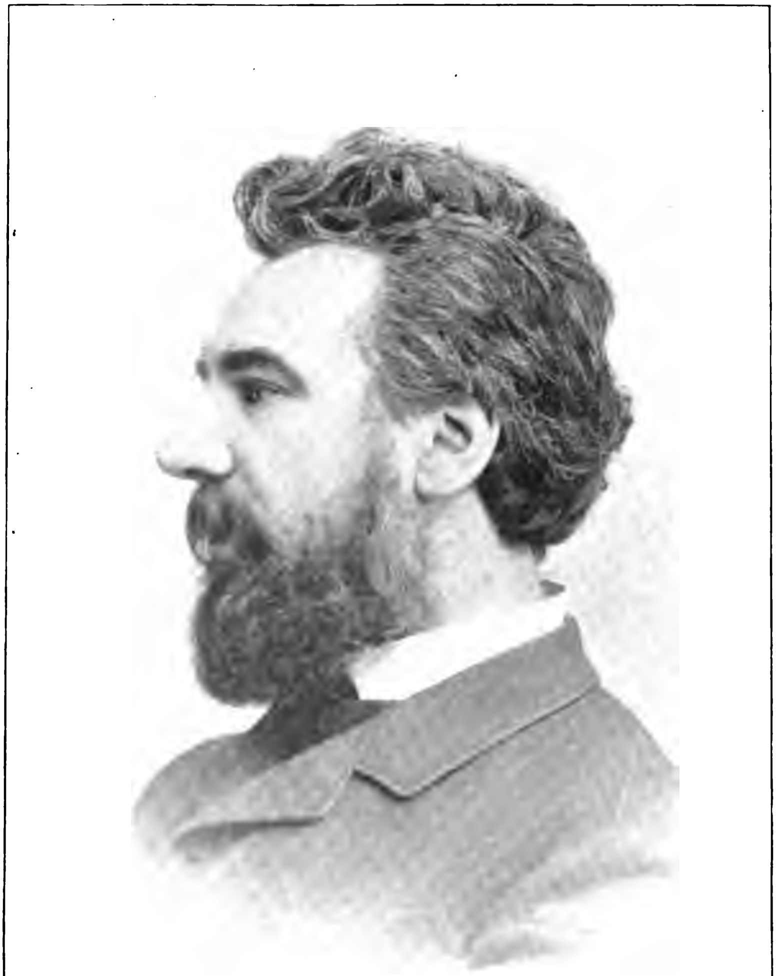

Alexander Graham Bell

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OF

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REPORT OF THE SUB-COMMITTEE ON PROVISIONAL PROGRAMME FOR THE INTERNATIONAL ELECTRICAL CONGRESS OF 1893.

To the General Congress Committee of the American Institute of Electrical Engineers:

GENTLEMEN:—The Sub-Committee on Provisional Programme respectfully submits to you the following report:

DUTIES.

The Sub-Committee understands its duties to be as follows:

To suggest what work it is desirable to have done at the coming International Electrical Congress to be held in Chicago in 1893.

To suggest a programme for carrying out this work in the most satisfactory manner to all parties, and with the least loss of time.

RECOMMENDATIONS.

Regarding the work to be done, your Committee's recommendations are as follows:

RATIFICATIONS.

(1.) *Ratification of the adoption of units, terms, symbols and definitions by previous International Electrical Congresses.*

This is a mere matter of form and an act of courtesy to the preceding Congresses. Your Committee understands that this was done at both the Paris Congress of 1889 and the Frankfort Congress of 1891. (For the work done at previous Congresses see Appendix I.)

NEW UNITS.

(2.) *Defining and adopting practical units for measuring and designating the measurements of the following quantities: Magneto-motive force; magnetic flux; magnetic intensity; magnetic reluctance; electric conductivity; illumination.*

It will be noticed that this refers only to the question of what the magnitude of the units shall be, or, in other words, what multiples or sub-multiples of the absolute units they are to be.

Your Committee is aware that objections are raised by some, to the establishment of units not already in universal use. Past experience, however, has shown that the proper time to establish units is *before* their need is universally felt, in order to avoid the introduction by different persons or nations, of units of different values, a contingency which will be very likely to arise if not anticipated by concerted action.

Your Committee recommends the following:

The value of the practical unit of magneto-motive force to be one-tenth of the absolute unit, that is, equal to $\frac{1}{4\pi}$ ampere turn.

The value of the practical unit of magnetic flux to be 10^8 absolute units or lines.

The value of the practical unit of magnetic intensity to be 10^8 absolute units, that is, 10^8 lines per square centimetre.

The value of the practical unit of reluctance to be 10^{-9} absolute units. (For further explanation of these magnetic units see Appendix II.)

The value of the practical unit of electrical conductivity to be 10^{-9} absolute units, that is, to the reciprocal of the ohm. This makes it equal to the unit proposed some time ago and known to some extent by the name of "mho." It should be given this value in order that it correspond with the already adopted units.

The value of the practical unit of illumination to be a bougie-decimale at the distance of one metre. The bougie-decimale is the unit of light or candle power already established and is practically equal to one English standard candle; by making the distance a metre, the practical unit will be approximately equal to one-tenth of a carcel-metre, one-tenth of a foot-candle (or "lux") or to one metre-candle or metre-kerze, all three of which units are already in use to some extent.

It has been announced that a proposition will be made at this Congress to change the values of some of the practical units which have been adopted by previous Congresses and are already in universal use. Among these are the ampere and the farad. It is urgently recommended by your Committee that such changes should not be favored, since they would necessarily be followed by great confusion and would of necessity have to be accompanied by some change in these well established names in order to distinguish these new units from those now existing.

NAMES FOR UNITS.

(3.) *Adopting names for the following practical units: Magneto-motive force; magnetic flux; magnetic intensity; magnetic reluctance; inductance; electrical conductivity; illumination.*

The following names are suggested for these units:

For the practical unit of magneto-motive force, the name "gilbert."

For the practical unit for magnetic flux, the name "weber." This term was formerly applied to a unit of current, but its use in this sense was so limited, and it has been abandoned for so long a time that no confusion would be likely to arise; the context alone would always be sufficient to prevent any possibility of misunderstanding. The name "weber" is preferred on account of the intimate relation between this unit and that of magnetic intensity for which the name "gauss" is suggested.

For the practical unit of magnetic intensity, the name "gauss." This name has already come into use to such an extent that objections to it will not be likely to arise.

For the practical unit of magnetic reluctance, the name "oersted."

For the practical unit of inductance, the name "henry." This name has already come into use quite extensively and it would therefore be very undesirable to change it. The value of this unit has already been fixed as equal to 10^9 absolute units or nearly to the length of an earth's quadrant. The definition "quadrant" is sometimes employed as the name itself, but its use as a name is evidently objectionable, as it already has several other meanings, which might readily lead to confusion.

For the practical unit of electrical conductivity, equal to the reciprocal of the ohm, the name "mho." This name has been in use for some time and is already well known. It is thought better to recommend it than to select and introduce a new name.

For the practical unit of illumination defined above, the name "bougie-metre."

It has been suggested to name the units "kilowatt-hour" and "ampere-hour." Your committee, however, recommends that inasmuch as these terms explain themselves, and are not longer than some others in use, to give them special names would burden a system of nomenclature unnecessarily. The principal objections to the term "Board of Trade Unit," which is in use in England for the former, are, that it could never become international, that it is longer by one syllable than the term "kilowatt-hour," and that there are now, or may soon be, units of other denominations adopted by the same Board of Trade, from which this one will have to be distinguished by an additional affix or prefix.

CONCRETE STANDARDS.

4. *Defining and adopting modes of embodying the following principal units of measurement in concrete standards capable of being readily reproduced, and adopting names for them or for the theoretical units, by which they can be distinguished from each other:—Ampere, ohm, volt, watt, standard candle.*

The following definitions of these units are recommended:

An ampere shall be that unvarying current, which, when passed through a solution of nitrate of silver in water, in accordance with the specifications recommended in the recent report to the British Board of Trade (See Appendix III), deposits silver at the rate of 0.001118 of a gramme per second.

An ohm shall be the resistance offered by a column of mercury at the temperature of melting ice 14.4521 grammes in mass, of a constant cross-sectional area, and of a length of 106.3 centimetres. It is recommended that material standards of this value, constructed in solid metal, should be preserved under the care of the several governments, as standards of comparison, and that they should from time to time be verified by comparison with the mercury standard defined above; also that for the purpose of replacing the standard, if lost, destroyed, or damaged, and for ordinary use, a limited number of copies should be constructed which should be periodically compared with the standard ohm.

A volt shall be the product of this ampere and this ohm.

A watt shall be the product of the square of this ampere and this ohm.

The other units such as the coulomb, the farad, and the joule, shall be taken as derivatives in terms of this ampere and this ohm.

In order to create as little confusion as possible by the introduction of this set of units which are to become the universal standards, and in order to distinguish them readily from the true theoretical, or abstract units defined in terms of the absolute units, it is recommended to call the former simply "amperes," "ohms," "volts," etc., or more specifically "standard amperes," "standard ohms," etc., and to call the theoretical units "true amperes," "true ohms," etc. The latter term has already come into use in this sense.

The unit of resistance known as the B. A. unit, shall be taken as equal to 0.9866 of this ohm.

The electro-motive force of a Clark cell at 15° C., prepared in accordance with the specification recommended in the recent report to the British Board of Trade (see Appendix III.), shall be taken as not different from 1.434 of these volts by more than one part in one thousand. The coefficient of temperature shall be taken as..............................?

The standard candle shall be taken as equal to the light from a lamp like that known as the Hefner-Alteneck standard amyl acetate lamp, which is to be defined by its dimensions and the height of the flame, the dimensions being such that the light shall be equal to that of the "bougie-decimale," the practical unit adopted at the Paris Congress of 1889, which is equal to one-twentieth of the absolute platinum standard adopted in 1884.

It has also been suggested that a universal wire gauge be defined and adopted. Desirable as this may at first seem, your committee recommends that no action be taken by the Congress in this matter, as it is not likely that any one scale would ever be universally adopted by manufacturers, even if defined and adopted by a Congress. The universal introduction of the metric system and the designation of wires by their diameters in millimetres, is thought to be the only satisfactory solution of this question for international work.

NOTATION AND SYMBOLS.

5. Adopting an international system of notation and conventional symbols, for designating different quantities.

The system suggested to the last Congress by Mr. E. Hospitalier, of Paris (see Appendix IV.), is recommended by your committee, although doubtless some amendments may be found desirable.

DEFINITIONS.

6. *Defining the following terms:—Impressed electromotive-force, inductivity, inductance, reluctivity, Matthiessen's standard, north and south pole.*

The following definitions are suggested:

The impressed electromotive force is the ratio of the total activity in an electrically conducting circuit to its instantaneous current-strength.

The inductivity at any point in an isotropic medium is the ratio, added to unity, of 4 π times the intensity of magnetization there existing, to the magnetizing flux density. Inductivity is synonymous with "permeability." The electro-magnetic dimensional formula is $L^0 M^0 T^0$; the conventional symbol is μ.

Inductance.—(*a*) Self inductance is the ratio of the total magnetic induction linked with, and established by an electric current, to the uniform strength of the same. The inductance of a conducting circuit is constant when its environing medium has constant inductivity. (*b*) The mutual inductance of one electric circuit upon another is the ratio of the total magnetic induction linked with the second, due to a uniform current in the first, to the strength of that current. The mutual inductance between two electric circuits is reciprocally equal when the environing medium has constant inductivity. The electro-magnetic dimensional formula is $L^1 M^0 T^0$. The absolute unit is one centimetre; the practical unit, one henry.

The reluctivity of a medium at any point is the volume differential of the reluctance thereat. Simple definition: Reluctivity is the reluctance per unit volume. The electro-magnetic dimensional formula is $L^0 M^0 T^0$. The absolute unit is one C. G. S. unit of reluctance per cubic centimetre; the proposed practical unit is the same or about one oersted per cubic earth quadrant.

The resistance of copper known as Matthiessen's standard shall be defined as follows:—The resistance of a soft copper wire one metre long, weighing one gramme, is 0.14365 B. A. units at 0° C. This definition is recommended by the Committee of the American Institute of Electrical Engineers on Matthiessen's Standard (see Appendix V.). The reasons for selecting it are given in that report.

The north pole of a magnet to be defined as being that which seeks the geographic north pole, and the south pole that which seeks the geographic south pole. This is the generally accepted definition, but it is thought desirable to formally define it.

EXPRESSIONS.

Defining and adopting expressions:—For alternating currents of more than one phase; for describing phenomena of alternating currents and of electro-magnetic waves.

It is recommended to adopt the following expressions: "simple" alternating current for the usual alternating currents in which there is practically but a single phase; "di-phase" alternating currents for two alternating currents whose phases differ in time by 90 or 270 deg.; "tri-phase" alternating currents for three alternating currents whose phases differ in time by 60 or 120 deg.; "poly-phase" alternating currents for such as have more than three phases.

For expressions describing phenomena of alternating currents and electro-magnetic waves, your committee solicits suggestions.

UNIVERSAL USE.

8. *Recommending the more universal use:*

Of the term "voltage" as synonymous with "difference of electrical potential" or P. D. *in place of the terms "potential," "tension," or "pressure," the use of which in this sense it is recommended to abandon.*

Of the term "transformer" instead of "converter";

Of the term "dynamotor" for a continuous current transformer;

Of the term "continuous current" instead of "direct current";

Of the term "kilowatt" instead of "horse power";

Of the metric system of weights and measures, and suggesting means by which its introduction will be facilitated.

PROGRAMME.

Regarding the programme for carrying out this work in the most satisfactory manner to all parties, and with the least loss of time, your Committee's recommendations and suggestions are as follows:

PREPARATORY DISCUSSIONS.

It cannot be too strongly urged that an International Congress is the place to close, but not to open a discussion on questions about which international agreement is to be had; it is therefore urgently recommended that action be taken at once to open the discussion of such matters now, in order that expressions of opinions, and, if possible, agreements, may be arrived at prior to the meeting of the Congress. To this end your Committee recommends that the suggestions embodied in this report, or as many of them as you may see fit to designate, be printed and sent to all the leading electrical and physical societies and journals in this country and abroad, with a request that they aid this work by making them public, by freely discussing them, and by sending copies of such discussions and any further suggestions to the General Congress Committee of this Institute, care of the Secretary, 12 West Thirty-first street, New York City. That a sub-committee of the Institute prepare from time to time a classified summary of such discussions to be sent to the leading electric and physical journals for publication here and abroad, and that it present a final summary of this international discussion at the Congress. This discussion ought to be closed by July 1, 1893.

It is recommended also that this Institute invite other societies to coöperate with it by appointing special committees to discuss and report to their respective societies on these international questions and to publish their conclusions in the electrical journals of their country.

It has been suggested that various committees be appointed to prepare reports on the international questions on which agreement is desired, and to submit these at the meeting of the Congress. Your Committee, however, believes that to postpone the discussion until the Congress is in session, would lead to no satisfactory results, as was shown at the last Congress of 1891, in Frankfort. It therefore urges that the discussion take place through the medium of the journals *prior* to the meeting of the Congress, and that the only report submitted to the Congress be a summary of such discussions.

MEETINGS.

Regarding the meetings of the Congress, your Committee recommends that, as in the Paris Congress of 1889, the meetings be divided into (a) *General Meetings*, one at the opening and one at the closing of the Congress, which are to be devoted to the consideration of general questions and those on which agreements are desired, and to the reading of papers of a general character; (b) *Sectional Meetings* for the reading of all other papers. The following division into sections is recommended:

1. Electro-physics, units, measurements, and all electrical matters of a purely scientific nature.
2. Dynamos, motors, transformers, etc.
3. Systems, central stations, installations, lamps, etc.
4. Electric telegraphy, telephony, and signaling.
5. Electric railways.
6. Electro-chemistry, batteries, electro-metallurgy.
7. Electro-physiology and electro-therapeutics.
8. Legal questions.

PAPERS.

It is suggested that specific invitations be extended by the Congress Auxiliary to prominent electrical engineers and physicists to prepare papers on specified subjects to be read and discussed at the Congress meetings. Also, that a general invitation be extended for other papers, which are to be submitted not later than July 15, 1893, to a committee on papers appointed by the Chairman of the Congress Auxiliary; that this committee shall examine and accept or return the latter, and at its discretion shall print any or all of those accepted as well as those solicited, in form for circulation at least one week prior to the meeting at which they are to be read; that the papers shall be read in full, in abstract or by title, as that committee shall direct.

The following subjects of papers are offered as a suggestion to the Congress Auxiliary merely as a basis and not as a complete list.

The criterion of sensitiveness of galvanometers; or on the theory of their construction.

The working of National or Municipal Laboratories for testing meters and instruments.

The practical results and economy of the employment of accumulators in Central Station systems of supply.

On the relation between weight of copper and iron in dynamos and their output.

Nomenclature and notation of magnetic circuit—or on Standard and Units of Magnetism.

Magnetism.

On the economic use of transformers under various conditions of supply.

Electricity meters from an European standpoint.
Electricity meters from a British standpoint.
Electricity meters, from an American standpoint.
Choice of materials for standard of electrical resistance.
Nomenclature of phenomena of electro-magnetic waves.
Standards for electrical measurement.
Alternating current transformers from an American standpoint.
High frequency and high potential phenomena.
Dynamo construction.

Your Committee suggests that papers should be solicited by the General Congress Committee of this Institute on the work of the early electricians of this and foreign countries considered from a modern standpoint, and their importance in the development of the science of electricity, including descriptions and illustrations of models and apparatus used.

TRANSACTIONS.

Your Committee suggests that the proper authorities be requested to have a stenographic report made of the proceedings and discussions at all the meetings of the Electrical Congress, and that this report, including such of the papers and discussions as a commitee appointed for the purpose shall direct, shall be printed and published in book form; that each member of the Congress be entitled to one copy of this report on the payment of $1 (one dollar), and non-members on the payment of $5 (five dollars).

MEMBERSHIP.

It is suggested that the membership of the Congress be divided into delegated members and members; that the dues for each shall be $5 (five dollars); that only delegated members and members be admitted to the meetings, and be permitted to take part in the discussions; that any one shall be entitled to become a member; that delegated members shall be appointed by governments, physical and electrical societies, and shall be approved by an international committee whose decision shall be final; that questions on which international agreement is desired, be decided by the delegated members, whose decisions shall be submitted to the whole Congress for approval or rejection in part or in whole, but not for alteration.

It is recommended that the United States Government be requested to extend a formal invitation to foreign governments and societies to send delegates to this Congress.

ACKNOWLEDGEMENTS.

In the present report, your Committee has embodied recommendations from the reports of the Institute's Committee on Matthiessen's Standard, and on Magnetic Units, from the recent reports of the British Board of Trade Committee, and from the suggestions of Dr. Silvanus P. Thompson to Dr. Elisha Gray, in reference to the Congress.

Respectfully submitted,

(Signed) CARL HERING, Chairman,
WM. A. ANTHONY,
A. E. KENNELLY.

New York, December 28th, 1892.

By a resolution adopted at the meeting of the General Congress Committee of the American Institute of Electrical Engineers, December 28th, the following proposed subjects, suggested by a Sub-Committee of the British Royal Commission for the Chicago Exhibition, were ordered to be published with this report:

Subjects on some of which it is proposed that short and suggestive special papers might be read by specially selected members.

Comparison between procedure in different countries.

Methods of avoiding electrical interference and risks to person and property.

Units of magnetic quantities and mode of embodying them in concrete standards.

The adoption of the name "henry" for the unit of self and mutual induction.

Adoption of the "kilowatt" instead of the "horse power" as the unit of power.

International nomenclature for describing phenomena of alternate currents and of electro-magnetic waves.

National and municipal testing laboratories.

Materials for standards of electric resistance.

Points of difference of the electrical vocabulary used in different countries.

The direct conversion of the energy of fuel into electric energy.

Comparison of the various methods employed for the electric transmission of power.

The cost of insulation in relation to high pressure for the electric transmission of power.

Comparison of the economies of the various systems of electric distribution.

Alternate current motors.

The behavior of transformers when supplying power to alternate current motors.

The construction of condensers for alternate current purposes.

The measurement of power in polyphase currents.

Direct coupled and non-direct coupled dynamos.

The use of equalizing dynamos in a three and a five-wire system.

The use of accumulators in central stations.

The proportions between output of dynamos and the weight of copper and iron employed in their construction.

Electric traction.

Application of electric power in mining.

The adoption of a uniform method of distinguishing positive and negative mains.

Electric supply meters—American, British, Continental.

Criterion of sensibility of galvanometers.

Commercial instruments for measurement of electric quantities.

The relation between the voltage of the arc and the quality and composition of the carbons.

The aging of glow lamps.

The electric working of metals.

The use of electric and magnetic tests for ascertaining the mechanical properties of metals and alloys.

The best material and mode of erection of lightning conductors in the light of recent researches in electric discharges.

The prospecting for iron by magnetic surveys.

International telegraphy.

Fast-speed and long-distance telegraphy.

The use of batteries or other generators for telegraphy.

Telegraphic lines—land and sea.

Harmonic telegraphy.

Writing telegraphs.

Long distance telephony.

The possibility of providing telephonic communication without wires.

Application of electric signaling to the working of railways (alarms, time, etc.), and to naval and military purposes.

Magnetic separators.

The use of electricity in engraving and in art reproductions.

[SUPPLEMENT TO REPORT OF THE SUB-COMMITTEE ON PROVISIONAL PROGRAMME FOR THE INTERNATIONAL ELECTRICAL CONGRESS OF 1893.]

APPENDIX I.

BRIEF SUMMARY OF THE UNITS, TERMS, SYMBOLS AND DEFINITIONS ADOPTED BY PREVIOUS ELECTRICAL CONGRESSES.

The Paris Congress of 1881 adopted, on September 21st, 1881, the following:

The fundamental units: centimetre, mass of a gramme and the second, for electrical measurements. The practical units, ohm and volt, defined in terms of the absolute units. The ampere defined by the volt and the ohm. The coulomb defined by the ampere and the second. The farad defined by the coulomb and the volt. It also resolved that the ohm be represented by a column of mercury of one square millimetre cross-section at 0° C., and of such a length, to be determined by an international commission, that its resistance be equal to the ohm.

The Paris Conference of 1884 adopted, on May 3d, 1884, the following:

The legal ohm of 106 centimetres. The ampere defined in terms of the absolute units. The volt defined by this ampere and the legal ohm. The unit of light defined as the quantity of light emitted perpendicularly from a square centimetre of platinum at the temperature of its solidification.[1]

At the Philadelphia Conference, in 1884, it was proposed to adopt the "watt," but, from the official records, this term does not appear to have been formally adopted.

The Paris Congress of 1889 adopted, on August 31, 1889, the following:

The joule defined in terms of the absolute units, and in terms of the ampere, ohm and second. The watt in terms of the absolute units, and also in terms of the joule and second. The bougie-decimale[2] defined as the twentieth part of the absolute standard

1. The name "violle," sometimes applied to this standard of light, was not adopted by this Conference, but came into use subsequently. It does not appear to have been formally adopted by any international congress. This standard is equal to about 2.08 carcels, or, approximately, to 20 English candles.

2. The bougie-decimale is a close approximation to the English standard candle, as also to one-tenth part of a carcel.

of light adopted by the conference of 1884. The quadrant as the practical unit of induction, defined in centimetres. This congress also defined the following terms:—The period of an alternating current is the duration of one complete oscillation. The frequency is the number of periods per second. The mean intensity of an alternating current is defined by the relation

$$C_{\text{mean}} = \frac{C}{T}\int_0^T C\, d\, t.$$

The effective intensity is the square root of the mean square of the intensity of the current. The effective E. M. F. is the square root of the mean square of the E. M. F. The apparent resistance is the factor by which the effective intensity must be multiplied to give the effective E. M. F. The positive pole of an accumulator is the one which is connected to the positive pole of a dynamo during the charge, and is the positive pole during the discharge. The term interurban is to apply to all telephonic communication between two subscribers or public stations belonging to different groups. The metallic circuit was adopted for urban telephone circuits and for interurban lines.

The Frankfort Congress of 1891 adopted, on September 12, 1891, the following:

Physical quantities shall be represented in italics. Units shall be represented in Roman type. Physical constants and angles shall be represented by Greek letters. The practical electrical units shall be represented by their initial letters in Roman capitals as follows:—Ampere by A, coulomb by C, farad by F, volt by V, joule by J, ohm by O, watt by W.

The proposition made by Mr. Hospitalier,[1] to establish an international system of notation, abbreviations and symbols was referred to a committee, to report at the next congress.

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1. See Appendix IV.

APPENDIX II.

The following is taken from the Report of the Committee of the American Institute of Electrical Engineers, on "Units and Standards.".

LIST OF NEW UNITS PRACTICALLY NEEDED FOR CONVENIENCE IN DEALING WITH MAGNETIC CIRCUITS.

1st. *Magneto-Motive Force, and Difference of Magnetic Potential.*

Simple Definition.—The analogue in a magnetic circuit of voltage in an electric circuit.

Strict definition.—The magneto-motive force in a magnetic circuit is 4π multiplied by the flow of current linked with that circuit.

The magneto-motive force between two points connected by a line, is the line integral of magnetic force along that line.

Electro magnetic dimensional formula, $L^{1/2}\ M^{1/2}\ T^{-1}$.

The absolute unit of M. M. F. is $\frac{1}{4\pi} \times$ unit current of one turn.

The practical unit is $\frac{1}{4\pi} \times$ ampere of one turn, or one-tenth of the absolute unit—*i. e.*, 0.0796 ampere-turn gives the unit. The prefix kilo would perhaps be occasionally used for practical applications.

2d. *Magnetic Flux.*

Simple definition.—Total number of lines of force or total field.

Strict Definition.—The magnetic flux through a surface bounded by a closed curve, is the surface integral of magnetic induction taken over the bounded surface, and when produced by a current is also equal to the line integral of the vector potential of the current taken round the boundary.

The uniform and unit time rate of change in flux through a closed magnetic circuit, establishes unit electro-motive force in the circuit.

Electro-magnetic dimensional formula, $L^{3/2}\ M^{1/2}\ T^{-1}$.

The absolute unit is one C. G. S. line of induction.

The practical unit is 10^8 C. G. S. lines.

Fluxes range in present practical work from 100 to 100,000,000 C. G. S. lines, and the working units would perhaps prefix milli- and micro-.

3d. *Magnetic Intensity*, or induction density.

Simple Definition.—Flux per sq. cm.

Strict Definition.—The induction density at a point within an element of surface is the surface differential of the flux at that point.

Electro-magnetic dimensional formula, $L^{-\frac{1}{2}} M^{\frac{1}{2}} T^{-1}$.

Absolute unit, one c. g. s line per sq centimetre.

Practical unit, 10‘ c. g. s. lines per sq. cm.

In practice, excluding the earth's field, intensities range from 100 to 20,000 lines per sq. cm., and the working unit would perhaps have the prefix milli- or micro-.

4th. *Magnetic Reluctance.*

Definition.—Unit reluctance in a magnetic circuit permits unit magnetic flux to traverse it under the action of unit magneto-motive force.

Dimensional formula, $L^{-1} M^0 T^0$.

The practical unit is 10^{-9} the absolute unit.

Reluctances vary in present practical work from 100,000 to 100,000,000 of these practical units, so that the working unit would perhaps employ the prefix mega-.

A. E. Kennelly, *Chairman.*
Francis B. Crocker,
Wm. E. Geyer,
Geo. A. Hamilton,
Geo. B. Prescott, Jr.

APPENDIX III.

The following specifications are those contained in the recent report of the British Board of Trade Committee on Electrical Standards:

The Silver Voltameter.

In the following specification, the term silver voltameter means the arrangement of apparatus by means of which an electric current is passed through a solution of nitrate of silver in water. The silver voltameter measures the total electrical quantity which has passed during the time of the experiment, and by noting this time, the time-average of the current, or if the current has been kept constant, the current itself can be deduced.

In employing the silver voltameter to measure currents of about one ampere, the following arrangements should be adopted:—The cathode on which the silver is to be deposited should take the form of a platinum bowl, not less than 10 cm. in diameter, and from 4 cm. to 5 cm. in depth. The anode should be a plate of pure silver, some 30 square cm. in area, and two or three millimetres in thickness. This is supported horizontally in the liquid near the top of the solution, by a platinum wire passed through holes in the plate at opposite corners. To prevent the disintegrated silver which is formed on the anode from falling onto the cathode, the anode should be wrapped around with pure filter paper, secured at the back with sealing wax. The liquid should consist of a neutral solution of pure silver nitrate, containing about 15 parts, by weight of the nitrate, to 85 parts of water. The resistance of the voltameter changes somewhat as the current passes. To prevent these changes having too great an effect on the current, some resistance besides that of the voltmeter should be inserted in the circuit. The total metallic resistance of the circuit should not be less than 10 ohms.

Method of Making Measurement.—The platinum bowl is washed with nitric acid and distilled water, dried by heat, and then left to cool in a desiccator. When thoroughly dry, it is weighed carefully. It is nearly filled with the solution, and connected to the rest of the circuit by being placed on a clean copper support, to which a binding-screw is attached. This copper support must be insulated. The anode is then immersed in the solution, so as to be well covered by it, and supported in that position; the connections to the rest of the circuit are made. Contact is made at the key, noting the time of contact. The current is allowed to pass for not less than half an hour, and the time at which contact is broken is observed. Care must be taken that the clock used, is keeping correct time during this interval. The solution is now removed from the bowl, and the deposit is washed with distilled water. and eft to soak for at least six hours. It is then rinsed successively with distilled water and absolute alcohol, and dried in a hot-air bath, at a temperature of about 160 deg. C. After cooling in a desiccator, it is weighed again. The gain in weight gives the silver deposited.

To find the current in amperes, this weight, expressed in grammes, must be divided by the number of seconds during which the current has been passed, and by .001118. The result will be the time-average of the current, if during the interval the current has varied.

In determining by this method the constant of an instrument the current should be kept as nearly constant as possible, and the readings of the instrument taken at frequent observed intervals of time. These observations give a curve from which the reading corresponding to the main current—time-average of the current—can be found. The current, as calculated by the voltameter, corresponds to this reading.

The Clark Cell.

Definition of the Cell.—The cell consists of zinc and mercury in a saturated solution of zinc sulphate and mercurous sulphate in water, prepared with mercurous sulphate in excess, and is conveniently contained in a cylindrical glass vessel.

Preparation of the Materials:—

1. *The Mercury.*—To secure purity it should be first treated with acid in the usual manner, and subsequently distilled in vacuo.

2. *The Zinc.*—Take a portion of a rod of pure redistilled zinc, solder to one end a piece of copper wire, clean the whole with glass paper, carefully removing any loose pieces of the zinc. Just before making up the cell, dip the zinc into dilute sulphuric acid, wash with distilled water, and dry with a clean cloth or filter paper.

3. *The Zinc Sulphate Solution.*—Prepare a saturated solution of pure ("pure recrystallized") zinc sulphate by mixing in a flask distilled water with nearly twice its weight of crystals of pure zinc sulphate, and adding zinc oxide in the proportion of about 2 per cent. by weight of the zinc sulphate crystals to neutralize any free acid. (See notes below.) The crystals should be dissolved with the aid of gentle heat, but the temperature to which the solution is raised should not exceed 30 deg. C. Mercurous sulphate treated as described in paragraph 4 should be added in the proportion of about 12 per cent. by weight of the zinc sulphate crystals, and the solution filtered while still warm, into a stock bottle. Crystals should form as it cools.

4. *The Mercurous Sulphate.*—Take mercurous sulphate, purchased as pure, and wash it thoroughly with cold distilled water, by agitation in a bottle; drain off the water, and repeat the pro-

cess at least twice. (See notes below.) After the last washing drain off as much of the water as possible. Mix the washed mercurous sulphate with the zinc sulphate solution, adding sufficient crystals of zinc sulphate from the stock bottle to insure saturation, and a small quantity of pure mercury. Shake these up well together to form a paste of the consistence of cream. Heat the paste, but not above a temperature of 30 deg. C. Keep the paste for an hour at this temperature, agitating it from time to time, then allow it to cool; continue to shake it occasionally while it is cooling. Crystals of zinc sulphate should then be distinctly visible, and should be distributed throughout the mass; if this is not the case add more crystals from the stock bottle, and repeat the whole process. This method ensures the formation of a saturated solution of zinc and mercurous sulphates in water. Contact is made with the mercury by means of a platinum wire about No. 22 gauge. This is protected from contact with the other materials of the cell by being sealed into a glass tube. The ends of the wire project from the ends of the tube: one end forms the terminal, the other end and a portion of the glass tube dip into the mercury.

To Set Up the Cell.—The cell may conveniently be set up in a small test tube of about 2 cm. diameter, and 6 cm. or 7 cm. deep. Place the mercury in the bottom of this tube, filling it to a depth of say 1.5 cm. Cut a cork about .5 cm. thick to fit the tube; at one side of the cork bore a hole through which the zinc rod can pass tightly; at the other side bore another hole for the glass tube which covers the platinum wire; at the edge of the cork cut a nick through which the air can pass when the cork is pushed into the tube. Wash the cork thoroughly with warm water, and leave it to soak in water some hours before use. Pass the zinc rod about 1 cm. through the cork.

Clean the glass tube and platinum wire carefully, then heat the exposed end of the platinum red-hot, and insert it in the mercury in the test-tube, taking care that the whole of the exposed platinum is covered. Shake up the paste and introduce it without contact with the upper part of the walls of the test tube, filling the tube above the mercury to a depth of rather more than 2 cm. Then insert the cork and zinc rod, passing the glass tube through the hole prepared for it. Push the cork gently down until its lower surface is nearly in contact with the liquid. The air will thus be nearly all expelled, and the cell should be left in this condition for at least 24 hours before sealing, which should be done as follows:—Melt some marine glue until it is fluid enough to pour by its own weight, and pour it into the test tube above the cork, using sufficient to cover completely the zinc and soldering. The glass tube should project above the top of the marine glue.

The cell thus set up may be mounted in any desirable manner. It is convenient to arrange the mounting so that the cell may be immersed in a water-bath up to the level of, say, the upper surface of the cork. Its temperature can then be determined more accurately than is possible when the cell is in air. In using the cell, sudden variations of temperature should, as far as possible, be avoided.

Notes.

The Zinc Sulphate Solution.—The object to be attained is the preparation of a neutral solution of pure zinc sulphate saturated with $ZnSO_4$ 7 H_2O. At temperatures above 30 deg. C., the zinc sulphate may crystallize out in another form; to avoid this, 30 deg. C. should be the upper limit of temperature. At this temperature, water will dissolve about 1.9 times its weight of the crystals. If any of the crystals put in, remain undissolved, they will be removed by the filtration. The amount of zinc oxide required, depends on the acidity of the solution, but two per cent. will, in all cases which will arise in practice with reasonably good zinc sulphate, be ample. Another rule would be to add the zinc oxide gradually, until the solution became slightly milky. The solution, when put into the cell, should not contain any free zinc oxide; if it does then, when mixed with the mercurous sulphate, zinc sulphate and mercurous oxide are formed; the latter may be deposited on the zinc and affect the E. M. F. of the cell. The difficulty is avoided by adding, as described, about 12 per cent. of mercurous sulphate before filtration; this is more than sufficient to combine with the whole of the zinc oxide originally put in, if it all remains free; the mercurous oxide formed, together with any undissolved mercurous sulphate, is removed by the filtration.

The Mercurous Sulphate.—The treatment of the mercurous sulphate has for its object the removal of any mercuric sulphate which is often present as an impurity. Mercuric sulphate decomposes in the presence of water into an acid and a basic sulphate. The latter is a yellow substance—turpeth mineral—practically insoluble in water; its presence, at any rate, in moderate quantities has no effect on the cell. If, however, it is formed, the acid sulphate is formed also. This is soluble in water, and the acid produced affects the E. M. F. The object of the washings is to dissolve and remove this acid sulphate, and for this purpose the three washings described in the specification will, in nearly all cases, suffice. If, however, a great deal of the turpeth mineral is formed, it shows that there is a great deal of the acid sulphate present, and it will then be wiser to obtain a fresh sample of mercurous sulphate, rather than try by repeated washings to get rid of all the acid. The free mercury helps in the process of removing the acid, for the acid mercuric sulphate attacks it, forming mercurous sulphate and acid, which is washed away. The cell may be sealed in a more permanent manner by coating the marine glue, when it it set, with a solution of sodium silicate, and leaving it to harden.

APPENDIX V.

The following is taken from the report of the Committee of the American Institute of Electrical Engineers.

REPORT OF THE STANDARD WIRING TABLE COMMITTEE.

Matthiessen's Standard of Resistance of Copper.

Revised by the Committee on Units and Standards.

The Council of the American Institute of Electrical Engineers, at its regular monthly meeting held December 3d, 1889, appointed a Committee "To formulate and submit for approval a Standard Wiring Table for lighting and power purposes."

The Committee appointed consisted of the following members of the Institute:—Thomas P. Conant, Dr. Louis Duncan, Prof. Wm. E. Geyer, A. E. Kennelly, Geo. B. Prescott, Jr., E. Wilbur Rice, Jr., Prof. E. P. Roberts, Prof. Harris J. Ryan, William Stanley, Jr., Dr. Schuyler S. Wheeler, and Francis B. Crocker, Chairman.

This action was taken by the Council with the object of overcoming or reducing the great confusion which now exists in regard to the standards and constants of electrical conductors.

At the first meeting of the Committee, held January 10, 1890, it was decided to confine the work for the present to the three subjects of "Standard of Resistance," "Temperature Co-efficient" and "Safe Carrying Capacity of Copper," since these are of fundamental importance.

The subject of "Matthiessen's Standard" alone is so confused and involved, and the discrepancies in regard to it are so great between the best authorities, as shown by the accompanying table No. 1, that the Committee has devoted its attention almost entirely to this subject up to the present time.

TABLE No. 1.

Table of valuations for the Specific Resistance of Pure Copper at 0° *C.* in Legal Microhms, selected from various electrical papers and text-books.

AUTHORITY.	Sp. Res. of Copper		PROBABLE SOURCE.
	Hard.	Soft.	
Matthiessen, Phil. Mag., 1865, No. 1 ...	1.624		Table No. II., p. 362.
" " No. 2 ...	1.634	1.598 *	Metre-Gramme standard, Table No. III., p. 363.
" B. A. Report, 1864, No. 3..		1.557	Standard Mile Unit corrected for temp. (15.5°c).
F. Jenkin's "Electricity & Magnetism." " & B. A. Report, 1873	1.634	1.598	Matthiessen's Standard for copper, other values in same report, from lead standard.
Everett's "Units & Phys'l Constants," 1886	1.611		Based on mercury ohm=94,340.
" "	1.620	1.584	Jenkin's table.
" "C. G. S. System Unit," 1891 ..	1.616	1.58·	
Gordon "Electricity & Magnetism," 1880	1.634	1.598	From Everett's "Units and Physical Constants," 1st edition.
Maxwell's "Electricity," 1881 ...	1.624		Matthiessen's No. 1.
Mascart & Joubert, "Electricity and Magnetism," 1888............	1.621	1.584	Matthiessen's No. 1.
Gray's "Absolute Meas.," 1888		1.598	Jenkin's table of Matthiessen's No. 2 value.
" " " "	1.624		Wire table from Matthiessen's No. 1 value.
Ayrton's "Practical Electricity"	1.634	1.598	
Stewart & Gee's "Practical Physics"...		1.598	
Hospitalier, "Formulaire," 1891..........	1.620		
Munro & Jamieson's pocket book, 1891, p. 189 and p. 194 b	1.642	1.615	
Munro & Jamieson's pocket book, 1891, p. 194 a..........................		1.598	Matthiessen's No. 2.
Dr. Fleming "Electrician," London. Vol. XX., p. 471 ...	1.624		Matthiessen's No. 1.
W. H. Preece, Paper, London Inst., E E.. Dec. 10th, '91..	1.616	1.580	
Anthony & Brackett, "Text-Book of Physics", 1887.........		1.611	
Slingo & Brooker's "Elec. Engineer'g," 1890	1.634	1.598	F. Jenkin.
Houston's Dictionary of Elec. Terms, 1889	1.634	1.598	F. Jenkin.
Boult's Comprehensive International Wire Table, 1890............		1.557	Matthiessen's mile standard.

*Standard recommended by committee.

The Committee after careful consideration came to the conclusion that Matthiessen's "mile-standard" (one statute mile of copper wire $\frac{1}{16}$ inch in diameter having a resistance of 13.59 *B. A.* units at 15.5° C) is not the correct one, although very commonly used. Matthiessen himself did not place much confidence in this "mile-standard." The Committee, acting under instructions from a meeting of the Institute, held September, 1890, has based all standards and values in this report upon soft or annealed copper, since its properties are reasonably constant and reliable. It has purposely excluded from its recommendations all standards and values based upon hard copper, although several were given by Matthiessen, because the hardness of copper is merely relative, and the resistance of hard copper may vary between wide and uncertain limits depending upon the *degree* of hardness.

As to the fact often brought up that copper may be found which shows one or two per cent. higher conductivity or less resistance than Matthiessen's standard, we are of the opinion that this is no real objection provided the value of the standard is definite and generally accepted. A standard which is not the highest attainable value may even be considered an advantage since the average commercial wires will approximate to it more closely.

Although we believe the standard we recommend will answer the purpose temporarily and probably permanently, nevertheless we think that if a thoroughly correct and complete redetermination of the standard resistance of copper could be accomplished, it would be a benefit to electrical science and industry. Favorable offers in this direction have already been received by this Committee from Johns Hopkins University, Cornell University, and Columbia College, and it is likely that this redetermination may be undertaken.

The following statement of the most important and reliable figures and facts given by Matthiessen will serve to show the derivation of the standard which we recommend.

A hard-drawn copper wire 1 metre long weighing 1 gramme ("metre gramme") has a resistance of .1469 *B. A.* unit at the temperature of 0° centigrade.[1]

Matthiessen also gives the resistance of a hard-drawn copper wire 1 metre long and 1 millimetre in diameter ("metre millimetre") as .02104 *B. A.* unit at 0° C.[1]

This implies a specific gravity of 8.89 for the copper used by Matthiessen, but unfortunately he neglected to actually determine the specific gravity.

Matthiessen's figures for relative conducting power are:[2]

Silver	100
Hard or unannealed copper	99.95
Soft or annealed copper	102.21

From this the resistance of Matthiessen's hard copper is found to be 1.0226 times that of soft copper, therefore, the resistance of a soft copper wire 1 metre long, weighing 1 gramme "metre gramme" is 0.14365 *B. A.* unit at 0° C., and this is the fundamental standard recommended by the Committee.

From this standard, with the specific gravity of copper 8.89 assumed by Matthiessen, are derived the following sub-standards:

A soft copper wire 1 metre long and 1 millimetre in diameter ("metre-millimetre") has a resistance of 0.02057 *B. A.* unit at 0° C.

A cubic centimetre of soft copper has a resistance of 0.000001616 *B. A.* unit at 0° C.

1 Philosophical Magazine, May, 1865.

2 Philosophical Transactions, 1864,

A soft copper wire one foot long, and one thousandth of an inch in diameter ("mil-foot") has a resistance of 9.720 *B. A.* units at 0° C.

Taking one *B. A.* unit as .9889 legal ohm or 0.9866 new standard ohm, any of the above values may be converted into these units.

Table No. II.

Matthiessen's Standard recommended by the Committee.

	B. A. U.	Legal Ohms.	New Ohms.
Equivalent length of a sq. mm. mercury column.	104.8 cms.	106.0 cms.	106.3 cms.
Resistance at 0° C. of Matthiessen's Standard—"Metre-Gramme" Soft Copper......	.14365	.14206	0 14173
"Metre-Millimetre" Soft Copper.	.02057	.02035	0.02030
"Cubic Centimetre" " "	.000001616	.000001598	0.000001594
"Mil-Foot" " "	9.72	9.612	9.59

The conductivity of copper at temperatures other than 0° centigrade may be determined by using Matthiessen's formula $C_t = C_0 (1 - .00387\, t + .000009009\, t^2)$ in which C_t is the conductivity at the given temperature, C_0 is the conductivity at 0° and t is the given temperature in degrees centigrade. It should be carefully noted, however, that this formula refers to *conductivity*. Therefore in order to apply it to resistance it is necessary to take the reciprocal and this should not be done by merely changing signs, which is not mathematically correct although often given in that way. The correct modification of Matthiessen's formula when referred to resistance is difficult to express accurately for any considerable range of temperature without increasing the number of terms in the formula, which would be very objectionable for practical work. The Committee has therefore calculated a table of temperature coefficients within the range of ordinary requirements. This seems to be the best plan to secure accuracy and convenience.

Table No. III.

Table of Temperature Variations in the Resistance of Pure Soft Copper according to Matthiessen's standard and formulæ.

I.	II.	III.	IV.	V.	VI.
Temp. °C.	Temperature coefficient of resistance.	Logarithm.	Matthiessen Metre Gramme Standard Resistance.		
			B. A. Units.	Legal Ohms.	New Ohms.
0	1.0	0.	0.14365	0.14206	0.14173
1	1.003876	0.0016801	0.14421	0.14261	0.14228
2	1.007764	0.0033588	0.14477	0.14317	0.14283
3	1.01166	0.0050362	0.14533	0.14372	0.14338
4	1.01558	0.0067121	0.14589	0.14427	0.14394
5	1.01950	0.0083864	0.14645	0.14483	0.14449
6	1.02343	0.0100593	0.14702	0.14539	0.14505
7	1.02738	0.0117307	0.14759	0.14595	0.14561
8	1.03134	0.0134003	0.14815	0.14651	0.14617
9	1.03531	0.0150683	0.14873	0.14708	0.14673
10	1.03929	0.0167346	0.14930	0.14764	0.14730
11	1.04328	0.0183993	0.14087	0.14821	0.14786
12	1.04728	0.0200621	0.15045	0.14878	0.14843
13	1.05129	0.0217230	0.15102	0.14935	0.14900
14	1.05532	0.0233821	0.15160	0.14992	0.14957
15	1.05935	0.025039	0.15218	0.15049	0.15014
16	1.06339	0.026694	0.15277	0.15107	0.15071
17	1.06745	0.028348	0.15334	0.15164	0.15129
18	1.07152	0.029999	0.15393	0.15222	0.15186
19	1.07559	0.031648	0.15451	0.15280	0.15244
20	1.07968	0.033294	0.15510	0.15338	0.15302
21	1.08378	0.034939	0.15569	0.15396	0.15360
22	1.08788	0.036581	0.15628	0.15455	0.15418
23	1.09200	0.038222	0.15687	0.15513	0.15477
24	1.09612	0.039859	0.15746	0.15572	0.15535
25	1.10026	0.041494	0.15806	0.15631	0.15594
26	1.10440	0.043127	0.15865	0.15689	0.15653
27	1.10856	0.044758	0.15925	0.15748	0.15711
28	1.11272	0.046385	0.15985	0.15808	0.15770
29	1.11689	0.048011	0.16044	0.15867	0.15830
30	1.12107	0.049633	0.16105	0.15926	0.15889
40	1.16332	0.065699	0.16711	0.16526	0.16488
50	1.20625	0.081436	0.17328	0.17136	0.17095
60	1.24965	0.096787	0.17952	0.17753	0.17711
70	1.29327	0.111687	0.18578	0.18372	0.18329
80	1.33681	0.126069	0.19204	0.18991	0.18946
90	1.37995	0.139863	0.19823	0.19604	0.19558
100	1.42231	0.152995	0.20432	0.20206	0.20158

Respectfully submitted,

FRANCIS B. CROCKER,

Chairman.

LOUIS DUNCAN, E. WILBUR RICE, JR.,
WM. E. GEYER, E. P. ROBERTS,
A. E. KENNELLY, HARRIS J. RYAN,
GEO. B. PRESCOTT, JR., WM. STANLEY, JR.,
SCHUYLER S. WHEELER,

Committee on Standard Wiring Table.

The report of the "Standard Wiring Table Committee" presented at a meeting of the Institute Nov. 18th, 1890, was referred by the Council to the regular committee on "Units and Standards," and has been somewhat modified, particularly in regard to the correction for temperature.

Committee on Units and Standards.	A. E. KENNELLY, *Chairman*, FRANCIS B. CROCKER, WILLIAM E. GEYER, GEO A. HAMILTON, GEO B. PRESCOTT, JR.

Appendix IV. embodying an international system of notation and conventional symbols for designating different quantities, suggested to the last Congress by Mr. E. Hospitalier of Paris will be published hereafter.

CARL HERING,
Chairman, Sub-Committee on Provisional Programme.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

NEW YORK, January 17th, 1893.

The seventy-third meeting of the Institute was held this date, at 12 West Thirty-first street. The Secretary of the Institute called the meeting to order, and said: I have just received a note from President Sprague, saying that he was just out of a sick-bed and expected to be ordered back again, and that it would not be prudent for him to be out this evening. We have with us the senior Vice-President, from Boston, Mr. Lockwood, who, I presume, will be willing to take the Chair, as he has frequently done.

At the meeting of Council this afternoon the following associate members were elected.

