B.

Denver.

Searing, L., 842 Equitable Building.
Stearns, J. W., Jr., Mountain Elec. Co.

2

Pueblo.—Chubbuck, H. E.

Silverton.—Holt, M. B.

CONNECTICUT.

Ansonia.

Grower, George G.
Wallace, William.

2

Bridgeport.

Bryant, Waldo C.
Waddell, Montgomery
Waldo, Dr. Leonard, 57 Coleman St,

3

Deep River.—Selden, R. L., Jr.

Hartford.

Robb, Prof. Wm. L., Trinity College.
Strong, F. G., Box 959.
Waterhouse, F. G., 302 Asylum St.

3

Middlefield.—Lyman, James

Middletown.—Knowles, E. R.

New Haven.
Daniell, Francis G., Box 394.
Rogers, Edward H.
2

Noroton.—Delafield, A. Floyd

Norwich.—Ely, W. G., Jr., 297 B'way

South Norwalk.—Winchester, A. E.

Waterbury.—Brady, E. D. A.

Windsor.
Corson, William R. C.
Heath, Harry E.
Rodgers, Howard S.
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Hall, John L. 300 Market Street.
2

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Chamberlain, F. H., 2411 P St., N. W.
Dodge, Prof. O. G., Care of Navy Dep't.
Freeman, Dr. Frank L., 931 F St.
Hollerith, H., 1054 31st St.
Jannus, Frankland, 928-30 F. St.
Mauro, Philip, 620 F. St.
Maynard, G. C., 800 H St., N. W.
Millis, Capt. John, Lighthouse Board.
Royce, Fred W., 1410 Pennsylvania Ave.
Spicer, C. W., 1054 31st St.
Stratton, M. G., 213 14th St., N. W.
Sturtevant, Charles L., Atlantic Bldg.
Tapley, W. H., Gov. Printing Office.
Watson, Robert, 931 F. St., N. W.
18

Takoma Park.—Foote, Allen R.

FLORIDA.

Jacksonville.—Hammatt, C. S.

St. Augustine.—Best, A. T.

GEORGIA.

Atlanta.—Schoen, A. M.

Augusta.—Edwards, Jas P.

Columbus.—Boileau, W. E.

Macon.—Nyhan, J. T.

Rome.—Smith, C. H.

Savannah.—Nunn, Richard J., *M. D.*

ILLINOIS.

Alton.—Porter, J. F.

Champaign.
Shea, Danl. W., 403 W. Hill St.
Swenson, B. V., University of Ill.
2

Chicago.
Abbott, Arthur V., 203 Washington St.
Armstrong, C. G., 1400 Auditorium Twr.
Arnold Bion J., 574 The Rookery
Badt, L. Francis B., 1215 Monadnock
Barton, Enos M. 227 South Clinton St.
Bell, Dr., Louis, Monadnock Bldg.
Comstock, L. K., Monadnock Bldg.
Crandall, Chester D., 4438 Ellis Ave.
Croxton, A. L., 7118 Drexel Ave.
Cushing, F. W., 1106 The Rookery.
Cutter, George, 851 The Rookery.
DeLand, Fred., 436 The Rookery.
Dommerque, F. J., 203 Washington St.
Dorr, Frank H., Monadnock Block.
Eddy, H. C., Room 22, 170 La Salle St.
Ellicott, E. B., 227 So. Clinton St.
Etheridge, C. L., 203 Washington St.
Frantzen, Arthur, 84 La Salle St.
Galletly, J. Fred., Care of Swift & Co.
Gutmann, L., Care *St. Railway Rev.*
Hancock, L. M., 227 So. Clinton St.
Haskins, Clark C. 582 W. Congress St.
Hibbard, Angus S. 203 Washington St.
Insull, Samuel, 513 Rookery.
Izard, E. M., 315 Dearborn St.
Kammeyer, C. L., 1439 Monadnock.
Keller, C. L., 203 Washington St.
Kreidler, W. A., 6 Lakeside Building.
Larned, S. J., 203 Washington St.
MacFadden, Carl K., 128 So. Clinton St.
Malia, J. P., 5314 Union Ave.
Mayer, Geo. M., 79 Fifth Ave.
Munns, C. K., 1002 W. Monroe St.
Myers, L. E., 917 Monadnock Bldg.
Neiler, S. G., 1409 Manhattan Bldg.
Norton, E. F., 15 City Hall.
O'Connell, J. J., 203 Washington St.
Pierce, R. H., 1409 Manhattan Bldg.
Pratt, Chas. A., 39th St. & Stewart Ave.
Rhodes, S. A., 203 Washington St.
Richardson, R. E., 1409 Manhattan Bdg
Rodman, Sam'l, Jr., 106 Pullman Bldg.
Sackett, W. M., 203 Washington St.
Sage, Henry J., 4467 Oakenwald Ave.
Scheible, Albert, 486 North Park Ave.
Stine, Prof. W. M., Armour Institute.

Chicago—Continued.

Summers, Leland L., 436 Rookery.
Wait, Henry H., 4919 Madison Ave.
Warner, E. P., 227 So. Clinton St.
Wilson, Charles H., Monadnock Bldg.
Wray, J. G., 162 Centre St.

51

Highland Park.—Gray, Dr. Elisha

Oak Park.

Bentley, M. H., 221 Scoville Ave.
Nutting, S. E., 241 So. Scoville Ave.
Privat, Louis

3

INDIANA.

East Chicago.—Flegel, G. C.

Fort Wayne.

Barnes, Edw. A. Ft., Wayne Electric Co.
Duncan, Thos., 407 Broadway.
Hadley, A. L., 149 Griffith St.
Hunting, F. S., 330 W Washington St.
Serva, A. A., 29 W. Jefferson St.

5

Lafayette.