Name.	Address.	Endorsed by
CORSON, WILLIAM R. C.	Assistant Electrician, The Eddy Electric Mfg. Co., Windsor, Conn.	H. S. Rodgers H. G. Reist. Franklin Sheble.
LOW, GEORGE P.	Electrical Inspector, Pacific Insurance Union, 307 Sansome St., San Francisco, Cal.	W. F. C. Hasson. Horace B. Gale. F. G. Cartwright.
SCHREITER, HEINR	Editor, *Der Techniker*, 11 Chambers Street, New York City.	C. P. Steinmetz. F. W. Tischendoerfer. Ralph W. Pope.
SEE, A. B.	A. B. See Manufacturing Co., 1235 Bedford Ave., Brooklyn, N. Y.	T. C. Martin. Joseph Weztler. Ralph W. Pope.

Total, 4.

The following associate members were transferred to full membership.

CHURCHILL, ARTHUR	Electrician, Cable and Wire Department, General Electric Co., Schenectady, N. Y.
HERRICK, CHARLES H.	Manager and Engineer, Wright Engineering Co, 196 Summer St., Boston, Mass.
WELLS, DOUGLAS	Late Supt. of Telegraphs and Engineer to Government, Nassau, Bahamas.
SCOTT, CHARLES F.	Assistant Electrician, Westinghouse Electric and Mfg. Co., Pittsburg, Pa.
MARVIN, HARRY N.	In Electric Percussion Drill Work, General Electric Co., Schenectady, N. Y.

Total, 5.

Mr. Lockwood, upon taking the Chair, said :

Gentlemen, Members of the Institute and guests: as Mr. Haskins, the speaker of the evening, comes from New England, it seems perhaps in order that a brother of the Eastern Star, like myself, should preside this evening, and hence I have less hesitation in presiding than I otherwise should have.

Some of you may have already, from advance copies, read the paper. All of us, I am sure, will be interested in following Mr. Haskins as he reads his paper; and although he stated to me a few moments ago that he was something of a crank on meters, I would like to remark before he starts that we must not forget that it is the cranks that make the wheels go round. With these few preliminary remarks, I commend him to your tender mercies, and I am sure he will feel that the more thoroughly you discuss his paper after he finishes reading it, the better he will like it. There is nothing equal to a counter-irritant for getting all the real good there is, out of a medicine.

A paper presented at the Seventy-third Meeting of the American Institute of Electrical Engineers, New York, January 17, 1893. Vice-President Lockwood in the Chair.

ELECTRICAL RECORDING METERS.

BY CARYL D. HASKINS.

It was my first intention to present this paper on "Electrical Recording Meters" or "Electricity Meters" in the form of a strictly technical monograph, dealing with each of the more prominent theories of meter construction separately and fully. Upon outlining this plan, it at once became evident that to do so I should be obliged to devote some four or five hundred pages of closely written manuscript to my subject matter, and should have been obliged to deliver the final three quarters of my paper during the early morning hours succeeding the session. I therefore changed my plan, and now propose to briefly describe and discuss the leading elements which, singly or combined, have gone to make up the typical meters which have been presented to the public up to the present time. I find myself limited to generalities, and strictly technical considerations have necessarily been neglected that the field might be approximately covered.

A brief history of the evolution of the electric meter would be very appropriate, but repetition is odious, and I beg to refer all who are interested in the early history and genealogy of the meter to that most interesting and complete paper on this subject read before the Institute[1] by Mr. George W. Walker, May 21st, 1891.

The earliest meter patent was granted in 1872 to Mr. S. Gardiner, Jr., of New York City, and the principle of a magnetic or electro-magnetic release for a simple clock movement, is preserved in two or three so-called time-counters to-day, and is doubtless very useful for many purposes; as for example in the Spaulding clock for registering the hours of use of a motor, or in

1. Transactions, vol. viii., p. 351.

other similar devices for registering the hours of use of arc circuits. These devices I shall neglect; they are not meters within the true sense of the word, and their simplicity is obvious.

Before undertaking any description, a few words may be appropriately devoted to the question of the unit by which it is most desirable to measure electrical power in use.

The ampere has been, perhaps is to-day, the popular unit for the measurement of electricity supplied; yet, on careful consideration, how very meaningless for work of this kind, is the ampere unit. It would only find a parallel in the very ingenious early settler of Maryland, who bought six linear miles of land from the trusting Indians.

Had we but one standard voltage in use for lighting and for power, and for other purposes, no unit could be better than the ampere-hour for meter measurement; but unfortunately or fortunately, as the case may be, there is scarcely a potential, between 5 and 2,000 volts, which does not more or less imperatively call for measurement by meter to-day, and if the ampere unit is to be retained, then it will become necessary to reconcile ourselves to the use of an endless number of constants, or to a still more endless schedule of ampere-hour rates.

We wish to measure power delivered; in fact we wish to know how much coal a group of lamps is consuming. This points directly to the watt, and I venture to assert that careful consideration will invariably show that the watt is the only true unit for the measurement of electricity by meter. Unless, perchance, we adopt the cubic foot, as has at least one central station in the United States; a very amusing but equally practical demonstration of American ingenuity.

The earliest successful meters if we consider classes rather than individual instruments, were the chemical meters, closely followed by thermo-meters.

The chemical meter is obviously capable of giving most accurate results: in fact, with proper manipulation, it is very doubtful whether any measuring device, which has up to to-day been designed, could more correctly sum up passing current. It is in the manipulation and care which such meters require, that their fault lies—if fault there be.

It will be useless for me to waste the time and patience of my kind listeners, by describing to them the eminently successful and generally popular Edison chemical meter, and I will not attempt to do so.[1]

An electro-plating bath in its meter form as generally used, does not, however, give a dial indication, and the consumers ask for a dial indication almost invariably, unless they have already become thoroughly familiarized with Edison meters as used by many large Edison stations.

Many very ingenious and some quite successful attempts have been made to actuate a train of gears by the electro-deposition of an electrolytic bath. Thus, we have two electrodes suspended at the opposite ends of a walking-beam, as shown in Fig. 1. This

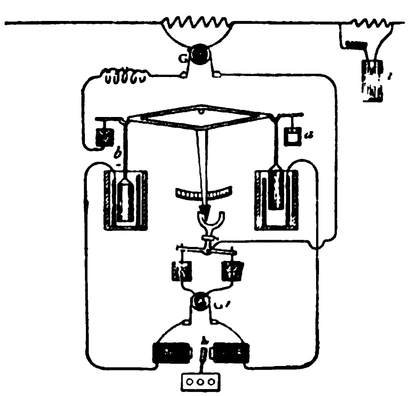

FIG. 1.

walking-beam is in various ways connected with a pole-changer, and as but a small portion of the current being measured, passes through the true meter (for of course almost all chemical meters are shunted) the pole-changer is not perhaps a very serious objection.[2]

The action of such a meter as this is obvious; we have a deposit from one electrode onto the other, until the second electrode becomes the heavier, when the beam tips and the recording device is set one notch ahead, the pole-changer is thrown over, and the deposit takes place in an opposite direction, the former plus electrode becoming the minus, and so on. This device deposits and reposits the same zinc, or rather electrode material. Again, we have a modification of the same device in the form of a wheel bearing a number of electrodes, and on the same principle setting up continuous rotation.

1. See Jenks on the Edison Chemical Meter, TRANSACTIONS, vol. vi., p. 26.

2. See also, TRANSACTIONS, vol. vi., p. 32.

It should be noted, that in this first digression from the chemical meter, we at once meet with the prime factor of difficulty in all motor meter construction—that of friction, which, if uncompensated, must invariably introduce more or less serious error. We shall touch on this point more fully hereafter.

Another form of self-registering electrolytic meter has a cathode plate suspended from a spring-balance, an ordinary sensitive spring weighing machine, and the heavier the cathode grows, so much greater is the registration of the spring indicator. This device is perhaps preferable to the reciprocating movement just described, but is limited in the capacity of the spring, and probably lacks sensitiveness to small amounts, being dependent of course solely upon the nicety of construction in the spring-balance.

Mercury has at times been employed in the construction of electrolytic meters, and with at least moderate success, for with a mercury, anode and a cathode of the same or other material, a record easily measured may be obtained, and such a meter may even be made self-registering in a graduated tube or by half a-dozen other more or less simple means. Such are the more typical electrolytic meters. There are other similar devices which have not been mentioned, for it is absolutely impossible to deal with everything within the brief limits of a single paper.

Another form of chemical meter formerly quite popular among inventors, depended for its registration upon the decomposition of water, generally acidulated water, and sometimes upon the decomposition of more volatile substances. This class of meter may very properly be divided under two heads :

First, are those meters simply dependent upon the measurement of the gas developed by the decomposition of water through any gas registering device. We may say that such meters are mere decomposing baths connected to a gas meter. There are some devices of merit which may be classed under this head, but the principle is probably not commercial, for we have nothing very successful of this kind in use to-day.

One of the more ingenious meters of this class provides a diagonally placed rotating wheel with pockets; the decomposition takes place directly under each pocket progressively, and as the air pocket fills with gas, the wheel rotates sufficiently to free this gas at the surface of the fluid, bringing another pocket into place. Others have a rising and falling diaphragm like the popular gas meter, and still others, a delicately poised air fan over a minute aperture. This last device is obviously most inefficient.

The second group of meters under this classification brings us to the thermo-meters; a typical group containing a few meters of more or less pronounced merit.

Those thermo-meters depending upon volatilization of a fluid, generally have two or more sealed bulbs partly filled with some volatile fluid, as for example, naphtha or ether. When two such bulbs are used, they have generally been mounted on a walking-beam mechanism combined with a pole-changer; each bulb containing some kind of a rheostat or heat developing device dependent for its heat on the current passing through the meter, the two bulbs communicating with one another. The rheostat in but one bulb is in circuit. The heat developed in the rheostat in circuit, volatilizes more or less rapidly the fluid contained in this bulb, according to the current passing through it. The gas developed either passes in gaseous form into the second bulb and condenses, or else, as is more common, forces the fluid remaining by the simple increase of pressure into bulb No. 2, which at once becomes heavier and causes its end of the beam to fall. This throws the pole-changer, and the rheostat in the second bulb is thrown into action, repeating the operation as just described. To be successful, such a device must be very sensitive, and to be sensitive, the construction must be of a more or less expensive character, and so delicate as to be to a greater or less degree prohibitive. Like the walking-beam meters just described, instruments of this class have been designed with a number of bulbs mounted on a rotating wheel; the same actuating principle holding true for all such devices.

Another ingenious form of thermo-meter, no longer in any sense a chemical meter, is an instrument dependent for its action upon the heat in a confined but circulating atmosphere. Thus a rheostat dependent for its heat upon the amount of current passing, is so arranged as to heat a body of air, which, by the peculiar construction of its receptacle, at once commences to circulate more or less rapidly, dependent upon the heat. It is obvious, that if a delicate air fan, a screw propeller in fact, be suspended over such a column of circulating air, its speed would increase with the speed and volume of circulation. One of the most ingenious and most interesting meters that it has ever been my good fortune to see, is the Forbes meter,[1] constructed on this plan. But here friction is the most serious consideration; the torque obtained in this manner being necessarily small.

While the Forbes meter cannot perhaps be properly considered as a motor meter in the true sense of the word, it still must be classed as such in a certain sense, and I think it may safely be accepted as an axiom, that to be successful in practical operation, a motor meter of any kind must be of high torque, for it is only by the combination of high torque and compensated friction that accurate results can be obtained on light loads. I might say that almost any one can build a meter which will record fairly accurately on heavy loads; the difficulty is to build a sensitive and accurate meter for very light loads down to one lamp.

Another ingenious and quite positive form of heat meter consists of two metallic rods, or in some cases of two bi-metallic rods, somewhat similar to an ordinary thermostat; such meters being in fact a simple application of the thermostat principle to a recording device. Two rods of this kind may be placed vertically, one under each end of a walking-beam mechanism, the familiar pole-changing device being again brought into play to throw the current first through one rod and then through the other. The character of the walking-beam must be such, that only a slight movement is needed to actuate the recording mechanism and the pole-changer.

The method of operation of a meter of this kind is obvious; the alternate expansion and contraction of the two rods as they are thrown in and out of circuit, will occur with a rapidity dependent upon the current passing, and, as each rod lengthens, it throws over the walking-beam onto the shorter cool rod, and the same action follows from that side.

I think no comments are needed on such a device as this; it probably lacks sensitiveness to a marked degree and is open to other objections which are sufficiently apparent.

Probably better than this is the system of bi-metallic rods with one contracting and one expanding side, which bend to right

1. TRANSACTIONS, vol. v., p. 85.

and left, or up and down, with a rapidity dependent upon the strength of the current, a pole-changer of course being used as before.

The method of actuating a train of gears by this system is comparatively simple, and not open to as many objections as the device just described.

I have neglected to state heretofore that the bulk of the thermo-meters are actuated on the shunt principle. It is obviously out of the question to operate a pole-changer by means of a meter, which shall break the full current passing to the lamps. As a rule, a very small portion of the total current is carried through the meter around a positive shunt. Instead of shunting the current, this device has at times been applied to alternating work by the introduction of a kind of transformer, which takes a few turns of wire in series with the lamps and transfers the energy through a secondary to actuate the meter; thus the meter is not on the lamp circuit at all, but on the independent secondary. But this intervention of induction is probably not desirable in cases of this kind.

A patent issued in the year 1889 to a Mr. Dahl, tells us of another quite unique form of thermo-meter, and this is probably the only heat meter actuated by an actual field and armature arrangement. In this instrument, the inventor provides a field of iron excited by a few series turns on the lamp circuit. The strength of the field should therefore bear a more or less definite relation to the amount of current passing through its turns. I will not comment upon how direct this relation would be. It will suffice to say that the introduction of iron in the construction of any meter of this character is probably detrimental to its accuracy to a quite marked degree. This, however, is aside from the subject in hand.

On a shaft in the meter which we are describing, is suspended a kind of armature consisting of a flat disk-like ring of iron, supported by a number of spokes and mounted as a wheel; in fact it is mounted on a quite ordinary shaft mechanism suspended in proper bearings. The iron portion of the wheel, rotates between the poles of the electro-magnet, or at times permanent magnet just described. We have, in fact, a disk armature rotating in such a manner as to pass between two poles of a more or less strong field. On the principle of the familiar thermo-magnetic motor, rotation is obtained in this meter by so placing coils of wire or other suitable material under certain portions of the disk and in series with the lamp circuit, that the disk is differentially heated at the proper point; thus, with the peculiar armature construction described, a thermo-magnetic action is set up, somewhat akin probably to the action of the thermo-pile. The portion of the disk heated, is attracted toward the field coils, and thus (the disk being progressively heated and cooling rapidly owing to its thinness,) continuous rotation is set up. Sometimes in this construction the field is in series with the heating device and sometimes the heating device alone, is in series with the lamp circuit, and the field is either a permanent magnet or independently excited by a battery. Neither of these changes involve the principle of the meter, which is assuredly ingenious.

As in almost all other of the arts, electric meter construction shows the definite mark of evolution; thus, one class of meter cannot be distinctly separate from its neighbor, the one merging into the other.

The final form, and I think I may say the last form of thermo-meter at all worthy of description, is distinctly a clock meter also. The clock meters will come under our consideration somewhat later in this paper, but this meter which I am about to describe can scarcely be separated from its kindred, the heat meters. Briefly, however, I may define the clock meter as one whose recording device or whose movement is actuated by an independent source of power, generally a clock, though sometimes a constant speed motor or other device.

I might add here, at the risk of digressing from my subject, that there is probably no known indicating device from the Cardew voltmeter to the Sir Wm. Thomson balance which could not be so applied as to make a more or less accurate clock meter.

The thermo-meter now under consideration is also a thermometer (a commentary perhaps on the English language.) An ordinary thermometer of somewhat modified construction, is surrounded at the bulb by a coil of wire in series, or in shunt series with the circuit to be measured. It is apparent that the rise and fall of this thermometer will be *more or less* dependent upon the heat developed by the coil, and the heat again will be more or less dependent upon the current passing, but, of course, the temperature of the surrounding atmosphere must necessarily introduce serious error.

Now, it has been more than once suggested, though never patented, I believe, that a constant ray of light supplied, we will say by an electric lamp burning when the meter is in circuit, if thrown upon this thermometer could be made to photographically mark on rotating sensitized paper, actuated by clockwork at uniform speed, a line, or rather a solid block of light whose lower termination or line of demarkation would represent the height of mercury in the tube.

Thus, we have a curve of registration which only needs to be integrated with a planometer to determine the consumption, always supposing that the indicating device is correct. I have said, "only needs to be integrated." How desirable a meter would be in practical service whose record needs to be integrated can best be left to the judgment of the station manager and the public. It is largely a matter for personal experience and personal opinion. To compensate for external temperature it was suggested that the meter be so encased as to practically insulate it from heat or cold; but this is probably not feasible.

There is another device, or perhaps I had better say there might be another device for accomplishing the object of this last meter in a somewhat similar manner. The actinometer is probably familiar to all who have indulged in amateur photograhy; it consists of a piece of glass covered with small cubes, each cube of a more intense ruby red than the one beyond it, merging, in fact, from an almost clear glass to an almost perfectly non-actinic medium.

Now, it suggested itself to a certain electrician that if a number of these squares were arranged in a piece of glass, one above the other, and a lamp whose light should vary more or less directly with the potential on the lines, be placed before this glass or actinometer, a sensitive film being rotated behind the actinometer at a constant speed, the varying light of the lamp would draw a curved line, or rather a curved block of light and shade on the paper. This could be measured by a planometer to get the average voltage, or could be taken at points, to see what the voltage was at certain times; in fact, a recording. voltmeter. This device seemed very nice indeed It had only one fault—it would not work. I might say that the device was my own.

At this point I think we may venture to dismiss the subject of chemical, thermo and similar meters, and give some attention to the clock meter.

The general character of the clock meter has already been briefly stated; that is, we have already defined the character of one form of clock meter. There is an almost endless variety of instruments of this kind. They are not all electricity meters by any means; some are recording voltmeters, others recording ammeters, and others have still different purposes. The familiar recording steam gauge is only a modification of this instrument. Many such meters have been designed for station work and for testing purposes, where great sensitiveness and considerable cost of construction were permissible. But these hardly come within the province of a treatise of this kind.

A strip of paper is almost invariably rotated over a drum, sometimes being fixed to the drum, and sometimes being drawn from one drum to another, the paper moving at uniform speed, and generally being divided into hours or other fractions of time by abscissæ lines.

This clock mechanism can be combined with any indicating device; it is only necessary to supply the connection which shall cause the indicator to mark the paper, and draw a crooked line. First attempts of this kind were generally made with ordinary solenoids, or sometimes with a simple coil and iron core rising and falling with the current. Whatever the character of the indicator or method of communication between the indicator and paper, it remained necessary to keep the friction of contact low.

First attempts were made with a pencil, bearing directly upon the paper, but the friction introduced by this device was fatal to accuracy. A glass pen has been substituted for the pencil with better results, but even this caused too much friction. The photographic method of line drawing is one of the best systems yet introduced, and is probably more familiar to the majority of us than the other methods, because it forms a component part of the Walker meter which has attracted attention at various times.

With an instrument of this kind a diaphragm is provided, attached to the indicating mechanism, and raising and falling with the variations of the indicator. Through this diaphragm is a small aperture, the best plan being to form this aperture by the crossing of two slits in two diaphragms. Through this small hole, light from a burning lamp is shed upon sensitized paper with the result already described.

One of the best methods of accomplishing the registration, perhaps, when all things are considered, the very best, is to attach to the pointer of the indicator, a steel point or needle with an iron armature mounted in the form of a spring, or in some similar manner. By placing an electro-magnet behind the paper to be marked, and sending through this magnet an electrical impulse at fixed periods of say, one, two or five minutes, the pointer is drawn sharply down to the paper, puncturing it, and is immediately released by the cessation of energizing current in the electro-magnet. Thus, the paper when removed, will have a continuous marking of punctures tantamount to a curved line. This device presents practically no friction, and is more or less simple and easy to carry out. The make-and-break necessary for the electro-magnet is easily actuated by the clock movements. But we may say of these forms, as well as of the forms of clock meter which are to follow, thas there is one serious objection to them all—they have to be wound up, which is certainly a fault. We may modify this statement by saying that many of these devices have an electrical attachment which makes them self-winding, and this is a step in the right direction at least. Whether it fully meets commercial necessities, is, I believe, an open question.

All of these instruments being curve-drawing meters, they cannot properly be considered as the thoroughly commercial article, which the successful electric meter must be. For, I think I am safe in assuming that to be successful the meter of to-day positively must have a direct reading dial, equally available for the consumer and supplier.

This brings us to a second form of clock meter of a preferable character. This class comprises those meters whose clock mechanism is accelerated or retarded by the strength of the current passing through them. There are several methods of accomplishing this, all of which apply the influence of the current to the pendulum of the clock.

For example, we have a pendulum with an iron end forming an armature which is attracted first to the right, and then to the left by two electro-magnets dependent for their strength upon the lamp circuits. This is an early and very ineffective form of clock meter, hardly to be considered as a practical affair, but typical of the class.

The most successful and probably the best form of meter of this kind consists of two clocks with a differential gear between. One of these clocks runs at a constant speed, the speed of the other being governed by the circuit to be measured. At times by a pressure coil forming an armature, and a fixed series coil forming a field, or again, by a horse-shoe magnet pendent from the pendulum and vibrating back and forth above two coils in series with the lamps, the former being a wattmeter and the latter an ammeter. Both were designed by the same inventor. When nothing is passing through the meter, the speed of both clocks is the same, and the differential gear does not move; but when lamps are turned on, and the regulating device begins to act, one clock is accelerated or retarded according as the principle may be applied, and the difference between the constant speed clock and the varying speed clock indicates the consumption in directly read units.

I can safely say that this is an excellent meter, but is obviously open to the same objection as are other clock meters, since there are two clocks and the presence of the differential gear requires very nice adjustment between the two. There is another important factor here, which bears directly upon the question in hand, and that is, the cost of building meters of this class. The clock-work must not be of a cheap character, but must be good clock-work and good clock-work, is invariably expensive and as invariably delicate.

I might say, however, that the Aron meter, the typical meter of this type, proved itself so accurate at the recent meter competition in Paris, that it succeeded in dividing the first prize of 10,000 francs and a gold medal, with the Thomson recording wattmeter, the principle of which we shall consider later. Your attention may be directed to the fact that the Aron meter is a watt-hour indicator, something unusual, for almost everything considered up to the present point has been typically an ammeter or a modification of an ammeter.

Before turning to the final portion of this paper and considering the most popular and as yet the most successful class of recording instruments, viz. the motor meters, a little attention should be given to those clock meters whose principle is dependent upon the function of a cone or cones. There are a considerable number of meters of this class. It appealed to the inventive mind early, and our patent records are full of cone meters.

Briefly, the principle of all of these instruments is about as follows:—Let us assume a wheel rotating at a constant speed; let us assume a cone mounted on a shaft on which it is free to rise and fall; let the cone rise and fall with one of its sides in a vertical plane with the periphery of a constantly rotating wheel; the speed of the cone regulates the speed of rotation of the recording gear and the position of the cone is determined by an indicator device which obviously must be of high torque, because friction must be a more or less serious factor. Some very fair instruments of this class have appeared from time to time. One of the most ingenious of them provides two cones, or rather two cone-like drums, somewhat like a beehive in shape. One of these cones is rotated at a constant speed, and the other is inclined on a movable right angle axis in such a way that its point of contact with the neighboring cone varies with the load. The same end being accomplished as in the rising and falling cone just described without the introduction of as much friction. This is a type of the Maxim meter.

Another similar device provides for a small wheel rising and falling between two cones whose inner sides are parallel, thus transferring the power from a constant speed cone, to a cone with varying speed, the same end being accomplished in all cases, and invariably involving friction.

Another form of cone meter, still a clock meter, differs somewhat from those just described. Let us assume a conical drum, shaped like the bullet of an ordinary rifle, and some four or six inches in diameter at the base rotated at a constant speed by a clock mechanism beneath it; let us suppose a spool of thread to be placed upon a pin a foot or so removed from the drum; the thread from the spool is attached to the drum which should be covered by some felt-like material; the thread passes through an eye or ring which in its turn is attached to an indicating mechanism. It is apparent that the indicator will raise and lower the string according to the amount of current or energy passing. Since the drum rotates and at a regular speed, the thread will be wound onto the drum and the point where it is wound on will depend on the position of the indicator. Thus, if the load were very small, the indicator would be at its highest point and the thread would be wound around the smallest diameter of the drum. As the load increased the indicator would be depressed and the thread would be wound on a greater diameter of the drum. The inspector of meters, on his monthly visit, would remove the thread which had been wound onto the drum, measure it on a yard stick, and ascertain perhaps that the consumer had used

15 yards of electricity. This is certainly reducing things to a very practical standpoint.

In conclusion, I would say of the clock meters, that those which actuate a drum or marking mechanism, and those akin to them, generally have a stopping and starting device which prevents the clock from running at times when there is no current passing through the meter; this is quite essential. Those which have a lamp within them for photographically marking the fluctuations of current, have an automatic cut-out for the lamp, which turns out the light in the meter when the last lamp on the supplied circuit goes out. I might add that in a few of these meters the clock is displaced by a constant speed motor.

There is one other class of clock meter which actuates a dial or recording train, and for the typical instrument of this class we will select the Pilkington-White meter. This instrument has a wheel rotating at a constant speed; into this wheel are inserted a number of pins of different lengths, projected from the face of the wheel parallel to its axis: a lever arm whose position is determined by the indicator mechanism is so placed that it shall engage with these pins. If the load be light it will only engage with the longest pins. If, for example, the load be one lamp, then the lever will engage only with the longest pin. On engaging with the pin, the lever is pushed in the direction in which the wheel is rotating, and gives an impulse of one tooth to the recording wheel. Thus, on the lightest loads the meter would receive but one impulse for every revolution of the motor wheel, and just in proportion as the load increased, so this lever would engage with more pins and more frequently actuate the recording dial.

This meter is very good in many respects; but it should be noted that it reads to points. There is necessarily a limit to the number of pins which can be used, and with a 100-light meter having, we will assume, 10 pins, it is clear that there can only be 10 variations in speed from one lamp to full load, consequently the speed will only change every 10 lamps, and we should get the same record on 19 lamps as on 10. The figures which I give for a 100-light meter having 10 pins, are purely arbitrary and not representative of the number used. They are given to indicate the idea which I wish to convey.

We may now turn from the consideration of the clock and separate motor meters, and give our attention to what is to-day themost important class of electrical recording instruments; the motor meters: those whose registering train is actuated by the current to be measured. There are quite a large number of meters of this class, although invention until recently has not been as prolific in this particular direction as in the clock meter field.

In the majority of cases, motor meters have been designed for alternating circuits, and they again naturally divide themselves into two classes, those actuated by inductive principles, and those whose motive powers act direct, and which are similar in character to the electric motor. Of the first class there is a large number, designed of course, for alternating circuits.

These are almost exclusively of two forms—the wound and the unwound armature types. But few, if any, of the former are in practical use, though several have, I believe, shown very fair results. As a rule, they consist of a field formed of a coil or coils in series with the lamps, and a small drum, or other closed circuit armature, preferably without iron, whose commutator is short-circuited across, by directly connected brushes, or other similar means. This meter will record with considerable accuracy, but in the presence of better and simpler plans, it has not met with general favor.

The other class of inductive meters is, in its various forms very familiar to almost every one. It consists, in general, of a coil or coils, in series with the lamps forming the primary. A short-circuited coil of low resistance within and at angle to this, forms a secondary, and within this again is a more or less solid metal armature of disk, drum or other shape, generally, wholly or in part of iron, and mounted on a carefully balanced shaft, which, on rotating, actuates the recording dial.

Meters of this construction while highly practical and decidedly useful, are scarcely all that could be desired for several reasons, all of which tend to introduce errors. In the first place their construction and principle prohibit entire accuracy on very low readings. This is quite largely due to the fact that nothing is present to balance friction.

Now the theory of inductive meters as set forth in most of the patents on file, claims a torque proportional to the square of the current, which doubtless would be correct, could we neglect the effect of certain minor influences. In reality, careful tests seem to show that torque falls somewhat below the square, and above the direct proportion, and the proportion does not seem to hold good at various points of load.

The speed of motor meters is naturally approximately proportional to the torque, and they therefore tend to run too fast and have to be restrained, which is accomplished in various ways as we shall presently see, while discussing the question of drag or damping mechanism.

None of the inductive meters will run on direct current, of course.

We now come to the second form of motor meters—those which are not dependent upon inductive action for their rotation, and are consequently equally serviceable for alternating or direct circuits.

Of these there are but a few, all of them having a field in series with the lamps, and an armature—sometimes in series and sometimes in shunt. Of the former kind but few have been tried carefully enough to thoroughly prove their merits.

Perhaps the most successful of these has a heavy iron field, with resultant poles above and below a shallow basin of mercury. The current for the lamps passes through a copper coil around, and excites the field, thence through the mercury from a portion of its periphery to center or *vice versa*. This naturally causes the mercury to rotate, its speed being proportional to the amount of current, minus, of course, the effect of friction, the mercury in this case forming the armature.

Friction is balanced in this meter quite successfully by the introduction of a fine coil of high resistance around the field. This coil is " in shunt across the line," like a lamp and serves to intensify the field on the lower readings, while on the higher ones it bears too small a proportion to the total field to exert any appreciable influence. This coil can be nicely adjusted to balance friction, by varying an outside resistance.

While such a meter has many weak points, still it has one marked advantage, the balancing of friction. This cannot be given too great importance in motor meters, for friction is their weakest point, and by successfully neutralizing this, they may be brought to almost any degree of accuracy, provided always that their fundamentals remain proportional throughout.

The remaining class of motor meters embraces those with a series field and shunt armature. These are somewhat of a departure from the others, since they are not current meters, but power or watt-meters. Their construction is in all cases quite similar to an ordinary shunt motor.

The field is in series with the lamps; the armature, (which is of high resistance,) and generally of Siemens construction, with a commutator of a few segments, is placed "in shunt across the line," the shunt being taken off beyond the field coils. An outside resistance is placed in this shunt circuit, to reduce the current in the armature to the necessary small quantity and to prevent any appreciable waste across the line.

The strength of armature then, may be seen to be dependent on the pressure, whilst that of the field depends on the expenditure of current. The torque of this meter then, is directly proportional to the power passing, and the speed is naturally proportional to the torque (again minus the friction) but the friction is balanced in these meters, as in the previous one, by the presence of a "shunt field," since the armature circuit is taken off beyond the fields, and consequently passes through them in addition to the current for the lamps. As in the previous meter, this serves to counteract the retarding influence of friction in the lower readings where it is dangerous, whilst in the higher ones it bears so small a proportion to the total field as to be unappreciable. This meter is, of course, equally useful without change of calibration on either a direct or an alternating current of any frequency.

Motor meters, as a class, rotate far more rapidly than is allowable in practice. It has been found necessary, therefore, to introduce a "drag," or resistance to rotation, to slow the meter to a reasonable speed. This has been done in several ways; perhaps the most common method being to attach to the shaft a number of air fans. These are quite largely used, and with fair success. The resistance of an air fan to rotation is approximately proportional to the square of the speed. Therefore this device is only fitted for combination with such meters as have a torque increasing with the square of the current. But since the torque of such meters does not quite reach the square, the retarding effect increases rather too rapidly and has a tendency, though not always pronounced, to cause the speed of the meter to fall off proportionately on high loads.

An effort has been made to overcome this disproportion of the fan's resistance by substituting folding fans which close by centrifugal force, on the principle of the steam-engine governor. This novel idea, whilst a decided improvement, scarcely overcomes the difficulty.

Another method of drag, which has been used with some success is the rotation of a small fan in a liquid, a method perhaps rather better than the previous one, since resistance to rotation falls below the square of the speed, when the liquid itself begins to rotate. Much depends in this case on the shape of the receptacle containing the fluid.

A third method, certainly by far the best, consists in rotating a small inefficient dynamo, generally a mere disk, turning between permanent or electro-magnets. This resistance is of course directly proportional to the speed, and therefore to the torque, being indeed the machine reversed. By applying this drag, friction is again the only difficulty to contend with.

One other important point in connection with the drag and the friction we must not neglect. The greater the drag (commensurate, of course, with sufficient speed), the smaller the proportion of friction to the total load will be, and the greater the accuracy of the instrument's registration.

To sum up then, it would seem that the most practical and useful meter, according to our present light, would be a motor meter of other than inductive type—preferably an energy rather than a current meter—with some force to balance friction, with a drag which is directly proportional to the speed, and which will be comparatively heavy, making the irregular friction as small a portion of the load as possible.

With all these features properly worked out and applied there can be no reason why a meter should not be accurate, efficient, comparatively cheap, and thoroughly able to take care of itself without undue attention.

There is another class of motor meter somewhat akin to the clock meters, which we discussed some little time ago. This comprises those instruments whose motor would rotate at a constant speed, provided the resistance to rotation were also constant. In other words, the current passing through the meter would be so arranged as not to cause fluctuations of speed in the motor directly. This is easily accomplished by placing the motor in shunt across the line like a lamp, and properly proportioning its windings. Variation in speed of the motor which is geared directly to the dial, is accomplished in this class of meters by varying the resistance to rotation or drag.

This may be accomplished in several ways. One plan suggested, provides for fans which can be more or less fully immersed in a fluid, the immersion of the fan being dependent upon the strength of the current, the fan, in fact, moving up and down the shaft in accordance with the influence of a kind of indicator mechanism. Thus, on very light loads, the fan would be immersed and would strongly retard the motor, while, on very high loads, the fan would be almost removed from the fluid in which it rotated, and the motor would race in consequence.

This same end has also been accomplished by an electro-magnetic drag, somewhat similar to the damping device which has already been described. The electro-magnets being so arranged that their strength varied with the current in use on the line, and also at times with the potential on the line, and thus accomplishing the same purpose in a better way than the fluid fan just mentioned.

It is with regret that I find myself at the end of my subject, or rather of my paper. The subject is so vitally interesting to me that I would gladly have continued and made my paper much longer, had I dared further to trespass upon the patience of my hearers. I trust that there is matter in this paper which will be found worthy of discussion and further attention.

Discussion.

The Chairman :—Gentlemen, it is impossible that one of your number can sit in this chair and preside over your sessions at intervals for more than a year, without noticing the demeanor of the several listeners to a paper like this, and forming, in some sense, a conclusion as to the degree of interest which it excites in the minds of the members. I have learned to observe the faces and the habits of the several members, and I have learned to consider it an excellent sign when several members who are reading the paper and following the speaker, turn over the leaves a brief instant of time before the reader of the paper turns over. This has been uniformly the case this evening, and I observed this evening that about three words before the speaker reached the end of the page. the listeners were ready for him, and would turn over the pages.

I will not undertake to indicate the trend that discussion should take at all. I think the members who have heard this paper read are well able to appreciate its value; but the Chair does desire to say that he believes that, when incorporated in our proceedings, it will form a valuable addition to our Transactions and will be well worthy of re-reading over and over again, as we shall reach it, not only as we receive the proceedings by mail in the separate numbers, but after they are bound. I notice with pleasure the brief intimation at the head of page 28, where Mr. Haskins speaks of the inspector of meters measuring the thread on his yard-stick and finding out how many yards of electricity the consumer had used since the inspector last visited his place, and, although perhaps a little out of order, I may say that it reminded me of an incident which occurred, not in my own history, but in that of my father, who lived, as I did in my youth, near the Soho Foundry, where Boulton and Watt made their first steam engines. Murdoch, as most of you know, worked there and was one of the earliest, if not the earliest to strongly advocate the general introduction of coal gas as a means of illumination. It took him some time to find that his views were feasible. It took some time before coal gas became recognized as a valuable means of illumination. But Murdoch was early impressed with a strong sense of its value. He told to my father an anecdote concerning its introduction, to the effect that after they began to manufacture coal gas at the Soho Foundry, very frequently farmers would come with sacks on their shoulders, for a sackful of it, and would go away greatly disappointed on finding that they could not carry it away in sacks. There is perhaps more sense in measuring electricity by the yard, and I am glad to see that we have got down to such a practical method of measurement. This paper deserves full and free discussion and I trust it will get it, because that is the greatest compliment that can be paid to any person who prepares a paper and comes to read it to us.

Mr. Townsend Wolcott:—Mr. Haskins speaks of a thermometer. I would say that thermic meter would be more in accordance with the rules of the English language. It is hardly fair to lay the blame of a word having two meanings to the English language. Thermic meter would indicate one in which heat played some part, but not one which measured heat.

Mr. Haskins:—I am very glad to be corrected in that matter, I am sure. I used the word thermo-meter because it appeared in Patent Office literature. It is a term commonly used to designate this class of meter.

Mr. Wolcott:—Yes, sir; but the language of patents is generally a very poor place to go for English.

Again, on page 45, Mr. Haskins says:—"This meter is, of course, equally useful without change of calibration, on either a direct or an alternating current of any frequency." I have understood that the meter was correct on alternating currents; but the reason I mention this is, that the "of course" is not so plain to me, why the meter having the fine wire armature in the shunt should be accurate with the same constancy of calibration, with alternations of all frequencies. Unless the inductance of the armature is a negligible quantity, it would seem to me that there should be corrections for each change in frequency.

One other point. The Thomson meter seems to be a very accurate and beautiful instrument. There is only one fault I can find with it. There is a series of pointers passing around by a continuous motion, and if you look at the meter frequently, there is never any trouble about it. But sometimes you read a hundred or a thousand wrong. When the hand gets on the other side of the graduation, you are not sure which side of the graduation it is on. There is a device for overcoming that, by using a hand that moves all at once. Sometimes we use what is known as the Geneva-stop movement in watches, but that introduces a friction. It seems to me that by running a little extra energy from the main current, that friction could be overcome at the point where it interferes with the meter.

Prof. R. O. Heinrich:—Having heard from Mr. Haskins about the general lines on which the inventive genius has been at work in this very serious meter question, we would all I am sure, be thankful to the gentleman if he would give us from his large experience some information on the general behavior of those meters which came under his observation. There is very little literature on this subject, beyond the mere description of meters usually in connection with the indefinite statement that the apparatus is extremely accurate.

I beg to anticipate such information in giving some details on the Teague meter, of English make which was shown to me as one of the very few which were approved by the Board of Trade. From this statement we must assume that the London Board of Trade is satisfied with an accuracy as shown by this meter.

This Teague meter is a motor meter, in principle and construction exceedingly simple and certainly very commendable as far as that is concerned. It is very compact, up to a capacity of 50 amperes, it is 6½ inches in height, 4¾ inches wide and 6¾ inches deep at its base. In the cross-section shown in Fig. 2, I represents a cast-iron cylindrical shell, with the cast-iron parts I_1 and I_2 screwed in at top and bottom. The latter two form the two concentric poles of an electro-magnet which is excited by the field winding W. A very intense field is thus formed in the circular space between I_1 and I_2. The armature consists of a copper

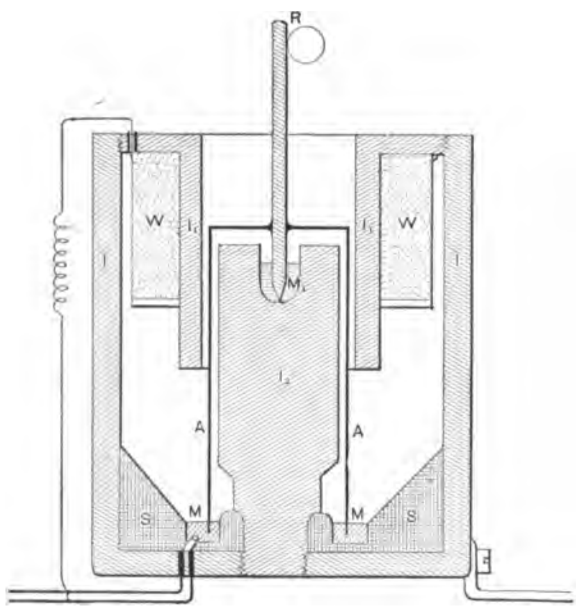

Fig. 2.—Cross-Section of Teague Meter.

I. Cast-iron shell, with pole pieces 1 and 12.
W. Shunt wound field.
A. Armature consisting of a nickel plated copper cylinder.
S. Insulating material.
M. Mercury Troughs.
R. Recording device.

cylinder closed at the top and pivoted in a hole drilled into the central iron core I_2. This copper cylinder is thus free to rotate about its central axis, this rotation being communicated to a registering device in the usual way.

The lower part of the interior at S S is filled with an insulating material, so as to form a circuit through M M partially filled with mercury. Mercury connection is likewise made at the point where the armature is pivoted. The main current passes into the lower mercury through M M, thence through the copper cylinder to M, where the metal of the electro-magnet forms the other connection with the main circuit. The field winding W W is connected in shunt to the main circuit. Those parts of the armature which are immersed in the mercury are nickel plated, which is claimed to be preferable to amalgamated contacts.

In a similar meter shown to me at the same time by the inventor, Professor Perry, the concentric poles of the electro-magnet had a fluted surface so that the toothlike projections of the external pole were placed opposite to similar projections of the internal pole. [See Fig. 3.] The armature would thus rotate in magnetic field of varying intensity, the induced currents generated during rotation in the armature increasing the "drag" which is a very desirable feature, as Mr. Haskins has pointed out. Professor Perry also proposes to immerse his armature almost entirely in mercury, so that the surface tension and friction connected therewith, will only exist where the shaft of the arma-

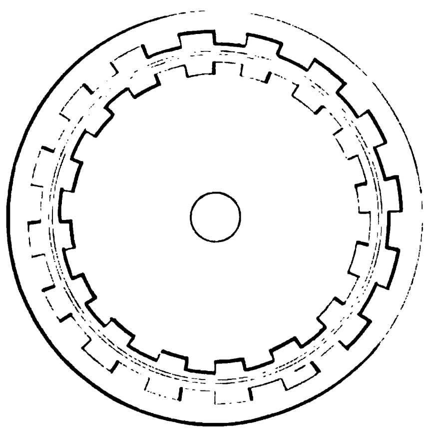

FIG. 3.—Perry's Meter. Horizontal cross-section of field magnets.

ture emerges from the mercury. (Mercury to be used for the purpose of making contact for large currents does not seem to me to be a desirable and practical feature of a meter under any circumstances.)

Returning to the Teague meter, I was informed by the maker, that its greatest number of revolutions does not exceed 25 per minute, which would be a very good feature of this meter. The speed curve given to me and represented in the diagram, Fig. 4, represents most likely the meter at its best, otherwise it would not have been made public. This curve is taken from a meter which is to register from half an ampere up to 25 amperes. The following table is compiled from the speed curve, in which the heavy line indicates rising, the dotted line falling current. The readings were taken at a temperature in the case of 25.5 C.

TABLE I.

CURRENT	WATT-HOURS PER TURN.		PERCENTAGE DIFFERENCE FROM MEAN.			
			Rising Current.		Falling Current.	
	Rising Current.	Falling Current.	Mean 2.755	Mean 2.67	Mean 2.755	Mean 2.67
0.6 amp.	3.70	4.20	+ 35 per c	+ 38.7 per c	+ 51 per c	+ 57 per c
1 "	3.60	3.90	+ 30 "	+ 34.8 "	+ 42 "	+ 46 "
2 "	2.90	2.90	+ 5 "	+ 8.6 "	+ 5.5 "	+ 8.6 "
3 "	2.68	2.68	− 2.5 "	+ 0.4 "	− 2.5 "	+ 0.4 "
4 "	2.68	2.70	− 2.5 "	+ 0.4 "	− 1.8 "	+ 1.1 "
5 "	2.68	2.72	− 2.5 "	+ 0.4 "	− 1.1 "	+ 1.9 "
6 "	2.68	2.73	− 2.5 "	+ 0.4 "	− 0.78 "	+ 2.2 "
7 "	2.70	2.60	− 1.8 "	+ 1.1 "	− 5.5 "	− 2.6 "
8 "	2.70	2.50	− 1.8 "	+ 1.1 "	− 9.0 "	− 10.1 "
9 "	2.63	2.70	− 4.3 "	− 1.5 "	− 1.8 "	+ 1.1 "
10 "	2.63	2.70	− 4.3 "	− 1.5 "	− 1.8 "	+ 1.1 "
11 "	2.63	2.70	− 4.3 "	− 1.5 "	− 1.8 "	+ 1.1 "
12 "	2.63	2.70	− 4.3 "	− 1.5 "	− 1.8 "	+ 1.1 "
13 "	2.63	2.70	− 4.3 "	− 1.5 "	− 1.8 "	+ 1.1 "
14 "	2.63	2.70	− 4.3 "	− 1.5 "	− 1.8 "	+ 1.1 "
15 "	2.63	2.70	− 4.3 "	− 1.5 "	− 1.8 "	+ 1.1 "
16 "	2.64	2.70	− 4.0 "	− 1.1 "	− 1.8 "	+ 1.1 "
17 "	2.65	2.65	− 3.6 "	− 0.75 "	− 3.6 "	− 0.7 "
18 "	2.66	2.58	− 3.2 "	− 0.4 "	− 6.2 "	− 3.4 "
19 "	2.68	2.57	− 2.5 "	− 0.4 "	− 6.5 "	− 4.0 "
20 "	2.69	2.58	− 2.1 "	+ 0.75 "	− 6.2 "	− 3.4 "
21 "	2.70	2.59	− 1.8 "	+ 1.1 "	− 5.8 "	− 3.0 "
22 "	2.68	2.60	− 2.5 "	+ 0.4 "	− 5.5 "	− 2.6 "
23 "	2.60	2.65	− 5.0 "	− 2.6 "	− 3.6 "	− 0.7 "
24 "	2.64	2.69	− 4.0 "	− 1.1 "	− 2.2 "	+ 0.7 "
25 "	2.69	2.69	− 2.1 "	+ 0.75 "	− 2.2 "	+ 0.7 "
Total mean.	2.74	2.77				

Mean of total number of observations 2.755 Watt-hours per turn.
Mean of last 24 observations 2.67 " "

This meter shows bad frictional errors for the lower ranges, and is not reliable below three amperes, that is below 12 per cent. of its maximum capacity. Taking the useful range, from 3 to 25 amperes, this meter is right within about 2 per cent., the error being in this case mostly in favor of the consumer apparently, although the percentage error for rising and falling current would to some extent have an equalizing effect. It would however, be only a question of time and of a very short time at that, when the error would be entirely against the consumer, since the frictional errors in this type of meter must increase very rapidly. It is quite a different thing to criticize an instrument from a laboratory test, and from tests made in actual practice extending over a longer period. The Board of Trade of London seems then to be satisfied with an accuracy as exhibited by this meter; I do not think we would be satisfied with it in this country.

Mr. Haskins mentions in his paper, "that the watt is the only true unit for the measurement of electricity by meter." In principle I agree in this completely with Mr. Haskins, but the general public seems to think otherwise. This very day I was informed by a gentleman from one of the Edison illuminating companies, that his customers want their bills in ampere, or rather lamp hours. As an advantage of this way of rendering the bills they claim, that they know how many amperes a lamp takes, and if they wish to check their meters they can note the number of hours and the number of lights which they use, and thus check their bills. If the bill was rendered in watts or kilowatts, 99 per cent. of the customers would be entirely at sea as to what it meant, and therefore, they claim would not have the means of making a rough check on the meter. At the present state of the meter question, I do not know whether it is so very desirable for the producer to have the customer get a check on the meter.

In most of the motor meters, I think, though I am not so very familiar with this subject, the energy consumed is considerable, and I have heard frequently in connection with the Thomson meter, that in comparison with the chemical meter, it is decidedly inferior so far as the energy consumed is concerned.

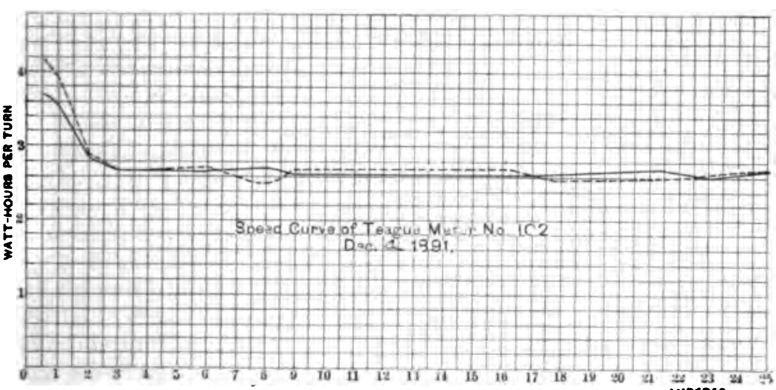

Fig. 4.

The mentioning of the use of graphical registering devices reminds me of a patent which I noticed only recently in which puncturing by means of a spark passing from the pointer to a metallic drum is made use of. I have serious doubts whether this would be practical, since I should certainly think that the static charge on the pointer and on another conductor in its neighborhood, would seriously interfere with the indications of the instrument.

I should not think that the winding up of a clock meter, (and I am convinced that the difficulties are so far best solved in clock meters) is a very serious objection. We have to send somebody to read the meter and it would be just as well for the man to wind it up at the same time.