Boggs, Lemuel S.
Flather, J. J., Purdue University.
Goldsborough, W. E., 164 Columbia St.
MacMullan, R. H.
Smith, H. B., 164 Columbia St.

5

Notre Dame.—O'Dea, M. T.

KANSAS.

Fort Riley.—Reber, Lieut. S.

KENTUCKY.

Louisville.—Hubley, G. W.

LOUISIANA.

New Orleans.

Ayres, Brown, Tulane University.
Carroll, Leigh

2

MAINE.

Lewiston.—Robinson, Almon

MARYLAND.

Baltimore.

Burton, W. C., 1425 Maryland Ave.
Duncan, Dr. L., Johns Hopkins Univ.
Hall, Clayton C., 810 Park Ave,
Hering, H. S., 2122 Mt. Royal Terrace.
Keilholtz, P. O., U. S. E. P. & L. Co.
Knox, F. H., 2116 N. Charles St.
Liebig, G. A., Jr., Johns Hopkins Univ.
McCay, H. K., 106 E. German St.
Morrison, J. Frank, 15 South St.,
Peirce, Wm. H., Keyser Building.
Ries, E. E., 430 So. Broadway.
Rowland, Prof. H. A., Johns Hopkins University.
Smith, F. H., 216 Equitable Bldg.
Varney, W. W., 118 E. Lexington St.
Young, C. G., 706 Equitable Bldg.

15.

Chestertown.—Humphreys, W. J.

MASSACHUSETTS.

Adams.—Magenis, James P.

Auburndale.—Blake, Francis

Boston.

Adams, A. D., 620 Atlantic Ave.
Almon, G. H., 620 Atlantic Ave.
Archbold, Wm. K., 328 Exchange Bldg.
Berthold, V. M., 125 Milk St.
Blodgett, Geo. W., B. & A. R. R.
Bubert, J. F., 620 Atlantic Ave.
Burleigh, Chas B., 620 Atlantic Ave.
Burton, Geo. D., 194 Washington St.
Claflin, Adams D., 116 Bedford St.
Coffin, Chas. A., 620 Atlantic Ave.
Corey, Fred. B., 114 Chandler St.
Craig, J. Hally, 69 Broad St.
Crosby, J. W., 38 Bedford St.
Cross, Prof. Chas. R., Mass. Institute of Technology.
Cumner, A. B., 69 Broad St.
Cushing, H. C., 55 Kilby St.
Davenport, Geo. W., 61 Ames Bldg.
Doolittle, Thos. B., 125 Milk St.
Erickson, F. Wm., 3 Head Place.
Eustis, Herbert H., 61 Hampshire St.
Farnham, I. H., 125 Milk Street.
Ford, Wm. S., Room 73, 125 Milk St.
Hart, Francis R., 1 Court St.
Haskins, Caryl D., 620 Atlantic Ave.
Hayes, Hammond V., 42 Farnsworth St.
Herrick, Charles H., 133 Oliver St.
Hood, Ralph O., 15 Federal St.
Hudson, John E., 125 Milk St.
Lockwood, Thomas D., 125 Milk St.
Morss, Everett, 303 Marlboro St.
Paine, F. B. H., 328 Exchange Bldg.
Paine, Sidney B., 180 Summer St.

Boston.—Continued.

Parcelle, Albert L., 157 Washington St.
Puffer, Wm. L., Mass. Inst. of Tech.
Robb, Russell, 4 P. O. Square.
Sanderson, E. N., 328 Exchange Bldg.
Spaulding, Hollon C., 31 Milk St.
Stone, Charles A., 4 P. O. Square.
Sweet, Henry N., 4 Spruce St.
Taber, Robert B., 620 Atlantic Ave.
Webster, Edwin S., 4 P. O. Square.
Whitney, Henry M., 81 Milk St.
Williams, Chas. Jr., 100 Sudbury St.
43

Brookline.

Bliss, D. M., 1 Davis Ct., Washington St.
Lee, J. C., Mountfort St. Longwood.
Wason, L. C., 199 Harvard St.
3

Cambridge.

Adams, C. A. Jr., 13 Farrar St.
Hall, Prof. Edwin H., Gorham St.
Woodward, F. L., 22 Perkins Hall.
3

Fall River.—Beattie, John, Jr.

Holyoke.

Newell, Arthur J., 366 High St.
Winchester, S. B., 9 Laurel St.
2

Jamaica Plain.—Brophy, William.

Lawrence.—Humphreys, C. J. R.

Lynn.

Ekstrom, Axel, 92 Hamilton Ave.
Everest, A. R., Thomson Elec Welding Co.
Fish, Walter C., Gen. Elec. Co.
Fiske, J. P. B., General Electric Co.
Lemp, H. Jr., Thomson Elec. Welding Co.
Thomson, Elihu, General Electric Co.
6

Marlboro.

Bottomley, Harry.
deKhotinsky, Capt. Achilles.
2

Melrose Highlands.

Mansfield, Geo. W.

Newton Centre.—Stearns, C. K.

Pittsfield.

Chesney, C. C., Stanley Laboratory.
Fridenberg, H. L., Stanley Mfg. Co.
Stanley, William,
Tobey, W. B., Dawes Ave. & High St.
4

Springfield.

Anderson, H. S., United E. L. Co.
Hyde, Jerome W.
Taintor, G., New England T. & T. Co.
3

Stockbridge.—Field, Stephen D.

Worcester.

Kimball, Prof. A. S., Polytechnic Inst.
Webster, Dr. A. G., 936 Main St.
2

MICHIGAN.

Calumet.—Bosson, Fred. N.

Detroit.

Blades, Harry H., 1343-55 Cass Ave.
Dow, Alex, 411 Hammond Bldg.
Rae, Frank B., Home Bank Bldg.
Smith, Jesse M., 36 Moffatt Block.
Wilkes, G., 149 Griswold St.
5

Ishpeming.—Mills, Frank P.