Mr. Wolcott has already touched upon the question of the influence of self-induction in wattmeters for alternating current, inasmuch as they might be claimed to give correct readings for very different frequencies. In watt measurements in inductive circuits, we may be able to reduce such errors to a minimum for a special case, but not for all possible cases.

The construction of a wattmeter equally well adapted for the measurement of energy in continuous and alternating current circuits presents very great difficulties, and I am satisfied, that at the present time we are not in possession of a meter which will do this satisfactorily. It would be interesting to hear more from Mr. Haskins about the efficiency of such meters as have come under his observation.

MR. FRED. W. TISCHENDOERFER :—In regard to Mr. Haskins's conclusion about motor meters, I would like to call your attention to a recording wattmeter invented by Mr. Hummel, which fulfills all the conditions mentioned as necessary for an accurate and sensitive apparatus. These motor meters I refer to, have been in practical use since 1886, and were exhibited at the Frankfort exposition. In Schuckert and Company's catalogue, including their exhibited machines and apparatus, the plates and description of this direct-reading meter are given. The armature and field have no iron. The field is in the main circuit, and the armature, which carries a commutator with a few segments, is connected in derivation. To balance the mechanical friction, and at the same time to start the motor at a small percentage of the maximum current, for which the instrument is built, a fine wire field coil, connected in shunt to the line, is provided for. For damping the speed of the motor, the armature shaft carries a copper disk which revolves between the poles of an electro-magnet, the latter being also in shunt to the line terminals, thus securing a constant damping coefficient. The sensitiveness of the apparatus is such, that readings are made at one per cent. of the maximum current the meter is made for, and the exactness of the apparatus, found by many tests, gives a maximum error of less than one and a half per cent. The patent of this recording wattmeter dates from the year 1887. I would like to show the plates to any one interested in it.

MR. A. E. KENNELLY :—In regard to the question whether a meter should be made an ammeter or a wattmeter, I have the honor to differ with the author to some extent. When power is delivered, when a motor is operated, from a continuous current circuit, a wattmeter is indeed preferable, since if the pressure falls below the normal, the consumer is deprived of the full measure of power to which he is entitled, but in electric lighting the case is so different that surely a different consideration applies. The consumer seeks illumination, and not power. He is indifferent to the amount of power absorbed by his lamps, when the terms of his contract have once been decided. If the illumination from an incandescent lamp varied directly with the voltage, then indeed it might be desirable to record the volts and amperes conjointly, but as no such simple relationship obtains, the necessity for a wattmeter seems to disappear. All the recording ammeters in use to-day are merely lamp counters, and the charge is usually based simply upon the lamp-hour and not upon the watt-hour. The necessity for regulating the voltage is forced upon the central station, partly owing to the excessive lamp breakage that would result from too high a pressure, and partly because the consumers complain when the illumination is not up to the standard.

The important question which has been raised concerning the applicability of meters to alternating current, and to continuous current circuits, without a change in their constants, is a very important one and depends almost wholly—does it not?—upon the amount of non-inductive resistance, which is necessarily in one or the other circuit. If the field, say as in the Thomson meter, is in the direct circuit of the lamp, then the armature, we will suppose, is in shunt, and the armature must have a certain inductance. If there is no iron in the armature, then that inductance can be quite appreciable, provided that the non-inductive resistance in the circuit is so large that the impedance of the two in series shall not be more than, say, one per cent. greater than the simple resistance to the continuous current, and knowing the inductance and the frequency, I think it will be found that a considerable margin is allowed in this way for meters not employing iron.

Mr. C. R. Van Trump:—Regarding what Mr. Kennelly has said respecting a motor meter or a watt-meter, as a central station engineer I beg to approve, and to further say that I cannot see that the meter should be the exception in the case. In fact having operated a great number of Edison chemical meters, and recently taken up the Thomson meter, the wattmeter which has been so ably described. I have put the chemical meter, the ampere-meter, on the motors, and the watt-meter on the lights. We are operating a great many cranes, quite large cranes, all the way from 10, 15, 20 and 25 tons. In one case the factory is about a mile away from the station, and operated entirely by electricity. Ordinarily the average load on the cranes, perhaps, would not be over five horse-power for a whole day, but at times the operator will throw on as much as 50 or 60 horse-power. Now from a central station point of view, are we to put an investment of copper down there to keep his pressure up to the normal, or are we to put an ampere-meter on, and charge him with the loss in the line, which would occur when he put enormous loads on the mains at a considerable distance from the station? It is merely a question of who is to pay for the drop, the customer or the central station, supposing of course that the line is of ample capacity for the *normal* load. We think it should come on the consumer. One feature that I have found in looking into, and selecting the Thomson meter is just the question of what the variation is, under different loads which was brought up in the discussion referring to a foreign meter, and I would like to know if it is possible, just what the number of turns under different loads, or what the variation in the number of turns per watt-hours should be on the Thomson meter for different loads.

Mr. J. Overbury :—There is one objection to the Thomson meter, or to any wattmeter that employs an armature in shunt, that I think has not been mentioned, and that is the question of operating expense. To get sufficient torque, the armature requires a current of about one-tenth of an ampere, and as this current is flowing, even when the meter has stopped and the lights turned off, it would amount to over $20.00 a year for each meter, allowing that the current could have been sold instead of wasted.

Now, if we could by any means cut out this armature during the fifteen or eighteen hours that the meter is idle, it would overcome one of the strongest objections some operating superintendents and users have to this type of meter.

This cutting out of the armature could be accomplished by at least one method, as shown :

In this device, a fine coil connected as a shunt to the field coils, by its magnetic pull, closes a switch placed in the armature circuit,

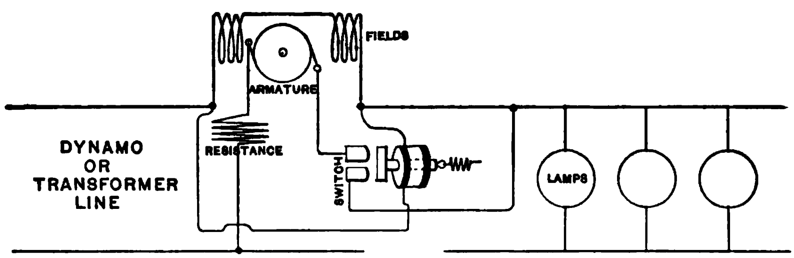

Fig. 5.

whenever the current is being used. When the lamps are turned off, the shunt ceases to work and a spring opens the armature circuit.

Mr. Caryl D. Haskins :—I do not know that I succeeded in catching notes of all that was brought forward.

I do not think I need go into the question of mutual and self-induction in such meters as the Thomson meter. I regret that my use of the term "of course" was unfortunate. I think Mr. Kennelly covered the ground and explained the matter better than I could. It must be remembered that meters of this class are, I think I may say, uniformly made without iron, and again, the resistance of the armature winding is not as great as the resistance in series with it; the total resistance of armature circuit is high, and the turns of the field are low, so that while there doubtless is a certain amount of self and mutual induction effect there, it is so slight that the most careful experiments find it to be a negligible quantity—a quantity which may be neglected, not only in calibrating meters, but in testing them for close and accurate results.

The same gentleman who brought up this question, brought up the question of the dial. He called our attention to a fact which everybody who has to read gas-meters found out some time ago: namely, that the dial is not always where it seems to be. [Referring to Fig. 6.] For illustration, suppose there are two dials, and, as is generally the case with dials of this kind, each hand or each wheel runs ten times slower than the one next nearer to the motor. Now, if the very simple rule be followed, that dials of meters, whether electric-meters, gas-meters, or any other kind, must be read from the dial which is nearest to the motor mechanism—the dial which runs fastest—in the case of the Thomson meter, the dial which is on the extreme right, and if we remember that the dial last read must always confirm the one next to be read, we cannot possibly make a mistake. For example, suppose the hand to the extreme right stands approximately on 9. That tells us that this hand has made nine-tenths of the revolution. Each of the divisions in the next dial is equal to a revolution of

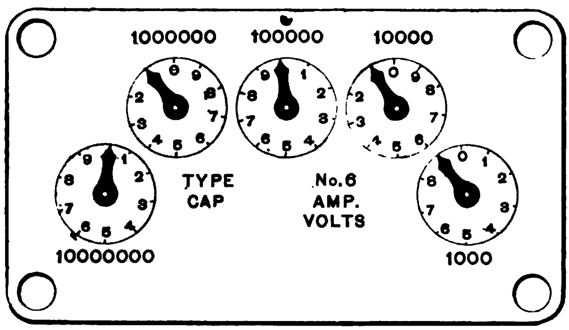

Fig. 6.

this dial. Consequently, if this hand has made nine-tenths of a revolution, that hand has made nine-tenths of the division. Therefore, it is nine-tenths of the way from one number to the next—it may be ten-tenths of the way to the one it may appear to rest on; but this shows us positively, and I think any one who has been used to noting gas-meters much, will not have that difficulty.

Referring to the other kind of dial, the dial which moves along at points, it is familiar to me. It operates, I believe, by exposing the figures in square holes. I would say there are many devices like that. and most of them involve a great deal of friction, and friction is a very bad thing. I have been asked to say what the various meters that we have with us to-day will do, and I think I can combine with that, a reply as to what the Board of Trade over the water asked for, and what accuracy they were satisfied with. I think I can safely say that we are not satisfied, the public is not satisfied, in this country, with medium-sized meters intended for average loads, which will not do better than 35 to 45 per cent. error, even on the very lightest load. I do not wish to go into the question of just what each individual meter will do. I think it would hardly be my right to do so. But I will give a few illustrative curves. Let us make a number of ordinates here. Let us assume that this line, here, which I have marked 0, is the line of 0 error. Let us suppose that each line above the 0 indicates that the instrument at that particular load is five per cent. fast, and that each line below the 0 indicates that the instrument at that particular point is five per cent. slow. Let us assume that A is one lamp, and B three lamps, C is five lamps, D ten and E fifteen. Then the inductive meters, as a class, as I have found them in this country, tend to show us a curve which starts at one light somewhere down here. (X).

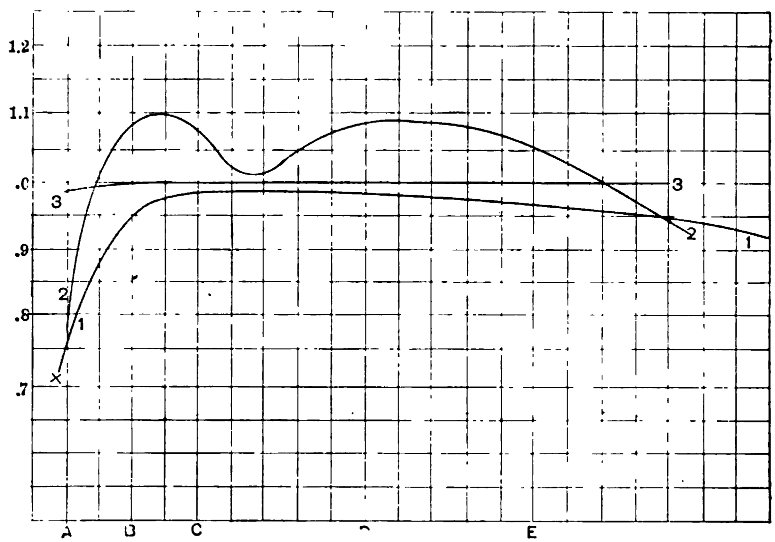

FIG. 7.

Please bear in mind that I do not bind myself to any particular percentage of error there, because no two individual meters will give us the same error. I have seen meters in the same shipment, of these inductive meters, which would run all the way from 50 per cent. slow on one lamp, to not more than 15. So that I cannot lay down any positive rule. But the meter starts slowly. That is friction which is not compensated for. The curve comes up here, but as a rule, I have found it in medium sizes running rather slow, till past the three-light point. It then strikes this line (0). It generally goes a little above it. It keeps on following it with a falling tendency till it gets to its maximum load. I have found that that curve varies. Sometimes the error here is less, and the error above is more, and *vice versa*. But it maintains that characteristic, and as soon as it begins to be over-loaded it drops off—that effect I mentioned, of drag increasing more rapidly than the torque. The meter which I spoke of in my paper which had folding fans, started in something like this, (curve 2) I am speaking of individual meters which I have tested. I think I can explain this. That curve is greatly exaggerated. That low start is friction again. The curve goes up here. That is all right. Perhaps our zero line ought to be there. In reality the meter may have been calibrated too high. Here the fans begin to shut up by the device which closes them like a steam engine governor. That causes the retarding effect—the drag—to increase much less rapidly, and the meter speeded up again. This fall here was caused before the fans began to be moved by centrifugal force. Here the fans got closed up entirely, and the resistance increased more rapidly than the torque.

Taking out these lines, I will draw a characteristic curve, as I have found it with a great many tests of Thomson meters of average size. (Curve 3.) This is our zero line again. The law of the meter is correct, the only error which we find being on the lower reading where possible friction is not properly compensated for. I have tested meter after meter which did not show that dropping curve even on one lamp. I might go into an endless number of curves of various meters, but I do not think it is called for. I do not know that I need to comment on the meter described just now. It has found its parallel in two or three other meters, and after all, it is not so much unlike the meter I described as having a rotating basin of mercury. It might be described as a form of the disk meter with modifications.

As to the question of the watt-hour, whether the watt-hour is the proper basis to charge by: first, I think we ought to consider that the watt-hour is a *universal basis.* A watt-hour means just the same thing whether a man is using 500 volts, or 50 volts, or 10 volts, or 1,000 volts. It is a watt-hour, and has a meaning, no matter what the voltage is. That enables us to have uniformity of rates and uniformity of constants, and to do away with the confusion that results from a number of constants. Again, it is undoubtedly the unit of power, and it seems to me that what the station man wants to measure, is what he is putting into the lamps. I think that measurement by ampere-hours, which by the way, is generally warped by the customer into the term "lamp-hour" (which is very deceptive), is much as if we had started by charging for gas by the burner-hour, irrespective of what is burned. Now, it is very common in some places for the consumer to furnish his own lamps—very common indeed—especially among small stations, and if the consumer furnishes his own lamps, perhaps he furnishes a lamp that takes 70 or 80 watts; then he will, as a rule, object to his bill very strongly, because he counts his lamps and he counts his hours, and his meter does not agree and does not tell him the some story. The same, again, with fluctuations of the pressure—the old story that the "lamp-hour" meter is not what is wanted, because every fluctuation of pressure introduces an opportunity for a quarrel between the consumer and the station man. I think I might enlarge on this much more fully, if I dared to take more time.

A very important question was touched on here to-night, and that is the energy that is expended in the meter. I think the statement which was made is misleading. Surely, what the station superintendent or the station manager wants to know is, "how much more will it cost me to operate my system with a meter using a certain fraction of an ampere all the time, than it does to operate my system without that meter?" Well, to generate that amount more current, it is not likely that he would have to put in any more dynamos, or any more engines, or add to his battery of boilers. He would probably not employ any more linemen, or any more station men, or any more wire men. He would burn a little more coal. He would probably evaporate a little more water. He might possibly use a little more oil. In other words, it is the difference between the cost of running the station without, and running the station with, meters that he must consider; he must not say, "Here are so many amperes that my meters 'consume,' and I get a cent an ampere-hour, and my meters are costing me four million dollars a minute." I think that if the drop on the line, or any of the many losses the station man has to meet, were considered on the basis of what he could sell that amount of current for, he would soon find the debit side of his books discouraging.

I do not think that I need reply specifically to the difficulties which a gentleman presented relative to the use of the wattmeter on an electric crane, the crane being remote from the station, and there consequently being a considerable drop on the line. I do not quite see how this bears upon the meter question at all. Yet it seems to me that it simply means that a good deal of energy was being wasted in getting the power to the motor, and I think heavy wire would be the remedy for that. Certainly what the consumer wants to pay for is the service of the motor, not the maintenance of the system.

I was asked, I think, what could be considered an admissible percentage of error for a meter on an average load. That is a very nice question, because no error is really permissible; but I think it has been generally accepted by almost everyone, that an average error not to exceed three per cent. is not to be grumbled at, in the present state of the art, and there is more than one meter existing to-day which will do better than that on average loads, and even on light loads.

Referring to such meters as mark a curved line of fluctuations in current or pressure, or anything else on paper, the device of a sparking coil connected to the indicator point and puncturing or marking the paper was mentioned. I might say that I did not mention that in my paper because I did not think it really important enough. I have seen such instruments and have experimented with them. The only objection seems to be that the spark is erratic. It does not go straight through the paper. I have seen it vary from one-sixteenth, to one-eighth of an inch from the right direction, and it will not always puncture the paper directly under the pointer. That is the only objection I know of to that. Of course, in using such a device it is best to use a paper which has been so treated that it will be discolored by the spark.

The Board of Trade question perhaps needs a little further consideration. The Board of Trade was evidently satisfied with instruments which certainly showed considerable error, but if my memory serves me, the Board of Trade insisted more on certain details of mechanical construction. Of course they insisted on certain accuracy, but they were more inclined to dictate as to the character of the mechanical construction, the facilities for installing and similar details, than they were as to what was really the essential thing, what the meter should record, what it should do. It was a great surprise, I think, to more than one meter man in this country that they should do so. Methods of sealing were given great importance.

A meter was mentioned just now as having been shown at the Paris exposition, which embodied, it was said, the qualities which I mentioned as probably the best to apply in an ideal meter. I notice in the description of this meter, which, by the way, I think I know, that electro-magnets were used for the damping mechanism. Whilst it is undoubtedly very good practice to use them, they are probably not the best thing to use. In the first place, electro-magnets, unless they are separately excited, are scarcely fit for alternating work, and, again, they expend energy in the meter, and, again, they are apt to vary somewhat in saturation, with fluctuations of potential in the line. A properly constructed permanent magnet is, I believe, preferable for this work. I think I have replied more or less definitely to everything that was suggested in the discussion. I thank the members for their kind attention.

THE CHAIRMAN ;—The Chair has been much interested to note that the habits which any form of apparatus acquires as a genus, when there is but one species, is usually perpetuated when the species is increased, and from casual remarks whick Mr. Haskins and the other speakers have dropped, it is evident that the bad habits ascribed by consumers to the gas-meter are perpetuated in the electric-meter. It is perhaps satisfactory to those who are not consumers to know that.

I would wish also to call attention to one feature of the paper which has not been commented on by any gentleman, and that is the remarkable performance of the Aron meter, as noted on page 40. If any one of us possessed a meter which produced for its owner five thousand francs, and half a gold medal, I think you would all agree with me, that that person had become possessed of the goose which laid the golden egg. Meters do not ordinarily do that for their inventors.

I am sure that the Institute will join with me very heartily when I propose a cordial vote of thanks to Mr. Haskins for his paper, and I will ask all who agree in that motion to say "Aye."

[The vote was carried, and the meeting adjourned.]

[COMMUNICATED, AFTER ADJOURNMENT, BY MR. TISCHENDOERFER.]

Mr. Haskins is right in saying that the damping effect of a shunted electro-magnet depends on the line potential. This variation of the damping effect can be made very small, if the electro-magnet is practically saturated for the lowest working potential, so that the increase of M. M. F. of the electro-magnet, due to the increased potential, increases the magnetism, and consequently the damping effect, to a very small extent only. This increase of the damping effect, however, is counteracted by the increase of motor torque, due to increased strength of the shunt field.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

New York, February 21st, 1893.

The seventy-fourth meeting of the Institute was held this date at 12 West 31st Street, and was called to order at 8.30 P. M. by Vice-President Lockwood.

The Secretary announced that at the meeting in the afternoon the Council had officially adopted the design for a badge submitted by the Committee which had been appointed for preparing designs for both badge and certificate. Arrangements had been made for the manufacture of the badges from 14 carat gold, and they could be furnished to the membership at $3.00 each, provided a sufficient quantity are ordered. Members desiring to purchase should order them at their earliest convenience.

The following Associate Members were elected:

Name.	Address.	Endorsed by
ALBERT, HENRY	Superintendent, Royal Light, Heat and Power Co., Front Royal, Va.	T. C. Martin. Geo. M. Phelpe. Ralph W. Pops.
BURNETT, DOUGLASS	Instructor in Physics, Pratt Institute, 42 Livingston St., Brooklyn, N. Y.	Samuel Sheldon. James Hamblet. Chas. E. Emery.
CANFIELD, MILTON C.	Post-Graduate Student of Electrical Engineering, Columbia College, 242 W. 48th St., New York City.	M. I. Pupin. F. B. Crocker. W. H. Freedman.
COWLES, JOSEPH W.	Electrical Engineer, General Electric Co., 620 Atlantic Ave., Boston, Mass.	H. J. Ryan. E. L. Nichols. Louis Bell.
EGGER, ERNST	Electrical Engineer, Eickemeyer and Osterheld, 134 E. 58th St., New York City.	C. P. Steinmetz. Jos. Wetzler. F. W. Tischendoerfer.
LEVIS, MINFORD	Superintendent, Novelty Electric Co., 54 North 4th St., Philadelphia, Pa.	Wm. D. Marks. Edw. J. Houston. L. Knowles Perot.
PARMLY, C. HOWARD	Tutor in Mathematics, College of the City of New York, 344 W. 29th St., New York City.	F. B. Crocker. M. I. Pupin. W. H. Freedman.
RANDOLPH, L. S.	Electrician, Baltimore Electric Refining Co., Canton, Baltimore, Md.	W. H. Peirce. Leonard Waldo. Morgan Brooks.

SANDS, H. S.	Constructing and Consulting Electrical Engineer, Fairmont, W. Va.	Harris J. Ryan. Edw. L. Nichols. Frederick Bedell.
WATERMAN, F. N.	Electrical Engineer, Westinghouse Electric and Mfg. Co., Pittsburg, Pa.	Ludwig Gutmann. Edw. L. Nichols. Alex. Jay Wurts.
WHITE, ANTHONY C.	Electrical Engineer, American Bell Telephone Co., 42 Farnsworth Street, Boston, Mass.	T. D. Lockwood. V. M. Berthold. F. A. Pickernell.

Total 11.

The following Associate Members were transferred to Full Membership, their applications having been approved by the Board of Examiners.

SPERRY, ELMER A.	Electrical Engineer, Sperry Electric Mining Machine Co., Chicago, Ill.
BOSSON, FREDERICK N.	Electrician, Calumet and Hecla Mining Co., Calumet, Michigan.
LEMP, HERMANN, JR.	Electrician, Thomson Electric Welding Co., Lynn, Mass.
PEARSON, F. S.	Chief Engineer, West End Street Railway Co., and Brooklyn City R. R. Co., 439 Albany St., Boston.
BROWN, ALFRED S.	Electrical Engineer, Western Union Telegraph Co., New York City.

Total, 5.

[The following paper on "The Most Economical Age of Incandescent Lamps" was then read by Mr. Carl Hering.]

A paper presented at the seventy-fourth meeting of the American Institute of Electrical Engineers, New York, February 21st, 1893, Vice-President Lockwood in the Chair.

THE MOST ECONOMICAL AGE OF INCANDESCENT LAMPS.

BY CARL HERING.

It is generally supposed that it is an advantage for incandescent lamps to have a long life; the object of this paper is to show that this is a delusion, and that it is not substantiated by facts, but that on the contrary under normal conditions a very much shorter life represents an actual gain in dollars and cents; or in more homely language, if the lamps do not die a natural death, it is more economical to break them at a comparatively early date than it is to keep them in use. The results obtained in the present deductions are so different from what is generally believed, that they will doubtless be discredited by many.

At the June meeting of this Institute last year a very able and valuable paper, [1] was presented by Prof. Thomas and Messrs. Martin and Hassler on "A Life and Efficiency Test of Incandescent Lamps." This paper gives a very complete life-history in the form of data and curves, of representatives of almost all the incandescent lamps made in this country. With this very complete and reliable data as a basis I have endeavored in the present paper to deduce some figures to show what the best life of a lamp is, everything considered. It is well known that lamps deteriorate and blacken with age, becoming less efficient, and it is therefore an important question to know at what age they should be replaced by new ones; it is this point which the present paper is intended to answer.

The method by which the best age is to be deduced, is the new and very rational one suggested by Mr. O'Keenan and described

1. TRANSACTIONS, vol. ix. 1892, p. 271.

in *L'Industrie Electrique*, Nov. 25, 1892, page 510, also in *The Electrical World*, Dec. 24, 1892, page 404. As it is fully described in these articles I will merely state it briefly here. Calculate *for each hour* in the life of a lamp the *total quantity of light* given off by it in candle power hours *from the time the new lamp was first started;* similarly, calculating *for each hour* in the life of a lamp the *total cost* of this light, that is, the original cost of the lamp added to the cost of all the energy consumed by it *from the time the new lamp was first started*, up to that particular hour. Now, it is evident that if for each hour in the life of a lamp we divide this total cost up to that hour by the total quantity of light up to that hour, we will obtain the *average* cost of a candle power hour of light from the beginning up to this hour. From this final data it can readily be shown at what age this total average cost is the least. If based on actual measurements of the lamp made throughout its life, it is evident that this method will give the most rational and only strictly correct way of determining the real cost of the light actually produced, the only factor which it does not include is the interest on the cost of the lamp and power which evidently are negligibly small. It includes the effect of the blackening of the bulb and of the lowering of the efficiency. It should be distinctly understood however that this method is based on the actual amount of light produced, and on the actual cost of the power; whenever therefore either or both of these factors are not of importance, the results deduced here are also of less importance. I desire to emphasize this as it will doubtless be claimed by many, and with right, that the diminution of the light is of less importance to some consumers than the cost of a new lamp would be.

Having given the candle power and the power consumption of a lamp throughout its life, any one can by this method deduce the final results as described. In the present case, however, the work, which is somewhat tedious, is greatly simplified by taking the data from Table V. in Prof. Thomas's paper giving the watts per mean candle power. This data is given here in Table I. for the seven most important of the thirteen makes of lamps; the last column giving the average, is the average of all of the thirteen lamps of that paper and not of the seven which we have here taken as the most important. By using the watts per *one* candle power it is evident that the candle power curve in the present calculations becomes a straight line, thus reducing the work by more than one half.

TABLE I.

	Watts per mean horizontal candle power.							
	A.	B.	F.	G.	K.	L.	M.	Av.
Hours.								
0	3.9	4.4	4.1	4.2	3.9	3.8	4.8	4.2
100	4.3	4.8	4.3	4.6	4.3	4.2	4.7	4.5
200	4.6	5.1	4.6	4.9	4.6	4.6	4.7	4.8
300	4.9	5.4	4.9	5.0	4.8	4.9	4.8	5.0
400	5.2	5.6	5.1	5.3	5.0	5.2	4.9	5.3
500	5.5	6.1	5.5	5.5	5.2	5.5	5.1	5.6
600	5.8	6.4	5.8	5.7	5.4	5.8	5.5	5.9
700	6.1	6.7	6.1	6.0	5.6	6.1	5.8	6.3
800	6.4	6.9	6.4	6.3	5.9	6.3	5.9	6.6
900	6.7	7.1	6.6	6.6	6.2	6.4	6.1	6.8
1,000	6.8	7.1	6.8	6.7	6.4	6.2	6.1	6.8
1,100	7.0	7.2	6.7	6.8	6.5	....	...	7.0
1,200	7.1	7.2	6.7	6.7	6.5	.	..	7.0

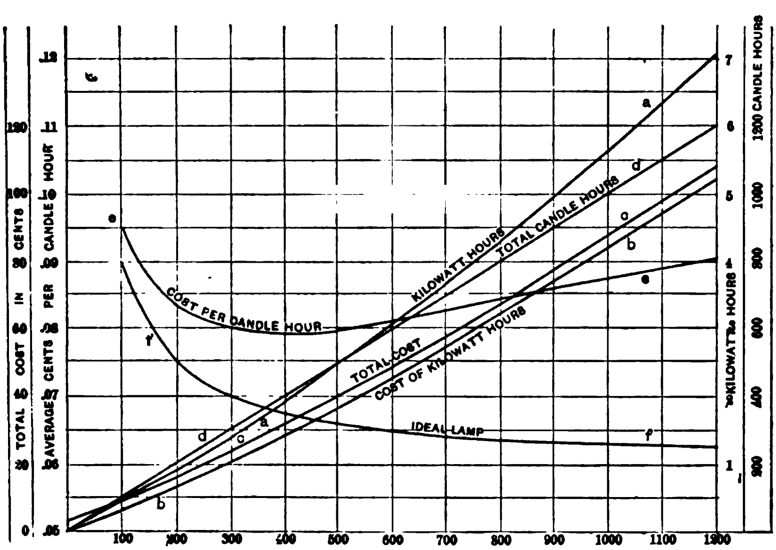

FIG. 1.

To illustrate the method let us deduce the results given for the average lamp in the last column. Instead of calculating the results for every hour, which is a very tedious process, it is sufficiently accurate to do so for every hundred hours. From the last column of Table I, calculate for each hundred hours the total kilowatt-hours consumed per candle up to that age, the result gives the curve *a*, Fig. 1. Multiply these values by the cost of a kilowatt-hour of energy; fifteen cents is the price assumed here; this gives curve *b*. Add to these values the original cost of the lamp *per* *candle*, that is, the total cost of the lamp divided by the candle-power at the start; this gives curve *c* which is evidently parallel to curve *b*. Draw curve *d* representing the total candle-power-hours from the start; in this case this line will evidently be straight; it would be curved if the watts consumed by the whole lamp and if the whole candle-power had been used instead of the watts per one candle. Curves *c* and *d* are those sought for. These curves are evidently the integrals of those represented by the numbers in the table. The process of integrating is effected most readily by purely arithmetical means as described above, that is by simply calculating the totals for each hundred hours and it can, therefore, be performed by any one without a knowledge of algebraic integration. These curves, however, need not be plotted, they are given here only to illustrate the method. Now divide each ordinate of the cost curve *c* by the corresponding ordinate of the light curve *d*, and the result is the curve *e* which is the final curve sought for.

This curve *e* gives for each age the cost of a candle-power-hour of light, that is, *the average cost for the whole time from the time the lamp was first started up to that particular hour.* This is the curve which gives us the final results. The interesting feature about it is that it has a minimum point at about 400 hours, which means that at the end of 400 hours you are paying the least for a candle-power-hour of light, that is to say, your light is then the cheapest during the whole life of the lamp, all things considered. If you continue to use those lamps longer than 400 hours the *average* price for the light will again increase and will continue to do so. Strange as it may seem, therefore, it is more economical to break the lamp at that age rather than to continue running it, even though this means buying a new lamp. This result will naturally be discredited by many, but it is nevertheless true, always remembering that we are concerned with the cost of a candle-power-hour of light under normal conditions and over an indefinite period. If the amount of light is of no great importance, but the price of the lamp is, then run them until they die a natural death; the same if the power cost nothing. If your lamp costs nothing, but your power does, then replace them as often as possible. The effect of different relative costs of lamps and power will be discussed later. The data assumed in this curve is that the average lamp costs 45c. and the power costs 15c. per kilowatt-hour; the latter figure was obtained from a well known central station manager as a fair average for 110 volt continuous current circuits from central stations or for private installations of about 500 lamps; for installations of less than 500 lamps the cost will be greater, and for more it will be less.

As this curve must always have a minimum point for lamps which deteriorate, there is no question that there is such a minimum value in the cost of the light. Mr. O'Keenan calls this point by the appropriate name "smashing point." To show the results of an ideal lamp, curve *f* has been drawn. This ideal 16 C. P. lamp costing 48 cents and consuming 4 watts per candle, is supposed not to blacken or to change in any way. It is evident that in this case there will be no minimum point and the longer such a lamp is run, the cheaper the light is, although after a few hundred hours the cost diminishes very slowly, being practically the cost of the power only.

To show what the curve *e* really means in dollars and cents let us calculate an actual case. If run 1200 hours the average cost per candle-power-hour is .0903 cents or a total of 108.4 cents per candle, for 1200 hours. A 16 C. P. lamp therefore represents a total cost of $17.344 for power and lamp, assuming for the sake of simplicity that it continues to give 16 candle power. If stopped at 400 hours the cost of the light is only .0792 cents per candle hour or a total of 31.7 cents per candle for 400 hours; if renewed three times in the 1200 hours the cost will be 95.1 cents or $15.22 for 16 C. P. This shows that replacing the lamp after every 400 hours represents an actual saving of 13.3 cents per candle during a period of 1200 hours or a total of $2.12 saved per 16 C. P. lamp every 1200 hours. This amount of money represents the cost of almost five lamps at 45c. which might be interpreted so as to show some very curious results, namely, if you bought eight lamps originally, used only three of them for 400 hours each and threw the other five away, it will in the long run, cost you the same as to buy one lamp and run it for 1200 hours, assuming that you are paying for light actually received. As the cost of the light at 400 hours is over 12 per cent. less than that at 1200 hours, it is an actual saving of 12 per cent. in dollars and cents if the lamps are replaced every 400 hours.

Another curious result which it will be hard for many to believe, is shown by the fact that the curve *e* cuts the .09 line at 1200 hours and also at about 130 hours, which shows that the light costs just as much whether the lamp is broken at 130 hours or run for 1200 hours. The curves being flat near their minimum points, shows that it makes practically no difference (always remembering that we are considering the cost of a candle-power-hour of light) whether the lamps are replaced every 300, 400 or 500 hours. It might at first be thought that at 300 hour renewals the lamps will make the cost greater than at 500; but this is not the case as the curve includes the cost of these new lamps. The explanation of this is that we are considering a constant amount of light and if run to 500 hours, more lamps must be used to give the same amount of light owing to their reduced candle power. Of course in practice they will be renewed at 500 instead of 300 hours because the difference in the light would be noticed less than the price paid for the more frequent renewals.

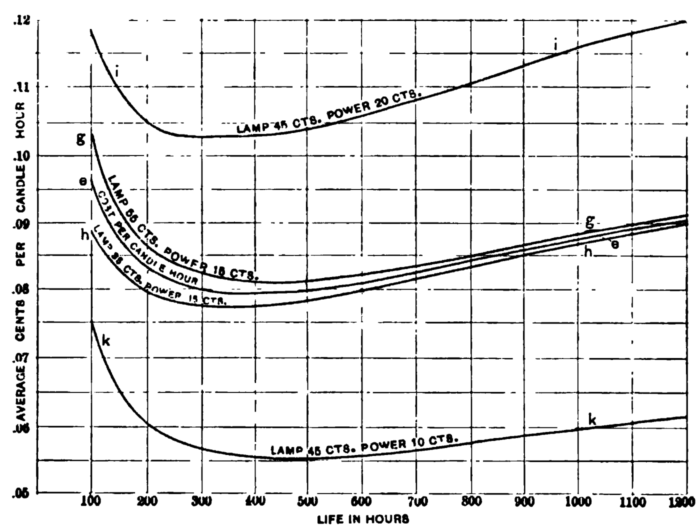

FIG. 2.

Another interesting and somewhat surprising result which may be deduced from the curves, is that an ideal lamp of 16 C. P. which remains constant, may cost originally as much as $5.75 and will then give precisely as cheap light during 1200 hours as the existing lamps, and even then it will be cheaper during the next 1200 hours than the others. An ideal lamp equal to the others at 400 hours instead of 1200 can cost as much as 75 cents and will even then be considerably more economical after those 400 hours. This theoretical deduction only goes to show how very great the value in dollars and cents is of a lamp which remains constant.

To show the effect of the price of the lamp and the price of the power, the curves in Figure 2 have been drawn. Curve *e* is, as before, that for the average lamp costing 45 cents while the power costs 15 cents per kilowatt-hour. In curves *g* and *h* the cost of the lamp was taken at 55 and 35 cents respectively. They show that the minimum point is reached only about 50 hours later when the lamp is 10 cents dearer, and only about 50 hours earlier when it is 10 cents cheaper. The minimum cost is about 2½ per cent. more in the first case, and 2½ per cent less in the second. The cost of the lamp therefore makes comparatively little difference in the best age of the lamp; it affects the cost considerably more at 400 hours than at 1200 hours where it is practically insignificant.

If for the same price of the lamp, namely 45 cents, the cost of the power is taken at 20 and 10 cents, the resulting curves will be *i* and *k* respectively. This shows that the minimum point is about 100 hours earlier in the first case, and 100 hours later in the second. This point is furthermore more decided when the power is more expensive, (see curve *i*) showing that for expensive power it is very much more important to replace lamps earlier, while for cheaper power it will make less difference. For cheap power with cheap lamps the position of the minimum point with respect to time, will be altered very little from that of the mean curve *e*; this is also true for expensive lamps with expensive power. The five curves show the following: That even for considerable ranges the best age remains at between 300 and 500 hours; that the effect of the cost of power is much more important than that of the price of the lamps; that the latter is not of so much importance as is generally believed. All the curves show that while there is a minimum point it is not very definitely located as to time, that is, it makes but very little difference if the lamps are replaced at 300, 400 or 500 hours, but after that the difference becomes apparent.

So far we have been considering the average of all the lamps in Professor Thomas's paper. In order to show the relative differences as well as the absolute values for the different lamps, the curves in Fig. 3 have been drawn for seven of the most important of the thirteen lamps; they refer, though not in order, to the Thomson-Houston, Packard, Edison, Beacon, Perkins and Pennsylvania lamps. The letters of reference are the same as those in Professor Thomas's paper. He selected these seven as the most important, and gave me the price of each lamp. These prices were divided in each case by the initial candle-power of each make, as given in that paper, to obtain the cost prices per candle. These candle powers were as follows: A = 16.0, B = 13.1, F = 14.2, G = 17.1, K = 17.0, L = 13.2, M = 12.5.

A comparison of these curves with those in Professor Thomas's paper will show the effects of certain features. Such a comparison would be far more interesting and instructive were we allowed to know the names of each one of the lamps.[1] We can merely say here that *B* and *G* are evidently poor lamps, while

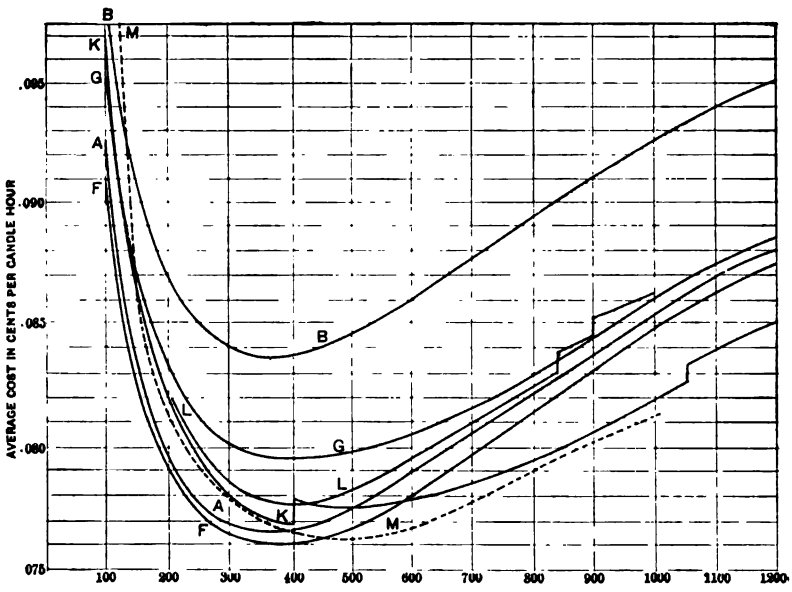

FIG. 3.

M, *F* and *A* are the best, notwithstanding that one of the *M's* died very early, which is equivalent to increasing the price of the other nine lamps. *K* would have been among the best had not one lamp broken at the critical point. The best age of all but *M* are between 300 and 400 hours.

The scale of ordinates in Fig. 3 is much larger than that in Fig. 2 and the minimum point is therefore much more marked. The offsets in curves *K* and *L* are due to breakages among the 10

1. Just before going to press I have received permission from the makers to state that the lamp marked M is the "Pennsylvania" lamp.

lamps of which these results are the mean. Curve *M* is dotted merely to distinguish it from those which it crosses at a number of places.

In addition to these curves attention is called to those shown in *L'Industrie Electrique*, Nov. 25, 1892, page 510, or in *The Electrical World*, Dec. 24, 1892, p. 404, deduced by Mr. O'Keenan for foreign lamps, in which the best age is less uniform and somewhat earlier.

Whatever other deductions may be made from this investigation, the most important is that lamps might to advantage be forced much higher than is customary, at the expense of their life. This, though it requires more frequent renewals, will be a gain to the consumer as well as to the central stations; the former will get better and cheaper light, while the latter will be able to increase their output in lights or run the same number of lights at less loss in the mains, or what is equivalent, run the same number of lamps on considerably smaller mains. How far the lamps may be forced before the other extreme is reached, is a question to be determined when we have the data for such new lamps.

In conclusion I desire to state that while some of the results obtained from the present investigation are somewhat striking, it must not be forgotten that we have been considering the cost of *a candle-power of light*; as it is far more difficult to notice slight differences in the light than it is to notice the bills for lamp renewals, it would not be wise to adhere too strictly to what theory tells us. In practice, therefore, lamps need not be renewed as early as these deductions point out; at the same time however. the results clearly show that it is absurd to keep lamps running too long, and that the life of our present lamps is more than sufficiently good. Perhaps the best practical rule for renewing lamps is to run them until the diminution in candle power becomes noticeable, after that point is reached, we may rest assured that it is economical to throw them away. It might be argued that the original data is of too crude a form to admit of making deductions involving small differences. The differences however are not slight but very apparent, and, I presume, much greater than any possible inaccuracies in the original measurements. Furthermore, they show the existence of a uniform law. This paper should be considered more as an investigation of the subject and not as one claiming to show that present practice is radically wrong. I merely wished to prove conclusively the well known fact, that it is absurd to run lamps too long. By far the most important conclusion is, that it would be far better to force lamps at the expense of their life than to run them at the present efficiency.

COMMUNICATIONS.

PROF. BENJAMIN F. THOMAS:—I am indebted to Mr. Hering for a copy of his very interesting and valuable paper, and for the opportunity to contribute a little to its discussion. All who are interested in any way in incandescent lighting, must, with me, feel under obligation to Mr. Hering for the application of the O'Keenan analysis to the data of the Ohio test, and for his skilful interpretation of the results. The original paper and its discussion at Chicago established the fact that lamps last altogether too long for economical results, and also the fact that the lamp of highest initial efficiency is not always the most economical. Mr. Hering's paper now turns the conclusions there expressed in general terms, into a complete demonstration, and adds other facts which will doubtless prove quite suprising to many who have made lamp questions a study. If the results are properly understood and applied, they must prove to be of great value to the lamp manufacturer, the station manager and his customers, and all who have anything to do with incandescent lamps.

To the owners of isolated plants, Mr. Hering has demonstrated the importance of renewing lamps when they reach the point of minimum cost. It hardly seems right, however, to advise running lamps until they "die a natural death" when the amount of light afforded is not important. Economy considered, it is better to use, in such place, lamps of the lowest allowable candle power, and to renew them as elsewhere.

Central station managers, who furnish lamps to their patrons, without specific charge for lamp renewals, and who charge by meter record, will of course find it pays best to run the lamps just as long as they can, without causing enough dissatisfaction to seriously affect the number of customers they have. A rough calculation shows that even this case may profit by the deductions of Mr. Hering. The manager who runs a lamp 1200 hours, instead of renewing at 400 hours, loses 12 cents of profit which he would have made on the larger current which would have passed through the lamp and meter, if he had renewed at the proper time. The strength of the current which flows through the lamp diminishes as the lamp grows old, and therefore the watt hours which will be recorded, will be less during the last half of the natural life of the lamp than during the first half. This is of course equivalent to decreasing the station out-put, when lamps are burned as long as they will last. If we ever have lamps produced and sold at about 15 cents it will then be best for station managers of the class supposed, to renew at the 400 or 500 hour point.

It is interesting to note that the man who is a customer of such a central station, will find it profitable to buy his own lamps, even though no deduction be made in the price charged by the station. The higher average candle power of the lamps when renewed each 400 hours, will enable him to light his premises equally well with a smaller number of lamps, and the saving in his meter bill will more than pay for all the lamps used, and he will have a more uniform and more satisfactory light. It seems not improbable that the central station of the future will limit itself to supplying current through its mains, the one who buys current furnishing his own lamps, fixtures, wiring, etc., and the fact last considered ought to be helpful in bringing about that state of things.

Mr. Hering's Fig. 3, furnishes a very striking view of the relative values of several lamps as money savers. At 400 hours, lamps A, K, F and M, are very close together. the extreme difference in cost of 1000 candle hours being only 1 cent. Where the difference in cost of light is so small, it matters little which lamp be chosen, unless the user of the light is particular about uniformity of candle power during the 400 hours. Referring to Table IV. of the paper read at Chicago,[1] the percentage of original candle power was, at 400 hours, for A. 74%; for F, 77%; for K, 78%; and for M, 90%. The M lamp is therefore best, when uniformity of candle power is deemed important. It must be borne in mind, however, that the number of lamps of each make tested, was too small to base a sound judgment of relative merit on, and the above comparison must be considered as an illustration only.

PROF. ELIHU THOMSON:—I have received the advance proofs of Mr. Hering's paper on incandescent lamps. I have very little time at present to add anything to the discussion of the matter. I think his points are well taken, but I also think he is quite right in pointing out that they are theoretical rather than practical. I think his practical rule for the running of lamps, which, as stated by the paper, is to run them until the diminution of incandescence becomes noticeable and then discard them, is a very good one. Whether to break them or not is another question. They are of course still capable of being run at a high incandescence for a short time if the voltage be increased. In fact, looking at the matter again from a purely theoretical standpoint, it strikes me that the ideal condition for running is, to start the lamp at the potential for which it is made, and gradually increase the potential so as to keep up a fair or uniform light efficiency regardless of the life of the lamp, letting the lamp go when it will. Of course this is not practical in most systems, but it can be approximated by putting the older lamps on parts of the system which have the highest potential where there exist differences of potential. It also strikes me that the calculations will need to be varied with

1. TRANSACTIONS, vol. ix., p. 271.

every change in the time of running of a lamp; in other words, if out of a set of lamps there are some which only run a short time for each day while others run a much longer time, the time for discarding a lamp will vary, theoretically, in this case. The short-time lamps demand, theoretically, a higher light efficiency throughout their use.

It still remains, after the discussion, that we should not relax ourefforts to obtain a lamp with long life, and one having an economical efficiency during that life. Furthermore, it is evident that the longer the life of the lamp, if the economy of the light production is maintained, the less in proportion becomes the cost of renewal to the other expenses. There is something to be said also on the score of the convenience of use of the long life lamps as against a lamp which runs down rapidly, which, in a discussion like this is liable to be left out of the question. Where lamps require to be renewed very frequently and are somewhat inaccessible in their placing, the long life lamps have an advantage in the saving of labor.

In my opinion, the development of the manufacture of incandescent lamps will give rise to the production of lamps of short and long life to suit the varying conditions in practice and the relative cost of power, to cost of lamp renewals.

PROF. E. P. ROBERTS :—It was my intention to attend the meeting of February 21st; having, however, changed my plans and being very much rushed, I only have time to drop you the following brief memoranda :

1st. An instructive line of investigation, following the method of Mr. Hering's valuable paper, would be to determine the comparative value of lamps "L" 3.8 watts at start, 5.5 at 500 hours and "M" 4.8 at start and 5.1 at 500 hours.

2nd. Undoubtedly, blackened lamps should be either destroyed or used where amount of light is of little consequence, or used for resistance racks.

3d. That as long as lamps have a 90% probability of being operated anywhere from 2% below normal voltage, to from 10% and upwards above that, the practical rule, "Smash blackened lamps," should be supplemented by, "Pay the interest on $75.00 for a first-class portable voltmeter, use same frequently and systematically all over circuits and give the medicine called for by the diagnosis," which might be further supplemented by, "If, after knowing the disease, you will not pay for the medicine necessary to make a cure, do not complain if patients die young."

DISCUSSION.

MR. W. D. WEAVER :—The paper of Mr. Hering is one of unusual value, not only on account of the matter contained but also from the important practical bearing of the subject.

The graphical method which Mr. Hering adopts is very interesting, and the curves enable one to easily follow and thoroughly understand the steps that lead to results that at first sight are rather startling. In this respect the graphical method is superior to an analytical one, but it has the misfortune in this case of being extremely laborious in application, and lacking in accuracy unless the curves are laid down on a large scale.

An inspection of Prof. Thomas's table giving the variation of watts with the life of a lamp shows that the rate is quite uniform in the majority of the tests and numerically equal to an increment of .3 watt per candle power per hundred hours. The regularity in these cases enables an analytical method of remarkable simplicity to be applied to the solution of the various problems considered graphically by Mr. Hering, the development of which is as follows:

Let T = total cost of one-candle power of light for a given number of hours.
" y = average hourly cost during the same period.
" a = cost of lamps per candle power.
" b = cost of one electrical horse-power per hour delivered at the lamp.
" w = initial watts per candle power.
" k = increase of watts per candle power for each hour of the life of the lamp.
" x = hours that the lamp is used.

Assuming that the increase in watts per candle power is by equal increments for equal increments of the time the lamp is burning, the watts being used at the time x are $w + k\,x$, and the total watts used up to the time x are

$$\int (w + k\,x)\,d\,x = w\,x + \frac{k\,x^2}{2}$$

The total cost, including the lamp cost, is therefore

$$T = a + \frac{b\,w\,x}{764} + \frac{b\;k\,x^2}{2 \times 746}$$

The average cost is

$$y = \frac{T}{x} = \frac{a}{x} + \frac{b\,w}{746} + \frac{b\,k\,x}{2 \times 746}$$

which is a minimum when

$$x = \sqrt{\frac{2\,a \times 746}{b\,k}}$$

If A is the cost of a 16-candle power lamp and K the increase of watts per hour for a 16-candle lamp, the formula becomes

$$x = \sqrt{\frac{1492\,A}{b\,K}}$$

The value of k from Professor Thomas's experiments is .003 and consequently $K=.048$; substituting, we have finally for the life, L, or the "smashing point,"

$$L = 176 \sqrt{\frac{A}{b}}$$

By substituting the corresponding values of A and b, the minimum values of the life measured from the final curves laid down in the graphical method, are at once obtained from this formula, and it is remarkable that such a simple equation can replace a graphical method so unusually involved. The accuracy of the method of course depends upon the assumption in regard to the arithmetical increase of watts with the life of the lamp, but an inspection of Table I. shows that in most cases an error in this respect is less likely to cause a serious error in the final result than the graphical method, unless the scale of the latter is large.

A singular deduction from the formula is that the "smashing point" does not vary with the watt efficiency of the lamp. Consideration, however, shows that the factors of the question of efficiency are merely the relation between the hourly lamp cost at a given time, and the increment of lamp energy at the same time, and as this is independent of the watt efficiency of the lamp, the latter does not enter.

As the most economical life varies directly as the square root of the cost of the lamp, and inversely as the square root of the cost of the power, considerable variations in these values will not introduce large ones in the value of L, as the following table shows:

VALUES OF L.

	$b=$ 10 cents.	$b=$ 4 cents.
$A=35$ cents.	333	510
$A=45$ "	370	580
$A=55$ "	404	650

The formula just deduced is based upon a constant value of K. Where this value varies materially, the following arithmetical method, which will be explained by an example, enables the "smashing point" to be determined, and also gives the ordinates of the curve showing the variation of the average cost of the unit light, the same that Mr. Hering constructs graphically.

In the tables below, which are based on lamps A and M of

Prof. Thomas's experiments, the hours the lamp has been used are in column *A*, and the watts at the end of each 100 hours in column *B*. The increase for each 100 hours is in column *C*, and the total increase at the end of a given period in column *D*. The cost of this increase at 15 cents per 1,000 watt hours is given in column *E*, and the average cost per 100 hours in column *F*, while column *G* gives the corresponding average cost of the lamp per C. P. per hundred hours, the cost of a 16 C. P. lamp being assumed 45 cents. Column *H* gives the gain per C. P. per hundred hours from the continued use of the lamp, and column *I* the loss, these quantities being the differences of the values in columns *F* and *G*, and correspond to the ordinates of Mr. Hering's curve E.

Referring to the first table, it will be seen that at 300 hours the average cost of the increased watts required to maintain the unit length, is .8 cent per hundred hours, while an average of .94 cent is saved by continuing the use of the lamp; at the end of 400 hours, however, these quantities are 1.031 and .7 respectively, showing that the cost of the increased power has become greater than the saving from the continued use of the lamp. The exact time when the average increased power cost is equal to the average lamp cost is the "smashing point," and may be determined approximately by a proportion between the quantities for 300 and 400 hours.

The following formula enables the quantities in column *D*, to be quickly calculated:

$$D = \left(n\,a + (n-1)\,b + (n-2)\,c + d + \right) - \frac{a + b + c + d +}{2},$$

where $n =$ the hours in hundreds, and a, b, c, d, etc., the values for 100, 200, 300, 400, etc. hours from column C.

LAMP A.

A	*B*	*C*	*D*	*E*	*F*	*G*	*H*	*I*	
0	3.9	0	0	0	0	0	....	....	Minimum at 324 Hours.
100	4.3	.4	.2	.3	.3	2.81	2.51	....	
200	4.6	.3	.75	1.125	.56	1.40	.84	....	
300	4.9	.3	1.6	2.40	0.80	.94	.14	....	
400	5.2	.3	2.75	4.125	1.03	.70	....	.33	
500	5.5	.3	4.2	6.30	1.26	.56	....	.70	
600	5.8	.3	5.95	8.925	1.48	.46	....	1.02	
700	6.1	.3	8.	12.	1.71	.40	....	1.31	
800	6.4	.3	10.35	15.52	1.94	.35	....	1.59	
900	6.7	.3	13.0	19.5	2.18	.31	....	1.87	
1000	6.8	.1	15.85	23.77	2.37	.28	....	2.09	
1100	7.0	.2	18.85	28.27	2.57	.25	....	2.32	
1200	7.1	.1	22.50	33.75	2,82	.23	....	2.59	

LAMP M.

A	*B*	*C*	*D*	*E*	*F*	*G*	*H*	*I*	
0	4.8	0	0	0	0	....	....	....	Minimum at 680 Hours.
100	4.7	—.1	—.05	—.075	—.075	2.81	2.88	....	
200	4.7	.0	—.15	—.225	—.112	1.40	1.47	....	
300	4.8	.1	.05	.075	.025	.94	.91	.. .	
400	4 9	.1	.2	.3	.075	.70	.62	...	
500	5.1	.2	.5	.75	.15	.56	.41	...	
600	5.5	.4	1.1	1.65	.276	.46	.18	....	
700	5.8	.3	2.05	3.075	.44	.40	....	.04	
800	5.9	.1	3.1	4.65	.58	.35	....	.23	
900	6.1	.2	4.5	6.75	.75	.31	. ..	.44	
1000	6.1	.0	5.9	8.85	.88	.28	....	.60	

Mr. Hering :—The formula given by Mr. Weaver is certainly very interesting. I have not had an opportunity to examine it carefully, but if it is correct, which I do not doubt, it certainly is a valuable formula. But it seems to me that it is applicable only in a general way, that is, it might not show all the characteristics of the different lamps, because it is based on a uniform rate of variation in the efficiency of a lamp. It does not seem to be correct for the two ends of the curve, but that is perhaps of little importance as long as it is correct in the neighborhood of the minimum point and as long as its application is limited to that point only, and not to any other parts of the curve. It seems to me that by using this formula the characteristics of the curves of different lamps, which are caused to a great extent by their different rates of increase of the watts per candle power, would be lost, or in other words, curves constructed for different lamps would probably be parallel; but as the important part is the minimum point, this objection may not be of much importance. I am very glad to see such a simple algebraic formula for determining this point, as it makes such deductions as these much more valuable. It should be remembered, however, that it is to be used only in the neighborhood of the minimum point. Before endorsing the formula, however, it would be well to see how closely the results for each of the different lamps agree with those in Fig. 3. The arithmetical calculations are not as tedious as might at first seem; a curve could probably be calculated in ten or fifteen minutes.

The method which I used for calculating the results is purely arithmetical and not graphical; it is merely the final result which is given graphically because it shows so much more than a column of figures would. Although Mr. Weaver's second method of calculation, shown in the tabulated matter, is interesting, it involves more calculations and more work than the simple arithmetical method which I used; besides, curves appeal to one much more than a column of figures do, particularly when two sets of similar figures are compared with each other. As there is no need of great accuracy in determining the best age, it is not unlikely that the purely arithmetical method, used in the paper, might after all, be the simplest, especially when the data is not given in as convenient a form as in Prof. Thomas's paper.

MR. GEORGE HILL :—We should all feel grateful to Mr. Hering for again calling attention to this important subject, and those of us who have to look after clients' interests, predict probable results and the like, should appreciate these simple curves that will enable us to reach approximate results with considerable accuracy, while the formula that it called forth in the discussion will enable us to come still closer.

Apart from the perhaps, insufficient data on which the conclusions were based, which were insufficient for very exact results or broad generalizations as to costs, but were ample for the underlying truths, the matter of interest should have been considered as one easily introduced and, under certain conditions materially affecting the result.

For example, take the case of a factory running the lamps for a few hours per day during nine months in the year, say the lamps cost $0.35, the power $0.04 per K. W. hour, 16 C. P. lamps, and a life of 500 hours, current would cost $2.27, and interest $0.136, making the total $2.41; this is of course 6% per annum, which is too much to neglect. Or take an office building with some of the lights burning the same number of hours as those just noted, then with power costing $0.15 per K. W., we should have the total cost, including interest at 6%, $8.00 for the 500 hour life, while in the same building there would be lamps burning 9 hours every day, and, as a consequence, being renewed every 55 days for a 500 hour life, the cost of which, taking interest into account, would be for the first, $7.62; for the fifth, at the end of the year say, $7.90, and for the eleventh, or the second year. $8.35; a gain at the end of two years of $0.73, or 9½%. The investigation could be carried along this line much farther with interest, but it is not the line from which the best results can be obtained.

No one who has kept posted has been willing to deny that there was an economical limit or "smashing point" in the life of the lamp and many have thought that it was about 400 to 600 hours; in fact, two years ago I wrote a specification for a plant that we were installing, calling for an initial efficiency of three watts per candle, and a life of 600 hours, which was hardly commercially possible then, although one manufacturer thought that he could comply with the conditions. This was done simply as the result of something that I heard or read and does not deserve any particular credit.

The point that is most desirable of solution in my mind is: How can we take advantage of this knowledge in a practical way in the operation of our plants? How can the manager of the small central station replace the lamps that are not doing their full duty or are taking too much current? In my own house I have some lamps that have been unchanged for nearly five years, burning an average of two hours per day for 270 days in the year, which would be a life of about 2700 hours. They have been satisfactory, as they are all hall lights and not easily reached, and have never been changed.

There should be first, a new curve plotted, or a new formula deduced, with the element of the wages of a lamp collector added, so as to get a new economical life and then, second, some form of apparatus devised that would make it easy to determine the efficiency of each lamp. Possibly the candle power test spoken of by Mr. Hering if applied as a photometric test by the station management would be the simplest, but if we wait until there are complaints about the light, we shall certainly run the lamps far beyond the economic point. Under such conditions we could call for a testing apparatus in each installation and instruct those who were to run it, in its use, so as to do the best that present knowledge permits of for our clients.

Prof. Thomas's conclusion from Mr. Hering's paper is that the development of the future will be in the line of the central station furnishing so many watts of energy to the consumer to do as he pleases with, and he may be right for central station cases, although I am disposed to think that it would be found to be economy to exercise an oversight in these matters, the station taking away the lamps every few months, replacing them with new ones, testing those taken away, rating them and using them on circuits of known duty, and instead of charging for the renewals, lumping it in the charge for current. For the isolated plant, especially in large office buildings, the owner as a matter of course supplies the tenant with light, and there he must look after the lamps himself or waste much energy, since he is only required to supply a certain amount of light, and if the lamps are dull there will be more turned on until finally all the lamps are in use and then when they are insufficient there will be a complaint, and an increase in the output of current of about 100%. Keeping in mind then the lesson of the paper, it will be seen how necessary it is to have some way of watching the lamps that is quickly applied, then the engineer of the building would each month change all of the lamps, test them, put those that are nearly used up in the halls and new ones in the offices, so keeping them always under supervision, and smashing them when the proper time comes.

MR. TOWNSEND WOLCOTT:—Mr. Howell read a paper[1] before the Institute April 10th, 1888, in which it was assumed that the watts per candle power for a given lamp, were constant during its life. Yet, notwithstanding this assumption, it was pointed out in the discussion that a lamp could live too long, its efficiency

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1. TRANSACTIONS, Vol. v, p. 237.

being impaired by blackening. Mr. Howell stated that the relation between cost of power and first cost determined the most economical life of lamps. That is, the greater the cost of power in relation to the first cost, the hotter we can afford to burn the lamps. The cost of lamp manufacture (or at all events, the selling price of lamps) has been reduced since that time, while that of power has not, and it ought to pay to burn them hotter and I should be glad to see that done. I do not consider that a bottled angle worm is a suitable light. The incandescent lamp lacks just a little of being a beautiful light: just when every one begins to admire the light we find that we are away above the voltage marked on the lamp. In some places where lamps are run from existing power, or where the cost of power is not considered, they think more of the cost of lamps than anything else. In one place I have in mind, 112 volt lamps are run from a dynamo which does not give above 110 volts. Where a good light is required the tendency is to use them up quicker. Although Mr. Hering's paper starts from a very different basis from Mr. Howell's, it leads to the same conclusion, so far as the most important point is considered, namely, that we can afford to have better light than we have been getting.

Mr. Hammer:—The paper referred to was read while I was out of the country, and having had occasion to look into the matter recently, I was much interested in the published tests. Some of the tables have a direct bearing upon Mr. Hering's conclusions. One curve shown by Mr. Howell was stated to have cost $10,000, and the tests covered an enormous period of time. In laying down a law and forming conclusions such as shown in Mr. Hering's paper it seems to me they should be based on various types, and upon a large number of lamps tested under the same conditions.

The Chairman:—There is much food for thought in Mr. Hering's paper, and its ideas as elucidated by him, and by the members who have favored us by discussion, are clearly of such practical importance that they merit the mature consideration of all who are interested in illumination by glow lamps, whether producers or consumers.

While the idea of cutting short the life of an incandescent lamp is an absolutely new one to me, I have thought while listening to the discussions that it is one capable of much wider application than the paper suggests. For example, to human beings who have outlived their usefulness and whose life is devoted to making the lives of their fellow-creatures miserable.

My early years were embittered by an aged and crabbed male relative, and I think now, if I had had then the suggestions contained in Mr. Hering's paper I might have taken into consideration the circumstance that it was a waste of energy to keep him going, and that in the interests of law and order, seeing that he had reached that period which Mr. O'Keenan, impelled by the hereditary instincts of his race, calls the "smashing point," it was my duty to disconnect him.

Vice-President Hammer from the Committee on Badge and Certificate exhibited samples of each and made the following report:

Mr. Hammer:—Although this question has been considered by the Council at intervals during the past four years, various committees having been appointed, I personally have had no connection with the matter until the appointment of the present Committee of which I have the honor to be a member. The Council has now officially and unanimously adopted the design as shown to the Institute at its last meeting, with some very slight modifications. The badge will be ready for distribution in about a month. The badge is of 14 karat gold, and the price will be $3.00 each. The design is in the form of Franklin's kite, which demonstrated the identity between lightning and electricity, and is a compliment to America's first electrician and philosopher, Benjamin Franklin. The arms or border of the kite form a diagrammatic representation of the Wheatstone Bridge. In the centre is a tiny galvanometer, the pin thus embodying two important electrical measuring instruments. The galvanometer contains a blued steel needle, above which is a small sheet of amber. Its use is particularly appropriate as our first conception of electricity dates back to 600 years B. C., when Thales, the Greek philosopher, recorded the fact that amber when rubbed attracted like particles to it, and the Greeks worshipped it believing that it possessed a soul and that the gods had endowed it with life.

Amber also represents the derivation of the word electricity as Dr. Gilbert, Court Physician under Queen Elizabeth in the year 1600, A. D., coined the word electricity from the Greek word for amber, which is elektron. Above the galvanometer are the letters A. I. E. E, the initials of the Institute. Below is Ohm's law $C = \frac{E}{R}$.

I wish also to call attention to the certificate which has been submitted by the Committee to the Council and which I take pleasure in presenting to the Institute. It is the result of the labors of your Committee after a careful consideration of the designs used by the various engineering societies. The Committee will be pleased to receive any suggestions or criticisms from the members which they will kindly forward to the Secretary. I might also call attention to the fact that the Council at its meeting this afternoon decided that the design of the pin which was adopted should appear upon the certificate, all papers and other publications and the stationery of the American Institute of Electrical Engineers.

[The following "Note on the Disruptive Strength of Dielectrics," by Mr. Charles P. Steinmetz, was read by the Secretary in the absence of the author.]

A paper presented at the seventy-fourth meeting of the American Institute of Electrical Engineers, New York, February 21, 1893. Vice-President Lockwood in the chair.

NOTE ON THE DISRUPTIVE STRENGTH OF DIELECTRICS.

BY CHAS. PROTEUS STEINMETZ.

When an insulating medium is exposed to a great electrostatic stress, quite independent of its specific electric resistance at a certain E. M. F., disruptive discharge takes place, that is, a spark—followed by the arc, where the supply of electricity is sufficiently large—passes through the dielectric, and such mechanical and other changes take place therein that its resistance is practically reduced to *nil.*

To investigate this phenomenon further, especially to determine the dependence of the sparking distance upon the E. M. F., a number of tests were undertaken.

As E. M. F, an alternating E. M. F. had to be used, since the sources of high continuous potentials, the electrostatic machines, are not capable of giving perceptible currents of electricity, and since at these high potentials a very perceptible escape of electricity into the air takes place, the required constancy of potential in the discharge circuit can be ensured only by the use of a source of electricity capable of yielding perceptible currents. Hence, the required potentials were derived by converting a low pressure alternating current by step-up transformers.

As source of the low pressure current, a small Westinghouse 50 volt and 1 H. P. alternator was used, which was driven by a 3 H. P. Eickemeyer motor[1] with a speed corresponding to a frequency of approximately $N = 150$ complete periods per second. The E. M. F. of the alternator was varied by varying the exciting current, up to 80 ~ 90 volts.

1. The same two machines which were used for the electro-dynamometer tests communicated in previous papers, "On the Law of Hysteresis," vol. ix., p.p. 3, 621.

The potential was increased by means of a special transformer, except in the lowest readings (below 4,000 volts) of the tests made with air, where two small Westinghouse transformers (voltmeter converters) were used. Each of these transformers had two coarse-wire or 50 volt coils, and one fine-wire or 1,000 volt coil, and by suitably connecting the coils, the following ratios of transformation were derived:

$$1 \div 10,\ 1 \div 20,\ 1 \div 40.$$

The discharge plates were connected into the fine-wire circuit, the alternator into the coarse-wire circuit. The voltmeter—an electro-dynamometer[1] was connected across the coarse-wire or primary circuit, and the secondary potential calculated by means of

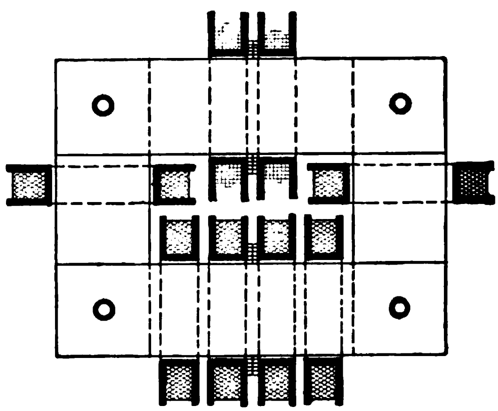

FIG. 1.

the ratio of transformation, which was permissible in this case, since the magnetization was always far from saturation.

For all the other tests, a special transformer was used, which is shown in Fig. 1. This transformer was built up in rectangular form of alternating sheet-iron plates of $12\frac{1}{2}'' \times 3''$ and of $9\frac{1}{2}'' \times 3''$, $3''$ high, giving a cross-section of the magnetic circuit of $4\frac{1}{2} \square'' = 29$ cm.^{2}, and a length of $30'' = 76$ cm. approximately. Eight spools were used, two primary spools of wire No. 10, A. W. G., one of 50 turns—the other of 20 turns. The 20 turn coil was excited by the alternator. The secondary circuit consisted of 6 spools of thoroughly paraffined wood of $\frac{3}{16}''$ thickness, each spool containing 1,000 turns wire No. 22, A. W. G. Usu-

1. Of which the constants are given in the first paper "On the Law of Hysteresis," vol. ix., p. 8.

ally, the secondary coils were all connected in series; for some tests, only 3 coils were used, the two halves of the secondary circuit being connected in parallel.

Since the magnetization was occasionally pushed up to high saturation, where the exciting current became large, and the ratio of turns does not equal the ratio of transformation, and since under these circumstances magnetic leakage is no longer negligible,

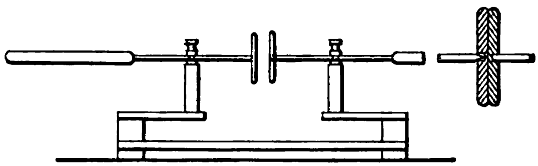

FIG. 2.

the voltmeter was not connected across the primary, but a special voltmeter circuit provided of 20 turns ordinary lamp cord, of which 10 turns were wound between the two primary spools, 10 turns between the middle spools of the secondary circuit, as shown in Fig. 1. The voltmeter was the same electro-dynamometer as in the former tests.

With open secondaries, and 50 primary turns, the equation of

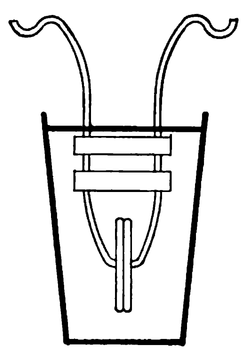

FIG. 3.

this transformer is, at $N = 100$ complete periods per second,

$$V = \frac{1{,}000\ A}{40 + 5.75\ A},$$

where V are the volts, A the amperes of the primary circuit. } (effective.)

With all the six secondary coils connected in series in *short-circuit*, at $N = 100$ complete periods, the equation of the transformer is, approximately,

$$V = 1.33\ A.$$

The E. M. F. of the high potential circuit was calculated from the voltmeter readings by the ratios,

$$1 \div 150 \quad \text{and} \quad 1 \div 300.$$

Since at the frequency $N = 150$ and this arrangement of the secondary coils for the potentials used in the tests, the influence of the capacity is still negligible.

In the tests made with the Westinghouse converters—which date back to July, 1891—as discharge plates, two polished rectangular ($2\frac{1}{2}'' \times 1\frac{1}{2}'' \times \frac{1}{2}''$) Norway iron blocks were used, with a piece of mica of measured thickness between them, which contained a hole of $\frac{1}{8}''$ to $\frac{1}{4}''$ diameter as spark gap.

As electrodes or discharge plates for the high potential converter, two brass disks of $2''$ diameter and $\frac{3}{16}''$ thickness were used, well polished and with their edges carefully rounded off. They were held by two brass rods, or wires, of $\frac{1}{16}''$ diameter on hard rubber pillars, which were fastened on overhanging hard rubber sheets, so that by their elasticity the discharge plates were pressed against each other. The one electrode was fastened on the brass wire, the other rested on a point, to adjust itself to close parallelism. The whole was mounted on a hard rubber plate, as shown in Fig. 2. For testing liquids, the arrangement shown in Fig. 3 was used, consisting of two bent brass wires held $1\frac{1}{2}''$ apart, but pressed together by two soft rubber cross-pieces, and carrying at their lower ends the same discharge plates of $2''$ diameter.

In the tests with air or liquids, the electrodes were kept apart by pieces of glass or mica of measured thickness; in the tests with solid materials, the test piece was laid between the electrodes, which pressed against it.

The tests were made in the following manner:—The test piece was brought between the discharge plates; then the primary circuit of the alternator closed while the exciting current was at its lowest value, and then simultaneously the exciting current increased slowly, and with the other hand the voltmeter needle made to follow the increasing potential, until the test piece broke down, which was noticed by the sudden drop of the voltmeter, due to the short-circuit of the secondary. Then quickly the exciting current was broken, the voltmeter read, the electrodes cleansed, a new test piece adjusted, etc.

By the ratio of the turns of the voltmeter exciting coil, and of the high potential circuit, $1 \div 150$ *vs.* $1 \div 300$, from the effective voltmeter reading, the effective potential at the discharge plates was found, and by assuming the sine law of E. M. F. wave, that is, by multiplication with $\sqrt{2}$, the maximum potential at the electrodes or discharge plates calculated.

The connection between electrodes and secondary coils consisted of two short pieces of rubber insulated wire, which were carried through the air, so that practically no escape of current could take place from them.

The application of the potential always lasted but a short time —not over 15 seconds. Still, all the materials became more or less heated before breaking down.

The exactness of this method of testing is necessarily limited, and the results may possibly contain a constant-error due to the deviation of the E. M. F. from sine shape—so that the maximum E. M. F. is not exactly equal $\sqrt{2} \times$ effective E. M. F.—and an error in the counting of the secondary turns, which, though done very carefully, is unavoidable with such a large number as 6,000 turns. This possible constant-error, however, can amount to a very small percentage only, probably not exceeding two per cent.

Where several tests were made with the same thickness of material, and gave nearly the same result, the average of the readings was taken, and the number of readings marked in the table.

In the following tables, in the first column, the maximum difference of potential where the dielectric broke down, is given in kilo-volts or thousands of volts, as V.

In the second column, the thickness of the dielectric, or the sparking distance, is given in milli-centimetres, or thousandths of centimetres, as $\delta_{\text{obs.}}$

From these two observed quantities, in the third column, the electrostatic gradient $g = \frac{V}{\delta_{\text{obs.}}}$ is calculated, in kilo-volts per centimetre.

From these tests, empirical formulas, expressing the interdependence of V and δ, were calculated, and the values of δ calculated by means of these formulas, given in the fourth column as $\delta_{\text{calc.}}$

The fifth and sixth columns give the difference between $\delta_{calc.}$ and $\delta_{obs.}$ in milli-centimetres and in percentages of $\delta_{calc.}$.

First I tried the equation of the hyperbola,

$$\delta = \frac{a\ V}{b - V},$$

which would give a finite value $V = b$, where δ becomes infinite, that is, discharge takes place into the air. This function did not satisfy the results at all, as seen from the last column of Table I., and within the range of observation, no tendency to electrostatic saturation was noticed. It would give a limiting value of V at about 79 kilo-volts.

With some materials, the tests agreed fairly well with the linear function,

$$\delta = a\,V,$$

showing that at a definite electrostatic gradient, $g = \frac{1}{a}$, the dielectric breaks down, independent of its thickness.

Other materials did not follow this equation at all, but the sparking distance increased quicker than the E. M. F. The addition of a quadratic term,

$$\delta = a\ V + b\ V^2,$$

gave fair agreement with the tests.

A remarkable behavior was noticed with air. For higher E. M. F.'s, the sparking distance in air agreed fairly well with this quadratic law; but even better still with a formula, where the parabola did not start at the origin, but from an E. M. F. of about 650 volts, so that these tests were expressed by

$$\delta = a\ V + b\ V^2 - c.$$

For lower values, below $V = 1{,}500 \sim 2{,}000$, the curve left this parabola and sloped towards the origin.

Consequently, to express the tests over the whole range, a further term has to be added, which disappears for values of V beyond $1{,}500 \sim 2{,}000$ volts. The simplest function of this character is the exponential $c\,e^{-dV}$, and, indeed, the tests made with air, over the whole range, were fairly well—that is, within the errors of observation—expressed by the equation,

$$\delta = c\,(e^{-dV} - 1) + a\ V + b\ V^2.$$

This equation is merely empirical and can consequently be expected to hold only within the range covered by the tests. Still, the terms of this equation may have some theoretical foundation.

The term $a\ V$ gives the electrostatic gradient $g = \frac{1}{a}$, at which the air while at rest under ordinary pressure breaks down.

Mechanical or other changes taking place in the air under the electrostatic stress (for instance, air currents), and being proportional to the E. M. F., may justify the introduction of the quadratic term, $b\ V^2$.

As known, all the solid materials are covered—by molecular attraction—with a thin film of compressed air. The disruptive strength of compressed air is greater than that of air of ordinary pressure. Consequently, these two films of compressed air must behave like an increase of the distance of the electrodes, by that length, c, which is equivalent to the greater disruptive strength of the two films of compressed air covering the electrodes. Hence, the theoretical sparking distance, $a\ V + b\ V^2$, has to be decreased by a constant c, giving

$$\delta = a\ V + b\ V^2 - c.$$

But when approaching the electrodes so closely that the two films of compressed air unite more or less, the constant term c will be more or less decreased. Now, the density of the air near the surface of an attracting body is represented by an exponential function of the form e^{-dx}.

Hence, it is possible that this exponential term, which I noticed only in air, $c\ (e^{-dV} - 1)$, has a physical meaning.

This is corroborated by the numerical values of a, c and d. For, calculating the electrostatic gradient of air for extremely small distances,

$$g_0 = \left(\frac{V}{L}\right)_{V=o} = \left(\frac{V}{c\ (e^{-dV} - 1) + a\ V}\right)_{V=o} = \frac{1}{a - c\,d},$$

we get 139 kilo-volts per cm. Dry fibre, a porous material, gives almost the same value, 130 kilo-volts per cm.

Very interesting luminous effects take place when a thin sheet of good insulating material, as mica, is placed between the electrodes. At a difference of potential of 830 volts and a thickness of mica of 1.8 milli-centimetres, in darkness, a faint bluish glow becomes visible between the mica and the electrodes. This glow is very perceptible at 970 volts, and faintly visible in broad daylight at 1,560 volts. With increasing difference of potential, this bluish glow increases in intensity, forming a sharply defined, smooth, blue line around the electrodes at their point of contact with the mica.

At a difference of potential of 4.5 kilo-volts—thickness of mica of 2.3 milli-centimetres—violet creepers of about 2 mm. length break here and there out of the line of bluish glow. These creepers are distinctly different from the blue glow surrounding the electrodes and increase in number and length with increasing potential, until they form a broad electrostatic aurora surrounding the electrodes on either surface of the mica-sheet, consisting of an infinite number of small violet streamers, rushing with a hissing noise over the mica. This corona increases rapidly in width until it reaches the edges of the mica-sheet. Then white sparks of intense brightness pass from electrode to electrode over the surface of the mica, first few in number, then with increasing potential, covering the whole sheet with an infinite number of streaks of lightning, with a roaring noise. The amount of current passing through these sparks is exceedingly small, for no perceptible reaction upon the primary circuit was noticed. The length of these sparks is many times larger than the sparking distance in air, being tenfold at 17 kilo-volts. They are intensely hot, and leave whitish marks, due to calcination, on the mica when passing over it. The sheet of mica and, especially, the electrodes become heated very rapidly, the mica twists and begins to splinter, to separate into sheets, until finally it breaks down.

To determine the difference of potential required for the production of these sparks, a circular disk of mica, of 7″ diameter and 19 milli-centimetres thickness, was inserted between the electrodes, the E. M. F. increased until the first sparks passed over the edge of the mica, the voltmeter read—then the mica disk cut down to 6″ diameter and the test repeated, etc. These tests are given in Table XIII. and Fig. 8.

The width of the electrostatic corona is half the length of these sparks. The length of these sparks depends somewhat upon frequency and the thickness of the mica-sheet, being greater for higher frequency and thinner mica disk, but apparently only inso-far as the capacity, or, rather, the charging current of the condenser, represented by the mica disk as dielectric, is increased thereby.

Since these sparks behave entirely different from an arc passing between the electrodes, and are usually unable to start an arc, I believe they are merely a condenser phenomenon. That is, I explain it so, that the corona is the charging current of the condenser formed by the mica disk as dielectric, and the whole surface covered by the corona as condenser plate, while, when the corona reaches the edge of the dielectric, disruptive discharge of the condenser takes place, by these sparks passing over the edge of the dielectric.

I do not know whether this explanation really covers the phenomenon, but merely offer it as a suggestion. Of all the high-potential phenomena, this is undoubtedly the most brilliant

Referring now to the tables and figures:

Table I.,	Fig. 4,	Disruptive	Discharge	Through	Air.
II.,	5,	"	"	"	Mica.
III.,	6,	"	"	"	Vulcanized Fibre.
IV.,	6 and 7	"	"	"	Dry Wood Fibre.
V.,	7,	"	"	"	Paraffined Paper.
VI.,	7,	"	"	"	Melted Paraffin.
VII.,		"	"	"	Boiled Linseed Oil.
VIII.,		"	"	"	Turpentine Oil.
IX.,		"	"	"	Copal Varnish.
X.,		"	"	"	Crude Lubricating Oil.
XI.,		"	"	"	Vulcabeston.
XII.,		"	"	"	Asbestos Paper.
XIII.,	Fig. 8,	Luminous Effects and Creeping Discharge.			
XIV.,	Electrostatic Gradients and Specific Electric Resistances.				

In Figs. 6 and 8, the air-curves, and

In Fig. 7, the air and the mica-curves are drawn in dotted lines for comparison.

As seen from Table XIV., the electrostatic gradients and the specific electric resistances have no relation with each other.

As a curiosity, it may be remarked that by extrapolating the sparking distance by means of the empirical formula of Table I.,

$$\delta = 36\,(e^{-1.3\,V} - 1) + 54\,V + 1.2\,V^2$$

we would get for 100,000 volts, a sparking distance of about 7″; for a quarter-million volts, a sparking distance of 3 feet; for one million volts, 40 feet sparking distance; while a lightning stroke of one mile in length would require 11,000,000 volts.

TABLE I.

DISRUPTIVE DISCHARGE THROUGH AIR.

$$\delta = 36\,(e^{-1.3\,V} - 1) + 54\,V + 1.2\,V^2$$

Maximum Difference of Potential in Kilo-Volts. V	Sparking Distance Observed, in Milli-Centimetres. δ obs.	Electrostatic Gradient, in Kilo-Volts per cm. g	Sparking Distance Calculated, in Milli-Centimetres. δ calc.	Difference, $\delta - \delta$ calc. obs. Δ	Difference in Per Cent. of δ calc. %	Remarks. $\delta = \frac{V}{65 - 29\,V}$
.18	3.0	60	2.2	− .8	(−36)	3.0
.26	4.6	57	3.8	− .8	(−21)	4.5
.29	5.1	57	4.5	− .6	−13.3	5.1
.46	8.1	57	8.9	+ .8	+ 9.0	8.9
.48	9.1	53	9.5	+ .4	+ 4.2	9.4
.53	11	48	11.0	0	0	10.7
.71	16	44	17.1	+ 1.1	+ 6.4	16.0
1.43	48	30	49.3	+ 1.3	+ 2.6	(62)
1.76	68	26	66.4	− 1.6	− 2.4	$\delta = \infty$ at $V = 2.3$
2.46	100	25	105.7	+ 5.7	+ 5.4	First term has disappeared.
3.96	190	21	197	+ 7	+ 3.5	$\delta = \frac{V}{17.8 - .226\,V}$
5.5	287	19	297	+10	+ 3.4	(332)
9.5	575	17	584	+ 9	+ 1.6	605
12.7	860	15	844	−16	− 1.9	852
15.7	1150	14	1110	−40	− 3.6	1097
19.6	1440	14	1480	+40	+ 2.7	1457
22.6	1730	13	1800	+70	+ 3.9	1780
24.0	2010	12	1950	−60	− 3.1	1935
						$\delta = \infty$ at $V = 79$
Average				±14.7	± 4.2	

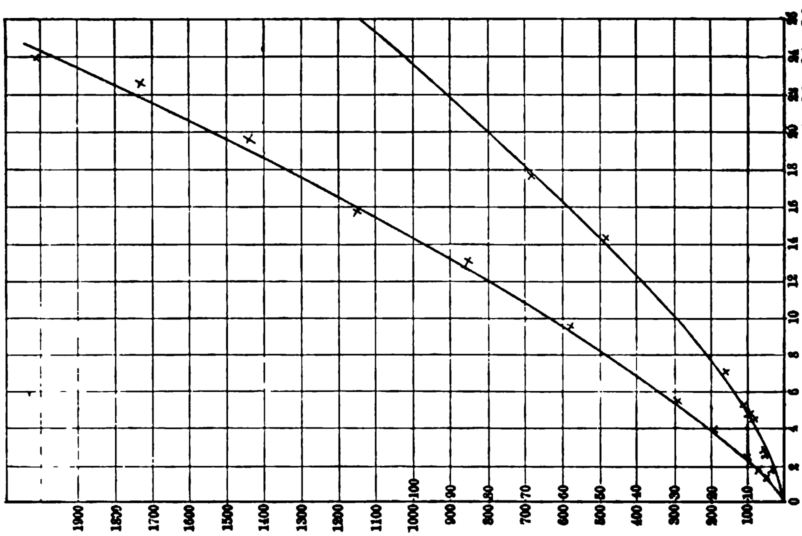

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FIG. 4.—Disruptive discharge through air.

TABLE II.

DISRUPTIVE DISCHARGE THROUGH MICA.

$$\delta = .24\ V + .0145\ V^2$$

Maximum Difference of Potential in Kilo-Volts. V	Sparking Distance Observed, in Milli-Centimetres. δ obs.	Electrostatic Gradient in Mega-Volts per cm. g	Sparking Distance Calculated, in Milli-Centimetres δ calc.	Difference, $\delta - \delta$ calc. obs. Δ	Difference in Per Cent. of δ calc. %	Remarks. Average of
.80	.30	2.67	.20	—.10	(— .50)	1 reading.
4.80	1.50	3.20	1.48	—.02	—1.3	1 "
6.04	2.00	3.02	1.97	—.03	—1.5	2 "
6.55	2.22	2.95	2.18	—.04	—1.9	2 "
7.18	2.44	2.94	2.46	+.02	+ .8	4 "
8.65	3.25	2.66	3.16	—.09	—2.9	2 "
9.66	3.60	2.68	3.66	+.06	+1.6	3 "
11.10	4.55	2.44	4.45	—.10	—2.2	5 "
12.90	5.55	2.33	5.52	—.03	— .5	4 "
16.50	8.00	2.06	7.91	—.09	—1.1	2 "
20.30	11.75	1.72	10.83	(—.92	+8.5)	1 "
12.50	6.20	2.02				1 "
13.60	7.50	1.81				1 "
14.00	8.50	1.65				1 "
16.20	10.00	1.62				1 "
19.20	15.40	1.25				2 "
Average				±.06	±1.5	

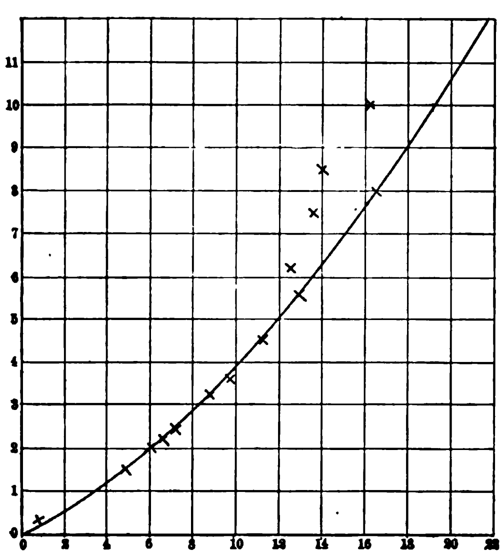

FIG. 5.—Disruptive discharge through mica.

TABLE III.

DISRUPTIVE DISCHARGE THROUGH VULCANIZED FIBRE.

$$\delta = 7.66\ V + 2.3\ V^2$$

Maximum Difference of Potential in Kilo-Volts. V	Sparking Distance Observed in Milli-Centimetres δ obs.	Electrostatic Gradient in Kilo-Volts per cm. g	Sparking Distance Calculated in Milli-Centimetres δ calc.	Difference, $\delta - \delta$ calc. obs. Δ	Difference in Percentage of δ calc. %	Remarks. Average of
2.2	58	35	28	—30	(—107)	
8.8	228	39	246	+18	+7.3	4 readings.
10.1	317	32	312	— 5	—1.6	3 "
14.9	640	23	624	—16	—2.5	4 "
22.5	1280	18	1332	+52	+3.9	
Average				±24	±3.8	

Material has perceptible electric conductivity, being apparently damp.

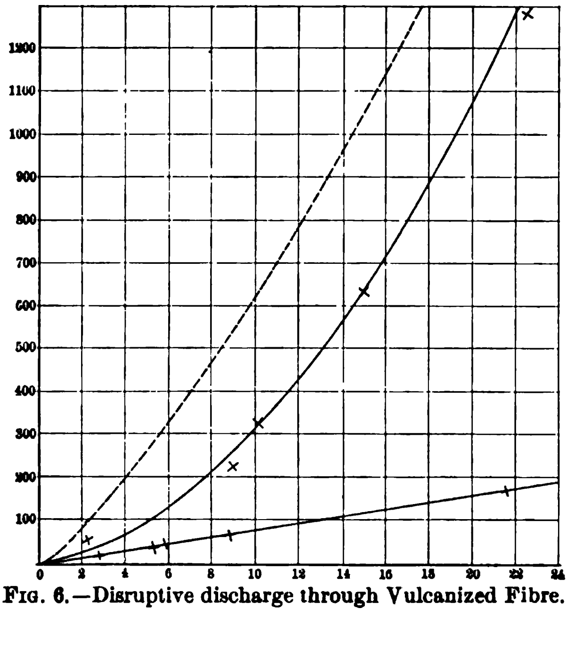

FIG. 6.—Disruptive discharge through Vulcanized Fibre.

TABLE IV.

DISRUPTIVE DISCHARGE THROUGH DRY WOOD FIBRE.

$$\delta = 7.66\ V.$$

Maximum Difference of Potential in Kilo-Volts. V	Sparking Distance Observed in Milli-Centimetres δ obs.	Electrostatic Gradient in Kilo-Volts per cm. g	Sparking Distance Calculated in Milli-Centimetres δ calc.	Difference, $\delta - \delta$ calc. obs. Δ	Difference in Percentage of δ calc. %	Remarks. Average of
2.8	22	127	21.5	− .5	−2.3	3 readings.
5.8	44	132	44.4	+ .4	+ .9	3 "
8.8	66	133	67.6	+1.6	+2.4	2 "
21.6	169	128	165.5	−3.5	−2.1	3 "
Average		130	Average..	±1.5	±1.8	

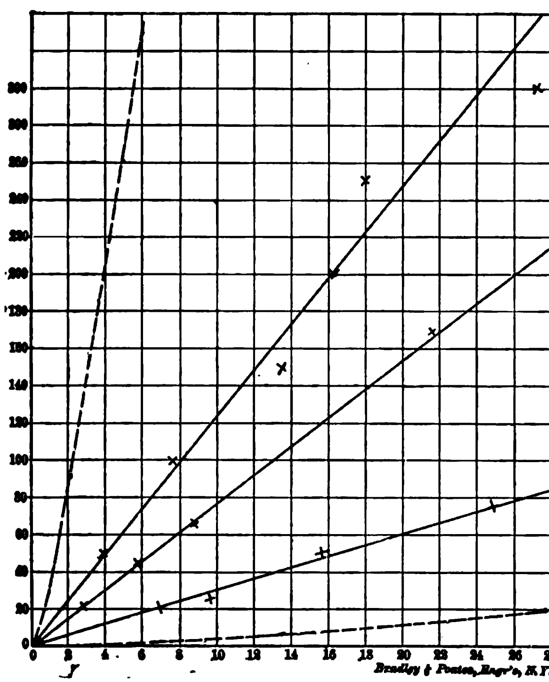

FIG. 7.

TABLE V.

DISRUPTIVE DISCHARGE THROUGH PARAFFINED PAPER.

$$\delta = 3\ V.$$

Maximum Difference of Potential in Kilo-Volts. V	Sparking Distance Observed in Milli-Centimetres δ obs.	Electrostatic Gradient in Kilo-Volts per cm. g	Sparking Distance Calculated in Milli-Centimetres δ calc.	Difference, $\delta - \delta$ calc. obs. Δ	Difference in Per Cent. of δ calc. %	Remarks. Average of
6.9	21	329	20.7	— .3	— 1.4	4 read's, 3 sheets
9.6	25	384	28.8	+3.8	+13.2	3 " 4 "
15.6	50	312	46.8	—3.2	— 6.8	1 " 10 "
24.8	75	331	74.4	— .6	— .8	2 " 15 "
Average		339	Average..	±2.0	± 5.5	

TABLE VI.

DISRUPTIVE DISCHARGE THROUGH MELTED PARAFFIN, 60° TO 70° C.

$$\delta = 12.4\ V.$$

Maximum Difference of Potential in Kilo-Volts. V	Sparking Distance Observed in Milli-Centimetres δ obs.	Electrostatic Gradient in Kilo-Volts per cm. g	Sparking Difference Calculated in Milli-Centimetres δ calc.	Difference, $\delta - \delta$ calc. obs. Δ	Difference in Per Cent. of δ calc. %	Remarks. Average of
3.9	50	78	48	— 2	— 4.2	2 readings.
7.6	100	76	94	— 6	— 6.4	2 "
13.5	150	90	168	+18	+10.7	3 "
16.3	200	82	202	+ 2	+ 1.0	3 "
18.0	250	72	223	—27	—12.1	2 "
27.1	300	90	336	+36	+10.7	1 "
Average		81	Average..	±15	± 7.5	

TABLE VII.

DISRUPTIVE DISCHARGE THROUGH BOILED LINSEED OIL, 21° C.

$\delta = 12.5\ V.$

Maximum Difference of Potential in Kilo-Volts. V	Sparking Distance Observed in Milli-Centimetres δ obs.	Electrostatic Gradient in Kilo-Volts per cm. g	Sparking Distance Calculated in Milli-Centimetres δ calc.	Difference, $\delta - \delta$ calc. obs. Δ	Difference in Per Cent. of δ calc. %	Remarks. Average of
7.6	100	76	95	− 5	−5.3	
12.1	150	81	151	+ 1	+ .7	3 readings.
15.5	200	78	194	− 6	−3.1	4 "
21.3	250	85	266	+16	+6.0	4 "
Average		80	Average..	± 7	±3.8	

TABLE VIII.

DISRUPTIVE DISCHARGE THROUGH TURPENTINE OIL.

$\delta = 15.7\ V.$

Maximum Difference of Potential in Kilo-Volts. V	Sparking Distance Observed in Milli-Centimetres δ obs.	Electrostatic Gradient in Kilo-Volts per cm. g	Sparking Distance Calculated in Milli-Centimetres δ calc.	Difference, $\delta - \delta$ calc. obs. Δ	Difference in Per Cent. of δ calc. %
7.1	125	57	112	−13	−11.6
12.0	167	72	188	+21	+11.2
15.6	250	62	245	− 5	− 2.0
Average		64	Average..	±13	± 8.3

TABLE IX.

DISRUPTIVE DISCHARGE THROUGH COPAL VARNISH.

$\delta = 30\ V.$

Maximum Difference of Potential in Kilo-Volts. V	Sparking Distance Observed in Milli-Centimetres δ obs.	Electrostatic Gradient in Kilo-Volts per cm. g	Sparking Distance Calculated in Milli-Centimetres δ calc.	Difference, $\delta - \delta$ calc. obs. Δ	Difference in Per Cent. of δ calc. %
9.7	300	32	291	− 9	−3.1
20.4	600	34	612	+12	+2.0
Average		33	Average..	± 10	±2.6

TABLE X.

DISRUPTIVE DISCHARGE THROUGH CRUDE LUBRICATING OIL.

$\delta = 60\ V.$

Maximum Difference of Potential in Kilo-Volts. V	Sparking Distance Observed in Milli-Centimetres δ obs.	Electrostatic Gradient in Kilo-Volts per cm. g	Sparking Distance Calculated in Milli-Centimetres δ calc.	Difference, $\delta - \delta$ calc. obs. Δ	Difference in Per Cent. of δ calc. %	Remarks. Average of
4.4	300	14.7	264	−36	−13.3	2 readings.
9.4	600	15.7	564	−36	− 6.4	2 "
15.9	900	17.7	954	+54	+ 5.7	
Average		16.0	Average..	±42	± 8.5	

TABLE XI.

DISRUPTIVE DISCHARGE THROUGH VULCABESTON.

$\delta = 28\ V.$

Maximum Difference of Potential in Kilo-Volts. V	Sparking Distance Observed in Milli-Centimetres δ obs.	Electrostatic Gradient in Kilo-Volts per cm. g	Sparking Distance Calculated in Milli-Centimetres δ calc.	Difference, $\delta - \delta$ calc. obs. Δ	Difference in Per Cent of δ calc. %	Remarks. Average of
4.0	100	40	112	+12	+10.7	3 readings.
6.7	200	34	188	−12	− 6.4	2 "
10.3	300	34	288	−12	− 4.2	2 "
12.6	355	36	353	− 2	− .6	1 "
3.7	156	24	104	(−52)	(−50	6 "
Average		36	Average..	±10	± 5.5	

Gets very hot before breaking down.

TABLE XII.

DISRUPTIVE DISCHARGE THROUGH ASBESTOS PAPER.

$\delta = 23\ V.$

Maximum Difference of Potential in Kilo-Volts. V	Sparking Distance Observed in Milli-Centimetres δ obs.	Electrostatic Gradient in Kilo-Volts per cm. g	Sparking Distance Calculated in Milli-Centimetres δ calc.	Difference $\delta - \delta$ calc. obs. Δ	Difference in Per Cent. of δ calc. %	Remarks. Average of
2.7	60	45	62	+2	+3.2	3 readings.
5.0	120	42	115	−5	−4.4	3 "
Average		43	Average..	±4	±3.8	

TABLE XIII.

LUMINOUS EFFECTS AND CREEPING DISCHARGE.

Width of Electrostatic Corona, $w = 27.5\,(V - 2)^2$.
Length of Creeping Discharge, $d = 55\,(V - 2)^2$.