Springport.—Higgins, Eugene

MINNESOTA.

Duluth.—Van Brunt, W.

Minneapolis.

Brooks, Morgan, 249 Second Ave., So.
Gerry, M. H., Jr. 3333 Cedar Ave.
Morse, Geo. H., 910 Wright Block.
Shepardson, G. D., Univ. of Minn.
4

St. Paul.—Byllesby, Henry M.

MISSOURI.

Columbia.—Shrader. Prof. Wm.

Kansas City.

Blood, W. H., Jr. 535 Delaware St.
Casper, Louis, Box 257.
Weeks, E. R., National Bank of Kansas City Building.
3

St. Louis.

Durant, Geo. F., 511 No. 4th St.
Kinney, H. A.
Randall, J. E., 1912 Olive St.
Schlosser, F. G., 1038 Leffingwell Ave.
4

MONTANA.

Great Falls.—Morrow, John T.

NEBRASKA.

Lincoln.—Owens, Prof. R. B.

Omaha.

White, W. F., 309 So. 13th St.
Lane, Vance

2

South Omaha.

Wiley, W. S., So. O. Elec. Light Co.

NEVADA.

Virginia City.—Fielding, Frank E.

NEW HAMPSHIRE.

Hanover.—Crehore, Dr. A. C.

Manchester.

Clough, A. L., Box 114.
Smith, J. Brodie, 142 Merrimack St.

2

Portsmouth.—Flanagan, T. F.

NEW JERSEY.

Bayonne City.

Bennett, E. H., Jr., 17 E. 33d St.
Fleming, Wilfrid H., 70 W. 3d St.
Uebelacker, C. F., 88 Avenue C.

3

Bridgeton.—Smith, Oberlin.

Bound Brook.—Loomis, O. P.

Camden.—Harrington, Walter E.

East Orange.

Bauer, W. F., 62 Steuben St.
Jackson, F. E., 61 So. Grove St.

2

Elizabeth.—Diehl, Philip.

Harrison.

Eyre, M. K.
Howell, Wilson S.
Marshall, J. T.
Moore, D. McF., 321 Sussex St.
Page, A. D.
Whittemore, C. F.

6

Hoboken

Cuntz, Johannes H., 325 Hudson St.
Denton, Prof. J. E., Stevens Institute.
Geyer, Dr. Wm. E., " "
Morton, Dr. H., " "
Verley, Horace S. L., " "

5

Jersey City,—Ockershausen, H. A.

Lakewood.—Stockly, Geo. W.

Maywood.—Jaeger, Chas L.

Montclair.

Broadnax, F., 47 Christopher St.
Harrison, Harold

2

Newark.

Anthony, Watson G., 32½ Webster St.
Atwood, Geo. F., Mechanic and Lawrence Sts.
Bosch, Adam, Fire Alarm Telegraph.
Colby, Edward A., Lock Box 313.
Criggal, J. E., 138 Bloomfield Ave.
Doane, S. Everett, 68 Park Place.
Donner, W. H., 241 6th Ave.
Gerry, E. M., 114 William St.
Gesseaume, Chas., 29 Plane St.
Heinrich, Richard O., 114 William St.
Howell, J. W., 761 High St.
Martin, J., 16 Oak St.
Osborne, L. A., 523 Prudential Bldg.
Saxelby, Frederick, 288 Summer Ave.
Weston, Edward, 114 William St.
Wharton, H. M., 800 Broad St.

16

Orange.

Edison, Thomas A.
Upton, F. R. 107 Day St.

2

Passaic.

Alden, James S., 486 River Drive.
Mott, S. D.
Varley, T. W., Okonite Co.

3

Plainfield.

Kinsman, Frank E.
McClurg, W. A., 207 Madison Ave.
Miner, Willard M., 339 East Second St.

3

Princeton.—Brackett, Prof. C. F.

Roselle.—Lawton, W. C.

Rutherford.

Buys, Albert
Petty, Walter M.

2

Trenton.

Roebling, Ferdinand W.
Elmer, Wm., Jr., 25 Forst Richey Bldg.

2

South Orange.—Delany, P. B.

Westfield.—Henshaw, F. V.

West Hoboken.—Reckenzaun, F.

NEW MEXICO.

Clayton.—Frye, H. W.

NEW YORK.

Albany.

McElroy, James F., 131 Lake Ave.,
Miller, Wm. C., 3 South Hawk St.,
Van Buren, Gurdon C., 84 Clinton Ave,
3

Auburn.—Case, Willard E., 6 Fort St.

Bedford.—Luquer, T. T. P.

Belmont.—Gorton, Charles.

Brooklyn.

Barstow, W. S., 360 Pearl St.
Berresford, A. W., 197 Van Buren St.
Bliss, Wm. L., 45 York St.
Broich, Jos., 448 8th Ave.,
Burnett, Douglass, 42 Livingston St.,
Caldwell, Fordyce S., 151 Henry St.,
Chinnock, C. E., 137 Sixth Ave.,
Dey, Harry E., 342 Tenth St.
Etheridge, E. L., 66 No. Oxford St.
Gerry, J. H., 163 Grand Ave.
Halsey, W. B., 246 Elton St.
Hochhausen, Wm., 76 Hanson Place.
Kirkegaard, J. Georg, 329 Union St.
Leslie, E. A., 343 Hancock St.
Mason, James H., 10 Fifth Ave.
McBride, James, 146 Kent St.,
McCarthy, L. A., 1053 Bedford Ave,
Mosscrop, Wm A., 29 Clifton Place.
Parker, Herschel C., 21 Fort Green Pl.,
Paul, Chas. M., 172 Remsen St.,
Peck, E. F., 14 Rockwell Pl.
Potts, Chas. E., 1356 Dean St.
Prince, J. Floyd, 868 Flatbush Ave.
Proctor, T. L., 45 York St.
Reilly, John C., 16 Smith St.,
Riker, A. L., 45 York St.
Robinson, F. G., 156 Macon St.
Roller, Lieut. J. E., 515 Clinton Ave.
Sargent, W. D., 16 Smith St.,
Searles, A. L., 291 Seventh Ave.
See, A. B., 116 Front St.
Sheldon, Dr. Samuel, 170 State St.,
Sinclair, H. A, 950 Bedford Ave.,
Sykes, Henry H., 75 Hicks St.
Treadwell, Augustus, Jr., 488 3d St.
Zimmerman, L. J., 57 Penna. Ave.
36

Buffalo.

Perkins, Frank C., 774 Prospect Ave.
Plumb, Chas., 89 Erie St.
Ricker, C. W., 109 White Bldg.
3

Cortland.—Dunston, R. E.

Dobbs Ferry.

Williamson, G. DeWitt.

Elmira.

Wolverton, B. C., N. Y. & Pa. T. & T. Co.,
Cheney, F. A., Elmira Ill'g Co.
2

Garden City.—Archer, Geo. F.

Greenbush.—Pratt Robert J.

Herkimer.—Lyman, C. W.

Ithaca.

Bedell, Fred. Dr., 117 E. Buffalo St.
Bergholtz, H., Ithaca Street Ry. Co.
Glading, F. W., 1 DeWitt Ave.
Lanphear, B. S, 106 Union Ave.
Macfarlane, Prof. Alex., 84 Heustis St.
Matthews, Chas. P., 211 E. State St.
Merritt, Prof. Ernest, Cornell University.
Nichols, Dr. E. L., Cornell University.
Ryan, Prof. H. J., Cornell University
Sharp, Clayton H., 122 Univ. Ave.
10

Jamaica.

Fleming, R.
Weller, H. W.
2

Jamestown.—Gifford, C. E.

Kingston.—Desmond, Jere. A.

Long Island City.—Shepard, W. E.

Mamaroneck.—Farnsworth, A. J.

New York City.

Agnew, C. R., 23 W. 39th St.
Alexander, Harry, 126 Liberty St.
Alexander, P. H., 45 Broadway.
Anthony, W. A., 5 Beekman St.
Ashley, F. M., 136 Liberty St.
Auerbacher, L. J., 136 Liberty St.
Ayer, Jas. I., 73 Watt St.
Barberie, E. T., 159 W. 66th St.
Baillard, E. V., 106 Liberty St.
Bartlett, Edw. E., 23 Rose St.
Barrett, John A., 126 Liberty St.
Batchelor, C., 33 West Twenty-fifth St.
Bates, J. H., 126 Liberty St.
Baylis, R. N., 81 Fulton St.
Benjamin, Park, 203 Broadway.
Bennett, J. C., 44 Broad St.
Bethell, U. N., 18 Cortlandt St.
Bijur, Jos., 41 W. 53d St.
Binney, Harold, 38 Park Row.
Birdsall, E. T., 18 Broadway.
Bishop, James D, 234 W. 29th St.
Blake, Henry W., 26 Cortlandt St.
Blizard, Chas., 66 Broadway.
Bogart, A. Livingston, 22 Union Sq.

New York City.—Continued.

Bogue, Chas. J., 206 Centre St.
Bohm, Ludwig K., 117 Nassau St.
Boughan, Edward L, 153 Cedar St.
Bourne, Frank, 26 Cortlandt St.
Bradley, C. S., 1919 7th Ave.
Brenner, W. H., 39 Cortlandt St.
Brixey, W. R., 203 Broadway.
Brown, Alex. S., 397 St. Nicholas Ave.
Brown, Alfred S, 195 Broadway.
Brown, E. D., 18 Cortlandt St.
Brown, J. Stanford, 126 Liberty St.
Buck, H. W., 14 E. 45th St.
Buckingham, Chas. L., 195 Broadway.
Bunce, Theo. D., Jr., 239 E. 27th St.
Burke, Jas., 203 Broadway.
Caldwell, Edw., 1320 Havemeyer Bldg.
Callender, Romaine, Decker Bldg.
Carichoff, E. R., 253 Broadway.
Carson, David I., 18 Cortlandt St.
Carty, J. J., 18 Cortlandt St.
Chamberlain, J. C., 135 E. 18th St.
Chandler, Prof. C. F., Columbia Coll.
Chase, H. S., 136 Liberty St.
Clark, Ernest P., 192 Broadway.
Clark, Le Roy, Jr., 229 W. 28th St.
Clarke, Charles L., 55 Liberty St.
Coho H. B., 203 Broadway.
Colgate, Geo. L., 136 Liberty St.
Colvin, F. R, 203 Broadway.
Compton, A. G., 17 Lexington Ave.
Cox, E. V., 50 E. 31st St.
Craigin, Henry A., 120 Broadway.
Crane, W. F D. 87 Maiden Lane.
Crocker, Francis B., 26 W. 22nd St.
Crosby, O. T., 29 Broadway.
Curtis, C. G., Box 412.
Cushman, Holbrook, 337 West 22d St.
Cuttriss, Chas., 1 Broad St.
Dana, R. K., 16 Cliff St
Davenport, C. G., 44 Broad St.
Davidson, E. C., 179 Times Bldg.
Davis, Charles H., 120 Broadway.
Davis Joseph P., 113 W. 38th St.
Davis, Minor M, 253 Broadway.
Decker, D. H., 5 Beekman St.
Denison, S P., 143 Centre St.
Dickerson, E. N., 253 Broadway.
d'Infreville, Georges, 10 Desbrosses St
Doremus, Dr. C. A. 59 W. 51st St.
Dressler, Chas. E., 17 Lexington Ave.
Dunbar, F. W., 425 W. 22d St.
Dunn, Gano S., 223 Central Park, West.
Durant, Edward, 39 Cortlandt St.
Dyer, F. M., 10 W. 23d St
Dyer, R. N., 36 Wall St.
Eidlitz, C. L., 10 W. 23d St.
Emery, Dr. Charles E., 95 Nassau St.
Emmet, H. L. R., 36 Cortlandt St.
Fay, Thomas J., 143 Liberty St.
Field, C J., 26 Cortlandt St.
Fiske, Henry G., 45 E. 22nd St.
Flack, J. Day, 252 W. 85th St.
Flint, B. P., 120 Broadway.