Maximum Difference of Potential in Kilo-Volts. V	Thickness of Mica Disk in Milli-Centimetres δ	Length of Creeping Discharge in Centimetres d obs.	Width of Electrostatic Corona in Centimetres. w	Electrostatic Gradient in Kilo-Volts per Centimetre. g	Calculated. d calc.	Difference, $d - d$ calc. obs. Δ	Difference in Per Cent. of d calc. %
.83	1.8	Faint bluish glow between electrodes, visible in the dark.					
.97	1.8	Perceptible bluish glow, visible in the dark.					
1.56	1.8	Considerable bluish glow, faintly visible in broad daylight.					
4.50	2.3	Intense bluish glow; begins to send out short creepers of about .2 cm. length. Considerable smell of ozone.					
4.50	2.3	.40	.20	11.25	.34	—.06	(—17.7)
9 60	19	3.28	1.64	2.93	3.18	—.10	— 3.2
12 35	19	5.16	2.58	2.40	5.89	+.73	+12.4
13.55	19	7.66	3.83	1.77	7.34	—.32	— 4.3
15.95	19	10.16	5.08	1.57	10.70	+.54	+ 5.0
17.10	19	12.50	6.25	1.37	12.54	+.04	+ .3
14.90	9	12.50	6.25	1.19			
Average						±.30	± 5.0

TABLE XIV.

ELECTROSTATIC GRADIENTS AND SPECIFIC ELECTRIC RESISTANCE.

MATERIAL.	Electrostatic Gradient at 0 Kilo-Volts, in Kilo-Volts per Centimetre.	5	25	Specific Electric Resistance in Millions of Megohm-Centimetres	Remarks.
Air....	139	16.7	11.9	∞	
Mica	4170	3200	1660	84	
Vulcanized Fibre, red ..	130	52	15.3	1.2 ?	Slightly damp.
Dry Wood Fibre .		130		1.2 ?	
Paraffined Paper		339		34,000	
Melted Paraffin		81			65° C.
Boiled Linseed Oil....		80			21° C.
Turpentine Oil		64			
Copal Varnish		30			
Crude Lubricating Oil (Mineral Oil)		15			Very impure.
Vulcabeston		36			
Asbestos Paper		43			
Creeping Discharge....		10.1	.86		

TABLE XV.

SPARKING DISTANCES IN AIR, IN MILLI-CENTIMETRES.

Difference of Potentials in Kilovolts.	Own Observations.—Alternate Current Transformer; N_1 = 150 Periods; Disks of 5 cm. Diameter.		Alex. Siemens (London).—Alternate-Current Transformer; N_1 = 100 Periods; Disks of 10 and 3.7 cm. Diameter.—(*Electrotechnische Zeitschrift*, 1892, No. 19, p. 246.)		Bourne.—Swinburne Transformer; Plates of 6.3 cm. Diameter.—(*Electrical Engineer*, N. Y., 1892, No. 205, p. 361.)		A. Schneller.—Alternate-Current Transformer; Ratio, 1 + 200.—(*Electrotechnische Zeitschrift*, Germany, 1890, No. 45, p. 589.)		Balls of 3 cm. Diameter.—(Uppenborn, Kalender fur Electrotechniker, 1893, p. 97, Germany.)	
V	δ obs.	δ calc.	δ obs.	δ calc.	δ obs.	δ calc.	δ obs.	δ calc.	δ obs.	δ calc.
1	30	29	24							
2	77	80	48							
3	132	138	73							
4	192	199	105				250	290	90	
5	255	264	139	144	150	165	320	380	116	
6	323	331	171	176			440	470	147	
7	396	401	206	209			600	590	177	
8	468	473	240	242			730	710	208	205
9	540	547	277	278			900	830	240	237
10	622	624	317	316	300	362	1030	960	273	270
12	800	785	400	393			1320	1260	343	341
14	988	955	480	473			1600	1570	416	417
15					570	590				
16	1173	1135	598				1900	1910	483	490
18	1320	1325	755				2200	2280	550	551
20	1476	1524	940		1000	850	2580	2690	615	612
25	2200	2064			1300	1140	3700	3850	768	765
30		2660			1610	1460	5300	5180	918	918
35		3320			1930	1810	7200	6350		
40		4040			2240	2190				
45		4820			2600	2610				
50		5660			3100	3050				
60		7520			4100	4030				
70		9620			5100	5130				
80		11960			6350	6370				
90		15740			7600	7710				
100		17360			9330	9200				
110		20420			11100	10790				
120		23720								
125		25460								
	$\delta = 36\left\{\epsilon^{-1.3V} - 1\right\} + 54V + 1.2V^2$		$5 \le V \le 14$, $\delta = 26V + .555V^2$		$\delta = 30V + .62V^2$		$\delta = 58V + 3.84V^2$		$V \le 14$, $\delta = 30.6V$; $8 \le V \le 14$, $\delta = 20V + .7V^2$	

TABLE XV.—Continued.

SPARKING DISTANCES IN AIR, IN MILLI-CENTIMETRES.

Difference of Potential in Kilovolts.	Balls of 1 cm. Diameter.—(Uppenborn, Kal. f. Electr.)		Balls of .5 cm. Diameter.—(Uppenborn, Kal. f. Electr.)		Warren de la Rue and H. Muller.—Silver—Chloride of Silver—Sal Ammoniac—Zinc Battery.—(Mascart and Joubert, Electricity and Magnetism, Vol. II., p. 198.		S. W. Thomson.—Between Disks.—(Mascart and Joubert, Vol II., p. 195.)		Mascart.—Electrostatic Machine; Balls of 2.2 cm. Diameter.—(Mascart and Joubert, Vol. II., p. 197.		Macfarlane.—(Mascart and Joubert. Vol. II., p. 196.)	
V	δ obs.	δ calc.	δ obs.	δ calc.	δ obs.	δ calc.	δ obs.	δ calc.	δ obs.	δ calc.	δ calc.	δ calc.
1	13		13		21	20	13	17				$V = 20{,}000\sqrt{\delta^2 + .205\,\delta}$, $.1 \le \delta \le 1.0$
2	30		30		43	42	42	39				
3	52		54		66	66	62	66				
4	78		80		91	91	100	97			112	
5	105		108		118	118	127	132	90		135	
6	133		134		147	148					160	
7	162		161		180	180					172	
8	190	190	189		215	213					213	
9	220	220	220		250	248					260	
10	254	250	252		286	287			180		305	
12	320	314	320		365	367					405	
14	388	384	387								505	
15									270			
16	460	458	460								605	
18	538	536	530								705	
20	623	620	608						360		800	
25	940		804						450			
30			1053						570			
35									690			
40									800			
45									900			
50									1100	1090		
60									1700	1700		
70									2430	2430		
80									3300	3300		
90									4370	4290		
100									5800			
110									7500			
120									10350			
125									12600			
	$8 \le V \le 20$; $\delta = 19\,V + .6\,V^2$				$\delta = 18.84\,V + .977\,V^2$		$\delta = 15\,V + 2.3\,V^2$		$50 \le V \le 90$; $\delta = -10.7\,V + .65\,V^2$		$100 \le \delta \le 1000$; $V = 20\sqrt{\delta^2 + 205\,\delta} \times 10^{-3}$	

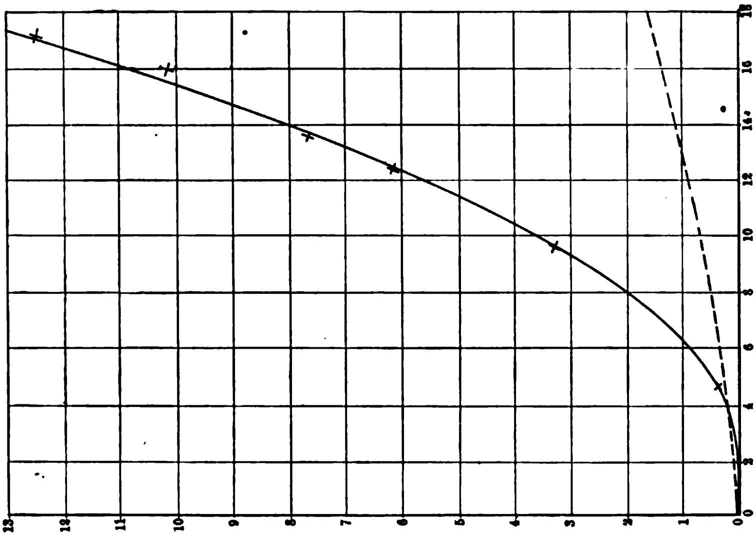

FIG 8.—Luminous Effects and Creeping Discharge.

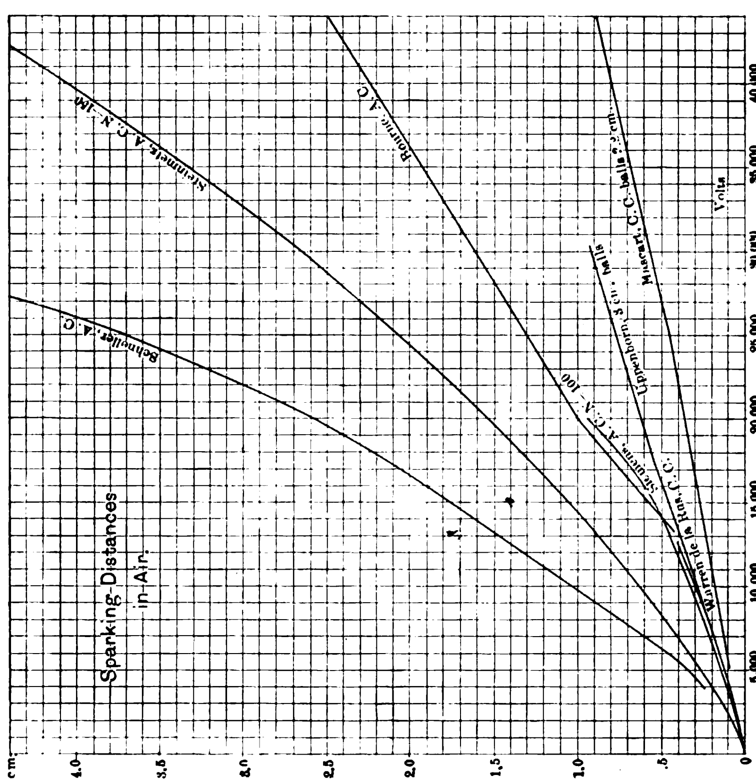

FIG. 9.

Bradley & Poates, Engr's, N. Y.

It will be of interest now to compare the results of the tests, especially of the sparking distance in air, with those found by other experiments. In Table XV. I have collected all the tests which were available to me, and represented a part of them in Fig. 9; with a very discouraging result however, for the values found by different observers disagree seriously.

Conceding even a large margin of uncertainty for the older tests, made by electrostatic machines, and leaving out of consideration those tests, where not sufficient explanation was given to permit critical discussion of the method, the discrepancies are still too large to permit of explanation by errors of observation. The different curves differ considerably in shape; the best and most reliable values however point fairly well to a formula:

$$\delta = a\,V + b\,V^2.$$

The tests made by Warren de la Rue, which were made by the chloride-of-silver battery, and hence are the only continuous potential tests free from the objection due to the electrostatic machine, agree completely with this formula, over the whole range up to 11,330 volts. Bourne's tests by means of alternating potentials, reaching up to 110,000 volts (?) agree fairly well also over the whole range. Other tests again show agreement with a quadratic formula only within a limited range.

Most noticeable, however, is the wide disagreement between the values of different observers, which seems to show that still other factors besides the difference of potential have a decisive influence on the sparking distance. Especially I suspect the frequency, for the values found with continuous potentials show the smallest, those with alternating potentials the largest distances. so that I almost believe that the sparking distance in air is larger with alternating than with continuous potentials, and is the larger, the higher the frequency is. This whole phenomenon of disruptive discharge. however, has been so little studied, in spite of the great importance it begins to assume by reason of the problem of long distance power transmission, that a closer investigation of the manifold phenomena connected therewith is very much to be desired.

Eickemeyer Laboratory, December 1892.

Discussion.

Dr. J. B. Williams:—I am satisfied that the discrepancies referred to by the author are largely due to leakage in the apparatus used by different experimenters, and that the figures obtained by calculation cannot always be accepted as correct. Referring to Mr. Steinmetz's apparatus, in which paraffined wood is used, I would say that this material cannot be relied upon to insulate currents of high voltage unless the wood be absolutely clean and dry before it is saturated with paraffin, and even then it must be used right after the saturation. If wood or any other porous material is saturated with paraffin, the wax rarely, if ever, solidifies in a solid form, but is in itself porous, and in a few hours absorbs moisture. I have demonstrated this fact by scores of experiments wherein dust and other bodies which would cause surface leakage were excluded. In 1886, when I succeeded in making an electrometer the quadrants of which would remain charged at potentials of 3000 volts and upwards for hours, with practically no leakage, I was astonished to find what an amount of leakage there was in the materials ordinarily used for insulating the various parts of testing instruments. Take, for example, hard rubber (ebonite). When this substance is *pure*, *especially made for insulation, and fresh*, its resistance to leakage, both through the mass and over the surface, is very great. But in order that it may *retain* its originally high insulating properties it must be kept scrupulously clean and dry, and also be kept coated with a material having at least as high a specific resistance as itself, which will prevent the deposition of moisture on the surface; and this coating must also be kept clean. Paraffin wax is an example of such a coating. I have seen electrostatic measuring instruments, made by European makers, in which hard rubber was used for insulation, and yet, upon examination, I saw that, owing to the condition, shape, and dimensions of the hard rubber, it could not possibly fulfill the conditions intended by the designers.

The perfect insulation of *any* electrical apparatus is a very important matter, and a difficult one as well, as any one may ascertain for himself if he has instruments which are sufficiently delicate to detect changes in the resistance of the dielectric on, say, six inches of a highly insulated wire, which result from changes of temperature, hygroscopic conditions of the atmosphere, etc.; and I am fully persuaded that if those who give us figures representing, and formulæ for determining, the voltage necessary to produce sparks of given lengths were provided with absolutely insulated apparatus—by "absolutely insulated" I always mean, unless otherwise specified, insulated to such perfection that no leakage can be detected during the time required for any one test, whether this time be five seconds or one hour, and at the highest potential used during the test—including machinery and testing appliances, these discrepancies would not occur. Almost all kinds of glass, if exposed to the air, are practically worthless for insulation when high potential currents or charges are used for testing purposes.

At the November meeting of the Institute it was suggested that the results of the tests of the insulated wires which were sent to the Institute be recorded for the use of the members. As the sizes of the wires and thicknesses of the dielectrics were not uniform, letters were sent by the Secretary to manufacturers of first-class wires requesting them to send short samples of their *best* wires of a specified size and thickness of dielectric. All but four complied. Of the four kinds that were lacking I found one on sale and obtained it. These wires are to be tested at a potential of over 3,000 volts; and I invite any and all who would like to assist me or to witness the tests—which will probably occupy the afternoons of 3 or 4 days—to do so at my office at No. 44 Broadway, Room 518. Notice of the dates of the tests will be given by mail. As the wires will be tested in short lengths I will make a large absolutely insulated air condenser which will not only have a much greater capacity than that of the wires, but will also serve to nullify any effects which might be due to differences in the specific inductive capacity of dielectrics of such short lengths.

I would add that I expect to soon commence a long series of tests upon all kinds of insulating material including insulated wires. The currents to be used in these tests will be alternating, and the periods per second, voltage, etc., can be varied as desired. Every portion of the apparatus used will have absolute insulation whenever necessary. Some of these tests I propose to exhibit before the Institute.

Prof. Francis B. Crocker:—This paper of Mr. Steinmetz is especially interesting to me, as it relates to a subject that I have already done considerable work upon and intend to follow still further. At the December meeting of the Institute when this subject was discussed in a general way, I then stated that it was important to have insulation tests made with a proper *source of current*. Mr. Steinmetz agreed with me entirely at that time. The insulation we require in electrical engineering is not for an electrostatic charge from a Leyden jar or anything of that kind. What we need is insulation which will stand direct or alternating *currents* of high potential and consequently to be reliable, the tests should be made with some precisely similar source of current. If we can get reliable data, the calculation of the thickness of insulation required in any given case will be as definite as that of the size of the conductor, and it would be as important an element in the designing of a dynamo as any other. It would be a very desirable state of affairs if we could be reasonably sure of accurate results in the calculation of the disruptive strength of insulation. In testing insulating materials, the alternating current is, of course, better than the electrostatic charge because of the *power* behind it, but differences will be found in testing with alternating currents due to different frequencies. The *form* of the alternating current wave would probably also affect the results. A perfectly smooth current wave might be a less severe test than a sharp one or *vice versa.* The alternating is a peculiar current, it is a special case. The current which I have been working with is a steady, direct current of high potential. It seems to me that this gives the most perfect conditions. If, however, insulation is to be used for alternating current apparatus then of course the test should be made with alternating currents. I have been using for some time a direct current dynamo of 5,000 volts E. M. F., and have just constructed one of 10,000 volts. In the 5,000-volt machine the remarkable fact is that there are but 32 commutator bars in all. I have run this machine up to 5,500 volts for an hour or more at a time without any trouble. I have measured 500 volts between two adjacent commutator bars at the point of maximum P. D. (half way between the two brushes.) The discrepancies between different experimenters referred to by Mr. Steinmetz, can be explained as Dr. Williams suggests, as being due to leakage, the tests having been made with electrostatic apparatus at a time when the art of insulation was very crude.

DR. WILLIAMS:—If a dielectric will leak at a certain potential when subjected to an electrostatic stress, it will also leak when subjected to the action of a steady current having the same potential.—This is an unscientific way of stating the case, but what I mean is this:—If we find that any dielectric will allow excessive leakage through itself of the small charge which is on the quadrants and a few inches of wire, why subject the same to the action of a steady current having the same potential as that of the charge? We *know* that it will break down if the current is continued long enough, because work is done on the material of the dielectric by the current. Electrostatic tests are simple and quickly performed, and a few inches of an insulated wire or a thin sheet of insulating material having a small area will suffice for the tests. If we test any dielectric by the electrostatic method and find that its specific resistance is low, we know that any wire insulated with the same material and conveying a direct current of the same voltage as that used during the tests would be dangerous. If the wire should suddenly become ruptured at a point distant from the generator, and the same amount of dielectric surface that was used during the tests could come in contact with a grounded body, the thickness of the dielectric on the wire being substantially that of the specimen tested. Conversely, we would be justified in using any suitable material for insulating wires if electrostatic tests showed that the material possessed a high specific resistance. Furthermore this method of testing will enable us to quickly and accurately compare the specific resistances of any two or more kinds of insulating material; and by using an absolutely insulated condenser of known capacity we can also determine the absolute resistance of any specimen of insulating material.

PROF. CROCKER:—An electrostatic pressure of 1000 voltsmight not break down a dielectric that a 500 volt *current* would break down. But that is rather a negative method; we should determine exactly what voltage the insulation will stand under working conditions. Furthermore, Dr. Williams's results do not represent an actual breaking down, but a leakage. Mr. Steinmetz's results are for an actual breaking down. Electrostatic charges may leak through, but may not be sufficient to break down the insulation. It tests the insulation, but not the disruptive strength. In dynamo or transformer construction we should know the point at which the insulation actually breaks down.

MR. HERING:—On page 72 Mr. Steinmetz estimates the voltage of a stroke of lightning one mile long at 11,000,000 volts. It may be of interest to note that Prof. Lodge in his book gives it as 5,000,000,000 volts.

The Chairman announced that since the annual meeting of the Institute, three honorary members had passed away, Cyrus W. Field, Dr. Werner Von Siemens and Dr. Norvin Green, who was the first President of the Institute, and also served a second term. It was now his sad duty to announce the death of George B Prescott, Jr., a member of the Committee on Membership, and of the Committee on Units and Standards. The Secretary had also received notice of the death of James P. Abernethy, who had been a member of the Institute since July 7, 1884, The Council had instructed the Secretary to arrange for the publication in the TRANSACTIONS, of appropriate memoirs of the deceased members.

Adjourned.

[COMMUNICATED AFTER ADJOURNMENT BY PROFESSOR ALEXANDER MACFARLANE.]

I have read with interest the valuable paper of Mr. Steinmetz on Disruptive Discharge through Dielectrics, more especially because I am the author of the investigation quoted in Table XV as taken from Mascart and Joubert's *Electricity and Magnetism*, vol. ii., p. 196. The work mentioned refers to only a few of the results which I obtained. The full record is contained in vols. xxviii. and xxix. of *Transactions of the Royal Society of Edinburgh*, and a summary of the results was published in the Philosophical Magazine for December, 1880.

I used a Holtz machine and a Thomson divided-ring electrometer, but the pole of the Holtz machine and the half-ring of the electrometer were not connected directly; they were each joined to an insulated ball, and these insulated balls were placed far from other conductors and at some distance from one another.

It was not difficult to tell when electricity escaped into the air, and our readings were taken when there was no such escape.

For liquid dielectrics I found the same result as that obtained by Mr. Steinmetz—the difference of potential is proportional to the distance between the plate-electrodes. I found this result for oil of turpentine, paraffin oil, liquefied paraffin, olive oil, and colza oil. This was observed up to a distance of 4 or 5 mm. I also made one observation for the above paraffin in the solid state. A comparison of the results with those of Mr. Steinmetz shows that the one method confirms the other.

ELECTROSTATIC GRADIENT IN KILOVOLTS PER CM.

	Macfarlane	Steinmetz
Oil of turpentine	94	64
Paraffin oil	87	
Olive oil	82	
Paraffin liquefied	56	81
Paraffin solid	130	
Paraffined paper		339
Colza oil [1]		

By taking sparks through different gases between two parallel plates 10 cm. in diameter and 5 mm. apart, I found the following gradients:

ELECTROSTATIC GRADIENT IN KILOVOLTS PER CM.

Air, - -	23.8.
Carbonic acid,	22.7.
Oxygen, -	22.2.
Hydrogen,	15.1.
Coal gas, -	22.3.

In the first of my papers published in the *Transactions of the Royal Society of Edinburgh*, will be found an investigation of the connection between the difference of potential required to pass a spark through air and through hydrogen, and the pressure of the gas, from the pressure of the atmosphere down to the lowest that can be readily obtained by an ordinary air-pump. I found for air

$$V = 13.7 \sqrt{\{p^2 + 202.9\,p\}}$$

and for hydrogen

$$V = 7.2 \sqrt{\{p^2 + 600\,p\}}$$

1. Report of British Association for 1881.

where V is expressed in volts, and p in millimetres of mercury. Messrs. De La Rue and Müller afterwards found the same result for air by their battery method, and Mascart says: "Experiments agree in showing that the law of variation is very closely represented by the branch of a hyperbola, the real axis corresponding to the pressures, and the imaginary axis to differences of potential."

The above comparisons give evidence of satisfactory agreement, how then are we to account for the wide disagreement of the results brought together by Mr. Steinmetz in Table XV under the heading, "Sparking Distance in Air?" He remarks, "Most noticeable, however, is the wide disagreement between the values of different observers, which seems to show that still other factors besides the difference of potential have a decisive influence on the sparking distance." Mr. Steinmetz suspects the frequency, but it is well known that the form of the two conductors is an all-important factor. The distribution of the electricity on the conductors depends on their form. With two parallel disks at a distance apart which is small compared with their diameter, we have a uniform distribution of electricity on the inner surface of the disks, and the equipotential surfaces between the disks ought to be planes parallel to the disks and equidistant from each other. But if the two conductors are balls, or a ball and a disk, the distribution of the electricity on the opposite surfaces is very complex, and the determination of the equipotential surfaces between is not a simple matter.. Hence a strict comparison is possible only when the sparks passed through a uniform field. The results of Sir Wm. Thomson quoted in Table XV were for a uniform field, the plates being 15 cm. in diameter and the greatest distance 1.5 mm., a ratio of 100 to 1. I used similar disks 10 cm. in diameter, and I observed that the spark passed through the uniform part of the field until the distance of the disks was increased to 1 cm. I did not continue the readings further, though the method permitted the observation of higher potentials, because the spark then passed under different conditions. The disks used by Mr. Steinmetz were 5 cm. in diameter, and the greatest distance for which an observation was made was 2.2 cm. A calculation is made for distances up to 25 cm.; but it appears to me to be unwarranted to extend an empirical formula so far beyond the limit of observation. At the greatest observed distance the ratio of the diameter of the disk to the distance between the disks was 2.3 to 1: at our limit it was 10 to 1. Hence the observations of Mr. Steinmetz were carried far beyond the region of uniformity, and the investigation really applies to a very complex field in which the electrostatic force ought not to be constant. It is more nearly akin to the case of the two balls than to the case of the two plates. The conditions of the investigation of Alexander Siemens and of Bourne up to .6 cm. satisfy approximately the conditions of uniformity, and I observe that their measurements agree better with mine.

I also investigated the case where the conductors were two balls, and as early as 1876 (*Proc. R. S. E.*, vol. ix, p. 332) showed that the difference of potential is approximately proportional to the square root of the distance. I subsequently carried the measurements up to a distance of 15 cm., and found that for the greater distances the curve tends to flatten to a straight line. The curve for a ball and a plate, when the distance is extended is not very different. In the case of a point and a plate the parabolic part is soon completed, and the curve becomes straight. At the page quoted in Mascart & Joubert's "Electricity" it is distinctly stated that I found a different result for a ball and a disk and for two balls, namely a parabola, while the hyperbola was true only of the uniform field between two plates.

Among additional results which I found were, that the capacity of the conductor has little or no effect on the difference of potential required for a spark; that heating a platinum wire electrode red-hot diminished the difference of potential in the ratio of 93 to 125; that heating the air and disks from 25° C to 250° C reduced the difference of potential to about one-half; that in the case of a point and a plate, the difference of potential is greater for the point being positive than for being negative; and that the difference of behavior of the gaseous dielectric from the liquid dielectric in the uniform field appeared to be due to their great difference in density.

UNIVERSITY OF TEXAS,
Austin, Tex., March 1st, 1893.

[REPLY TO DISCUSSION, COMMUNICATED AFTER ADJOURNMENT BY MR. CHARLES P. STEINMETZ.]

Having been absent during the presentation of my paper to the Institute, I am permitted to add a few remarks to the discussion in writing.

The results of Dr. J. B. Williams researches are of high interest undoubtedly, but they deal with a distinctly different property of dielectrics, with their electric resistance, which, as I have shown in Table XIV, has nothing whatever to do with the disruptive strength tested by me.

Leakage may have something to do with the discrepancies of tests made by electrostatic machines, but it has no influence upon my tests or the other tests made with "power behind the potential," since it is the very advantage of such tests, that they are independent of the necessity of "absolute insulation," because the engine behind the transformer is fully capable of covering all the losses of electricity due to leakage, and some thousand times more, and still to keep the potential difference constant at the electrodes. Hence it is of no importance whether the paraffined wood afforded absolute insulation or not, since it insulated sufficiently not to short-circuit the secondary.

If I had intended to measure the electric resistance of the dielectric, it would indeed have been necessary to have perfect insulation; for break-down tests, however, it does not matter whether some micro-amperes leak through here and there.

I also beg to disagree with Dr. Williams's statement, that an insulation is worthless if it lets an electrostatic charge escape, because work is done upon the dielectric, and it, therefore, must break down ultimately.

The electrostatic charge represents such an infinitely small amount of energy, that it may leak through the dielectric in practically no time, and still do no harm, for lack of energy. Dr. Williams leaves out of consideration here, that besides the work done upon the dielectric by the leakage current, at least under alternating stress, a very much greater amount of work is done upon the dielectric, outside of all leakage, by what I call *dielectric hysteresis*, a kind of molecular friction in the dielectric under changing electrostatic stress, analogous to magnetic hysteresis.

The effect of dielectric hysteresis upon the insulation no electrostatic tests show, and therefore electrostatic tests can not give us results reliable for alternate current practice.

Take for instance a long concentric cable, conveying alternating currents of say 10,000 volts. Perhaps 1,000, or even 2,000 watts are continuously exerted upon the insulation by dielectric hysteresis. This cable will certainly not break down sooner, if besides the 2,000 watts lost by dielectric hysteresis 10, or 20 watts leak through the insulation. Now suppose you replace the insulating material of this cable by a dielectric of much inferior insulating resistance, but of very small dielectric hysteresis, as oil. An electrostatic charge will disappear almost instantly, and perhaps some hundred watts leakage current act destructively upon the dielectric. But if the dielectric hysteresis of this cable is very low, the total amount of energy exerted upon the dielectric will be much less than in the first cable, and while electrostatic tests show in the first cable an almost absolute insulation, and show the second cable as "worthless," still in practice the first cable will break down very soon, the second stand for years and years.

Hence :—*Leakage* through the insulation is not the only work done upon the dielectric, and in an inferior insulating material not even the greatest, but more work may be expended by dielectric hysteresis, and though air is much superior to oil in insulating resistance, it is very much inferior to oil in dielectric hysteresis, and in disruptive strength; and, therefore, *air cannot compete with oil as high potential insulation.*

Of all this, electrostatic tests show nothing, but give only one quality, and not the most important one either.

The insulation of my transformer was very fair, the wood boiled in paraffin, until all the moisture was expelled, and cooled in the paraffin until it began to solidify, so that the pores were completely filled, and the wood at the outside covered with a layer of paraffin. In consequence thereof, nothing was noticeable when touching one terminal. If, however, the connecting wires between transformer and electrodes were laid on the table, from either electrode small sparks could be drawn by the finger. But still the results were exactly the same, as was to be expected.

Now with regard to the discrepancies noted with air in Table XV, I entirely agree with Prof. A. Macfarlane, that the shape of the electrodes has a considerable influence, especially in a certain range, and have therefore excluded all the tests with pointed electrodes, and brought only some tests with balls as electrodes, without laying great stress upon them. I cannot, however, accept the influence of the shape of electrodes as explanation of the discrepancies, because in this case the differences should be greater for greater sparking distances, and the different curves converge toward each other at small sparking distances, while rather the opposite is the case.

Calculating the curves of electrostatic force between the electrodes shows, that even if the distances of the plates is equal to their radius, the distortion of the field is not yet so great, and in agreement herewith I found that even at a sparking distance of 2.2 cm. the disruptive discharge took place about as often near the centre of the plates as at their edges, so that this can not account for the discrepancies, which amount to over 100 per cent. between the different observers. Besides, if we cancel the tests made at greater sparking distances, the tests up to $V = 10,000$ volts alone, already give the parabolic curve.

The agreement found with liquid dielectrics—which are known to have a very small dielectric hysteresis,—and the disagreement of the tests made with air—which has a very large dielectric hysteresis—rather point to an influence of dielectric hysteresis upon the sparking distance, and once more make the application of tests made under conditions where dielectric hystersis is absent, upon the alternating practice of suspicious value.

The values calculated from Prof. Macfarlane's formula agree with the parabolic formula also,

$$\delta = 19.1\ V + 1.13\ V^2$$

with anaveage difference of .011 cm., or about 4 per cent.

With regard to the extrapolations from my empirical formula, they certainly claim no reliability whatever, an empirical formula being reliable only within the range covered by the tests. I have given the potential calculated for a lightning stroke merely as a curiosity, of interest only in so far as it gives a *very much lower value* than former extrapolations of this kind, and therefore is perhaps somewhat nearer the truth than the former values, of 5,000,000,000 volts, etc. It is of interest, however, to note, that in the E. Thomson high-frequency transformer a sparking distance of about 2 metres has been reached, which according to my formula would correspond to about 400,000 volts, while from the ratio of transformation the E. M. F. has been estimated to about half a million volts.

Furthermore, comparative tests of the sparking distance in air and in oil (A. A. Campbell, *London Electrician*) gave:

2	inch oil equivalent	7.875	inch air.
$1\frac{1}{2}$	" " "	5.125	" "
1	" " "	2.75	" "

extrapolating for air by means of the parabolic formula, gives for oil:

V. in kilovolts.	$\delta_{obs.}$ in milli-centimetres	*g.* in kilovolts p. cm.
108	5080	21.3
83	3810	21.8
56	2540	22.0

that is, the sparking distance in oil is nearly proportional to the potential, a result which is in agreement with tests made at lower potential difference, and thereby confirms the parabolic air-formula to a certain extent.

[REMARKS BY DR. J. B. WILLIAMS, AT MEETING OF MARCH 21, 1893.]

DR. WILLIAMS:—Mr. President: Before the meeting closes I would like to say a word or two; for it is evident that I have been misunderstood—probably from not having been sufficiently explicit—both to my remarks on electrostatic testing, at the last meeting, and to the statements made in my paper on "Oil vs. Air as an Insulating Medium," which was read at the general meeting June 8th, 1892.

At the commencement of my paper, (p. 601 of the Transactions) I say, "The relative powers of the oils and air to resist disruptive discharges, and also to insulate alternating currents, will not be considered at this time, as the writer proposes to reserve those subject for a future paper."

Thus it will be seen that my statements were made with reference to *direct* currents only.

During the discussion at the February meeting, as I began by referring to direct currents, I supposed that it would be understood that my remarks would be confined to the consideration of electrostatic tests, with respect to direct currents. I should not like to be placed on record as one who does not know the difference between the values of the oils and the air as insulators for alternating currents; nor as one who claims that the electrostatic tests heretofore exhibited or described are suitable for insulated conductors, intended to convey alternating currents.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

New York, March 21st, 1893.

The seventy-fifth meeting of the Institute was held this date at 12 West Thirty-first Street, and was called to order at 8.30 P.M., by President Frank J. Sprague.

THE SECRETARY—At the dinner of the Institute, which took place last May, on the occasion of the Annual Meeting, it was voted that an annual dinner be made a regular feature of the Institute, and in accordance with that voted the Council has to-day appointed Messrs. Phelps, Hamilton and Pickernell as a committee on the dinner to take place on the occasion of the Annual Meeting May 16th.

At the Council Meeting this afternoon the following Associate Members were elected :

Name.	Address.	Endorsed by
BUTLER, WILLIAM C.	Monte Cristo Mining Co., Everett, Washington.	Leo Daft. Chas. H. Davis. John W. Howell.
CHISM, GEORGE F.	Standard Engineering Company of North America, 92 State Street, Albany, N. Y.	T. D. Lockwood. V. M. Berthold. I. H. Farnham.
ELY, WM GROSVENOR, JR.,	Edison General Electric Co., 226 Union St., Schenectady, N. Y.	Frederick Bedell. Edw. L. Nichols. Ernest Merritt.
GOLDSBOROUGH, WINDER E.	Adjunct Professor of Electrical Engineering, Arkansas Industrial University, Fayetteville, Ark.	Harris J. Ryan. E. L. Nichols. Harold B. Smith.
HARTMAN, HERBERT T.	Assistant Engineer, Canadian General Electric Co., 69 Front St., Toronto, Ont.	J. H Vail. Chas. Hewitt. A. E. Winchester.
HEATH, HARRY E.	Chief Draughtsman, Eddy Electric Mfg. Co., Box 189, Windsor, Conn.	H. S. Rodgers. Wm. R. C. Corson. Ralph W. Pope.
KEILHOLTZ, P. O.,	Superintendent, U. S. Electric Lighting Co., Baltimore, Md.	Cary T. Hutchinson. T. C. Martin. Joseph Wetzler.
MACKIE, C. P.	Manager, Electric Selector and Signal Co., 45 Broadway, New York City.	H. L. Webb. Joseph Wetzler. T. C. Martin.

McClure, C. E.	Superintendent, First District, So. Bell Telephone and Telegraph Co., Richmond, Va.	G. A. Hamilton. F. A. Pickernell. Thos. D. Lockwood.
Mottram, William T. M.	Electrical Engineer, New Orleans Traction Co., 102 Canal St., New Orleans, La.	Wm. J. Hammer. Francis R. Upton. Oscar T. Crosby.
Robertson, A. C.	President, The A. C. Robertson Co., Electrical Engineers and Contractors, Wilkesbarre, Pa.	M. J. Wightman. H. Bergholtz. Ralph W. Pope.
Spencer, Theodore	Assistant in Mechanical Department, American Bell Telephone Co., 2 Craigie St., Cambridge, Mass.	John C. Lee. H. V. Hayes. Thos D. Lockwood.
Storrs, H. A.	Professor of Electrical Engineering, University of Vermont, Burlington, Vt.	Geo. A. Hamilton. Geo. M. Phelps. F. A. Pickernell.
Zimmerman, L. J.	604 West 46th Street, New York City.	Townsend Wolcott. A. A. Knudson. Jas. B. Williams.

Total, 14.

Transferred from associate to full membership.

Perry, Nelson W.	Editor *Electrical World*, New York City.
Gutmann, Ludwig	Electrical Engineer, Cleveland, Ohio.

Total, 2.

REPORT OF MEETING OF BOARD OF EXAMINERS.

March 15th, 1893.

Present—Messrs. W. B. Vansize, *Chairman*, E. T. Birdsall, G. A. Hamilton 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 Re-considered.	9	1	1	11
New Applications for Transfer Considered.	4	5	4	13
Totals,	13	6	5	24

The President:—I take pleasure in announcing that the paper of the evening is by Dr. Charles E. Emery on "The Cost of Steam Power produced with Engines of different types under Practical Conditions; with Supplement relating to Water Power." Dr. Emery needs no introduction to his associates; he has been for a long time one of the leading members of the American Society of Mechanical Engineers, and a Vice-President, and is one of the best known steam experts in the United States. That he should after going through the ordeal of attaining that enviable position, have taken up electrical studies and become a member of the Institute is, I think, a matter for congratulation. The paper which he will read to-night is on a subject elaborated some years ago by him in a paper of now international importance. I am sure that those who hear it will be fully impressed with the results at which Dr. Emery has arrived.

[Dr. Emery then read the following paper :

A paper presented at the Seventy-fifth Meeting of the American Institute of Electrical Engineers, New York, March 21, 1893, President Sprague in the Chair.

THE COST OF STEAM POWER PRODUCED WITH ENGINES OF DIFFERENT TYPES UNDER PRACTICAL CONDITIONS; WITH SUPPLEMENT RELATING TO WATER POWER.

BY CHAS. E. EMERY, PH. D.

1. The paper of the writer on "The Cost of Steam Power," published in vol. xii, *Trans. Am. Soc. C. E.*, November, 1883, seemed to supply information desired on the general subject by many engineers not practicing in that branch of the profession, and the writer has often been urged to modify the paper to suit more recent conditions. On investigation it appears that the original paper is still substantially correct for the particular purposes to which it was originally applied. It was designed to show the capitalized or present value of steam power in different units maintained forever. The cost of the power in pounds of coal was for the larger engines based on testimony taken relative to large sized condensing engines operating regularly at Fall River, Mass. This was distributed by judgment to the amount of water evaporated in the boilers per pound of coal and required by the engines per horse-power, but such distribution evidently did not affect the final results. The prices of engines and boilers employed were higher than the ruling prices to-day, but these form a small percentage of the total capitalized values. The price of coal employed was also higher than the ruling prices in many localities at present, which directly affects the results, but it is evidently impossible to assume any price for coal which will apply to all locations. For these reasons it is believed that the table can still be employed with advantage by any one sufficiently familiar with the subject to make the necessary corrections for different conditions.

2. It has been decided at the present time, instead of revising the former tables, to compare the cost of developing a given amount of power with several of the different kinds of steam engines now in general use. A unit of 500 net horse-power has been selected for the purpose, though some of the comparisons are on the basis that several such plants are in the same station. In order to make the comparison at the same speed, it is assumed in all cases that the power is delivered at a speed of 250 to 350 revolutions per minute, corresponding to the jack-shaft speed of slow engines and the actual speed of high speed engines.

3. It will be attempted in this presentation to examine all the principal causes which affect the cost of steam power in engines of different types operated under practical conditions, but the substantial equalization of the cost of the power developed with engines of different types and different degrees of economy, when expenses independent of the coal consumed are considered, will necessarily form a prominent feature of the discussion, for the reason that such expenses have frequently been neglected or inadequately discussed, so that their very important bearing on the results is not generally understood. It will be shown that such additional expenses are fairly constant, independent of the type of engine, and that without considering interest or dividends they will in some cases equal the cost of coal. It will be seen, therefore, in comparing two engines both of which are good, so that one, for instance, will only effect a saving of coal compared to the other of, say, 12½%, making the relative costs of fuel as 8 to 7, that, if additional costs equal to the former be added to *both*, the relative economies will be as 16 to 15, and the saving reduced to 6¼% simply by the summation of costs. If then we assume that all expenditures should pay 10% interest or dividends on capital invested, and 10% of the difference in first cost of the engines equals an amount which represents a saving of 6¼% of fuel, then the cost to the owner of the steam plant will be exactly the same in the two cases, since in one case he will pay in additional interest or dividends on the capital invested the same sum as he will pay for additional fuel if he uses the cheaper engine.

4. In general it will be found as in the above illustration that the mere cumulation of costs, other than for fuel or interest, has the greatest effect in reducing the percentage of saving due to a more economical consumption of fuel, and that a consideration

of interest and dividends may simply neutralize or reverse percentages that were already made very small from the first named cause.

5. The original cost of the plant is evidently of great importance, where on one hand money is dear, or on the other coal is cheap or the work irregular. When the interest or dividend charges and others akin thereto are duly considered, it will readily be seen that the number of hours an engine operates in a year has a very important influence on the question of first cost, as such charges for a given steam machinery will be exactly the same whether the latter be operated one hour per day during part of the year, or 24 hours per day for the entire year, and the interest will also be the same whether the coal used cost $1.00 or $10.00 per ton. The saving in the total cost of coal consumed, either during the short hours or at the low price per ton, will evidently not be such as to warrant the adoption of high priced steam machinery.

6. It should be observed that various conditions in addition to those previously named operate to equalize the total cost of different kinds of steam machinery. For instance, with more economical engines smaller boilers are required, so the saving in the cost of the boilers partly compensates for the higher cost of the engines. Again, coal is at the present time cheap compared with former prices, and this is true also of steam engines of ordinary construction, which facts tend to maintain former conditions. On the contrary, steam machinery designed to secure economy, though lower than formerly, is still relatively high. It will be found that a consideration of all the facts available imposes very important conditions in relation to the selection of particular types of steam machinery for a particular duty and location.

7. Principally to obtain uniformity of expression we may preliminarily state, on a somewhat elementary basis, that steam engines at the present time may be divided into two general classes, distinguished as high speed and low speed engines, and though most low speed engines are operated at higher speeds than were employed years ago, there is still a definite distinction, although in many cases the gradations not only appear to reach, but perhaps cross each other. The high speed engines are best distinguished by the fact that they are of comparatively short stroke, and develop approximately the same piston speed as long stroke engines by an increased number of revolutions.

8. We have also simple, compound and triple compound engines of both the high and low speed types, and either of them may be horizontal or vertical. Most low speed engines of the power proposed are of the Corliss type, or at least have much the same general proportions, and some means of automatically cutting off the steam by the action of the governor to produce regulation of speed. There have been so many high speed engines brought out during late years of both the simple and compound type, and triple compound engines are being developed by so many builders, that it will be invidious to mention one name rather than another, though doubtless some engines possess advantages which others do not. In most high speed engines the steam is both distributed and cut off by a lap valve operated by a governor revolving with the main shaft, which acts to move the center of the eccentric in a line transverse to the crank at a distance from the center of the shaft equal to the lead. The effect is to modify the cut-off, substantially as with a link motion, by reducing the throw so that the angular motion required to move the valve through its double lap forms a greater or less proportion of the whole motion and therefore varies the angle of steam admission while the lead is maintained substantially uniform.[1]

9. Either of the above engines may also be operated with the steam escaping into the atmosphere or to a condenser, and in the latter case an air-pump may or may not be used. The first class of engines can no longer be distinguished as "high pressure engines." They are referred to herein as "non-condensing engines." Those of the second class are called "high pressure condensing, without vacuum," while the term "condensing engines" is a general one applied to those in which the steam is condensed and a vacuum formed.

10. Table I, submitted herewith, shows in detail the cost of one horse-power per year developed in engines of different kinds

1. While it is true that small high speed engines have been known for a long time, it is believed, and in relation to one of the principal forms it is known, that the present revival started with a discussion of the subject by the writer for a circular of the Novelty Iron Works about 1868, which, notwithstanding the closing of the works, was preserved to the profession by the late lamented Prof. W. P. Trowbridge, then Vice-President of the Company. See tables and "Formulæ Relating to Non-Condensing Engines," W. P. Trowbridge. N. Y., 1870. The present general adoption of the fly wheel governor revolving with the main shaft is undoubtedly due largely to the late lamented J. C. Hoadley, when at Lawrence, Mass. His successors were Messrs. Armington & Sims. See p. 152, Report Judges Group XX., Cent. Exh. 1876.

when operated for 10 hours per day for 308 days in the year and for 20 hours per day for every day in the year, with columns showing the results in each case for coal costing $2, $3, $4 and $5 per ton. The results are at first presented on the basis that the power required is comparatively steady so that no surplus machinery is required. A second presentation shows the results for electric light and other plants in case 50% surplus machinery be provided to supply the maximum power during certain portions of the day, and the power for the remainder of the day is sufficiently low to maintain the average.

11. In column *b* of Table I will be found the designation of the several types of engines compared, arranged by the amount of fuel required to produce one horse-power. Distinguishing letters are provided at the left in column *a* for convenience of reference. In general the kind of engine employed will be understood from the names at a glance. There are presented on different lines of the table, simple high and low speed engines, both condensing and non-condensing, compound high and low speed engines, both condensing and non-condensing, high speed triple compound engines, condensing and non-condensing. There are also three lines devoted to low speed condensing triple compound engines. Of these, line *J* shows the probable results with machinery designed to secure economy in construction rather than the highest economy of fuel. Line *K* refers to a low speed triple compound engine more expensively constructed, for which the economy is assumed lower than in the other case and for which the results are believed to be the best that can be secured under ordinary average practice even with the best machinery. There has, however, for comparison, been added another line, *L*, assumed to be operated at still lower economy by the use of boilers of unusual economy and careful attention to the details of operation, for which purpose $1 per day is added to the labor account. The results shown in this line are believed to be the maximum which can be obtained under the conditions of unusually good practice with the best care available.

12. It is assumed that the details and proportions of the different types of engines are of a kind which have proved reliable in practice, so that no large allowances are necessary for breakdowns or important repairs.

13. Column *c* shows the indicated horse-power required to produce 500 net horse-power. As previously stated the net power

is assumed as delivered to a shaft revolving at a speed of 250 to 350 revolutions per minute, such for instance as the main shaft of a so-called high speed engine, or the jack-shaft of a low speed engine, consequently the indicated power of the latter must be increased sufficiently to overcome the frictional resistance developed in transmitting the power to the jack-shaft. The total loss due to the friction of the engine and jack-shaft of the slow speed engine is considered to be 10% without reference to its distribution, which is substantially what has been obtained by experiment under average conditions. The friction of the high speed engines is fixed at 8%, which may appear somewhat high for such large engines, but is believed to be warranted by the short stroke, large bearings and high velocities necessarily employed, although it will undoubtedly show smaller in particular engines. In this way 542 is fixed as the indicated horse-power of the high speed engines and 556 as the indicated horse-power of the low speed engines.

14. Column *d* shows the assumed steam pressures as shown by gauge. The pressure regularly carried has been somewhat increased of late years, so that none of the engines are assumed to operate at a less pressure than 100 pounds, and for all the triple compound engines the pressure is assumed to be from 150 to 170 pounds.

15. Columns *e* and *f* relate to the feed water per indicated horse-power per hour; column *e* showing the probable limits within which the feed water required will vary for engines of the types stated, when constructed by different manufacturers or operated under different conditions. The lower limit is believed to have been fixed in each case at the minimum result which has been obtained by reliable experiments with the class of engines referred to; these figures are therefore too low for average practice. The larger figures in column *e* represent results which in the opinion of the writer may be obtained under less favorable but practical circumstances, and of course still larger costs would result from the use of apparatus imperfectly designed or improperly operated. Column *f* shows the feed water per indicated horse-power per hour assumed for comparison. The figures in this column are not intended to be averages of those given in column *e*, but those which can be safely depended upon under conditions of practice, with the load varying between considerable limits, thereby affecting somewhat the economy. It should be stated that the

desire to have these figures decrease progressively where possible has somewhat influenced the values selected, as well as the above considerations, which should be borne in mind in making the comparisons. In cases where the conditions are especially favorable, the results for a type of engine written in one line may be taken from the line below, or by an average of the results written opposite the name of such engine and those on the line below. It is believed, however, from a careful consideration of all the evidence available on the subject that the figures given are all that can be depended upon under average conditions of practice. Engines operating cotton mills or large numbers of small machines of any kind under conditions securing a substantially uniform load will necessarily give nearer the minimum results shown in column *e*, but engines generating electric current for electric railways, or subject to variable loads of any kind will rarely show economies as low as has been assumed for comparison in column *f*.