New York City.—Continued.

Foote, Thos. H., 215 W. 125th St.
Forbes, Francis, 32 Nassau St.
Foster, Horatio A., 35 Wall St.
Freedman, Wm. H., 120 W. 125th St.
Frey, C. P., 5 and 7 Dey St.
Fuller, F. G., 203 Broadway.
Gallaher, E. B., 253 Broadway.
Gardanier, George W., 195 Broadway.
Giles, W. A., 29 Cortlandt St.
Gladstone, J. W., 110 E. 23d St.
Goldmark, Chas J., 49 Liberty St.
Gordon, Reginald, 339 Lexington Ave.
Gossler, P. G., 108 Fulton St.
Graham, Geo. W., 203 Broadway.
Guy, Geo. H., 534 Temple Court.
Hadaway, W. S., Jr., 26 Cortlandt St.
Hadley, W. B., 30 Cortlandt St.
Hall, Edward J., 18 Cortlandt St.
Hall, William P, 80 Broadway.
Hamblet, James, 195 Broadway.
Hamilton, Geo. A., 22 Thames St.
Hammer, W. J., 1305 Havemeyer Bldg.
Harding, H. McL., 253 Broadway.
Haskins, C. H., 66th St. & Boulevard.
Hatzel, J. C., 114 Fifth Ave.
Haviland, F. L., 192 Broadway.
Hayes, Harry E., 153 Cedar St.
Hedenberg, W. L., 108 Fulton St.
Herzog, Dr. F. Benedict, 30 Broad St.
Higgins, Edward E., 26 Cortlandt St.
Hill, George, 44 Broadway.
Hix, E. R, 41 Wall St.
Holmes, Franklin S., 108 Fulton St.
Howson, Hubert, 38 Park Row.
Hubbard, Wm. C., 73 Watt St.
Hutchinson, Dr. Cary T., 253 B'way.
Idell, Frank E., 616 Havemeyer Bldg.
Jenks, W. J., 44 Broad St.
Jones, Francis W., 253 Broadway.
Johnston, W. J., 253 Broadway.
Keefer E. S., 22 Thames St.
Knox, James M., 32 W. 129th St.
Knudson, A A., 106 Fulton St.
Lanman, Wm. H., 44 Broad St.
Ledoux, A. R., 9 Cliff St.
Leonard, H. Ward, 44 Broad St.
Lester, W. B., 195 Broadway.
Levy, Arthur B., 810 Lexington Ave.
Lieb, J. W., Jr., 166 W. 97th St.
Lloyd, Robert McA., 1 W. 39th St.
Lozier, R. T. E., 465 Greenwich St.
Lowenherz, H, 18 Cortlandt St.
Lufkin, Harvey L., 39 Cortlandt St.
Lundell, Robert, 44 Broad St.
Mackie, C. P., 45 Broadway.
MacQuesten, W. D., 15 Cortlandt St.
Madden, O. E., 41 Wall St.
Mailloux, C. O., 45 William St.
Mansfield, A. N., 153 Cedar St.
Marks, L. B., 51 E. 67 St.
Martin, A. J., 121 Liberty St.
Martin, F., Madison Square Garden.
Martin, T. Commerford, 203 Broadway.

New York City.—Continued.

Maver, William, Jr., 31 Nassau St.
Mayer, M. M., 411 107th St., E. R.
Mayrhofer, J. C., 165 W. 82d St
McKibbin, George N., 80 Broadway.
Merrill. E. A., 42 Cortlandt St.
Metcalfe, George R., 343 9th Ave.
Meyer, Julius, 44 Broad St.
Mitchell, John Murray, 35 Wall St.
Monell, Joseph T., 236 W. 22d St.
Moses, Dr. Otto A., 1037 Fifth Ave.
Moses, P. R., 46 W. 97th St.
Mustin, H. S., 143 Liberty St.
Noll, Augustus, 8 E. 17th St.
Olan, Theo J. W., 69 W. 49th St.
Osterberg, Max, 232 E. 62d St.
Parmly, C. Howard, 344 W. 29th St.
Parsell, H. V., Jr., 31 E. 21st St.
Pattison, Frank A., 136 Liberty St.
Pearson, F. S., 621 Broadway.
Pedersen, F. M., 327 W. 34th St.
Perry, N. W., 6 Park Place.
Pfund, Rich'd, Room 70, 195 Broadway.
Phelps, Geo. M. 203 Broadway.
Pickernell, F. A., 153 Cedar St.
Poole, Cecil P., 253 Broadway.
Pope, Franklin L., 39 Cortlandt St.
Pope, Ralph W., 26 Cortlandt St.
Price, C. W., 13 Park Row.
Pupin, Dr. Michael I., 46 W. 72d St.
Read, Robert H., 39 Cortlandt St.
Reed, H. A., 420 E. 25th St.
Reed, H. D., 420 E. 25th St.
Rennard, J. C., 18 Cortlandt St.
Rittenhouse, C. T., 247 W. 138th St.
Roberson, O. R., 195 Broadway.
Rouquette, W. F. B., 47 Dey St.
Rosenbaum, Wm. A., Times Building.
Rosenberg, E. M., 784 Lexington Ave.
Sachs, Joseph, 32 Nassau St
Scheffler, F. A., 126 Liberty St.
Schreiter, Heinr, 11 Chambers St.
Seely, J. A., 121 Liberty St.
Serrell, Lemuel Wm., 253 Broadway.
Sever, Geo. F., 34 W. 32d St.
Shain, Charles D., 136 Liberty St.
Sheehy, Robert J., 570 Park Ave.
Slade, A. J., 62 E. 66th St.
Smith, J Elliot, 122 W. 73rd St.
Smith, T. Jarrard, 7 Dey St.
Sprague, Frank J., 253 Broadway.
Stieringer, Luther, 68 Broad St.
Stockbridge. Geo. H., 39 Cortlandt St.
Stump, C. E., 26 Cortlandt St.
Taltavall, Thos R., World Building.
Terry, Chas. A., 120 Broadway.
Tesla, Nikola, 35 So. 5th Ave.
Thompson, Edward P., 5 Beekman St.
Toerring, C. J., Jr., 73 Watt St.
Townsend, Henry C., 5 Beekman St.
Turner, Wm. S., 47 Times Building.
Tuttle, George W., 510 W. 23rd St.
Vail, J. H., 39 Cortlandt St.
Vail, Theo. N., 18 Cortlandt St.

New York City.—Continued.

Vansize, William B., 253 Broadway.
Van Wyck, P. V. R. Jr., 981 Madison Av.
Varley, Richard, Jr., 62 Cortlandt St.
Wacker, Geo. G., 1340 Vanderbilt Ave.
Wardell, Geo. P., 103 E. 10th St.
Warner, Chas. H., 50 Broadway.
Waterman, F. N., 120 Broadway.
Weaver, W. D., 253 Broadway.
Webb, Herbert Laws, 18 Cortlandt St.
Wetzler, Joseph, 203 Broadway.
Wheeler, S. S., 39 Cortlandt St.
White, H. C., 15 Cortlandt St.
White, J. G., 29 Broadway.
Whitmore, W. G., 44 Broad St.
Wiley, Wm. H., 53 East 10th St.
Wilson, Fremont, 108 Fulton St.
Wintringham, J. P., 36 Pine St
Wolcott, Townsend, 1002 Bennett Bldg.
Wood, E. J., 243 Broadway.
Woodbridge J. L., 47 Times Building.
Woolf, Albert E., 66 Broad St.
Yarnall, V. H., 29 Broadway.
Zalinski, Capt. E. L., 7 W. 43d St.

236

Official Stenographer.

Ryan, R. W., 262 W. 11th St.

Oswego.—Judson, Wm. Pierson.

Ovid.—Waring, John

Pelham Manor.—Gilliland, E. T.

Rhinecliff.—Philbrick, B. W.

Rochester.

de Lancey, D.
Redman, G. A.

2

Schenectady.

Andrews, W. S., General Electric Co.
Berg, E. J., 53 Washington Ave.
Blood, J. B., General Electric Co.
Cahoon. J. B., 226 Union St.
Churchill, Arthur, 5 So. Church St.
Curtiss, Geo. F., General Electric Co.
Greene, S. Dana, " " "
Emmet, W. L. R., " " "
Griffin, E , " " "
Hewlett, Edw. M., " " "
Hobart, H. M., " " "
Kellogg, J. W., " " "
Kirkland, J. W., " " "
Lovejoy, J. R. " " "
Oudin, M., " " "
Reid, Thorburn, " " "
Reist, H. G., 5 So. Church St.
Rice E. Wilbur, Jr., General Electric Co.
Rohrer, Albert L., " " "
Roper, D. W., " " "
Stebbins, Theodore " " "
Steinmetz, C. P., " " "

Schenectady.—Continued.

Wiener, A. E., 208 Liberty St.
Wirt, H. C., 842 Union St.

124

Syracuse.

Brady, Paul T.
Land, Frank, care of Globe Hotel.
Marvin, H. N., 208 So. Geddes St.
Wardlaw, Geo. A., 412 E. Willow St.

4

Troy.

Bernard, Edgar G., 43 4th St.
Chadbourne, H. R., Jr., Troy City R'y
Parks, C. Wellman, 1825 Fifth Ave.
Stevens, W. Le Conte, Rensselaer Poly. Inst.

4

Utica.

Hunt, A. L., State Hospital.
Nicholson, W. W., 73 Howard Ave.

2

Waterloo.—Mercer, A. G.

Yonkers.

Essick, Samuel V.
Ihlder, John D.
Slater, F. R.

3

NORTH CAROLINA.

Wilmington.—Barnard, J. H.

OHIO.

Akron.—Shaw, E. C.

Cincinnati.

Cabot, John A., 123 West 8th St.
Cothren, Wm. H., 164 W. 8th St.
Creaghead, Thos. J., 296 Plum St
French, Prof. Thos., Jr., Avondale.
Gray, W. N., 200 Neave Building.
Roberts, W. H., 15 Harrison St.

6

Cleveland.

Black, Chas. N., Belden St.
Brush, Chas F., 453 The Arcade
Canfield, Milton C., 18 Clinton St.
Cowles, Alfred H., 361 Arcade.
McKinstry, J. P., 316 Seneca St.
Roberts, E. P., Brainard Block.
Sperry, E. A., Mason and Belden Sts.
Wason, Chas. W., 1762 Euclid Ave.

8

Columbus.

Caldwell, F. C., State University.
Childs, S. W., Central Railway Co.
Thomas, Prof. B. F., State University.

3

Hamilton.—Cornell, Chas. L.

Marietta.—Shedd, J. C.

Salem.—Davis, Delamore L.