16. Column *g* shows the commercial horse-power of the boilers required to furnish steam to engines of the indicated power shown in column *c*. The power of a boiler is really represented by the quantity of water it will evaporate under normal conditions. Its rating should not be based on the maximum quantity it will evaporate, for in such case the steam pressure would fall during the operation of replenishing or cleaning the fires. There is really no such thing as an absolute boiler horse-power, for the reason that the quantity of feed water evaporated required by different engines varies through such wide limits. It is, therefore, necessary to arbitrarily fix the rating for boilers based on the evaporation of a definite quantity of water under definite conditions. In the report of the Judges of Group XX, Cent. Exh., the writer called attention to the fact that for economical engines the boilers were generally designed as part of the plant; that for portable engines the boiler and engine were generally attached together, and he suggested that the rated horse-power of a boiler could properly be fixed by the quantity of water required for engines of a class then most largely in use, to wit, automatic cut-off high pressure engines of 80 to 100 horse-power. He therefore, based the calculation relating to the tests of boilers at the Exhibition on the basis that the Commercial Horse-Power of a boiler should be fixed at 30 pounds of water evaporated at 70 pounds pressure at a temperature of 100 degrees. This rating

was adopted by the Judges at that time and has since been adopted by a Committee of the American Society of Mechanical Engineers. The increased duty required by the boiler to evaporate the water at an increased steam pressure is small and may ordinarily be neglected. The assumed temperature of feed water, to wit, 100°, can be readily obtained even with condensing engines, so the commercial horse-power of boilers for the different engines has been found by simply taking the product of the several indicated horse-powers in column *c* and of the water per indicated horse-power in column *f* and dividing each by 30, or the number of pounds of water per horse-power assumed for the rating of boilers. It will be interesting to observe that the high speed non-condensing engines in line *A* require 596 boiler horse-power to produce 500 net horse-power, and that the power of the boilers continually diminishes with the reduction in feed water per horse-power, so that for the case last named, line *L*, only 259 boiler or commercial horse-power is required.

17. While on the subject of boilers we proceed at once to the cost of the boilers given in column *i*. As shown by the heading, the prices stated not only include the original cost of the boilers proper, but the erection and connection of the same, which may add a considerable sum to the original cost, particularly where the work is of such character that steam pressure must be maintained either on one boiler or another at all times during the hours of operation as is desirable in all kinds of manufacture and necessary where public interests are at stake, as in electric work. All steam apparatus needs more or less repair. Stop valves will get off the stems occasionally or obstructions get upon the seats so that they cannot be shut; packing will blow out here and there, joints will become loosened and leaky; in fact, there are numbers of petty difficulties that are likely to arise which will require a particular boiler to be shut off pending repairs, independent of the regular suspension of operations for the purposes of inspection and cleaning. To provide for such contingencies it is necessary to insert plenty of valves in the pipes so that one boiler may be shut off independently of another. As accidents are likely to arise at the valves themselves so as to require shutting off main steam pipes occasionally, the writer felt that it was warranted when building the plant for the New York Steam Company to provide duplicate connections to each boiler so that the main steam pipes themselves could be repaired without suspend-

ing operations. It was done in that case by the use of smaller pipes for the duplicates and during the time they were used exclusively, the steam pressure was carried higher. Very many electric light plants have, however, been made since that time, in which every connection has been duplicated of full size, and valves placed in every branch where it was possibly thought any difficulty would arise likely to cause a suspension of operation. In such case the water feed pipes are also duplicated in the same way. In the work of the writer above referred to, the duplication of the feed pipes was provided for by arranging blow-off valves which could also be used as check valves, by leaving slack in the connection of the stems to the valves and then making connections from the pumps to the blow-off pipes. This arrangement answered the purpose perfectly and in fact proved of greater importance than the auxiliary system of steam pipes. It will be interesting to mention that the latter were kept in operation all the time, and used like what is called the "donkey system" on board ship to supply the pumps and auxiliary connections.

18. There has been some embarrassment in selecting a type of boilers for this comparison on account of the very considerable difference in cost. It has been decided to use a price sufficiently high to cover the cost of the better class of what are termed "safety boilers" or those in which the water is contained in tubes, small chambers or comparatively small shells of unusual strength compared to the large shells of ordinary boilers. Tubular boilers either of the horizontal or vertical type are undoubtedly cheaper in the first place, and since, with equally good combustion in different cases, the economy obtainable is increased slightly by increasing the heating surface available for a given power, ordinary tubular boilers are selected when large ratios of heating surface to grate are desired or boilers of the sectional type employed with economizers at the base of the chimney. Increased surface in tubular boilers chokes the draft. It follows, therefore, that generally the tubular boilers do not have the surplus power of the better class of boilers of the sectional type and that therefore when the power varies greatly and the machinery is to be forced for several hours per day to its utmost, as in electric railway work, a greater H. P. of boilers of the tubular type must in general be provided than of the sectional type. Again, some forms of tubular boilers, although showing high

TABLE II.—Showing the effect on the comparison, in Table I., due to reducing the cost of the boilers $5.00 per Commercial Horse Power.

a	b	H	H₁	L	L₁	P	P₁	T	T₁	a
	TYPE OF ENGINE.	TOTAL COST PER NET HORSE POWER PER YEAR. FIRST CASE. No Surplus Plant, Coal $3.00 per ton.				SECOND CASE. 50 per cent. Surplus, Plant Coal $3.00 per ton.				
Designation.	NAME.	10 hours day.		20 hours day.		10 hours day.		20 hours day.		
		H From Table I.	H Reduced.	L From Table I.	L Reduced.	P From Table I.	P Reduced.	T From Table I.	T Reduced.	
		Dols.	Dols.	Dols.	Dols.	Dols.	Dols.	Dols.	Dols.	
A	Simple High Speed, Non-Condensing	36.17	35.23	70.89	69.95	41.82	40.48	76.54	75.20	A
B	" Low " " "	34.20	33.35	65.55	64.70	40.28	39.08	71.64	70.44	B
C	Compound High " " "	30.86	30.12	59.61	58.87	36.12	35.06	64.87	63.81	C
D	Special Triple Compound High Speed Non-Condensing	30.39	29.70	57.43	56.74	36.28	35.30	63.32	62.34	D
E	Simple High Speed Condensing	27.49	26.87	52.82	52.20	32.32	31.43	57.65	57.65	E
F	" Low " "	26.77	26.19	50.25	49.67	32.07	31.24	55.55	54.72	F
G	Compound High " "	26.36	25.79	49.99	49.42	31.33	30.52	54.96	54.15	G
H	" Low " "	25.53	25.07	47.27	46.81	30.92	30.17	52.66	51.91	H
I	Special Triple Compound High Speed Condensing	24.85	24.37	45.93	45.45	30.20	29.51	51.28	50.59	I
J	Triple Compound Low Speed Condensing	25.27	24.80	45.25	44.78	31.44	30.78	51.42	50.76	J
K	do do	25.32	24.88	44.43	43.99	32.07	31.44	51.18	50.55	K
L	Do. For Probable Maximum Results	24.19	23.78	42.17	41.76	30.81	30.22	48.79	48.20	L

economies under the special condition of steady work and careful attention, are not calculated from their construction to run for a considerable time without attention, and any emergency which prevents regular cleaning, chokes them up so that their performance is very much reduced and inadequacy of boiler power soon becomes evident. This is particularly the case with the cheap and, under certain conditions, very efficient vertical boilers with small tubes. In the earlier form of sectional boilers all parts of the boiler were composed of tubes or small sections which would leak at the joints and relieve the pressure much below the explosive limit of the sections. This kind involved difficulties in the way of disengagement of steam, and were inconvenient to repair, so that in the more recent modified type, selected for comparison, all the water exposed to the direct heat of the fire is contained in tubes or small cells and disengagement of the steam takes place in drums at some distance from the fire. With this form of boiler, dangerous explosions are impossible, even when the water is low, and the cost of maintenance has proved to be very much less than for the ordinary types of boilers. It is thought, therefore, that though boiler explosions are rare, the small risk of an accident is sufficient, particularly when repairs are considered, to warrant the use of an apparatus with which a general disaster is impossible. A comparison is, however, made in Table II for cheaper boilers, which shows that the final result is not due to the price of the boilers selected. It has been assumed that sectional boilers for pressures not exceeding 125 pounds can be purchased and erected in place complete with necessary attachments, furnaces, brick walls, connections to chimney, steam and blow pipes and other connections, with fire tools and fire room fittings in place ready for regular use for $22 per commercial horse-power; similarly the price for boilers erected and connected complete to carry regularly 150 pounds and upward has been fixed at $25 per commercial horse-power.

19. In column *i* the above prices have been reduced to the prices per net horse-power by multiplying the ratio of the commercial horse-power given in column *g* to 500 net horse-power by $22 or $25 in the several cases, when, as would be expected, it will be seen that the price per net horse-power reduces as the economy increases in the same proportion as the commercial horse-power of the boilers required.

20. We now return to column *h*, which shows the cost of engines erected and connected ready for operation. It has been somewhat difficult to obtain satisfactory information for this column. Circulars were issued to different engine manufacturers, and although all responded, and some took special pains to furnish all the information in their power, the resulting data was nevertheless very incomplete, for the reason that few of the makers were prepared to manufacture high speed engines of 500 horse-power, and none of them could give prices throughout the whole list. Again there appeared to be a difference of opinion among the manufacturers as to the *kind* of prices which should be given for such a purpose. Some undoubtedly added a percentage to those for which they regularly sell the same engines in the market, while others evidently stated prices lower than would be furnished under strict specifications. Few of the manufacturers replied to questions as to the cost of erection, stating that such cost would vary greatly with conditions at each location. In some cases, however, the costs of plants complete, including erection, have been obtained which checked well, with estimates made up in detail in substantially the following manner: The costs of the engines at the shop, as furnished by manufacturers, were arranged in order, and the shop price adopted fixed not only by averaging different prices for engines of the same power, but by comparison with prices for engines of different sizes, and in some cases of different types, it being attempted to make a gradation in price from one type to another as nearly as the circumstances warranted. This method is the only practical one, for one manufacturer will quote prices for the same engine to develop either 400 or 500 horse-power, and perhaps in another case at lower pressure or reduced speed even for 300 horse-power. Several engines of different sizes are in some cases made by using the same frame and general details and simply changing the size of the cylinder so that the cost is not greatly modified for quite a range of power. In some cases it was necessary to consider double engines to produce 500 horse-power. The price of engines at the shop had therefore to be derived by digesting all the data based on the several conditions. To these prices were added the cost of foundations as calculated from drawings in some of the circulars, and an allowance was made for pipes, felting and the numerous incidentals which are always required in erecting an engine. The final results, though more

accurate on the average for different parts of the country than would be obtained by a less careful study of the subject, cannot be depended upon for special conditions. It would be the business of any engine manufacturer to claim that he would furnish machinery of a given kind for less than stated, and he could do it by using smaller engines or higher speeds than is customary, or in a variety of ways. The difference in price between an engine at the shop and one fully erected, and in operation, should in all cases be duly considered, and when a large number of engines are grouped on one system of piping for steam and another for exhaust, with condensing apparatus, air pumps and perhaps circulating pumps, the several items of cost mount up quite rapidly. There has been added to the cost of slow speed speed engines the cost of the main belt and a certain portion of the jack-shaft, so as to put the high and low speed types on the same basis. The allowance will not, however, include the expensive clutches frequently employed. The prices for high speed non-condensing engines with boilers complete, check very well with those collected by Mr. Wm. M. Schlesinger for a book he is preparing with Mr. T. C. Martin, of the *Electrical Engineer*, of which advance sheets have been kindly loaned me. Mr. Schlesinger gives the prices of six plants, varying from 250 to 1200 H. P., with high speed engines of various kinds and tubular boilers of various sizes. The total cost varies from $50.40 to $61 per H. P., and averages $53.70 per H. P., which checks well the $58.90 given in Table I for such engines with sectional boilers. A comparison for lower priced boilers is however given in Table II. The price given for triple compound engines with boilers, buildings and details complete, checks well with prices kindly furnished by F. S. Pearson, Esq., engineer of the West End Railway, Boston, for the large plant erected by that company. Mr. Pearson considers that $75.00 per H. P. will be sufficient to complete that work which includes surface condensers not considered in the table.

21. Column *j* shows the approximate cost per net horse-power of the chimney and the buildings required for the steam machinery. These have been based upon the actual cost of various chimneys in actual practice and upon the cost of comparatively cheap buildings to cover the engines and boilers. The prices could be reduced by using very cheap wooden buildings, but are insufficient to provide for elaborate buildings designed to secure architectural effect.

22. Column *k* shows the aggregate cost per net horse-power of the engines, boilers, buildings and chimney as given in the three previous columns.

23. Column *l* shows the amount in column *k* augmented 2½% for inspection and 6% for loss of interest during construction, a total of 8½%, which is designed to cover the expenses of the kind indicated and other contingencies incidental to the construction of a large steam plant or other important work. In starting a company there must be an organization formed in connection with raising the money, procuring the charter and initiating the construction, involving the salaries of several individuals. The engineers to take charge of the work must also be selected before the work is completed and in general very much of the expense must be incurred a considerable time before the plant is put in operation; and even after operation commences, a long time intervenes before everything is working on a sufficiently large scale to secure economy in operation and a return therefrom. Frequently these items are not considered in making estimates. Some of them are of a kind which are not necessarily included in those made by an engineer. Eventually, however, all these expenses must be charged in some way on the books and will usually be placed in the construction account, and it is, therefore, proper to make this charge of 8½% in excess of the ordinary preliminary charges for designs naturally included in construction, with the belief that it will in rare instances be sufficient to provide for the several contingencies referred to.

24. An inspection of column *l* shows unexpectedly small differences in the total cost of steam machinery of different types when everything is considered. It is a curious fact that the machinery which as a whole costs least originally is that of fairly good economy. The total cost of non-condensing steam machinery is, on the basis of performance assumed, higher than for more economical condensing engines, on account of the increased cost of the additional boilers, buildings and chimney made necessary by such reduced economy. If it be considered that the feed water per indicated horse-power per hour for the non-condensing engines is higher than would be obtained in particular cases, a comparison with the minimum quantities in column *e* shows that an assumption of the best possible results will not equalize the total cost of non-condensing and condensing engines of similar construction. The more economical engines near the bottom of

the table are comparatively so high priced that even when the reduced cost of boilers is considered, the total price is greater than for the steam machinery of reasonable economy shown near the middle of the table.

25. In column *m* is shown the weight of water which it is assumed will be evaporated per pound of coal. The assumption is 8½ pounds, except in the last case where it has been fixed at 9½ pounds. Although 9 and 9½ pounds evaporated can be obtained under actual conditions readily with boilers of good proportion and construction, it is thought that 8½ pounds is as much as can be depended upon in average practice. It is in fact greater than is obtained in most cases, though less than is claimed in exceptional ones. This rate must be considered somewhat in relation to the price of coal. If cheap coal is to be used which will not give 8½ pounds evaporation, the results must be sought in the column of the table applying to coal of a higher price.

26. Column *n* shows the coal per indicated horse-power per hour, which is simply deduced by dividing the feed water per horse-power, column *f*, by the feed water evaporated per pound of coal, column *m*. We notice that the coal per indicated horse-power per hour decreases from 3.88 to 1.47 lbs., which is about what should be expected for the various conditions heretofore discussed.

27. We next have in columns *o* and *p* the number of tons of coal, of 2240 pounds, required under the conditions stated; column *o* referring to a working day of 10 hours and column *p* to a working day of 20 hours, with coal for one hour added in each case for starting and stopping fires.

28. We have next in column *q* to *x* inclusive, the cost of coal per horse-power per year at the rates of $2, $3, $4 and $5 per ton; first on the basis of 308 days of 10 hours each, and second on the basis of 365 days of 20 hours each.

29. We have next in columns *y* and *z* the cost per net horse-power per year of supplies and average repairs. There has been allowed 12 cents per day of 10 hours for supplies and 29 cents for repairs, or 41 cents total for a 500 H. P. slow speed engines. These costs are based on testimony in the Fall River suit mentioned in the paper previously referred to. This amount has been increased to 48 cents for the high speed engines, which will probably be insufficient in many cases, but has been assumed correct for the better types.

30. We next have in columns *A* and *B* the cost per net horse-power per year for wages based on $3 per day of 10 hours for an engineer and $4 per day for 500 commercial or boiler horse-power for fireman and the labor incidental to passing the coal and disposing of the ashes. The latter amount has been entered fractionally, for although this would be impossible for a single unit it is the only way to obtain a correct progression when considering each unit as a part of a much larger plant. The cost amounts to $4.77 per day for simple engine in line *A* and $2.22 for compound engine in line *K*.

31. In column *C* is given the cost of insurance, taxes and renewals per horse-power per year. This is obtained on a basis of ½ of 1% for insurance, which is too low for an electric light plant, and in fact too low for other manufacturing operations except those insured on a mutual plan. Taxes have been assumed at $15 per thousand on three-fourths of the total cost, equivalent to 1.2%. The renewals for engines, boilers and buildings all averaged together have been assumed at 3.3%, making a total of 5%. The several percentages are believed to be low for general practice, but have been used in the first part of Table I so as to keep within bounds, the influence which quantities proportioned to the cost have on the result.

32. We have in column *D* the amount per horse-power per year to be provided for interest and dividends which has been fixed at the moderate rate of 10% on the capital invested. It should be realized that the interest account should not simply cover the interest on money borrowed or invested to do the work, but a percentage sufficient to pay expected dividends on the capital; in fact, interest and dividends must be considered together. In this country, at least, active business men demand at least 10% on their investments. It may be that they will borrow the money actually required to do the greater part of the work at a comparatively low rate of interest, say 5% to 6%, which the business is expected to pay in addition to a dividend of an equal or greater amount to recompense the promoters for their trouble and responsibility in the matter, so that 10% allowance for interest and dividends is none too much in a growing country. Indeed, it is altogether too small for operations in which the actual value of the plant is represented by a greatly increased face value of securities. Evidently the payment of 5% interest on a bonded issue equal to the cost of construction and 5% dividends

on an equal amount of stock to control the company, or any similar financial scheme based on the same total, is equivalent to an allowance of 10% for interest and dividends, which is the basis assumed for comparison, and this percentage must of course be paid on all expenditures including everything relating not only to the purchase, but the erection of the various details of the steam machinery.

33. Columns *E* and *F* merely sum up the cost per horse-power per year of all operating, current and interest expenditures, except coal, and include therefore the cost of wages, columns *A* and *B;* cost of insurance, taxes and renewals, column *C;* and the interest or dividends, column *D.*

34. We next have in columns *G* to *N* inclusive the *total* cost per net horse-power per year on the basis shown in the previous columns, for coal at $2, $3, $4 and $5 per ton, first for 308 days of 10 hours each, and second for 365 days of 20 hours each. An examination of the several columns shows clearly that for cheap fuel and short hours the engines of fair economy and least cost give the most economical results when both the cost of fuel and collateral and interest charges are considered. Such a result would be anticipated in comparison with non-condensing engines, but *it is somewhat surprising to find that the compound engines of comparatively moderate price show better economy, everything considered, than the higher priced triple compound engines*, if we reject the results shown in the last line, which, as already stated, it is believed cannot be obtained in average practice. For the 10 hours day, with coal at $2 per ton, the lowest result is, for the assumed conditions, shown on line *I*, referring to special triple compound high speed condensing engines. Unfortunately more conditions have had to be assumed in relation to this type of engine than for any of the others. They are being made specially for electrical purposes of extra weight and with extra length of bearings, and the prices available would with proper allowance for erection give prices higher than stated. However, the result is very little different from that shown in lines *G* and *H* for compound engines high and low speed, or even for the simple low speed condensing engine line *F*, on the one hand, or the triple compound, lines *J* and *K*, on the other. This similarity in final cost is certainly very interesting, and examining columns *H*, *I* and *J*, referring to coal at $3, $4 and $5 per ton, we find that although the total cost per year in-

creases, the relative cost for engines of different kinds varies but little. At the $5 rate, the high speed compound engine, line *G*, has fallen $1.43 per horse-power per year behind the low speed compound engine, line *H*, and $2.67 behind the high speed triple compound, line *I*, on basis assumed, but the latter with its lower assumed original price and higher coal consumption is holding its own substantially with the higher priced compound engine, line *J*. The same relations practically hold for 20 hours per day with cheap coal, and it is not until we reach column *N* for 20 hours per day and coal at $5 per ton that the higher priced engines (rejecting as before line *L*) show any decided superiority, and even under these circumstances the difference is comparatively not great.

35. We next examine columns *O* to *V* inclusive, which show the total cost per net horse-power per year for electric railroad and other variable work requiring 50% extra plant to obtain on the average 500 horse-power. That is, it must be supposed that a 750 horse-power plant, or the larger portion of the same, leaving one unit, perhaps, for emergencies, is employed during the times of heavy traffic, and that at other times the power is less than 500 horse-power so as to maintain the average. In making this comparison, the cost of fuel and labor per average horse-power has not been increased. Very great variations of load would certainly change these items, but as all conditions cannot be provided for in one table, it may be assumed that the charge for fuel in the first part of the table is rather high for uniform loads, and that charges for both fuel and labor have not been exceeded for the conditions involving surplus plant in the second part of table. The insurance in the latter case has, however, been increased to 1½% as an average on engines, boilers and buildings. The engine renewals have also been increased to 4% and the boiler renewals to 5%. These latter allowances are believed to be more nearly correct than those first assumed for many kinds of work. There are individual cases of boilers and engines lasting a long time, but in many cases the necessary changes in the character of the work done, requires a change of steam plant, so it is safer to charge off for renewals a sufficient sum to provide for conditions other than the actual wearing out of the apparatus. In any case we think it will be granted that these allowances are none too large for electric light plants. The comparisons are as before on the basis of coal costing $2, $3, $4

and $5 per ton, first for 308 days of 10 hours each and second for 365 days of 20 hours each.

36. Examining columns *O* to *V*, we are surprised at once to find that, although the costs have necessarily all been raised, the general relations for the different engines have been very little modified. For short hours and low priced coal the medium priced engines show, if anything, still better results than on the previous basis, and the higher priced engines only show to advantage for long hours and high priced coal, as shown in column *V*. The results even in this column for the engines referred to in the last four lines (excluding *L* as before) are remarkably near uniformity.

37. Before drawing further conclusions, it is desirable to ascertain the effect that will be had on the results by the use of cheap boilers. As has been stated, it is well known that boilers are to be had in the market at a cheaper rate per horse-power than the safety boilers which have been assumed. A mere examination of quoted prices would make it appear that such boilers can be purchased for $10 less per horse-power than the safety boilers, but the better types of the latter have much more surplus power than ordinary types of boilers, so much so in fact, that no allowance has been made in previous calculations for a greater boiler power than is required to supply engines with steam, as it is believed that the better class of safety boilers have so much surplus power that in large plants one or two can be laid off for cleaning purposes and the others take up the load. Even less than the rated power can be purchased in starting a plant, and no injury follow from forcing such boilers so as to obtain the power desired. When, however, boilers with small tubes, which furnish heating surface very rapidly at reduced cost, are employed, in general a higher rated horse-power must be furnished for a particular case, either originally or soon after operations are started. Moreover, the cost of extra steam and water connections and valves will be as high as in the previous case, and in general higher, for the reason that the units for such boilers are customarily smaller. It has been decided to make the comparison shown in Table II. on the basis that boilers will be employed costing $5 per horse-power less than those referred to in Table I. and from the modification thus shown, the effect of a further reduction if found possible can be estimated.

38. Table II. shows in columns *H*, *L*, *P* and *T* the total costs

per horse-power per year shown in columns of corresponding numbers in Table I., all being on the basis of coal at $3 per ton; the first two named being for the 10 hours and 20 hours day without surplus power, and the last two named being for the 10 hours and 20 hours day with surplus power. In connection with these columns and distinguished by the same letters marked sub-1 are corresponding columns showing the costs per net horse-power per year with the interest and other charges affected by the first cost reduced proportionally to a reduction of $5 per commercial horse-power on the original cost of the boilers. An examination of the parallel columns shows at once that the effect has been to favor the engines, requiring the use of most fuel, for the evident reason that, as they require more boiler power for a given net power, the reduction for them is proportionally greater than for the higher priced engines. The differences are comparatively not great, but sufficient to show the effect of reduction in this direction. The same effect would result from reducing not only the cost of coal as previously stated, but the cost of handling and firing the same.

39. Evidently, however, any decrease found possible in the cost of engines or of the numerous attachments and appurtenances necessary or claimed to be necessary in connection with a steam plant, and which do not increase in cost proportionally to the commercial horse-power, will decrease the interest charges in a higher proportion for the more economical engines and thus secure economy both by saving of fuel and by saving of interest. These considerations, though not governing ones, since interest is only one item of expense, show the desirability of exercising careful judgment in the details of a steam plant. There will be no economy in selecting poor material or in hurrying matters so as to secure cheap work. Everything must be of the best in the sense of being the best adapted for the purpose. The tendency to put in numerous details, each by itself of small cost, needs, however, to be checked, as such items amount in the aggregate to a large sum. It is recommended that the purchase of the various devices which are being urged upon the owners of steam plants should not be consummated unless they will surely save, say, 25% of their cost annually. It will be observed that the columns of the table foot up a percentage proportioned to more than two-thirds of this, and as the attention and repairs of miscellaneous apparatus is great, the additional percentage is not only

thought to be warranted but to be perhaps insufficient in some cases.

40. The rules adopted for calculating the various columns of Table I are shown by algebraic formulæ the notation being in terms of the letters designating the several columns.

41. The comparison shows the non-condensing engines inferior at every point to condensing engines, and even if better results be obtained for the former in certain cases, still, as has been referred to before, the principal difference will be found by causing the quantities in column *f*, Table I, to approximate more nearly to the minimum quantities in column *e*. Some forms of engines undoubtedly accomplish this, but it is believed that the quantities stated are nearly correct for average good practice, with variable loads. Non-condensing engines are wasteful of fuel when heavily loaded on account of low expansion and at light loads the back pressure forms a large proportion of the total resistance, whereas condensing engines will maintain their economy through a wider range on account of the reduction of the back pressure. The back pressure must particularly affect the economy of the new triple compound engines when used non-condensing. It does not seem possible that such engines used non-condensing can for irregular loads show any economy over well designed compound engines operated non-condensing, unless the steam pressure is carried to 200 pounds or upward, and the size of the cylinders carefully proportioned to the average load.

42. It may be claimed that it is unnecessary to add 10% for interest and dividends, for the reason that nearly every large owner of steam machinery can borrow a large proportion of the money required to construct the plant at a much lower rate of interest. This reasoning is good in some cases and not in others. If the manufacturer can in his regular business pay 10% or more on the capital invested, it is more economical for him to use his credit and borrow money to extend his business than to put such money into a steam plant at a lower rate. For such conditions there is no question but that every expenditure should be charged at 10% or upward. The case is different with municipalities which can borrow money at 3% and 4%, but this after all will not make a great difference in the results, as interest is only one of several items of cost independent of coal. If instead of adding 10% for interest and dividends in column *D* we add only 5%, the difference between the amounts so added for different

engines is very small; for instance, it would make only 42 cents per horse-power per year difference in engines in lines *I* and *J*, which is exactly the difference between the total cost per horse-power per year for these engines with coal at $3 per ton as shown by column *H*. That is, for this particular comparison the cheaper of the two engines is 42 cents per horse-power per year, more economical when 10% is added and of the same economy when only 5% is added. If we compare the engines referred to on lines *I* and *K* in column *H*, we find that the cost per horse-power per year is 47 cents less for the former, with 10% added for interest and dividends, and that the latter is 20 cents per horse-power per year more economical if only 5% be added for interest and dividends.

43. It is true also that the 8½% added for inspection and loss of interest during construction, in column *l*, would probably not be expended in making additions to a whole plant, though rather insufficient than otherwise for original construction. The rejection of this item will not, however, make a great difference. It has not been included in ascertaining the cost of insurance, taxes and renewals, shown in column *C* and if 10% of the increase be charged in column *D* it is only 10% of 8½% or .85 of 1%, and so would affect the result only about one-sixth as much as the former presentation.

44. A little more careful examination of the table shows, as was substantially stated at the outset, that it is not the interest charges which principally cause equalization of total costs in the operation of engines of different kinds. The principal difference is due to the fact that all the other expenses except coal and interest, are very nearly constant and the interest intensifies the difference by directly neutralizing, so far as it goes, the economy due to decreased coal consumption. This is made more clear by the following tabular presentation:

45. The table shows that the collateral charges, line (2), excepting interest, are substantially the same, and about equal to cost of coal, Col. (1), for the economical engines, and that the slight increase of interest, line (4), for the more economical engines tends to neutralize the slight increase of economy of coal shown on line 1, so that, though the costs for engine *I* still remain less than those for engine *G*, they are also less than for the more economical engine *K*.

46. In conclusion we will say that when this investigation was

	Simple High Speed Non-Condensing	Compound High Speed Condensing	Special Triple Compound High Speed Condensing	Triple Compound Slow Speed Condensing
LINES OF TABLE I.	*A*	*G*	*I*	*K*
(1) The costs of coal at $3.00 per ton, Col. *r*, for a ten hours day are for engines stated in the headings at the right.	$19.09	$11.57	$9.84	$8.91
(2) The collateral operating expenses, excluding interest, are (Line *E*—Line *D*)	10.69	9.18	9.06	9.11
(3) The interest charges, Col. *D*, are - -	6.39	5.61	5.95	7.30
(4) The total costs, Col. *H*, are - - -	36.17	26.36	24.85	25.32

instituted it was believed, as had indeed been hinted by others and proved for extreme cases, that under some conditions as to the relative prices of economical types of steam machinery compared to price of coal and number of hours operation, the saving in fuel would not warrant the increase of first cost, but it was not known that this would occur within the limits of ordinary practice as the comparison shows. It is hoped that the conclusions and the facts upon which they are based will be very closely examined by engineers and manufacturers, and any facts or reasons confirming or varying such conclusions be fully presented and discussed.

47. This paper should not be considered a criticism of the practice or views of others, or serve to discourage the higher development of the steam engine. On the contrary, an investigation of this kind, whatever the result, is calculated to broaden the view by taking into consideration more of the conditions of the problem, and thereby enabling the engineer to secure the best results for each particular case. The tables do show for short hours or low priced coal, or both, that types of steam

machinery to secure the greatest economy of fuel are not warranted, but by the same tables it is found that for longer hours and the higher prices of coal considered, the more expensive machinery begins to show a commercial advantage which evidently increases as the price of fuel increases. In some cases other conditions must be included for a complete solution of the problem. For instance, in large steamers making long voyages economical machinery secures in addition to the saving in the cost of fuel, a saving in the space required to carry the machinery and fuel, and thus increases almost in a geometrical ratio the efficiency of the ships. This may not be true for vessels making very short trips, or stopping a large proportion of the time in port. The very large expenditures made by some mining companies to save fuel by the use of expensive steam machinery of special design, has been fully warranted by the long hours and and high price of coal. The enormous mechanical operations at the Calumet & Hecla mines gave the profession unequalled opportunities, which have been grasped in the most creditable manner by our distinguished co-laborer in the engineering field, Mr. E. D. Leavitt, who has reared there a series of remarkable monuments of engineering skill with an expense warranted, doubtless, by the conditions. The development of the great West is, however, now so modifying the conditions that a change of policy may be initiated even at the Calumet & Hecla mines in the near future. During a recent business investigation with an electrical outlook, the writer ascertained that the prices of coal in Duluth and Superior, beyond the Calumet & Hecla peninsula, are even now reduced nearly to those ruling on the seaboard, which result has been brought about by the construction and operation of large whaleback steamers which take wheat eastward and coal on their return trips.

48. Until quite recently the construction of pumping engines of special design and expensive construction was warranted, even for locations near lines of communication with coal fields. At present, however, pumping engines of good economy are being manufactured regularly, as a business, at a comparative cost that will make the still more economical but much more expensive machinery less in demand than formerly. The further perfection of the steam engine will not be hindered by these facts, for with the development of our mining industries at great distances from the coal fields, the closest economy in the use of fuel will

secure the best commercial results. In order, however, to secure such results for our growing enterprises, electrical and otherwise, in the cities and towns along lines of communication already established, it is believed that the field will be occupied by cheaper engines of simple construction, which, though not securing the maximum economy of fuel, will so reduce the capital upon which interest and dividends are to be paid, as on the whole to represent not only better commercial policy but better engineering, because based on more complete conditions.

Supplement Relating to Water Power.

49. It will be impossible in this connection to make a satisfactory comparison of the relative advantages of steam and water power, under different conditions. The cost of steam power has, however, in the previous pages, been considered on a somewhat different basis than has heretofore been customary, particularly by including the interest and certain miscellaneous charges in the operating expenses, making the minimum practicable cost per horse-power per year appear higher than has been stated by others, so if the cost of water power calculated on the ordinary basis were compared with this increased cost of steam power it would cause the water power to show an unwarranted advantage. It, therefore, appears necessary to make a brief examination in the form of an inquiry as to what amount can be paid for the development of a water power in competition with steam power. The average cost of developing a water power cannot be based on locations where the natural conditions are specially favorable, nor on a case like that at the Falls of St. Anthony, where an enormous water power was developed at a minimum expense amid surroundings apparently permanent to those who had not studied former recessions. In due time, however, the water undermined the hard limestone cap defining the falls, which had already been worn through above, and started a new channel through the underlying sandstone, thereby threatening to change the falls into a series of rapids extending miles up stream, when by the intervention of the general Government and the use of large appropriations, on the plea of preserving navigation, with some private contributions, a concrete dike or dam was built in a cross tunnel in the sand rock back of the falls, from a firm foundation up to the limestone cap, and the falls and incidentally the water

power saved. Ordinarily such disasters must be provided for from the accumulations of an ample depreciation account. Information of great value as to the possible cost of the general development of a water power may be obtained from the experience on the Merrimack.

50. Prof. Swain has stated in the Census Reports the costs of several of the principal structures at Lawrence and Lowell, aggregating $650,000 at the former place and $752,000 at the latter place. There are wing dams and probably other improvements apparently not included at Lawrence, and the costs of the greater portion of the canals at Lowell are not mentioned. In addition to the above items must be added the original cost of the land and large expenditures which have been made by the several mill owners jointly for works to control and regulate the flow from several large lakes on the watershed and thereby help the low water flow of the stream. The testimony of the mill owners in various suits relative to water power shows that the original costs of development on the Merrimack were so high that it has been customary to gradually "charge off" from year to year a portion of the cost so that the final results would show better interest on the apparent capital invested. It is believed to be safe to assume that the original costs in connection with the Lawrence water power have been about $1,000,000, which is less than the amount stated to have been originally paid in, and that those for Lowell have been considerably more. The original costs at Manchester could not have varied greatly from those at Lawrence. Additional hydraulic plant has, however, been put in at all these places to utilize "surplus water" or that flowing in excess of the assumed low water capacity of the stream, and steam plants are also employed to keep up the power when the stream is low. The work is principally of a kind that requires steam for manufacturing purposes, independent of power, the year around for slashing, dyeing, etc., and steam also is necessarily required in the winter to heat the buildings. These other uses somewhat offset the cost of a steam plant, while the expenditures for surplus power by raising the average power developed on the stream, really lower the average cost per horse-power of the general expenditures which have been made to utilize it. There were originally sold at Lawrence 128 mill-powers aggregating 8,700 net horse-power and a little more power at Lowell where the fall is higher. The leases permitted the use of the water for 15 or 16 hours per day. In practice, however, but little water is used

during more than 60 hours per week, and the dams are so large that during low water little water is wasted and a larger power on a 10-hour basis is utilized, which fact together with the use of surplus water when available, probably brings the net power available at Lawrence up to 13,000 horse-power on a 10-hour basis throughout the year, and somewhat more at Lowell.

51. On the basis of a cost of $1,000,000 the general plant at Lawrence has therefore cost about $77 per horse-power and somewhat more at Lowell. Independent of this, the mill owners have incurred large expenses in erecting turbines and hydraulic connections from the head canals to the lower levels. These costs have been estimated by Mr. Main at $45 per horse-power, including provision for surplus water, or an average of $65 per horse-power for the average power utilized.[1] Adding this to $77, the general cost stated above, we find that the total cost of developing the water power on the Merrimack has been about $142 per horse-power, which it will be shown is about the limit of cost at which water power can be developed in competition with steam. The Merrimack owners are, however, obliged to add to this cost the greater part of the cost of a steam plant for use when there is no surplus water, which still further increases the capital account.

52. It has long been known that the water power on the Merrimack had cost so much for development, that could the expenditures be recalled it would be more economical to locate where coal can be obtained at cheaper rates, and steam power used exclusively. This was true before mill engines were constructed as economically as at present. Now the preponderance is so great, that unavoidable errors of fact as to the details of the original costs will not change the illustrative value of the presentation. It is true, notwithstanding these facts, that a new water power is being developed at Sewall Falls, near Concord, N. H., but it appears to be limited to the low water flow of the stream, and it is not known whether it will assume part of the original cost of the reservoir systems.

53. The highest allowable cost for the complete development

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1. "The Value of a Water Power," by Chas. T. Main, vol. xiii *Trans. Am. Soc. M. E.* The price stated seems high, but should be known to Mr. Main who resided at Lawrence when the paper was written. He has, however, overlooked the items of cost of development referred to above, not mentioned by Prof. Swain, and therefore gives $130 as the total cost, instead of $142 stated in the text.

of water power from the dam to the jack-shaft appears to be about $140 per horse-power utilized on a 10 hour basis. If we consider that accidents are liable to happen to the best constructed hydraulic work, we should charge at least 2½% of the original cost for depreciation, say 1¼% for repairs and about 1¼% for taxes, or about 5% independent of interest account. If then on the principles above developed we charge 10% to an account for interest and dividends, and allow 2% for operating expenses, the total annual charge becomes 17% of the original expenditure. At an original cost of $140 per horse-power, 17% represents an annual cost of $23.80, per horse-power per year, or about the same as shown in the tables with economical engines, and coal between $2 and $3 per ton. Generally, however, there will be one company to furnish water power and others to utilize it, which will have the effect of increasing this 17% so as to make the balance in favor of steam somewhat greater. This statement of course only applies to conditions where simply power is required, and no large quantity of water is necessary for purposes other than for power. When the power is used for 24 hours per day instead of 10 hours, a much greater original cost is permissible.

54. When the power of a waterfall is to be delivered at a distance, the allowable cost of actually developing the power must be decreased by that necessary to transmit the power and actually deliver it to a jack-shaft at a given distance. An electric transmission is undoubtedly the most economical for such a purpose. If we add to the cost of the electric dynamos that of the buildings, of the hydraulic connections to the canals, of the turbines, of the line and of the installation, and finally add the cost of the motors, so that the power is according to the assumption delivered to a jack-shaft, the total cost of what may be called the "electrical transmission plant" cannot probably at present prices be put in for $140 for each net horse-power delivered, so on a 10 hour basis no expenditure could be allowed for the general development of the water power, but only for the simplest hydraulic connections to existing canals, etc. If, however, power can be sold throughout the whole 24 hours, more than double the price can be obtained for the same, and this will warrant doubling the total cost of development unless a greater percentage of income is desired. As the cost of the electrical plant remains the same, the whole allowable increase may be applied to the de-

velopment of the hydraulic plant, thereby entirely changing the conditions.

55. The writer has not hesitated to recommend an original expenditure of $200 per horse-power for a combined hydraulic and electric plant near large cities, where not only the customary income due to incandescent and arc lighting and the use of small motors at high rates would be available for comparatively short hours, but where the industries were such that large units of power could be sold at remunerative prices on a 24 hour basis. Even higher costs for development would appear to be warranted in some locations, but there is no general rule on the subject. The allowable expenditure in a particular case can only be determined from calculations based on the actual conditions.

Discussion.

The President:—Members of the Institute, you have heard the very interesting paper which has been presented by Dr. Emery. I think the impression which many of you have received is the same as my own—of surprise that there is so little difference between the total cost of power for the different classes of steam engines. Any one who has been called upon to make a selection of a steam plant and has received from fifteen to twenty bids from different builders, together with the claims for economy made by each, has been much in doubt as to the wisdom of a final judgment. I must confess that I myself have often been confused by the conflicting claims, and I think the paper which has been presented to-night will aid every engineer in settling these oftentimes puzzling questions.

The conclusions which Dr. Emery has reached in the matter of the use of water power in plants for the electric transmission of power is similar to that which I arrived at some time ago. Before I go into any details, however, I shall ask Professor Forbes, who I see is present, and who by reason of his experience and his present connection should be well qualified, not only to give us his views upon steam plants, but particularly upon combined hydraulic and electric plants, to say a few words upon this subject. I see that we have also with us some members of the American Society of Mechanical Engineers whom we wish to hear.

Prof. George Forbes:—Mr. President, since you are good enough to call upon me to make some remarks I will rise, but I fear that I shall not have a great deal to say that will be of any value. The paper was only put into my hands a short time before I arrived here, and it is one which requires a good deal more attention than from hearing it read or the slight glance I was able to give it before arriving here. But everybody, I am sure, must appreciate, even from hearing the abstract read what enormous importance this paper is going to be to us all. No doubt there are many others here, who have been in the same position in which I have been placed in the course of the last year, who have had to make a special examination of this very question, and all those who have had to deal with the utilization of large quantities of water power, estimating the value which that water power is to those who are exploiting it, must have been forced to do what they could to estimate the value of the horse-power when developed in the most economical means by steam power. I have in these investigations during the last year had the good fortune to be assisted by the experience of some of the most experienced engineers in this country, including one whom we all respect, who is mentioned by the author of this paper, Mr. Leavitt; and I would only say generally that the conclusions which have been come to by myself, and those who have given me their assistance agree extremely well with the conclusions at which Dr. Emery has arrived. I must say that Dr. Emery has put the case in a much more general way, and much more applicable to a great variety of circumstances that actually do exist in practice than any estimates I have ever seen of the actual cost of the horse-power, and consequently it is possible for us in any particular case that we are considering, to pick out the conditions which most nearly approach what is in the case that we are studying. And while I may say that some estimates I have seen of the cost of a horse-power per annum, especially on a ten hours' a day service come out a little below what Dr. Emery has produced, I must add that in those cases certain items of expenditure have been omitted, and in the cases where a lower estimate has been arrived at, the compiler has been professedly trying to produce the lowest figure which the users of water power would ever have to contend against.

I would feel that it is hopeless, without having given a great deal of time to prepare remarks on this paper, to say anything much to the point. But I would ask the author one or two questions about it. In the first place as to the cost of the coal, I noticed that the heat producing power of the coal is assumed to be the same in all cases. Now I fancy that these tables may lead to a little misunderstanding in the minds of some. I imagine some one taking up this table and saying: "Now what will it cost us at such and such a place to produce a horse-power per annum? There is coal purchasable at this place at $2.00 per ton. There is also coal purchasable at $4.00 per ton. He will naturally say: I will choose the coal at $2.00 per ton. That gives him a certain definite price on this estimate; but, of course, the slack at $2.00 per ton would only produce about one-half the heating power of coal at $4.00 per ton. In giving the price of coal it seems to me it ought to be mentioned what the calorific power of the coal is, and I would ask Dr. Emery whether he has in all these cases considered the calorific power of the coal practically the same.

I would ask Dr. Emery whether he considers that 500 horse-power is a sufficiently big amount to be considered. I mean are the standing expenses of a 500 horse-power plant sufficiently divided over the power to make it fair? If you went to 10.000 horse-power, for instance, would the standing charges be less in proportion to the horse-power, or is 500 horse-power sufficient to take up the whole of the value of the fixed expenditure? That is to say; are all the brains, and all the bodies that are at work on the plant fully occupied with 500 horse-power or would a larger unit modify the result?

I will not take up the time of the meeting with remarks which I feel are not of very great value and I will only make a few remarks about the supplementary portion relating to water power. The cost of putting in the plant has been taken at probably $140 per horse-power without electrical transmission. Now it seems to me it is almost an impossible thing to make a definite estimate like that. The cost of the water power applied, depends so entirely upon the local conditions in the first place, and secondly upon the magnitude of them. Where you have got a large head, the cost is generally very much less than where you have a very low head. When you have to dam up a river, of course the expense per horse-power of the preliminary work is generally much greater than with a waterfall, and of course the cost of the actual turbines is very much greater when we have low heads than where we are using a very considerable head. Another point is that the cost per horse-power in the use of water power diminishes enormously with the magnitude of the works. When you are dealing with 100,000 H. P. the cost for each horse-power becomes very much less indeed. I should say that this $140 was a price that would be fair for a moderate fall, and for very few thousand horse-power. But when you come to larger amounts I think it would be considerably less.

The concluding paragraph of the paper I most thoroughly agree with, as to there being many cases where $200 per horse-power would be not too expensive in order to start work for the combined hydraulic and electrical plants in large cities; also, I agree with the writer's remarks that every single case must be computed from the beginning, and the comparison made depending on the particular conditions of the particular place.

The President:—I wish to say a few words concerning the transmission of power by electricity over long distances with water power as the prime source of energy. I have always tried to avoid being over-enthusiastic in dealing with a commercial question of this character. The habit is too common to assume, now that electric transmission is a possibility, that possession of a water power is necessarily something of a bonanza, and that all that is necessary to convert it into tangible wealth are a few electrical adjuncts. Both the economies and the possibilities in electric transmission are overstated, too many conditions are overlooked, too many possibilities of failure ignored. I made the statement some years ago that it was very questionable whether in an ordinary case, transmission from a water power to a city 20 to 25 miles away with reconversion in a sub-station and re-distribution for lighting or power purposes, was an attractive commercial enterprise, and that if a man wished to distribute power in a manufacturing district he would probably do better to put in a well equipped steam plant of large size, well located with regard to railroad facilities, with supply of coal at reasonably low rates, and distributing at a constant potential of 500 to 600 volts, than he would to attempt to replace the steam plant by a system of electric transmission with water power behind it 20 miles away. I believe the general statements which Dr. Emery has given here to-night confirm this conclusion, and that we should avoid expecting too much from the transmission of power by electricity. I am not authorized to make any statement concerning the intentions of the promoters of the enterprise with which the last speaker is connected, but I believe that I am safe in saying that this great development of water power of Niagara Falls, which is so interesting from the standpoint of the hydraulic engineer, is not designed so much for transmission of power to a distance as it is for local transmission, and for the local development of manufacturing industries requiring large units of power. The electric railway engineer is frequently called upon to pass upon the question whether for railroad purposes it is possible to use water powers even 10 or 15 miles away from the centres of distribution, and he cannot but appreciate the lack of sound information upon that subject, and the folly of many of the visionary assumptions made by railway managers with regard to this problem.

PROF. FORBES :—Might I say, as a sequel to what you have just stated, of course the question of the cost of the work at Niagara, we have worked out very fully and I think I may say that we know what it is going to cost, both the electrical transmission to Buffalo and everything, but you will readily see that it is hardly possible for me to speak very freely about that just at the present time. But as to the question of transmission to a distance, although it is naturally the desire of those associated with it to develop the power in the immediate vicinity, in the meantime the extra cost of transmission to a distance of 15 or 20 miles is almost insignificant. When the details of these plans are published you will be astonished to see what a very little difference that really does make when we come to the actual machinery that will be put down.

THE PRESIDENT :—Perhaps I do not make myself sufficiently clear in that matter. Take the case at Buffalo for example; let us see how electricity is used there. There are, generally speaking, the arc and incandescent light plants, the stationary power service and the railroad systems. The arc lamps run on constant current circuits with varying potential. Some of the incandescent lights probably run on alternating current circuits; others run on 110 and 220 constant potential circuit. Railway motors run in a 500 volt constant potential circuits. These various systems of distribution are distinct in themselves. They have practically but one thing in common, that is, they are driven by steam engines. The moment you leave the belt of the steam engine or its coupling and go to the dynamo, the character of the transmission is changed. Now to operate these various systems from a distant source of power there is only one thing that is practically possible, that is to substitute for the boilers and engines in the several stations represented by different financial interests, or in some common station controlled by all, motors driven by the electric plant 20 miles away, which is in turn driven by turbine wheels. The substitution of the secondary part of this plant is not practicable; it has a character determined by the local demands and conditions. There is undoubtedly a field for the transmission over long distances of large units of power for special purposes, and also under conditions where the cost of coal is prohibitory and where perhaps it cannot be obtained at all, but I think that from a general commercial standpoint the statement I have made will be found correct in practical experience.

I appreciate that Professor Forbes is in a somewhat delicate position in speaking of what is to be done at the Niagara plant, and perhaps I ought not to have suggested so plainly that he was somewhat better posted than most of us as to the actual cost of transmission for the distances in question, and for the larger units.

I understand that Mr. McElroy is present. If so, we should be glad to have the pleasure of hearing from him.

Mr. Samuel McElroy:—To an engineer who has been a long time in practice, a paper like this, replete with the study of a profoundly analytical mind, comes not only as a most valuable contribution to its subject matter, but as a characteristic index of the professional progress of the day.

The time when many professional men were much troubled with petty personal jealousies, and with the idea that their shop secrets were their most valuable stock for employment, has happily been changed. The advanced progress of science, keeping step with, but always in advance of, the material progress of the country, and the beneficial results of society life, have done much to improve the systems of mutual help, and to broaden the standards of professional service, and the result, as one of various indications, is shown in papers which condense in a few columns, a wide range of experimental and practical research and result, for the direct benefit of professional brethren.