Steubenville.—Flood, J. F.

Wyoming.—Lilley, L. G.

OREGON.

Portland.

Cheney, W. C., Portland General Electric Co.
Coolidge, C. A., 591 Hood St.
Dame, F. L., The Northwest General Electric Co.
Malcolm, P. S., 131 Sixth St.
Mitchell, Sidney Z., Fleischner B'ld'g.

5

Salem.—Anson, F. R.

PENNSYLVANIA.

Allegheny City.

Fessenden. Prof. R. A.
Vandegrift, James A., Robinson St.

2

Altoona.—Dudley, C. B.

Catasauqua.—Tait, F. M.

Chester.—Arnold, C. R.

Erie.—Boynton, E. C.

Germantown.

Condict, G. Herbert, 5328 Green St.
Wirt, Charles.

2

Haverford.—Griscom, Wm. W.

Monongahela City.

Acheson, E. G.

Nanticoke.—Sanborn, Francis N.

Norristown.—Watts, H. F.

Philadelphia.

Albright, H. F., 5304 Lancaster Ave.
Billberg, C. O. C., 3200 Arch St.
Blanchard, C. M., 1209 Betz Bldg.
Braddell, Alfred E., 316 Walnut St.
Bragg, Chas. A., 302 Girard Bldg.
Childs, A. E., 302 Girard Bldg.
Darlington, F. W., 503 Girard Bldg.

Philadelphia.—Continued.

Drysdale, W. A., Hale Bldg.
Eglin, W. C. L., 909 Walnut St.
Flagg, S.G., Jr., 19th St. and Penn. Ave.
Gerson, L. J., 809 Sansom St.
Hering, Carl, 927 Chestnut St.
Hewitt, Chas., 920 Spruce St.
Houston, Prof. E. J., 1105 Betz Bldg.
Hunter, Rudolph M., 926 Walnut St.
Ives, Lieut. E. B., 421 Chestnut St.
Johnston, A. L., 4300 Lancaster Ave.
Keller, E. R., 1210 Betz Bldg.
Kennelly, A. E., 1106 Betz Bldg.
La Roche, F. A., 2235 No. 16th St.
Levis, Minford, 54 North 4th St.
Lloyd, Herbert, Drexel Bldg.
Marks, Prof. W. D., Edison Elec. Lt. Co.
Perot, L. Knowles, 308 Walnut St.
Pike, Clayton W., 1210 Betz Bldg.
Reed, C. J., 609 Norris St.
Rowland, Prof. A. J., Drexel Inst.
Sheble, F., 1022 Arch St.
Shields, W. J., 829 Arch St.
Smith, T. Carpenter, 212 Drexel Bldg.
Spencer, Theo., 406 Market St.
Stevens, Prof J. F., 1419 Walnut St.
Uhlenhaut, F., Jr., 4101 Haverford St.
Wendle, Geo. E., 2433 N. 7th St.
Willyoung, Elmer G., 1210 Betz Bldg.
Winand, P. A. N., 3200 Arch St.
Wright, Peter, Drexel Bldg.,

37

Pittsburg.

Bliss, Geo. S., Telephone Bldg.
Eden, M. E., 83 Fourth Ave.
Fisher, H. W., Standard Und. Cable.
Floy, Henry, Westinghouse E. & M. Co.
Healy, L. W., Westinghouse E. & M. Co
Hutchinson, F. L. " " "
Ingold, Eugene, 1669 Second Ave.
Keller, E. E., 222 Murtland Ave.
Lange, Philip A., West'house E. & M. Co.
McCrosky, J W., " " "
Myers, Geo. Francis, Penn Building.
Phillips, Leo A. West'house E. & M. Co.
Requier, A. M., " " "
Schmid. A., Westinghouse E. & M. Co.
Scott, Chas. F., " " "
Smith, F. S., " " "
Stillwell L. B., " " "
Strauss, Herman A. " " "
Temple, William Chase, P. O. Box 800.
Vanderslice, G. H., P. O. Box 911.
Waring, R. S., 61 Westinghouse Bldg.
Waters, Edward G., 425 Wood St.
Wurts, A. J., Westinghouse E & M. Co.

23

Rochester.—Shallenberger, O. B.

Scranton.—Wightman, Merle J.

South Bethlehem.—Powell, W. H.

State College.

Jackson, Prof. J. P.
Lardner, H. A,

2

West Bethlehem.—Lattig, J. W.

Wilkesbarre.—Seitzinger, H. M.

Wilkinsburg.—Frost F. R.

RHODE ISLAND.

Bristol.—Blake, Theo. W.

Providence.

Lenz, Chas. O., 510 Ind. Trust Bldg.
Miller, Joseph A., 25 Butler Exchange.
Phillips, Eugene F., 67 Stewart St.
Trott, A. H. Hardy, 161 Friendship St.

4

SOUTH CAROLINA.

Columbia.—Johnson, J. N.

TENNESSEE.

Memphis.—Roessler, Capt. S. W.

TEXAS.

Austin.—McRae, Prof. A. L.

Dallas.—Mottram, W. T. M.

UTAH.

Salt Lake City.—McKay, C. R.,

VERMONT.

Brattleboro.

Childs, W. H.
Fuller, Hon. Levi K.

2

Burlington.—Storrs, Prof. H. A.

Rutland.—Francisco, M. J.

VIRGINIA.

Blacksburg.—Randolph, L. S.

Fortress Monroe.
Parkhurst, Lieut. C. D.
Squier, Lieut. Geo. O.
2

Front Royal.—Albert, Henry

Norfolk.—Crews, J. W.

Richmond
Leonard, M.B., Sup't C. & O. R.R. Tel.
McCluer, C. E., So. Bell T. & T. Co.
Tower, Geo. A., 109 S. First St.
Trafford, E. W., 104 No. 7th St.
4

WASHINGTON.