We have then, rising above the class which is content with a low standard of work, and adds little to the general fund of information; and the class which dwells continuously in the confined atmosphere of school-boy mathematics, rapidly developing into splendid, aggressive life, the men of true professional dignity; who neglect no details which involve principles, but give principles their true rank. These are the burners of midnight oil; men who love their work for its own sake, and not for what it pays; who are too high-minded to waste time in personal jealousies; students too ardent to turn aside for common places; and ever ready to put their most valuable acquisitions at the service of their professional brethren, as such papers as this illustrates.

One of the specially valuable features of this paper is its careful condensation into a brief space, of a wide range of experimental practice, and how a wide grasp of such practice simplifies apparently contradictory or complicated conditions, every experienced engineer learns, the more he learns.

In the feverish spirit of the day, which for want of just this balance of forces, tends to run to excess after every new theory, very great waste of time and money is made. which better habits of concrete analysis would have corrected. We go through spasmodic public excitements often, which are only spasmodic. Not so very long ago, the whole railway world was astir over the merits of the "broad gauge" railroad track, and trunk lines like the Erie and others, must have six and seven-feet gauges. Some years after, the same fever ran to a three and-a-half feet gauge; and it is only lately that common sense has brought our general railway system into standard gauge, and even this, the pedagogues of the profession, are continually tampering with.

The question of cost of steam power, in itself, and as developed by different classes of engines adopted, or now being adopted, is here carefully analyzed in its proper commercial condition. Steam power, we see here, means something more than a boiler, a cylinder and a line of shafting. It means not only boiler plant and cost, with all the accessories; not only engine plant and attachment; engine house, chimney, and appurtenances; coal supply, combustion rate, and cost; but other conditions are important. There are daily operation as to running time; relative engine friction; pressure economy; feed water temperature, evaporation, spare pipes and valves; repair, depreciation, supplies; interest on construction and plant; inspection, insurance, taxes, dividends, surplus power and like conditions, which enter into the general problem of use and cost, and are here carefully analyzed and tabulated.

As to these analyses, their basis of arrangement is so clearly explained that special corrections for special cases can readily be applied.

As, in my own expert practice, I have frequently been required to demonstrate the economy, fitness, and special value of water power, if I should venture any opinion on the valuable analysis here made, of comparative steam and water power, it would be to say only this:

That it is quite certain that water falls and flows to the sea, by gravity, much more cheaply than coal can be mined, and is likely to outlast coal; that water power plant is far less expensive, as a rule, than that of steam, and its daily care much less expensive; that at the centres of such power, Lowell, Lawrence, Holyoke, Cohoes, etc., their common annual rental of about $20 per H. P. (ordinary mill day), has induced the most elaborate outlay of capital, and resulted in very remunerative income.

I think, therefore, that on general analysis, water power does not cost as much as steam. While it must be admitted that very expensive reservoirs and other structures, as at Lowell and Lawrence, aggregate what Dr. Emery's analysis shows; yet at Lowell the Central Pacific mill prefers in low runs to pay $60 per H. P. for surplus water, to using its own steam plant; that for somewhat large consumption of special power, $3.00 per week is a common steam power price at Lowell, Boston, New York and other places.

The estimated cost of development on the Hydraulic Tunnel of Niagara is about $2,238,750 for a capacity of 119,000 H. P., or about $10.50 per H. P., for slopewalls, cribs, races and gates, and tunnel. The rates, as published, are for 5,000 H. P. or over, $10 per H. P. per year; 4,500 H. P, $10.50; 4,000 H. P., $11, down to 300 H. P., $21. On the old Hydraulic Canal, powers have been leased as low as $4 for 600 to 1,000 H. P., and $5.30 for 250 to 300 H. P. Such leases are, of course, improperly cheap, as are, in fact, the general rates of the new tunnel.

On the Kennebec River, Maine, I found a working power, at Carritunk Falls, under 28 feet fall, of about 7,000 H. P. This river, on its upper basins, has remarkable natural reservoirs of about 229 square miles, with about six feet available storage, in a main basin of 2,860 square miles. A main dam of 80 feet controls the flow, and nature has singularly formed the races and wheel pit; so that the cost of these items is much less than that of Niagara.

In cases like this, then, the superior economy of water power must be conceded; and in pulp mills, as at Carritunk, the large quantity of water needed is also an important item of cost.

These, however, are not criticisms on the elaborate and most valuable paper of Dr. Emery; they furnish modified conditions of use, which no engineer will apprehend and appreciate more thoroughly than he.

THE PRESIDENT:—I notice that Mr. McElroy in his interesting remarks has omitted mention of a rather well-known water power near by, and that is the Housatonic. My attention was called to it a short time ago, and although I have forgotten what the rates given were, my impression is that the price there, is considerably higher than those just given in other instances.

MR. MCELROY:—I am not able to say definitely as to that. Of course there are other cases. The company at Paterson, for instance, charges very much more. Their rates are more than double.

THE PRESIDENT:—Is Mr. Holloway of the Mechanical Engineers present?

MR. J. F. HOLLOWAY:—Mr. Chairman, I had no expectation of saying anything to-night, but I read with pleasure the paper which Dr. Emery has prepared, and I wish to say that I agree heartily with my friend, Mr. McElroy, in the complimentary remarks he made about the paper. The paper is one that will be of great value to mechanical, as well as electrical engineers. It is evidently the result of a great deal of study and care on the part of the author, and in its result it is to some extent surprising; and yet after all, it coincides very closely with ideas that I have had all of my life—that this is a world of compensation; that we do not get all the good things in one place, without having to take some other things that are not quite so good.

The results he shows are as between the more common, and the highly refined engines, and the ultimate results that are shown in his paper, are not so widely different, and that will be, I think, a matter of some surprise to many, until they investigate the matter in the direction and to the extent in which Dr. Emery has investigated it. The common idea is, that to get the best results, you must get the most refined mechanism, and considerable money is often spent in that direction without knowing how much in other respects, the total value of the saving is brought down to the results obtained by the plainer and more common machines.

Referring to the water power question, I know very little about that, except that it occurs to me that it is hardly a fair comparison that Dr. Emery has made between the steam engine and water power, in the location which he has selected. I think it would have been better to have said that he had made the comparison with a place where there would be a good water power if there was water. If you go to all the expense of arranging for using water, and then half the year you do not have water, of course that must add largely to the ultimate cost of water power.

PROF. JOHN H. BARR:—I came down here this afternoon to hear the third reading of this paper. I had the privilege a short time ago of hearing a dress rehearsal of it, and read it to-day on the cars. I hope this, however, is not the final reading as it is one of the things that grows on me. I thank you for the privilege of taking part in this discussion, though Dr. Emery has left little to discuss. The previous gentlemen have expressed their appreciation of this valuable paper to the engineering profession, in which I wish to concur.

I noticed a reference to the condition of affairs at the Calumet and Hecla Mines. My first engineering experience was gained at the Calumet and Hecla Mine, and, while I was there in a subordinate capacity and perhaps not entirely competent to judge of things, I think it quite probable, if not certain, that the conditions have by this time been so modified, that a different policy might be pursued with economy, so far as distribution of power is concerned. The great work done at Calumet by Dr. Leavitt, is of the highest interest to engineers, and if he has kept his eye rather constantly on columns *c* and *f* of the table given in this paper, it can be said that few men have done as much with a pound of coal as he.

The Falls of St. Anthony is cited as an exceptional case where nature has greatly favored the locality in water power; and while its water power had a very important influence in the development of the city located near it, I doubt if its present influence is as great as most people think. I believe that steam mills at other places can now compete on very fair terms with the mills on the water power at St. Anthony. The supply of water is so variable that all the more important mills have to have large steam plants—steam plants sufficient to practically run the entire mill. The interest, and all costs except running expenses, go on for this part of the plant, whether the mill is run by steam or water. The cost for labor also is largely maintained whether run by steam or water, because the engineers have to be employed throughout the year in order to have them when needed. The value of land and high taxation, or rent, puts these mills at a disadvantage, largely compensating the gain due to running by water part of the time.

MR. C. O. MAILLOUX:—In using engines for electrical station purposes, the tendency of modern practice, even with slow speed engines is to do away with the jack-shaft and to use the power either by belting from the engine direct or by attaching the armature direct to the shaft. Hence in a large number of cases the power of the engine will all be utilized excepting a very small percentage necessary for its own friction; and one need not, therefore, allow ten per cent. more for loss in jack-shaft or other intermediate transmission. This, of course has a direct bearing on the initial cost of the engine and the cost of maintenance, and indirectly upon the total cost of power per annum.

In regard to the utilization of water power when used to generate electricity to transmit to a distance, there is one factor that appears to me to be of importance, as affecting the question whether a given transmission scheme is financially practicable or not. I refer to accessory machinery and apparatus, particularly that needed for regulation and control, the importance and the cost of which I think is often underrated if not neglected, in making estimates on the cost of machinery necessary to utilize the energy of water power by transmission to distant points. I met a party some time ago who is operating an electric railroad by water power, and was not particularly happy over it, even though the power cost him but little. He said the principal trouble was in the great flunctuation of pressure, the voltage varying from 300 to 600. If several cars happened to start, or to be going up grade at the same time, the voltage would come down to 300, but if some of the cars stopped, the dynamo would raise the E. M. F. up to 600 or more. I asked myself, while thinking over this case, what would be the result when transmitting 5,000 horse-power electrically, from a generator driven by a turbine, supposing the load was suddenly and totally relieved by the circuit being opened through a main fuse blowing out, or a break in the wire, or supposing it were suddenly thrown on, without giving time for the governor to act; or again suppose the load were constantly fluctuating as it does on most railroad circuits. It occurred to me that something would happen of interest to science, and possibly the coroner also. We can hardly have fly-wheels sufficiently large to take care of these fluctuations, as they must be of enormous size. Calculations show just what would have to be their weight and proportions to prevent any serious variation of speed, when the whole load is thus thrown on or off. There are to-day many places where available and cheap water power is unused for the sole reason that no efficacious and satisfactory regulation has been found to compass the fluctuations of load occurring on railway and power circuits. At Oswego they use a resistance, so that when the load is thrown off from the working circuit it is thrown on an idle resistance, which is not a very economical means of handling the difficulty, to say nothing of its cost. Hence, even assuming that we can overcome all other difficulties, of a financial or engineering character, in connection with a transmission scheme, we must put into our estimate a very liberal allowance for accessories, to enable us to secure a successful transmission and control of the energy, and leave the energy at the other end where it has to be utilized, in such form that it can be used as successfully and as satisfactorily to the customer as the electricity obtained from a central station operated by steam.

I have always found the principal difficulty of electrical transmission projects to be, to dispose of the electrical energy after it is transmitted, and especially to distribute it, or deliver it to the consumer in a satisfactory manner.

PROF. FORBES:—I would say that in the case of the Niagara transmission that question has been thoroughly threshed out and we have got a fly-wheel, not at all gigantic in comparison with the revolving armature, which completely takes care of that. In two seconds the regulator will have acted and the fly wheel will have taken care of it up to that time.

THE PRESIDENT:—I think in the larger plants where five or six thousand horse power units are used, there will be less difficulty possibly than is anticipated, because as a rule where transmitting from a single source of such large units to distributed work for stationary purposes, there is a certain averaging up of the duty which will prevent such sharp variations of the load as have been indicated, and hence I do not think there will be so much difficulty on the large water power transmissions; still, these variations have to be guarded against, and it is more difficult to meet them in a water plant than in a steam plant.

If any other gentlemen wish to discuss the paper we will be glad to hear from them. Possibly some remarks may be prepared by members subsequent to this meeting; if so, I shall try to have them received at the next regular meeting, or they can be presented to the Editing Committee. Dr. Emery will reply to some of the comments which have been made.

DR. EMERY:—I can only say that I feel very much gratified with the complimentary remarks that have been made in regard to the paper and the way in which it has been appreciated by those present. It is unfortunate that the paper is so long that few have had time to study all the points in their different bearings and relations to each other, and I am quite sorry that one whom we esteem so highly as Prof. Forbes should have received a copy only an hour before the meeting. I should have been very much pleased to have his remarks, excellent as they were, based upon a more thorough study of the paper. I quite agree with him on the point he makes in regard to the quality of the fuel. That matter should be emphasized in every way. At the same time it is not neglected in the paper. I will read Section 25, page 133, upon that point:

"In column *m* is shown the weight of water which it is assumed will be evaporated per pound of coal. The assumption is 8½ pounds, except in the last case where it has been fixed at 9½ pounds. Although 9 and 9½ pounds evaporated can be obtained under actual conditions readily with boilers of good proportion and construction, it is thought that 8½ pounds is as much as can be depended upon in average practice. It is in fact greater than is obtained in most cases, though less than is claimed in exceptional ones. This rate must be considered somewhat in relation to the price of coal. If cheap coal is to be used which will not give 8½ pounds evaporation, the results must be sought in the column of the table applying to coal of a higher price."

The last clause of the quotation, it will be seen, exactly covers the point made by Prof. Forbes in regard to cheaper coals. They will not give 8½ pounds evaporation. The better grades of anthracite coal of buckwheat size, will rarely evaporate over 8 pounds of water from actual pressure and temperature, and for the poorer qualities 7½ pounds or even less can only be depended upon. If, therefore, such coal cost $2.00 per ton, the results in the table should be mentally interpolated between those for $2.00 and $3.00 per ton. A glance at the figures in the two columns will show that any such difference will not appreciably alter the relative performances of the different engines if the same coal be used in all cases. The absolute results for a particular engine would of course be changed.

The next question is, whether or not 500 horse-power is a sufficiently large unit for general consideration. Prof. Forbes did not use the term unit, but I think that will express his meaning. I used 500 horse-power as being more generally applicable than a larger unit. It is in fact much larger than the average, but sufficiently large to secure maximum performance in the engine. The results would not be materially changed if we were to consider larger plants as multiples of a 500 horse-power plant. The cost of fireman would not change at all, and though one engineer on watch might care for an engine of more than 500 horse-power in a large plant, a chief engineer would also be employed at a higher salary, so that the labor account would not be greatly modified. The table is therefore about right for any large plant.

Prof. Forbes:—For how small a plant is it right?

Dr. Emery:—It would be presumptious to say that it is exactly right for any engine. It cannot be expected that the friction of the engine, the weight of steam or coal used per hour, the cost of supplies, of wages, of interest, and of fuel will in any particular case exactly coincide with the assumptions in the table. I have attempted to cover a great deal of ground and have been obliged to generalize. I have stated all the details of the generalizations and show at some length that considerable changes in the assumed facts would not materially affect the comparative results for the different engines. Absolute results for any particular case can be obtained by substituting the particular conditions, using for ready reference the formulæ at the heads of the columns. Do not misunderstand me. I have assumed very *probable* conditions, collated by references and by personal recollections based on a very lengthy experience, so that the results are approximately right for average conditions. Recurring to the original question I do not think that the operating expenses of a steam plant of 2,500 horse-power, or containing several units of that size would vary from one of 500 horse-power, or made up of several units of that size, sufficiently to predict in advance which would have the advantage. As already stated, one engineer on watch would take care of more horse-power in large units, but the number of men required to do the overhauling on the spare engine, and the salary of a chief engineer or general superintendent, would balance or more than balance the apparent saving for attention. For electric lighting work the principal saving due to the use of 2,500 horse-power or other large units, would be in the cost of real estate and buildings. In New York City, for instance, the area is limited, land is high priced, and the larger units take less floor space, so that considerably more engine power can be crowded into the same space, and this may make considerable saving in first cost.

Prof. Forbes, in the short time he has had to examine the paper, has not quite understood my first remarks in relation to the cost of water power. Moreover, the preliminary statement in the supplement to the paper is necessarily incomplete, and was rendered much more so in attempting to abstract it. Prof. Forbes says, " The cost of putting in the plant has been taken at probably $140 per horse-power without electrical transmission." I wished to be understood that $140 per horse-power was about the maximum amount that could be expended to develop a water power on a 10-hour basis in competition with steam, and I showed that the expenditures on the Merrimack had reached this sum, and that if they could be recalled they would not now be warranted. It was not intended to intimate that water power could not frequently be developed for a less sum. The supplement is directed particularly to the question of allowable expenditure when interest on the first cost is considered. I of course agree with the general statement that the cost of developing water power when there is a high head and no dam to be built is comparatively small. I have simply emphasized the fact that all waterfalls are not thus advantageously located. I recently reported on a large water power 15 miles from large cities where there is over 70,000 horse-power running to waste, during the minimum flow which occurs in winter as in the Alps, and not in the summer. This power will in time be valuable, but it is not immediately available because a fall of nearly 500 feet is distributed through 10 miles, so that on the most economical plan of development there would have to be 10 power plants, some of which would be in gorges not readily accessible. This power may be utilized electrically, by locating a number of generating stations on the river, and transmitting it to the cities. The demand for so much power would, however, have to be created. The lower falls could be utilized at once for present demands, but the whole power must go together and a small development would not pay the interest on the large original first cost of the property. In this case I did not hesitate to recommend the original expenditure of $200 per horse power for combined hydraulic and electric plant in case the power could be immediately developed and utilized, but this was found to be at present impossible. In due time these conditions must change, but they are governing ones at present. When 100,000 horse-power or more can be obtained at a single location, necessarily the cost of development becomes very much less.

It will be observed that I have not attempted to discuss the details of electrical transmission. President Sprague has pointed out the nature of the difficulties. I, however, desire to call attention to the fact that the method I have adopted is the proper one. The total cost of plant to deliver the power from the waterfall to the work to be done, which I typify as a jack-shaft, cannot, in competition with steam and cheap coal, greatly exceed $140 per horse-power on a 10 hour basis and $200 and upward per horse-power on a 24 hour basis. This cost of plant includes, first, the original investment in the general development of the water power, which to the consumer may appear as rental charged by the water power company; second, the cost of installing turbines and dynamos to generate the electric current; third, the cost of the electric line, and, fourth, the cost of the motors which turn the wheels, or the illustrative jack-shaft, at the point of delivery. *The motor must be included in the cost*, as then only is the result comparable with steam power on the basis shown in the table. The comparison is independent of the work done by such jack-shafts, as in either case it can be employed in generating electric current at lower tension for general distribution, or utilized directly to operate large mills or manufactories.

I am pleased with the remarks of Mr. McElroy. Naturally he has had such an extended connection with water powers that his sympathies are in that direction. I have never personally administered the affairs of a water company, but one of the most important of my observations occurred when a brother engineer in charge, had to leave his comfortable office and brave pneumonia in directing the movements of several hundred men employed in clearing the canals and races of ice. Similar instances are frequent in our severe northern climate, and cheap as water power is under normal conditions, the irregularities due to freshets, droughts and ice must be duly considered in striking a balance with steam power.

Mr. Holloway, of the Henry R. Worthington Company, might have called attention to the fact that it was one of the chief arguments of the lamented head of that firm, that while he built a pumping engine not as economical in fuel as those of special design, the difference in first cost and of interest and repairs afterwards was in most cases sufficient to make his construction more desirable. Recent improvements of the engine under the direction of his son, C. C. Worthington, have had the effect to raise the economy, without materially reducing the simplicity, so that large pumping engines are now manufactured practically in considerable numbers, and the necessity for special designs to secure unusual economy of fuel is less frequent than before.

I am much obliged to Mr. Mailloux for initiating a very interesting discussion in regard to governing water-wheels operating electrical plants, and am also obliged to other speakers, but do not recollect any further question that requires response.

I trust that further discussions may be sent in by manufacturers and engineers, to whom the paper has been sent and will be specially obliged for any information tending in particular cases or under particular conditions to modify the statements as to costs and prices given in the paper as a basis for comparison. I have been assured this evening by the operating superintendent of an electric station, who does not wish to rise, that my estimates of the cost of supplies, repairs and renewals is too low to apply to high speed engines of large size, operating electric plants. This is an important point which I hope will be fully elaborated by those at liberty to talk freely on the subject, as it may show the desirability of using engines of fairly slow speed, rather than the cheaper ones running at high speeds compared with their size. Engines of the old slow speed type are now run much faster than formerly, and it may be that a compromise class of engine will soon be designed to which neither of the common names can be applied, which will be cheaper than the slow speed engine, and yet not run so rapidly as to cause the difficulties which some have experienced with certain types of high speed engines.

THE PRESIDENT:—I had intended to point out, which I will now do very briefly, that it is most important in considering the transmission of power by electricity from original water power plants, that the conditions of construction under least cost should be considered. Some few years ago I prepared a paper which was read before the Franklin Institute which brought up that question. The conditions were given for continuous current transmission, although they would probably need to be modified for alternating current transmission, and I presume the same general laws will hold.

[COMMUNICATED AFTER ADJOURNMENT BY THE AUTHOR.]

DR. EMERY:—Mr. Samuel Webber, the veteran hydraulic engineer, now residing at Charlestown, N. H., has, since the meeting, written me pleasantly, stating that the steam portion of the paper confirmed, in several respects, views he had held for some time; but in his first letter he criticized rather severely the costs given for the development of the water power on the Merrimack and, incidentally, the authorities cited in reference thereto. Some of the criticisms were, however, in part due to the fact that at first he did not quite appreciate the method of comparison adopted. The letters are not in shape for publication, but a full abstract is presented, which it is thought will be of great interest and value, although for reasons given in closing, all of his general conclusions are not accepted. He first suggests that probably the cost of land had been included in the total costs which "sold not only for the mill sites, but for offices, storehouses, dwelling houses and streets; for a complete factory village," implying, evidently, that but a portion of this cost should be included in the cost of the water power. He then criticizes the addition of about "50 per cent. of cost for additional wheel plant to provide against back water," a Lowell expression which means, in the language of the paper, the additional plant employed to utilize the "surplus water," as it is called in many of the places. He says:

"This additional wheel plant has been added, as the original power was outgrown, to enable the mills to use all the water possible, and it is now 19,000 H. P. in Lowell, which is used for a portion of the year."

He refers to the original comparatively low cost and the later high expenditures at Lowell, adding that "Lowell and Lawrence united, and bought at a moderate cost, the control of the outlet of Lake Winnipiseogee, which, by dredging, they are enabled to draw down six feet in summer. They do not raise the water in the lake. In the long run these improvements have raised the amounts expended to a large sum, but have doubled the original power."

He then adds:—"I still hold to my often expressed opinion that $100 per H. P. is an ample estimate for dams, canals, wheel-pits, etc., under 20 feet fall, and in many cases it can be done for $50."

He adds: "As to the statement that Lowell will never be rebuilt, it is all bosh. It would be done to-morrow if the power was unoccupied and it would not cost one-half the amount per horse-power which it did in the first place, by using modern turbines, and other improvements." He then expresses the opinion that the cost of the power at Manchester was not one-half that at Lawrence.

The attention of Mr. Webber was then, by letter, called to the fact, that the paper necessarily dealt with the average power throughout the year, since the power to be made up by steam would necessarily be the difference between the aggregate power required, and the power derived from the water, which latter would necessarily vary at different seasons. He was also asked if he would not kindly furnish a statement of what the actual costs had been as he recalled them. In responding, he regretted the loss by fire of a large collection of his notes and memoranda. He recalls that at the "first regular meeting ef the stockholders of the old Locks and Canals Co. at Lowell, Feb. 27, 1822," it was stated that the "property cost for land $18,339, and for the old dam and canals $30,217. In 1823, when the price per mill-power was fixed, Mr. Appleton stated that the expenditures up to date were $120,000 and would furnish 50 mill-powers. These were supposed to net 60 H. P. each, making 3,000 H. P., or $40 per H. P., on which the fixed rental was $300 per mill-power, or $5 per H. P., being 12½% on the investment." The mill-powers were gradually sold off, and "in 1845 the capacity of the old canals was over-taxed," and between then and 1875, expensive improvements were made, of which the costs are given in the paper of the writer and referred to hereafter. He recapitulates the costs at Lawrence substantially as given in the paper, but states the cost was greatly increased from the fact that "the town was originally intended to be on the south side of the river, but the agent employed by the original projectors to purchase the land was found to have reserved so much of the most desirable portion of it for his own private property, that the directors voted to build on the north side, where the ground was very uneven, and the cost of the canal increased enormously." The company, however, owned considerable land on the south side, so that the south canal was finally built to enable that land to be sold, though the water could all have been used by the first canal, which, however, should have been at least 20 feet wider to avoid the swift current which causes the banks to cave occasionally."

Mr. Webber adds:—"Old Mr. Samuel Batchelder, now dead, who began with Lowell, and afterwards built up Saco, says, in his 'History of the Cotton Manufacture,' that he estimates the total cost of water power at Lowell, 'including land,' at $15 per H. P. and he knew much more about it than these 'latter day' figurers."

Mr. Webber continues: "Were I to make a rough cast of the cost of a water power, imagined to give 5,000 H. P. on 20 ft. fall, I would allow for a dam 500 ft. long, averaging 10 ft. high near top of fall, base of dam 10 ft. wide, top 5 ft. or 7½ ft. average=75. c. ft. in one linear ft. This 75 × 500 = 37,500 c. ft. or 1390 c. yds. This, at $20 per c. yd. (ashlar in cement), would be $27,800. Assume guard gates and waste weir to cost half as much more, it would add $13,900, making $41,700. Then a canal a mile long, 100 ft. wide by 12 ft. deep, or 257,000 c. yds., at 25 cents per yard excavation = $64,250. This makes a total cost of $106,000. Add to this, facing canal walls with stone, say 1 ft. thick, average, or 4,280 c. yds., at $5 = $21,400, and you get dam gates and canals $127,400, or $25.48 per H. P. Now add about as much more for land for mill sites, and call this investment $50 per H. P. Then take Mr. Main's figures for average cost of wheel plant at Lawrence (which is far higher than it would be to-day) at $45 per H. P. and you get a grand total of $95 per H. P.

Mr. Webber states further, that the costs in some of the older mill towns "are so mixed up with land purchases and sales which have been very profitable, and furnished the means for great extensions over the original plant that it would be very difficult to separate them." He then refers to the very cheap development of some water powers in Maine, much in the same manner as the subject has already been treated by Mr. McElroy.

In reviewing the very valuable information furnished by Mr. Webber, it should be borne in mind that a writer in making a generalization usually obtains information from various sources, all of which can be considered with reference to the general conclusion. Mr. Webber's statement that the work on the Merrimack could be done now for much less than the actual cost, is of course true, though his remarks emphasize the fact. It is, however true, that nearly all new enterprises start small and then in increasing the plant incur very much more additional expense than if the final development could all have been carried out at one time. It is therefore proper to consider such facts in making a generalization. It is believed that, on the whole, the lesson of the Merrimack as pointed out in an illustrative way in the supplement to the original paper is still as valuable as ever. It was there stated, that changes in values due to more accurate sources of information would not much change the results, but the additional figures given by Mr. Webber rather confirm than otherwise, the rough estimate made as to the total cost at Lowell. The original company bought up an old canal company and probably at ruinous figures, yet the water company had expended $120,000 by 1823, which for the power then developed on a leasehold basis to the stockholders themselves, was very reasonable, being only $5 per H. P., but a profit would need to have been added to these figures, if the water had been obtained from a different company. The improvements referred to by Mr. Webber, however, increase the cost on an enormous scale. From the census reports it is learned that a new stone dam was built at the top of the fall, where the height was fortunately only from two to four feet, except for a short distance in a deep place, so although the dam was 1094 feet long it only cost $114,000. The new canal, which necessarily skirted the river so closely as to require a very expensive retaining wall for a considerable distance, cost $551,584, and one of the underground feeders in the city cost $86,131, making in round numbers the total given in the paper, $752,000, which with Mr. Webber's $120,000 gives $872,000 in definite items. The original work probably cost considerably more than was paid for it, and there is the additional expense of controlling the lakes. Mr. Webber considers that the outlet of Lake Winnipiseogee was purchased cheaply, but the census report shows that there are other lakes also utilized, and Mr. Francis in a prominent report has stated distinctly that the control of the headwaters of the Merrimack was obtained at very great cost. There is testimony in law-suits that part of the original costs for some for the mills have been charged off on the books. The general statement in the paper that the work at Lowell had probably cost considerably over $1,000,000 is therefore sustained. The rough estimate in relation to Lawrence cannot be very much in error. Mr. Webber, however, does not consider that the works at Manchester could have cost one-half as much, but this is an extreme statement. I now recollect that Mr. Manning, the engineer of the company, once told me that he estimated the total cost at that place as between $90 and $100 per H. P. If this meant the total wheel-power, the statement in the paper was approximately correct.

The evident difficulty in obtaining approximate prices at such a location is in distinguishing between the costs per H. P. of the total plant (all of which can only be utilized during high water) and the cost of plant per H. P. based on the average power throughout the year. The average power should be used as the divisor in obtaining the cost of the power, for it represents the quantity to be used in reduction of steam power in a given case. The total wheel power, was doubtless referred to by Mr. Batchelder in the extract noted by Mr. Webber. The $15 per H. P. necessarily refers to the leasehold or annual cost, not to the total cost. This on a 12½% basis, originally fixed upon among the owners, represents a total cost of $120 per H. P., and if the H. P. meant, is the maximum for which wheels are provided as we suppose, the total cost on the basis of the average H. P. developed throughout the years has been very much more than the $140 per H. P. assumed in the paper. When it is considered that, in a location like the Merrimack, the mill owners must provide not only the surplus water plant to utilize surplus water, but additional steam plant to do the work of a surplus water plant when the water is low, it will be seen, as briefly referred to in the original paper, that steam power exclusively, would cost much less, if the actual costs of developing the water power were even approximately what has been supposed, and it wouldneed careful study and estimates to determine whether, as Mr. Webber suggests, the work in being done over again could be accomplished enough more cheaply to change the conditions.

THE SECRETARY:—Alth
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standing of Professor Seeley'
by a dynamo. Mr. Greeley su [illegible]
build a dynamo machine and other apparatus, which were accordingly started at once, and on March 28th, 1867, Mr. Greeley published an editorial in the *Tribune*, entitled "Electric Light." The scientific points were, of course, furnished by Prof. Seeley, but it is evident that Mr. Greeley understood what he was writing about, as the editorial is interesting reading at the present day, considering the time it was written. I, therefore, quote it in full, as follows:

[From the New York *Daily Tribune* of Thursday, March 28th, 1867.]

ELECTRIC LIGHTING.

The Nineteenth Century is already most conspicuous among the centuries for those great inventions which minister to the wants of mankind. Three-tenths of it still remains; and the leaders of progress in science and the arts are more numerous and active than ever before. It is certainly far from impossible that many who are now living, may witness products of genius yet unthought of, which, for usefulness, may be ranked with the Locomotive, the Telegraph, and the Photograph. Indeed, we are not visionary in believing that we are already advancing into the shadow of something which may be as admirable and useful as any of them. Recent advances in science seem to indicate plainly the path which promises to lead to one of the most beneficial developments.

It has been demonstrated that what we call Light, Heat and Electricity, and supposed to be peculiar kinds of matter, are, after all, only forms or modes of motion—that the motions which constitute their peculiarities have no greater difference among themselves than the motions of our ordinary machines. It has been demonstrated that they may be converted into each other—that either of them may be converted into ordinary motion—and, conversely, that ordinary motion may be converted into either of them. In other words, their production and operation involves simple questions of mechanics. The precise amount of mechanical force required to produce heat and electricity have been exactly and mathematically determined. Thus, the force of the falling of a weight of one pound a distance of 772 feet will produce heat just sufficient to raise a pound of water one degree Fahrenheit. This exact transformation of falling force into heat may be accomplished with the greatest ease. The converse, however, we have not yet reached in practice. In our attempts by the steam engine to get heat into ordinary useful motion, we practically realize only about five per cent. of what we strive after. It is possible that, before the Nineteenth Century closes, we shall learn how to secure all, or nearly all, of that ninety-five per cent. which now escapes us. When we have that knowledge, one pound of fuel which we use for generating power may have its present value multiplied nearly twenty-fold.

The labors of scientific men, within the last five years, have developed methods of transforming ordinary force into electricity. When these methods are put into the most practical form, we shall have electricity so cheap that its applications may be multiplied a hundred-fold.

Practical results have not been so fully obtained in attempts to transfer mechanical force into light. But what we know is sufficient to fill us with the most exalted anticipations. The possibilities are too bright even for dreams. The precise amount of force required to produce a given degree of light has not been determined, mainly for the reason that it *is so small that it eludes our measurement.* It is asserted by savants that the strength of a child would be sufficient to turn night into day for the City of New York, provided that strength were wholly converted into, and should reappear as light. May not this be accomplished in the Nineteenth Century?

But, concerning Electric Light, wonderful things are already accomplished facts. We have transformed force into light through the medium of electricity, and the process has been a success. Such a light promises to come, ere long, into general use.

Thorough experiments in France and England, and official arrangements for tests in the United States, indicate the importance attached to electricity for illuminating light-houses. The results at Cape La Heve, on the French, and at Dungeness on the British coast, prove that, in clear weather, the electric light is visible seven miles further (27 miles against 20) than the best lights ever before shown at those points; while the advantages are still greater in thick weather, as its density, whiteness and brilliancy, enable it to penetrate fogs that almost totally obscure other lights. The successful experiments of the French Lighthouse Board, corroborated by the British experiments, may well encourage our American Lighthouse Board in the tests about to be instituted in this country, especially as the difficulties which cause the British authorities to hesitate about introducing the electric light generally along their coasts (" lest *their* heavy machinery might break down ") are understood to have been overcome by inventions of American genius.

It may be added that the brilliancy of the Electric Light will actually throw shadows from the flames of street lamps on a wall distant about 1,500 feet, and that it surpasses sunlight in photographing--effecting the object in one-third of the time required by Sol himself. Some of the British photographers now use the Electric Light at night for much of their work, especially for copying and enlarging pictures—a profitable part of their business.

The occasionally working of the Atlantic Cables (both being thrown into one circuit of nearly 5,000 miles) by the infinitesimally small battery in a lady's thimble has so utterly revolutionized the notions of electricians and telegraphers concerning the power required for working that vast telegraphic circuit, that intelligent people are now scarcely surprised at the latest assertions concerning the illimitable resources for cheaply generating Electric Light, and the quality of that light compared with solar light.

The electrical display on top of the Massachusetts State House one Fourth of July night, which shed a flood of light that enabled people to read newspapers on Boston Common, was useful in indicating what may be profitably done by apparatus less clumsy and less costly. So also with the " weighty machinery " that produces brilliant results in fog or clear weather off the Dungeness Lighthouse on the British coast. Simplify and cheapen the mode of effecting such results, and what is there to prevent the general use of electricity in producing light? Why should not our streets and our harbors, our factories and workshops, our offices and our homes, be thus brilliantly and cheaply illuminated, now that American scientific skill has devised certain modes of producing electric light which are claimed to be "more economical and more effective than any of the methods hitherto employed?"

We scarcely open a foreign scientific journal without finding allusions to various uses for which electric light are employed, or about to be employed. One of the latest papers (March 8) says that one of these lights is arranged for service on board the Prince Jerome, Prince Napoleon's yacht—not to illuminate the vessel itself; but, on the contrary, to throw light on other objects, such as a vessel, or a coast, for purposes of attack or defense. For such uses, especially in cases of fog, this light promises to prove very valuable, as ships at full speed, or lying-to, or at anchorage, can thus be kept free from danger of collisions, and, in battle, the object to be attacked can be illuminated. "However late in the field of practical adaptation," says *The London Chemical News*, "the Electric Light, once established on board of a vessel, will become a necessary adjunct to the marine and transport service," and invaluable on board of the fleet passenger steamers. "The illumination of this same light for railways and at the stations, and the approaches thereto, in tunnels, at curves, and otherwise," says a Parisian writer, "has been the order of the day, and the subject of deep study for some weeks past;" and the experiments on the East Railway of France are said to have been "so successful, that there is reason to expect the best results." Why may not this new light be advantageously used on locomotives?

Multitudinous ways will be found for employing a light so powerful, and so easily and cheaply generated; and, profiting by experience of "the way they do these things in France," and in England, our American inventors, who claim to have devised still better methods, can hardly fail to meet prompt and proper attention from people who are annoyed and overtaxed by the effluvia and extortion of the gas companies.

On April 3d, 1867, there was filed in the New York County Clerk's Office, the following certificate:

CERTIFICATE FILED IN NEW YORK COUNTY CLERK'S OFFICE, APRIL 3RD, 1867.

AMERICAN ELECTRIC LIGHT CO.

Was organized March 7th, 1867, by Henry O'Reilly, Charles A. Seeley, Lorenzo Sherwood and John W. Orr, for the manufacture, purchase and sale of machinery, material and apparatus for generating electricity for producing Electric light for illuminating cities and edifices of all kinds, on land and sea—Capital Stock $300,000—Duration of Company fifty years.

Names of trustees (of whom there are seven) for the first year are Henry O'Reilly, Charles A. Seeley, Lorenzo Sherwood, John W. Orr, Horace Greeley, A. E. Beach and J. Dwight Porter. The principal places of business are at New York City, Brooklyn and Rochester, N. Y.

The certificate of incorporation was executed by H. O'Reilly, C. A. Seeley and John W. Orr, April 2d, 1867, and sworn to before Gardiner Spring, Jr., Notary Public.

Of the gentlemen named in this certificate, Mr. A. E. Beach alone is living. Professor Seeley told me some years ago that Mr. Greeley had an idea of making the organization serve to some extent the same purposes as this society. That is, the advancement of electrical science.

Mr. Beach's recollection of the company is that it was intended to secure electrical inventions as fast as they made their appearance, in fact to build up a large organization on the plan of some of our modern companies. Mr. Beach published in the *Scientific American* of April 20th, 1867, an article entitled "Electric Light; Wilde's Magneto Electric Machine"

[From the *Scientific American*, April 20th, 1867, p. 248.]

ELECTRIC LIGHT—WILDE'S MAGNETO ELECTRIC MACHINE.

The subject of electric light is now attracting more attention than at any former period. Every day we find something about it in our foreign and domestic exchanges. Three of the city newspapers within a few weeks have printed full column editorials under the title of Electric Light. The lighthouse directors of England, France and America, simultaneously but independently, are instituting experiments on a liberal scale to determine if the electric light may not be the best for lighthouses. And as a natural consequence of these movements, a business association under the style "The American Electric Light Co.," has been chartered and will be soon in active operation. This new interest in electric light was originated by the exhibition in England of the machine which is described in this article. No wonder, for the light from it is the most powerful artificial light ever produced. Mr. Crookes testifies that when he saw it, it had three or four times the power of the sun light. The machines first built were found too powerful for lighthouse illumination, and it was found advisable to make machines for that purpose of only one-half the original sizes.

Professor Seeley had never seen a self-exciting dynamo, but had kept well abreast of the European work on the subject so far as possible by reading, and was also familiar with the arc of Davy and the incandescent carbon of Starr. The machine built by him is before you, and you can see that the design is not bad, considering the date of manufacture. It passed through a fire in Professor Seeley's office at 26 Pine Street, early in December, 1874, and was rewound with the same field wire with new insulation, and new armature wire as nearly like the original as possible.

Mr. Wm. H. Burnap, of 259 West Twenty-seventh Street, who made the repairs, confirms Professor [Seeley's recollection as to these points. Professor Moses G. Farmer saw the machine when it was comparatively new, but the exact date I have not been able to ascertain. Professor Farmer estimated the capacity of the machine as about two or three telegraph cells, meaning Grove cells. This estimate is evidently too low if the machine be run at a reasonable speed, but the machine was originally turned by hand power, and the estimate was probably about right. The necessity of a high speed to secure a reasonable output was not so well understood when the machine was built as it is now. Mr. Weston saw the machine in the summer of 1872.

Dr. Vander Weyde remembers the machine well, and is quite certain that it was built before the company was organized, which confirmed what has already been said, but cannot fix the date any more accurately. Dr. Vander Weyde also says that he remembers when the machine was first started, that there was some difficulty in getting it to excite, owing to the field not being connected in the proper relation to the direction of armature winding and rotation. That is, the machine instead of exciting itself, demagnetized itself until the connections were reversed. This was quite a common error a long time after.

THE PRESIDENT:—I take great pleasure on behalf of the Institute in accepting from Mrs. Seeley this very interesting machine, a forerunner of a large part of the electrical development of this country. I think it proper that a committee should draw up a resolution to be transmitted to Mrs. Seeley thanking her for her kindness in making this donation. I appoint for this purpose Mr. Martin, Chairman, and Mr. Kennelly and Mr. Birdsall as members. The sentiment of this meeting will be unanimous in favor of this action, so that it is unnecessary to put the motion.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

New York, April 18th, 1893.

The seventy-sixth meeting of the Institute was held this date at 12 West Thirty-first Street, and was called to order at 8.30 P. M. by Vice-President William J. Hammer.

THE SECRETARY:—At the Council meeting held this afternoon the following associate members were elected:

Name.	Address.	Endorsed by
ADAMS, ALTON D.	Electrician and Manager, Commercial Electric Co., 113 South Tennessee St., Indianapolis, Ind.	Fred. DeLand. L. L. Summers. W. A. Rosenbaum.
CRANDALL, JOSEPH EDWIN,	Electrician, C. & P. Telephone Co., 619 14th St, N. W., Washington, D. C.	J. J. Carty. T. D. Lockwood. F. A. Pickernell.
DEMING, EDWARD	General Manager, Deming Automatic Electric Safety System, 522 Hancock St., Brooklyn, N. Y.	T. C. Martin. Jos. Wetzler. Edwd. P. Thompson.
FLAGG, STANLEY G., JR.,	Stanley G. Flagg & Co., 19th St. and Penna. Ave., Philadelphia, Pa.	J. B. Cahoon. Harris J. Ryan. Ralph W. Pope.
GARDANIER, GEORGE W.	Electrician, Western Union Telegraph Co., 195 Broadway, New York City.	G. A. Hamilton. Alfred S. Brown. James Hamblet.
GERRY, M. H., JR.,	Engineer, N. W., General Electric Co., 3333 Cedar Ave., Minneapolis, Minn.	Chas. K. Stearns. H. M. Byllesby. Geo. D. Shepardson.
HALL, EDWARD J.	Vice-President and General Manager, American Telephone and Telegraph Co., 18 Cortlandt St., New York City.	Hammond V. Hayes. John J. Carty. Wm. S. Ford.
HAYES, HARRY E.	Asst. Electrician, American Telegraph and Telephone Co., 153 Cedar St., New York City.	F. A. Pickernell, H. V. Hayes. F. W. Dunbar.
HUMPHREYS, WM. J.	Professor of Physics and Mathematics, Miller School, Crozet, Va.	T. J. Smith. Ralph W. Pope. Geo. H. Stockbridge.
PFUND, RICHARD	Richard Pfund, Electrical Apparatus and Supplies, 153 Broadway, Brooklyn, N. Y.	James Hamblet. Alfred S. Brown. C. L. Buckingham.
PIERCÉ, RICHARD H.	Electrical Engineer, World's Columbian Exposition, 5434 Monroe Ave., Hyde Park, Ill.	F. A. Pickernell. Ralph W. Pope. F. W. Dunbar.

USINA, M. NELIGAN	Superintendent of Motive Power, City and Suburban Railway, 78 Bolton St., Savannah, Ga.	R. J. Nunn, M.D. Ralph W. Pope. T. C. Martin.
WILLIAMSON, G. DEWITT,	Electrician General Electric Co., New York City, Dobbs Ferry, N. Y.	J. H. Vail. Charles Hewitt. H. W. Weller.

Total 13.

And the following associate members were transferred to full membership:

WILLIAMS, JAMES B., M. D.	Electrician, 44 Broadway, New York City.
SMITH, FRANK STUART,	Supt. of Carbon Department, Westinghouse Electric and Mfg. Co., Pittsburg, Pa.
ROSS, ROBERT A.	Engineer in charge of Engineering Dept., Edison General Electric Co., Peterboro, Ont.
NOLL, AUGUSTUS	New York Electric Equipment Company, 59 Duane St., New York City.
HERING, HERMANN S.	Associate in Electrical Engineering, Johns Hopkins University, Baltimore, Md.

Total, 5.

THE CHAIRMAN [W. J. Hammer]:—The Chair wishes to announce that at the meeting of the Council this afternoon the certificate of membership which, doubtless, you have all seen in the adjoining room, was adopted with one or two slight changes.

This certificate will be issued to full members only. It will be a compliment from the Institute to those who have attained a high rank professionally, and whose work in their profession and for the Institute entitles them to receive it. These certificates will probably be ready within two or three weeks and will be printed upon heavy parchment paper at about cost, namely, one dollar. The Secretary has been instructed to send out a notice, stating the conditions under which this certificate is given and other matters pertaining thereto, as soon as they are ready for distribution. The American Institute of Electrical Engineers desires to raise its status as a scientific organization in every way possible, and it is believed that by issuing this certificate to full members only, the honor conferred will be more highly appreciated and it will result in a desire upon the part of many of our associate members to apply for and to secure full membership in the Institute. While in some of the engineering societies the majority of the members are active or full members, and there are comparatively few associate members, in the Institute of Electrical Engineers the opposite is the case, and the issuing of this certificate under the conditions as recommended by the Special Committee will prove advantageous to the interests of the Institute as well as to the members receiving the same.

I will refer to the subject of badges as several members have asked me about them to-night. Some of the badges were delivered to-day, and I might say that within the next week or ten days the first fifty will be ready. Five or six moulds have been made and a great deal of trouble has been taken to make them correct. Those who apply to the Secretary first will naturally receive the badges first. The price is $3.00.

There is one other point to which I would like to call the attention of the Institute before taking up the paper. On behalf of the Ways and Means Committee of the Institute in charge of the World's Fair matters I would like to state, as you have already heard, that headquarters have been secured at the Columbian Exposition. The authorities there have given us two excellent rooms adjoining the official headquarters in the Electricity Building. They have promised to give us excellent facilities there—long-distance telephone service, electric light, telegraph service, fire protection and police patrol. The Council has instructed its Secretary to go there and represent the Institute during the meeting of the International Electrical Congress and largely during the season. We have extended invitations to our foreign electrical friends to make use of our headquarters, to have their mail addressed there, to write their letters there and to see their friends there, and it is very desirable that we should make those headquarters interesting, and with that end in view, the Committee is endeavoring to secure a large number of exhibits of great historical interest. We want letters, books, small models and things of that character, autographs, photographs, etc., which will be of interest to our friends and to our members. We would like to make our headquarters the most attractive spot in the Electrical Building. You have doubtless noticed in the other room small reproductions of photographs of a great many eminent men. Those are largely based on a number of photographs that I have been collecting for many years, and there will probably be four or five of those large frames of photographs sent out there. The Committee has been promised some of the things that belonged to Professor Morse and to Franklin, and has already received some of them. The Secretary placed in the Committee's hands to-day the original list of the members who signed the call for the organization of the American Institute of Electrical Engineers. That will be placed in the exhibition. These and many other things which we shall have, will make the Institute headquarters very interesting to all our members and our visiting friends. I might state that one of the large publishers, Van Nostrand, has promised me to place in a few days a complete electrical library at the headquarters, and that this library, under the care of the Secretary will be at the disposal of our members. It will be a very complete and doubtless a very interesting exhibit. I trust that members who have interesting things will send them to the Secretary or to myself as Chairman of the Committee, and if they know of some particular feature which would be of interest and value that they would kindly communicate with the Secretary or the Committee.

We will now have the pleasure of listening to Mr. A. E. Kennelly's paper upon "Impedance."

[Mr. Kennelly then read the following paper.]

A paper presented at the seventy-sixth meeting of the American Institute of Electrical Engineers, New York, April 18th, 1893, Vice-President Hammer in the Chair.

IMPEDANCE.

BY A. E. KENNELLY.

The impedance of a conductor is its apparent resistance, and is expressible in ohms. More strictly, it has been defined as the ratio of the effective E. M. F. between the terminals of the conductor, to the effective current strength it carries. By voltage or current in the sequel, effective voltage and effective current, unless otherwise specified, will be intended,—such as would be indicated by properly calibrated alternating current voltmeters and ammeters.

With steady continuous currents, the impedance of a conductor is its simple resistance. With periodically fluctuating currents, the impedance will in general differ from the resistance, and will be greater or less, but in common practice greater. With such voltages and currents, Ohm's law becomes

$$C = \frac{E}{I}$$

where the impedance is represented by I.

We may first consider periodic currents and voltages of the simple harmonic type, following waves in time such as could be traced upon a band of paper, moving steadily lengthwise, by the vertical shadow of the crank-pin on a fly-wheel revolving uniformly in a vertical plane perpendicular to the band. [Fig. 1.] The ratio of the velocity between wheel and paper would control the spacing and steepness of the waves, but would not alter their sinusoidal type, while the motion of the shadow would be simply harmonic.

If the fly-wheel makes n complete periods or revolutions per second, and the distance of the crank-pin from the shaft axis—its crank radius—is unity, then the circumference of the circle it traces will be 2π, and the total distance it will run through in one second will be $2\pi n$ or $6.283\, n$ linear units, which we may call its speed, and denote by $p = 2\pi n$. If then the pin is shifted outwards upon the wheel until its crank-radius or distance from the axis is l units, where l is the number of henrys in any particular inductance, then the corresponding travel of the pin will be $2\pi l n$ linear units per second, and this speed $p\, l$ we may call the inductance-speed for the particular frequency and inductance l considered.