Everett.
Butler, William C.
Ray, Wm. D.
2

WEST VIRGINIA.

Huntington.—Wallace, G. S. Box 214

Morgantown.—Aldrich, William S.

Rome.—Babcock, C. D.

Wellsburg.—Brady, F. W.

Wheeling.—Sands, H. S.

WISCONSIN.

Eau Claire.—Bates, M. E.

Madison.
Davies, Prof. John E., 523 Carroll St.
Jackson, Prof Dugald C.
Jones, Prof. F. R., University of Wis.
3

Milwaukee.—Boardman, H. B.

DOMINION OF CANADA.

NOVA SCOTIA.

Baddeck.—Bell, Prof. A Graham

Halifax.
Cogswell, A. R., 34 Bishop St,
Spike, Clarence J.
2

Hazel Hill.
Colley, Benjamin W.
Dickenson, Samuel S.
2

New Glasgow.—Bowman, Fred. A.

ONTARIO.

Berlin.—Breithaupt, E. Carl

Ottawa.
Ahearn, T.
Dion, Alfred A., 72 Sparks St.
2

Peterborough.
Hartman, Herbert T.
Ross. Norman
Stephens, Geo.
3

Rat Portage.—McCrossan, J. A.

St. Catharines.
Thompson, William G. M.

Toronto.
Langton, John, Canada Life Building.
Rosebrugh, Thomas R.
Rutherford, W. M., 63 Front St
White-Fraser, Geo., 28 Toronto St.
4

QUEBEC.

Montreal.
Carus-Wilson, Prof. C. A.
Ross, Robert A., 94 Queen St.
Sise, Charles F., P. O. Box 1918,
3

MEXICO.

City of Mexico.—Peck, S. C.

CENTRAL AMERICA.

GUATEMALA.

Quezaltenango.—Morehouse, H. H.

SOUTH AMERICA.

UNITED STATES of BRAZIL.

Para.
da Cunha, Manoel Ignacio,
Empresa Industrial Gram-Para.
Chermont, A. L., Box 252.
2

WEST INDIES.

JAMAICA.

Kingston.—Wilson, Harry C.

AFRICA.

CAPE OF GOOD HOPE.

Cape Town.—Standford, William.

ASIA.

JAPAN.

Osaka.—Iwadare, Kunihiko.

AUSTRALIA.

NEW SOUTH WALES.

Sydney.

Fischer, Gustave J., Public Works Dep't.
Fitzmaurice, Jas. S., 210 George St.
Jones, A. W., care of H. H. Kingsbury.
Spruson, Wilford J., 169 King St.

4

EUROPE.

AUSTRIA.

Vienna.

Egger, E., X. Simmeringstr, 187
Sahulka, Dr Johann. Technische Hochschule.

2

GREAT BRITAIN.

ENGLAND.

Bolton-le-Moors.—Jackson, Henry

Bristol.

Dobbie, R. S., 43 Wellington Park Clifton.
Eley, Harris H., 88 Colston St.

2

Chester.—Keith, N. S.

Croydon.—Lewis, H. F. W.

Kingston-on-Thames.

Richardson, A. E.

London.

Forbes Geo., 34 Great George St.
Lawson, A. J., 49 Queen Victoria St.

London.—Continued.

Lorrain, James Grieve, Norfolk House, Norfolk St., W. C.
Mordey, Wm. M., 34 Montserrat Road, Putney.
Parshall, H. F., 38 Parliament St.
Preece, Wm. H., General Post Office.
Solomons, *Sir* D. L., 49 Grosvenor St.
Walter, Henry E. 3 Princes Mansions, Victoria St.
Wells, D., Hurstfield, The Avenue, Gipsy Hill
Winslow, I. E., 38 Parliament St.
Wharton, Chas. J., 82 Bond St.

11

Taunton.—Haynes, F. T. J.

SCOTLAND.

Glasgow.—Kelvin, *Lord.*

WALES.

Cardiff.—Walker, Sydney F.

BELGIUM.

Antwerp.—Inrig, Alec G.

FRANCE.

Creusot.—Stahl, Th.

Paris.

Albanese, G. Sacco.
Mann, F. P., 17 Rue Didot.
Mix, Edgar W., 27 Rue de Loudres.
Thurnauer, Ernst, 27 Rue de Loudres.
Welles, F. R., 46 Ave. de Breteuil.

5

GERMANY.

Berlin.

Bates, F. C., Union Elektricitats Gesellschaft.
Flesch, Chas., Allgemeine Elektricitats-Gesellschaft.
Magee, Louis J., Corneliusstr 1., W.
Otten, Dr. J. D., Kleinbeerenstrasse, 21, S. W.
Potter, H. N., Steglitzer Str. 10 Parterre
West, Julius H., Handjery Str., 58, Friedenau near Berlin.

6

Hagen.—Entz, J. B.

Munchen.—Voit, Dr. Ernst

Nuremberg.—Tischendoerfer, F. W.

ITALY.

Ivrea.—Olivetti, Camillo.

Torino.—Capuccio, M.

NETHERLANDS.

Amsterdam.
Hubrecht, Dr. H. F. R.

Delft.—Doijer, H.

RUSSIA.

St. Petersburg.
Sventorzetzky, Capt. L., Military Engineering Academy.

Addresses Unknown.

Barth Bartoshevitch
Cleveland, W. B.
Cobb, John S.
Danvers, A.
Handley, Arthur
Holcomb. E. R.
Moore, John J.
Raymond, Chas. W.
Reinmann, A. L.
Souza, Carlos Monteiro
Stadelman, Wm. A.
Vance, A. S.
Williams, A. S.
Wright, John D.

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Notice.

The Secretary would be pleased to receive the present addresses of the above.

Total 924.