Non-ferric inductances (if the term be permitted), or inductances without iron may be first assumed, leaving ferric inductances and conductors embracing iron, for later examination.

In order to find the impedance of any conductor whose resis-

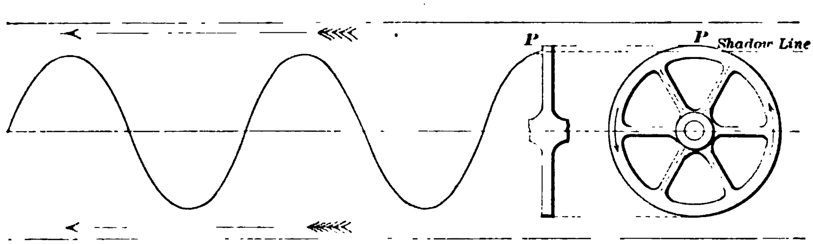


FIG. 1.

tance is r ohms, and inductance l henrys, we construct the triangle having as base the length r, the perpendicular of $p\, l$ units, and draw the hypothenuse which will have the length $\sqrt{r^2 + (p\, l)^2}$ between their free extremities. [See Fig. 1.] This length will be the impedance in ohms. The impedance is therefore the geometrical or vector[1] sum of the resistance and inductance-speed, when these are plotted on two rectangular axes. Calling this impedance $\dot{\imath}$, Ohm's law gives

$$c = \frac{e}{\dot{\imath}} \qquad e = \dot{\imath}\, c \qquad \dot{\imath} = \frac{e}{c}$$

corresponding to the usual formulas for continuous currents. In

1 The vector sum of two lines A B and B C is the line A C, since a transference from A to B followed by a transference from B to C is geometrically equivalent to a transference from A to C.

Fig. 2 a resistance of 75 ω having an inductance of 0.06 henry with a consequent inductance speed at 100 ~, of

$$6.283 \times 100 \times 0.06 = 37.7$$

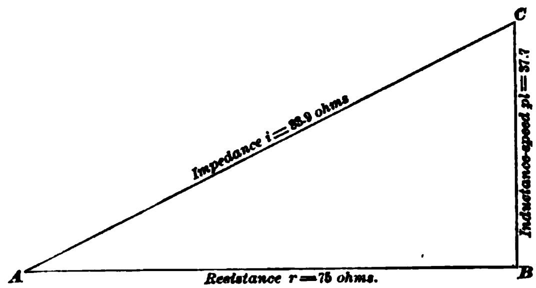


FIG. 2.

is shown to possess an impedance $\dot{i}$ of

$$\sqrt{(75)^2 + (37.7)^2} = 83.9 \text{ ohms.}$$

If two such impedances, $\dot{i}_1$ and $\dot{i}_2$, be connected in series, their total impedance [Fig. 3] will be the vector sum A C of $\dot{i}_1$ and $\dot{i}_2$ corresponding to the arithmetical sum for unvarying currents. This vector sum is most conveniently constructed geometrically by summing separately the component resistances r_1, r_2, A G, and then the component inductance-speeds, G C, as shown. Calling the vector sum I,

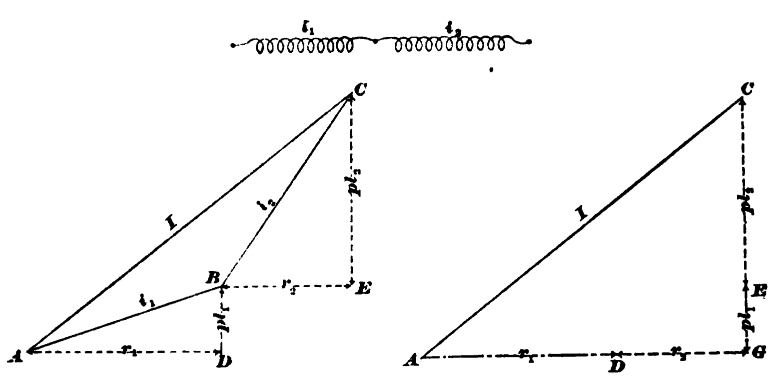


FIG. 3.

the voltage on I for a current of c amperes will be $c\,I$ volts.
" " " $\dot{i}_1$ " " " " " " $c\,\dot{i}_1$ "
" " " $\dot{i}_2$ " " " " " " $c\,\dot{i}_2$ "

but I will not be the arithmetical sum of i_1 and i_2, nor will the drop on I be the sum of the drops of i_1 and i_2, unless the separate time constants, $\frac{l_1}{r_1}$ and $\frac{l_2}{r_2}$ are equal, when A B will be in line

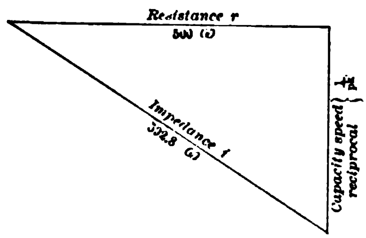


FIG. 4.

with B C; the reason being that otherwise the currents in i_1 and i_2 while of equal strength, are out of step.

In Fig. 3, r_1 represents to scale, a resistance of 30 ω, r_2 20 ω, $p\,l_1$ an inductance speed of 10, $p\,l_2$ 30. Then

$$i_1 \text{ is } \sqrt{900 + 100} = 31.63 \text{ ohms},$$

$$i_2 \text{ is } \sqrt{400 + 900} = 36.06 \text{ ohms},$$

$$I = \sqrt{2500 + 1600} = 64.0 \text{ ohms},$$

whereas the arithmetical sum of i_1 and i_2 would be 67.69 ohms.

A condenser of capacity k farads (millions of microfarads) has an impedance of $\frac{1}{p\,k}$ ohms, or in other words, its impedance

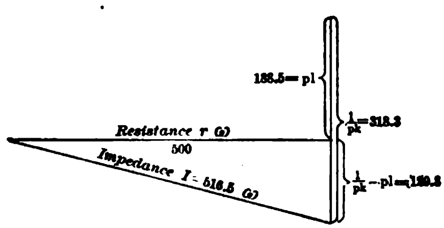


FIG. 5.

is the reciprocal of its capacity speed.[1] The current in a condenser supplied by an E. M. F. e at $n \sim$ is

$$e \div \frac{1}{p\,k} = e\,p\,k.$$

1 Fleming, "The Alternate Current Transformer," Vol. II., p. 378.

Thus, 1000 volts alternating give through 5 microfarads at 100 ∼

$$1000 \times 6.283 \times 100 \times \frac{5}{1{,}000{,}000} = 3.14 \text{ amperes.}$$

A condenser k in series with a resistance r has a total impedance

$$\sqrt{r^2 + \left(\frac{1}{p\,k}\right)^2}$$

which is found by taking the base r, and the capacity-speed

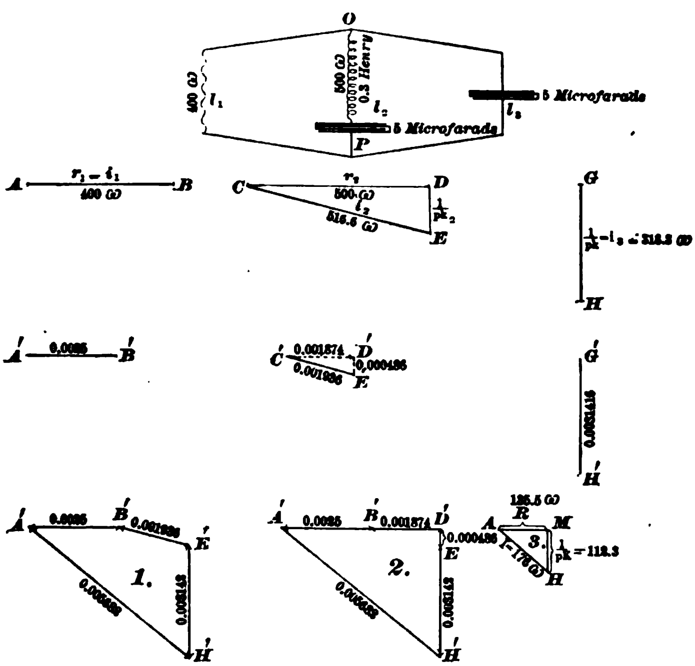


Fig. 6.

reciprocal $\frac{1}{p\,k}$ downwards perpendicularly. In other words, its impedance is the vector sum of the resistance and capacity-speed reciprocal.

Fig. 4 represents the five-microfarad-condenser above considered, whose impedance $\frac{1}{p\,k}$ at 100 ∼ is 318.3 ohms, in circuit with a non-inductive resistance r of 500 ohms. The resulting impedance

$$I = \sqrt{500^2 + 318.3^2} = 592.8 \text{ ohms,}$$

so that 1000 volts alternating harmonically at that frequency would, when connected to this series deliver

$$\frac{1000}{592.8} = 1.687 \text{ amperes through the same.}$$

If again, inductance is inserted in series with the condenser

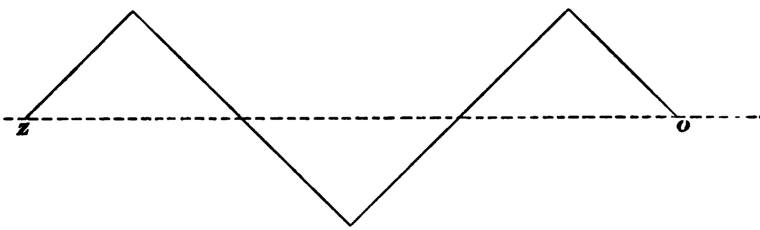


FIG. 7.

and resistance, the total impedance of the three, independent of their order, will be the vector sum of the resistance, the inductance-speed, and the capacity-speed-reciprocal, the second being drawn upwards, and the third negatively or downwards, as shown in Fig. 5, where an inductance of 300 millihenrys = 0.3 henry, giving at 100 ~ an inductance-speed of

$$628.3 \times 0.3 = 188.5,$$

is introduced into the same circuit of resistance and condenser. The effect is to diminish the impedance from 592.8 to 516.6

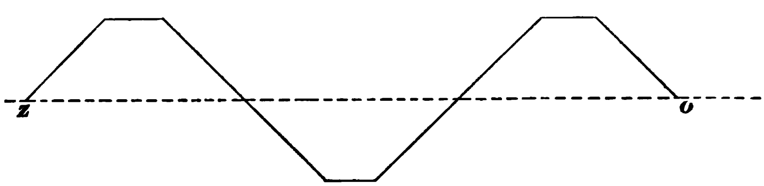


FIG. 8.

ohms, increasing the current to 1.936 amperes. If I be the resulting impedance,

$$c = \frac{e}{I},$$

and the voltage across the resistance and inductance supposing these combined in one coil will be $c\, i_1$ where i_1 is the impedance of these two considered together, and the voltage across the condenser will be

$$c \cdot \frac{1}{p\,k}.$$

It is evident that this latter may be greater than e when suitable values are chosen so that one or both of the factors, current and capacity-speed-reciprocal, are large. In such cases the condenser voltage exceeds the voltage of supply, and what is commonly called the "Ferranti effect," is developed.

It is also evident from the construction of the diagrams, that the inductance will neutralize the capacity in its series or *vice-versa*, when the verticals above and below are equal, or when the inductance-speed equals the capacity-speed-reciprocal, and in this case the impedance degrades into the simple resistance. This condition requires that

$$p\,l = \frac{1}{k\,p}, \text{ or } l = \frac{1}{k\,p^2}, \text{ or } k = \frac{1}{l\,p^2}.$$

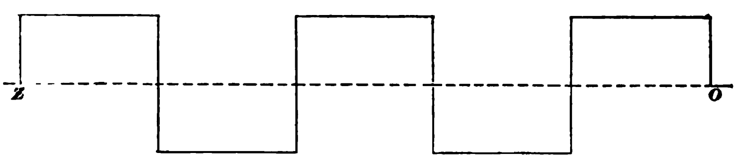


FIG. 9.

With unvarying currents, the combined resistance of resistances in parallel, is the reciprocal of the sum of their reciprocals. Thus, if three resistances be connected in multiple, of 3, 2, and 6 ohms respectively, their joint resistance is

$$\frac{1}{\frac{1}{3} + \frac{1}{2} + \frac{1}{6}} = 1.$$

In the same way, the joint impedance of impedances in multiple, is the reciprocal of the vector sum of their reciprocals. Thus suppose that three impedances, $\dot{i}_1$, $\dot{i}_2$, $\dot{i}_3$, are connected in parallel, the first being, for example, a simple resistance; the second a series of resistance, inductance, and capacity; the third a simple condenser. These three are diagramatically represented in Fig. 6. Take the numerical reciprocals of $\dot{i}_1$, $\dot{i}_2$, and $\dot{i}_3$, and lay them off in their respective directions. Take the vector sum of these, which is most conveniently done by resolving the reciprocals into components, summing r s alone and $p\,l$ s alone. Finally the reciprocal of this vector sum, laid off in its direction, will be the joint impedance of the combination, and the components of this impedance will represent the equivalent resistance, and the equivalent capacity-speed-reciprocal or equivalent inductance-speed of the combination.

Thus, in Fig. 6, $\dot{i}_1 = r_1 = 400\ \omega$ non-inductive: $\dot{i}_2$ is the series of inductance, resistance, and capacity, taken from the last case represented in Fig. 5, and $\dot{i}_3$ is a simple condenser of 5 microfarads or 0.000005 farads whose capacity speed will be $628.3 \times 0.000005 = 0.0031416$, and whose impedance, the reciprocal of this, is 318.3 ohms: $\dot{i}_1$, $\dot{i}_2$ and $\dot{i}_3$ are represented to scale

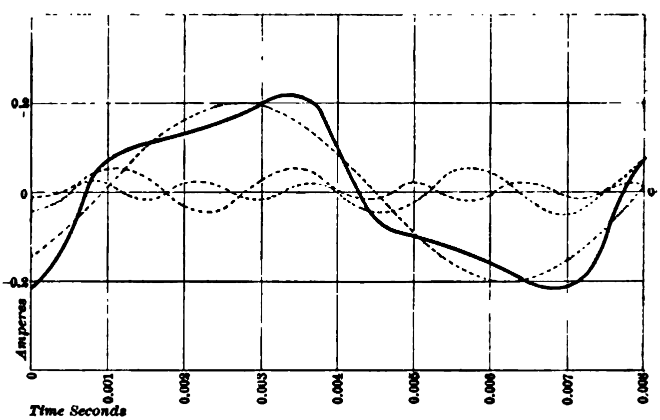


FIG. 10.

in their proper directions, A B $= \dot{i}_1$ horizontal, C E $= \dot{i}_2$ diagonal, and G H $= \dot{i}_3$ vertically downwards. Immediately beneath each of these are their respective vector reciprocals,

A′ B′ = 0.0025, C′ E′ = 0.001936, and G′ H′ = 0.0031416,

maintaining their directions, but to an altered dimensional scale for convenience in working. The vector sum of these reciprocals, A′ B′, C′ E′ and G′ H′, is A′ H′, indicated in the quadrilateral 1 and measures 0.005682. The most convenient way to obtain this practically is, however, shown at triangle 2, where the projected horizontal component of C′ E′, namely C D 0.001874, is added to A′ B′, and also the projected vertical component D′ E′ 0.00048, to G′ H′. The hypothenuse of the tri-

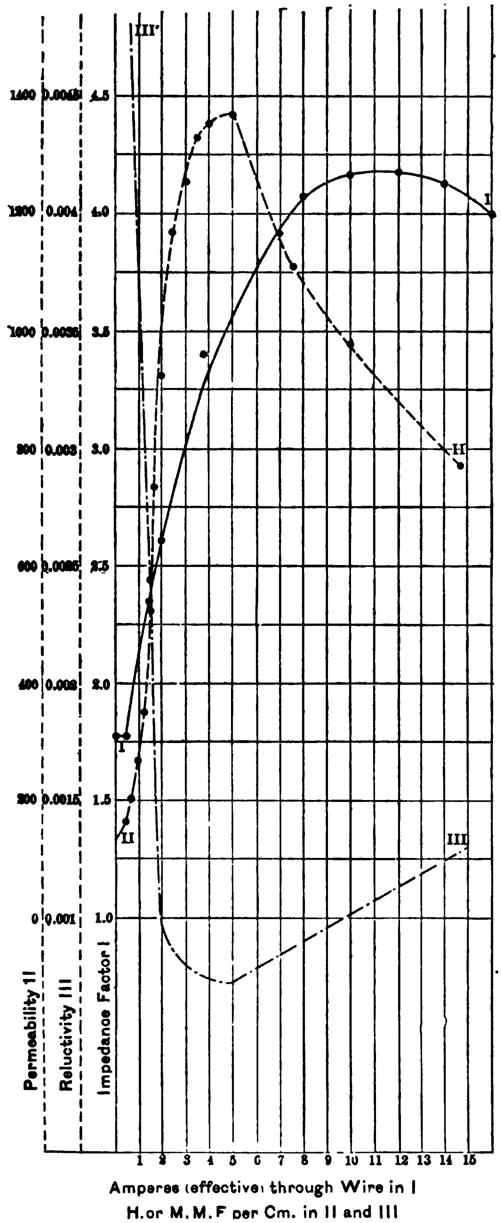


FIG. 11.

Curve I., Observed Impedance Factors; II., Observed Permeability; III., Observed Reluctivity.

Particulars: No. 5, B. W. G. soft iron wire, diameter 0.566 cm., stretched in one straight loop 2785 cms. long, with 10 cm. interaxial distance.

Resistance of iron wire 47.47 microhms per linear cm. @ 15° C.

Resistivity 11.84 microhms @ 15° C.

angle, A′ H′, then measures 0.005682 either arithmetically or geometrically. The vector reciprocal of A′ H′ is A H, represented at triangle 3, and measures 1/0.005682 or 176 ohms on the original scale, with projected components A M 135.5 ω and M H capacity-speed-reciprocal 112.3.

If the voltage measured between terminals O and P is 1000 volts, the current in the main leads to O and P is

$$\frac{1000}{176} = 5.682 \text{ amperes.}$$

But the current in the branch i_1 is $\dfrac{1000}{i_1} = \dfrac{1000}{400} = 2.5$ amperes

Also " " " " " i_2 " $\dfrac{1000}{i_2} = \dfrac{1000}{516.6} = 1.936$ "

And " " " " " i_3 " $\dfrac{1000}{i_3} = \dfrac{1000}{318.3} = 3.142$ "

Sum, 7.578

The sum of the branch currents is therefore greater in this instance than the current supplied through the main, for the reason that these currents are not in step; but at any one instant the main current and the branch currents' sum must be equal; it is only in the process of averaging over the cycle that the difference manifests itself.

If two conductors are connected in parallel, as for example a galvanometer and its shunt, of resistances g and s respectively, we know that if C is the unvarying current supplied through the combination, the portion through one of them,—the galvanometer say—will be $C\dfrac{s}{g+s}$. In the same way, if G and S are their respective impedances to a harmonic current, the galvanometer's share will be $C\dfrac{S}{G+S}$, where, however, $G+S$ is not the arithmetical sum, but the vector sum of the two impedances. Either G or S may contain capacity, provided that this is included in its impedance by the usual rule of laying off the capacity-speed-reciprocal against the inductance-speed.

Again, the joint resistance formula for galvanometer and shunt with steady currents being

$$r = \frac{g\,s}{g+s},$$

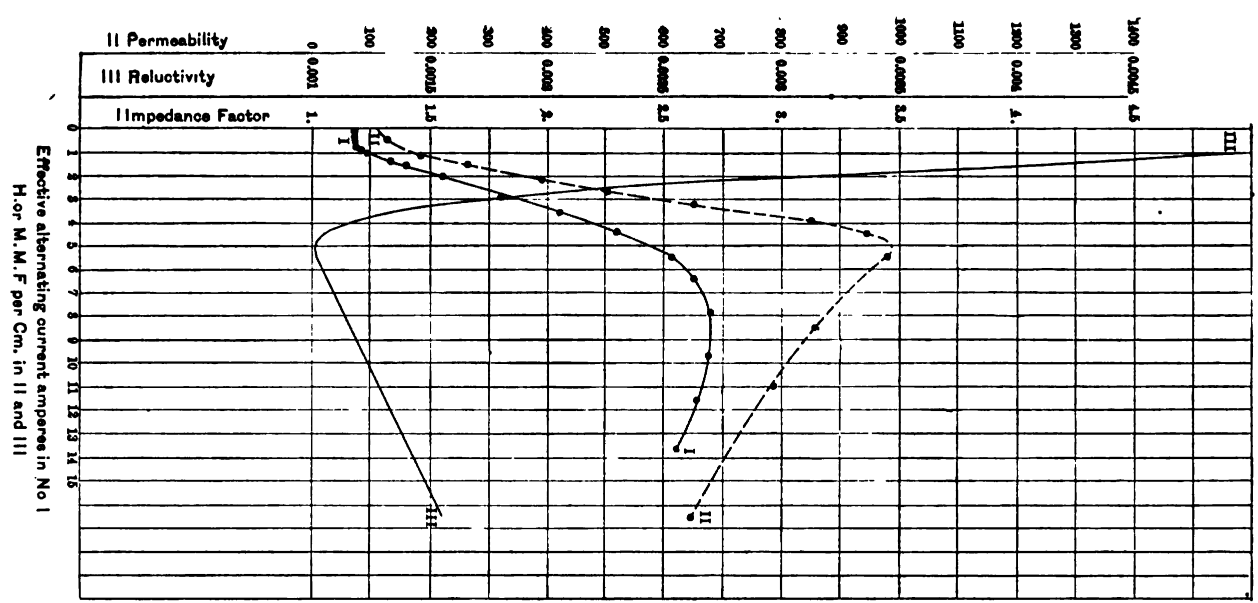


FIG. 12.

Curves of impedance and permeability observed in a galvanized iron wire No. 9, B. W. G. Diameter 0.148″ = 0.375 cm. drawn into one straight loop 2690 cms. long and 10 cms. interaxial distance. Specific gravity of wire, 7.76. Impedance of loop compared with a loop of german silver wire of diam. 0.28 cm. 1,000 cms. long and 7 cms interaxial distance by differential dynamometer. E. M. F. of generator very closely sinusoidal. Frequency 140 ~. Temp. of wire under test 10° C. Resistance of iron wire per linear cm. 208 microhms. Resistivity 12.09 microhms at 10° C.

Curve I., Impedance factor as function of current; II., Permeability as function of *H*; III., Reluctivity as function of *H*.

the corresponding formula is

$$i = \frac{G\,S}{G + S},$$

where $G\,S$ is the arithmetical product, but $G + S$ a vector sum. This case could also be dealt with by the rule for joint impedances

$$i = \frac{1}{\frac{1}{G} + \frac{1}{S}},$$

where the vector reciprocals are added vectorially.

All continuous current corollaries from Ohm's law thus become applicable to harmonic currents, when the sums and differences of impedances or their reciprocals are treated vectorially, and if all alternating currents were strict sinusoids, all inductances non-ferric, there would be almost as little difficulty in reckoning their quantitative relations as in continuous current circuits. But Kirchoff's laws, while true instantaneously, are only applicable to alternating current circuits, when the currents are all in step, and the vector sums degrade into arithmetical sums.

Algebraically, all, and more than all the foregoing statements concerning impedance combinations are contained in the following:

Any combination of resistances, non-ferric inductances, and capacities, carrying harmonically alternating currents, may be treated by the rules of unvarying currents, if the inductances are considered as resistances of the form $p\,l\,\sqrt{-1}$, and the capacities as resistances of the form $-\frac{1}{k\,p}\sqrt{-1}$, the algebraic operations being then performed according to the laws controlling "complex quantities."

A valuable application of the principles of impedance is to the determination of the limits of error in measuring apparatus—such as voltmeters—intended for both alternating and continuous current circuits.

For example, a particular sample of alternating and continuous current voltmeter, consisting of a dynamometer without iron, in circuit with a resistance coil, has a resistance of 2,085 ohms, and an inductance that varies with the relative position of the dynamometer coils, but which does not exceed 91.5 millihenrys = 0.0915 henry. At a frequency of 140 $\sim$ which is about the maximum in ordinary use, the inductance speed of this instrument will not be greater than $0.0915 \times 6.283 \times 140 = 80.5$. Constructing the right-angled triangle with base 2085, and perpendicular 80.5, we find the hypothenuse to be $\sqrt{2085^2 + 80.5^2} = 2086.5$ which is the impedance of the instrument at this frequency, so that 50 volts E. M. F. harmonically alternating would force less current through the dynamometer than 50 volts continuous E. M. F. in the ratio of $\frac{2085}{2086.5}$, and would therefore under-indicate by about 0.075 per cent.

A similar instance is afforded by the Thomson wattmeter. The armature of this instrument has a certain non-ferric inductance and is connected across the supply mains through a resistance. The equality of the meter's record for both alternating and continuous current circuits depends upon the equality of current strength through the armature under equal alternating and continuous voltages, and this is again dependent on the impedance of the armature circuit. In a particular instrument of 1,500 watts capacity (50 volts and 30 amperes) the total resistance of this circuit is 455.5 ohms, and its total inductance 25 millihenrys (0.025 henry). This represents at 140 $\sim$ an inductance-speed of 22, and an impedance of 456.0 ohms = $\sqrt{(455.5)^2 + (22)^2}$ so that the current in the armature will be about one-ninth of one per cent. less, on a supply pressure of 50 volts, alternating harmonically, than on 50 volts continuous. Strictly speaking the accuracy of a wattmeter depends not only upon the non-diminution of current strength through fine wire circuit in virtue of inductance, but also upon the lag in phase that is also there established. It is generally safe to assume, however, that when the error due to impedance is very small, the error due to lag is very small also.

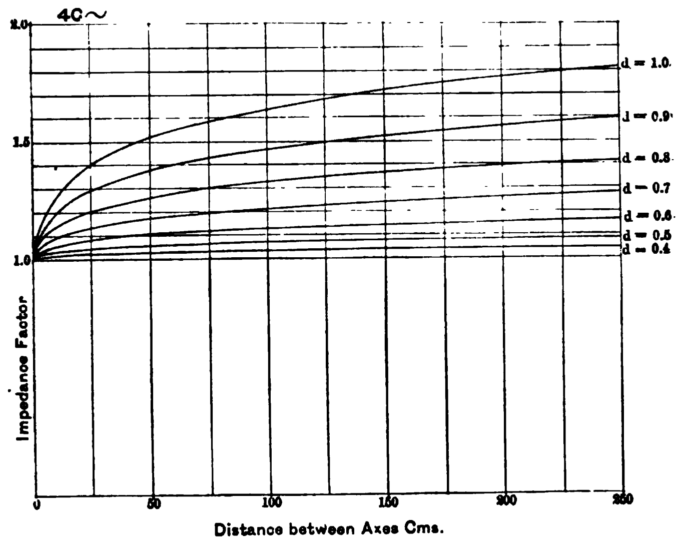
An important application of impedance principles is to conductors for the distribution of alternating currents. Let the common case be considered of two parallel overhead wires running from a central station to a consumption point one mile distant, each wire of copper, No. 4, A. W. G., 0.2043 in. (0.519 cm.) in diameter, fixed on pole insulators, at a distance between their axes of 30 inches. At a temperature of 15° *C* this wire of standard conductivity, would have a resistance of 1.285 ohms per mile of 5,280 feet, and if the wire actually possessed this conductivity, the resistance of the loop would be 2.57 ohms. An unvarying current of 10 amperes would establish in this wire a

TABLE I. Impedance factors for pairs of parallel copper wires carrying simple harmonic currents of 140 ~

$\frac{D}{r}$	Diam. 1 m. m. Rad. 0.5 m. m.		Diam. 0.2 cm. Rad. 0.1 cm.		Diam. 0.3 cm. Rad. 0.15 cm.		Diam. 0.4 cm. Rad. 0.2 cm.		Diam. 0.5 cm. Rad. 0.25 cm.	
	D. cms.	factor.	*D.* cms.	factor.	*D.* cms.	factor.	*D.* cms.	factor.	*D.* cms.	factor.
2	0.1	1.00003	0.2	1.0005	0.3	1.002	0.4	1.0075	0.5	1.019
3							0.6	1.0155	0.75	1.037
4	0.2	1.00009	0.4	1.0014	0.6	1.007	0.8	1.023	1.0	1.055
5							1.	1.029	1.25	1.070
6	0.3	1.0001	0.6	1.0022	0.9	1.011	1.2	1.035	1.5	1.084
8							1.6	1.046	2.0	1.108
10	0.5	1.0002	1.0	1.0035	1.5	1.018	2.	1.055	2.5	1.128
12							2.4	1.062	3.	1.146
16	0.8	1.0003	1.6	1.005	2.4	1.024	3.2	1.076	4.	1.176
20							4.	1.087	5.	1.200
25	1.25	1.0004	2.5	1.0065	3.75	1.032	5.	1.098	6.25	1.226
30							6.	1.109	7.5	1.248
40	2.0	1.0005	4.	1.0083	6.	1.041	8.	1.125	10.	1.284
60							12.	1.150	15.	1.338
80	4.0	1.0007	8.	1.0115	12.	1.057	16.	1.170	20.	1.378
100							20.	1.185	25.	1.409
150	7.5	1.001	15.	1.015	22.5	1.073	30.	1.214	37.5	1.469
200							40.	1.236	50.	1.513
300	15.	1.0012	30.	1.019	45.	1.092	60.	1.268	75.	1.576
400							80.	1.291	100.	1.622
500	25.	1.0014	50.	1.022	75.	1.108	100.	1.310	125.	1.658
600							120.	1.325	150.	1.688
700	35.	1.0016	70.	1.025	105.	1.119	140.	1.339	175.	1.713
800							160.	1.351	200.	1.736
900	45.	1.0017	90.	1.026	135.	1.127	180.	1.362	225.	1.756
1,000							200.	1.370	250.	1.773
1,500	75.	1.0019	150.	1.030	225.	1.145	300.	1.407	375.	1.842
2,000					300.	1.155	400.	1.434	500.	1.892
3,000	150.	1.0023	300.	1.036	450.	1.170	600.	1.472	750.	1.963
4,000					600.	1.181	800.	1.492	1000.	2.013
5,000	250.	1.0026	500.	1.040	750.	1.190	1000.	1.522	1250.	2.053
10,000			1000.	1.047	1500.	1.220	2000.	1.595	2500.	2.183
15,000			1500.	1.051	2250.	1.236	3000.	1.633	3750.	2.252
20,000	1000.	1.0034	2000.	1.054	3000.	1.249	4000.	1.664	5000.	2.305

$\frac{D}{r}$	Diam. 0.6 cm. Rad. 0.3 cm.		Diam. 0.7 cm. Rad. 0.35 cm.		Diam. 0.8 cm. Rad. 0.4 cm.		Diam. 0.9 cm. Rad. 0.45 cm.		Diam. 1.0 cm Rad. 0.5 cm.	
	D. cm.	factor.	*D.* cms.	factor.	*D.* cms.	factor.	*D.* cms.	factor.	*D.* cms.	factor.
2	0.6	1.038	0.7	1.069	0.8	1.115	0.9	1.179	1.	1.263
3	0.9	1.076	1.05	1.137	1.2	1.224	1.35	1.341	1.5	1.489
4	1.2	1.110	1.4	1.196	1.6	1.317	1.8	1.475	2.	1.671
5	1.5	1.140	1.75	1.247	2.	1.396	2.25	1.587	2.5	1.821
6	1.8	1.167	2.1	1.2925	2.4	1.464	2.7	1.683	3.	1.947
8	2.4	1.213	2.8	1.3683	3.2	1.577	3.6	1.839	4.	2.152
10	3.	1.251	3.5	1.431	4.	1.669	4.5	1.965	5.	2.316
12	3.6	1.284	4.2	1.484	4.8	1.746	5.4	2.070	6.	2.451
16	4.8	1.339	5.6	1.571	6.4	1.872	7.2	2.239	8.	2.668
20	6.	1.384	7.	1.641	8.	1.972	9.	2.372	10.	2.839
25	7.5	1.430	8.75	1.713	10.	2.073	11.25	2.507	12.5	3.009
30	9.0	1.469	10.5	1.773	12.	2.157	13.5	2.618	15.	3.151
40	12.	1.532	14.	1.869	16.	2.292	18.	2.796	20.	3.375
60	18.	1.624	21.	2.009	24.	2.485	27.	3.048	30.	3.693
80	24.	1.692	28.	2.109	32.	2.623	36.	3.229	40.	3.919
100	30.	1.745	35.	2.188	40.	2.732	45.	3.370	50.	4.096
150	45.	1.845	52.5	2.334	60.	2.930	67.5	3.627	75.	4.419
200	60.	1.916	70.	2.439	80.	3.073	90.	3.811	100.	4.648
300	90.	2.019	105.	2.588	120.	3.274	135.	4.071	150.	4.974
400	120.	2.093	140.	2.695	160.	3.418	180.	4.255	200.	5.202
500	150.	2.151	175.	2.778	200.	3.528	225.	4.40	250.	5.383
600	180.	2.199	210.	2.847	240.	3.622	270.	4.518	300.	5.530
700	210.	2.239	245.	2.904	280.	3.699	315.	4.617	350.	5.654
800	240.	2.274	280.	2.955	320.	3.766	360.	4.704	400.	5.675
900	270.	2.306	315.	2.999	360.	3.826	405.	4.780	450.	5.858
1,000	300.	2.333	350.	3.039	400.	3.879	450.	4.849	500.	5.942
1,500	450.	2.442	525.	3.193	600.	4.084	675.	5.112	750.	6.269
2,000	600.	2.519	700.	3.303	800.	4.231	900.	5.299	1000.	6.502
3,000	900.	2.630	1050.	3.458	1200.	4.437	1350.	5.563	1500.	6.831
4,000	1200.	2.707	1400.	3.566	1600.	4.581	1800.	5.747	2000.	7.059
5,000	1500.	2.769	1750.	3.655	2000.	4.698	2250.	5.897	2500.	7.244
10,000	3000.	2.968	3500.	3.933	4000.	5.067	4500.	6.366	5000.	7.827
15,000	4500.	3.073	5250.	4.079	6000.	5.261	6750.	6.615	7500.	8.134
20,000	6000.	3.153	7000.	4.222	8000.	5.409	9000.	6.802	10000.	8.366

"drop" of 12.85 volts on each conductor, or 25.7 volts in the circuit. But if 10 amperes are supplied harmonically at a frequency of 120 ~, the "drop" will no longer be 12.85 volts on each wire, but 19.27, or almost 50 per cent. more, making the total "drop" in the loop 38.54 volts. The impedance of the loop is one and one-half times its resistance, or the impedance factor is in this case 1.5. The additional drop in the wires is owing to the inductance of the loop. The magnetic flux from the current moves back and forth through this loop at each pulse of the current, and establishes a back pressure or counter E. M. F. that compounds with the ordinary *c r* drop, rectangularly. No attention is here paid



Curve-Plate I.

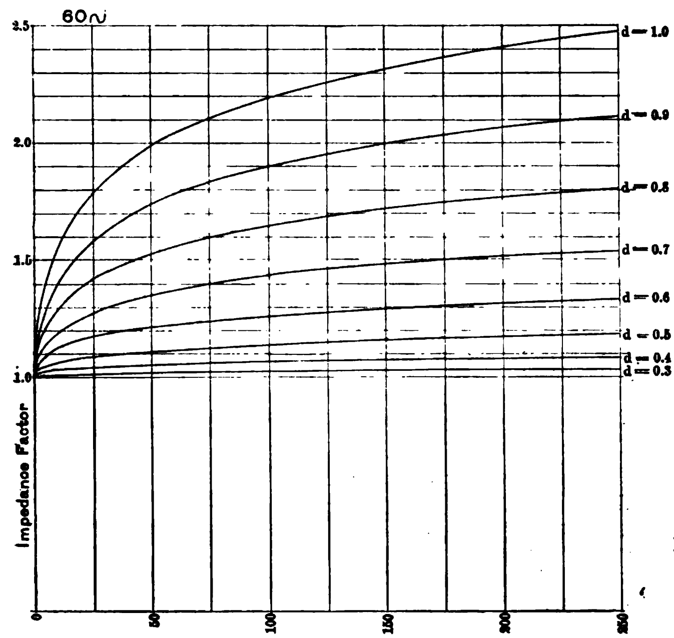
to the static capacity of the line, which is indeed of minor importance in overhead wires of only a few miles in length, but whose influence on the impedance factor increases with the capacity of the line, the frequency, and the E. M. F.

In order to reduce the impedance of our pair of overhead wires as far as possible, we must bring them close together, so that the loop may hold less flux, and the counter E. M. F. from the oscillation of this, will be correspondingly lessened. A safe practical distance might be ten inches (25.4 cms.) apart. By bringing them to this distance their impedance would be reduced to 34.7 ohms for the whole loop, and the impedance factor from 1.5 to

TABLE II. Impedance factors for pairs of parallel copper wires carrying simple harmonic currents of 120 ~ at 15° C.

$\frac{D.}{r}$	Diam. 0.1 cm. Rad. 0.05 cm.		Diam. 0.2 cm. Rad. 0.1 cm.		Diam. 0.3 cm. Rad. 0.15 cm.		Diam. 0.4 cm. Rad. 0.2 cm.		Diam. 0.5 cm. Rad. 0.25 cm.		Diam. 0.6 cm. Rad. 0.3 cm.		Diam. 0.7 cm. Rad. 0.35 cm.		Diam. 0.8 cm. Rad. 0.4 cm.		Diam. 0.9 cm. Rad. 0.45 cm.		Diam. 1.0 cm. Rad. 0.5 cm.	
	D. cms.	factor.	*D.* cms.	factor.	*D.* cms.	factor.	*D.* cms.	factor.	*D.* cms.	factor.	*D.* cms.	factor.	*D.* cms.	factor.	*D.* cms.	factor.	*D.* cms.	factor.	*D.* cms.	factor.
2	0.1	1.00002	0.2	1.0004	0.3	1.002	0.4	1.0056	0.5	1.0135	0.6	1.028	0.7	1.051	0.8	1.086	0.9	1.134	1.	1.199
3							0.6	1.0114	0.75	1.028	0.9	1.057	1.05	1.102	1.2	1.169	1.35	1.260	1.5	1.3765
4	0.2	1.00006	0.4	1.001	0.6	1.005	0.8	1.017	1.	1.040	1.2	1.082	1.4	1.147	1.6	1.241	1.8	1.365	2.	1.522
5							1.	1.022	1.25	1.052	1.5	1.105	1.75	1.187	2.	1.303	2.25	1.455	2.5	1.644
6	0.3	1.0001	0.6	1.002	0.9	1.008	1.2	1.026	1.5	1.062	1.8	1.125	2.1	1.222	2.4	1.357	2.70	1.532	3.	1.747
8							1.6	1.034	2.	1.080	2.4	1.160	2.8	1.281	3.2	1.447	3.6	1.659	4.	1.916
10	0.5	1.00017	1.	1.003	1.5	1.013	2.	1.041	2.5	1.100	3.	1.190	3.5	1.330	4.	1.521	4.5	1.761	5.	2.051
12							2.4	1.046	3.	1.109	3.6	1.215	4.2	1.372	4.8	1.583	5.4	1.848	6.	2.163
16	0.8	1.00023	1.6	1.004	2.4	1.018	3.2	1.056	4.	1.132	4.8	1.258	5.6	1.442	6.4	1.685	7.2	1.987	8.	2.344
20							4.	1.064	5.	1.151	6.	1.293	7.	1.498	8.	1.767	9.	2.098	10.	2.487
25	1.25	1.0003	2.5	1.005	3.75	1.024	5.	1.073	6.25	1.170	7.5	1.329	8.75	1.556	10.	1.851	11.25	2.210	12.5	2.631
30							6.	1.081	7.5	1.187	9.	1.360	10.5	1.604	12.	1.920	13.5	2.303	15.	2.750
40	2.	1.00038	4.	1.006	6.	1.031	8.	1.093	10.	1.215	12.	1.410	14.	1.683	16.	2.031	18.	2.451	20.	2.938
60							12.	1.113	15.	1.257	18.	1.484	21.	1.797	24.	2.192	27.	2.663	30.	3.207
80	4.	1.00053	8.	1.008	12.	1.042	16.	1.127	20.	1.288	24.	1.539	28.	1.880	32.	2.307	36.	2.815	40	3.399
100							20.	1.13	25.	1.313	30.	1.582	35.	1.945	40.	2.398	45.	2.934	50.	3.550
150	7.5	1.00068	15.	1.011	22.5	1.050	30.	1.161	37.5	1.360	45.	1.663	52.5	2.066	60.	2.564	67.5	3.152	75.	3.822
200							40.	1.178	50.	1.395	60.	1.721	70.	2.153	80.	2.683	90.	3.306	100.	4.018
300	15.	1.00087	30.	1.014	45.	1.069	60.	1.203	75.	1.445	90.	1.806	105.	2.278	120.	2.854	135.	3.527	150.	4.294
400							80.	1.221	100.	1.482	120.	1.866	140.	2.366	160.	2.975	180.	3.684	200.	4.490
500	25.	1.0010	50.	1.016	75.	1.080	100.	1.235	125.	1.511	150.	1.915	175.	2.437	200.	3.070	225.	3.807	250.	4.643
600							120.	1.247	150.	1.535	180.	1.954	210.	2.493	240.	3.147	270.	3.907	300.	4.769
700	35.	1.0011	70.	1.018	105.	1.088	140.	1.258	175.	1.556	210.	1.987	245.	2.542	280.	3.212	315.	3.991	350.	4.875
800							160.	1.267	200.	1.574	240.	2.017	280.	2.585	320.	3.270	360.	4.066	400.	4.967
900	45.	1.0012	90.	1.020	135.	1.095	180.	1.275	225.	1.590	270.	2.043	315.	2.622	360.	3.322	405.	4.130	450.	5.049
1,000							200.	1.283	250.	1.605	300.	2.065	350.	2.656	400.	3.364	450.	4.188	500.	5.120
1,500	75.	1.0014	150.	1.023	225.	1.108	300.	1.312	375.	1.660	450.	2.156	525.	2.785	600.	3.540	675.	4.413	750.	5.400
2,000							400.	1.333	500.	1.701	600.	2.220	700.	2.878	800.	3.663	900.	4.572	1000.	5.598
3,000	150.	1 0 17	300.	1.026	450.	2.128	600.	1.363	750.	1.759	900.	2.312	1050.	3.009	1200.	3.839	1350.	4.797	1500.	5.878
4,000							800.	1.385	1000.	1.801	1200.	2.378	1400.	3.102	1600.	3.964	1800.	4.956	2000.	6.077
5,000	250.	1.0019	500.	1.030	750.	1.143	1000.	1.403	1250.	1.833	1500.	2.429	1750.	3.174	2000.	4.061	2250.	5.080	2500.	6.231
10,000					1500.	1.166	2000.	1.460	2500.	1.940	3000.	2.596	3500.	3.410	4000.	4.374	4500.	5.482	5000.	6.730
15,000	750.	1.0024	1500.	1.037	2250.	1.178	3000.	1.492	3750.	1.997	4500.	2.684	5250.	3.536	6000.	4.540	6750.	5.694	7500.	6.992
20,000	1000.	1.0025	2000.	1.040	3000.	1.188	4000.	1.516	5000.	2.041	6000.	2.752	7000.	3.630	8000.	4.666	9000.	5.854	10000.	7.191

1.35. At this distance of ten inches between wire axes, the impedance of the circuit of No. 4 A. W. G. will be a little less than with No. 3, A. W. G., at 30 in, although No. 3 has 26 per cent. more weight and cross-section. The energy expended thermally in the wires as $c^2 r$, is of course strictly in proportion to the ohmic resistance, so that in respect to loss of energy, No. 3 would have the full advantage of its weight, but as far as regards drop in voltage, with its attendant influence upon the pressure regulation over the system, No. 3 at 30 in. is almost the exact equivalent of No. 4 at 10 in.



Curve-Plate II.

If the frequency had been 140 ∿ in place of 120 ∿, the impedance factors of 1.5 and 1.35 would have become 1.9 and 1.45 respectively, and the Nos. 4 wires at 10 in. would have had 4 per cent. less impedance and drop, than Nos. 3 at 2 ft. 6 in. apart.

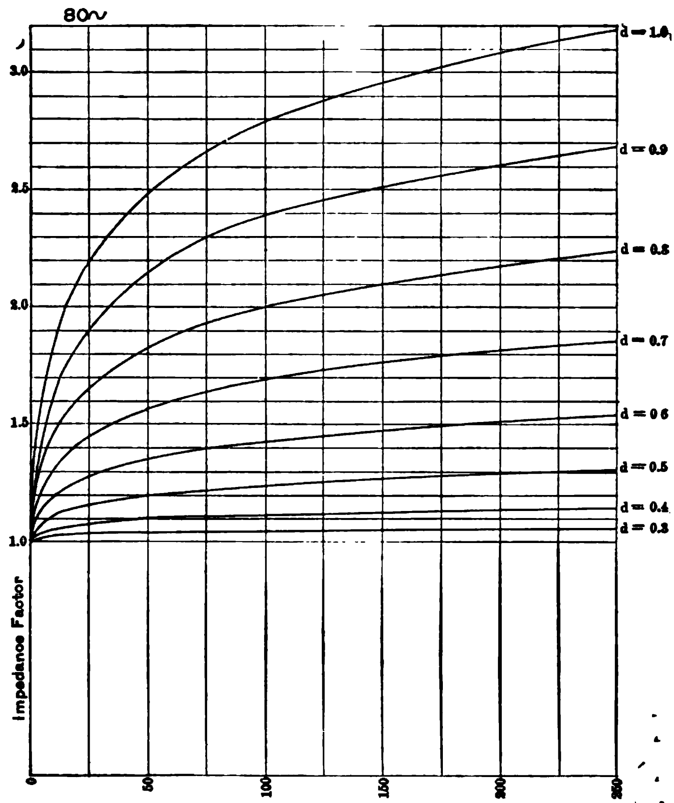
It is evident that for alternating current supply circuits which commonly consist of pairs of overhead line wires, the influence of their proximity is of practical importance, and grows with the size of wire. They should clearly be placed for lowest impedance at the minimum intervening distance consistent with

TABLE III. Impedance factors for pairs of parallel copper wires carrying simple harmonic currents of 100 ~ at 15° C.

$\frac{D}{r}$	Diam. 0.1 cm. Rad. 0.05 cm.		Diam. 0.2 cm. Rad. 0.10 cm.		Diam. 0.3 cm. Rad. 0.15 cm.		Diam. 0.4 cm. Rad. 0.2 cm.		Diam. 0.5 cm. Rad. 0.25 cm.		Diam. 0.6 cm. Rad. 0.3 cm.		Diam. 0.7 cm. Rad. 0.35 cm.		Diam. 0.8 cm. Rad. 0.4 cm.		Diam. 0.9 cm. Rad. 0.45 cm.		Diam. 1.0 cm. Rad. 0.5 cm.	
	D. cms.	factor.	*D.* cms.	factor.	*D.* cms.	factor.	*D.* cms.	factor.	*D.* cms.	factor.	*D.* cms.	factor.	*D.* cms.	factor.	*D.* cms.	factor.	*D.* cms.	factor	*D.* cms.	factor.
2			0.2	1.0002	0.3	1.0012	0.4	1.004	0.5	1.010	0.6	1.020	0.7	1.036	0.8	1.061	0.9	1.095	1.0	1.142
3							0.6	1.008	0.75	1.019	0.9	1.040	1.05	1.072	1.2	1.120	1.35	1.187	1.5	1.274
4			0.4	1.0007	0.6	1.004	0.8	1.012	1.	1.028	1.2	1.058	1.4	1.104	1.6	1.172	1.8	1.265	2.	1.384
5							1.	1.015	1.25	1.036	1.5	1.074	1.75	1.133	2.0	1.218	2.25	1.332	2.5	1.477
6			0.6	1.0011	0.9	1.006	1.2	1.018	1.5	1.044	1.8	1.088	2.1	1.158	2.4	1.258	2.7	1.391	3.	1.557
8							1.6	1.024	2.0	1.056	2.4	1.114	2.8	1.202	3.2	1.326	3.6	1.489	4.	1.689
10			1.0	1.0018	1.5	1.009	2.	1.028	2.5	1.067	3.	1.135	3.5	1.239	4.0	1.383	4.5	1.569	5.	1.796
12							2.4	1.032	3.	1.077	3.6	1.154	4.2	1.279	4.8	1.431	5.4	1.636	6.	1.886
16			1.6	1.0025	2.4	1.013	3.2	1.039	4.	1.093	4.8	1.185	5.6	1.323	6.4	1.509	7.2	1.746	8.	2.030
20							4.	1.045	5.	1.107	6.	1.211	7.	1.366	8.	1.573	9.	1.834	10.	2.145
25			2.5	1.003	3.75	1.017	5.	1.051	6.25	1.121	7.5	1.238	8.75	1.410	10.	1.638	11.25	1.923	12.5	2.261
30							6.	1.057	7.5	1.133	9.	1.270	10.5	1.447	12.	1.693	13.5	1.997	15.	2.357
40			4.	1.004	6.	1.021	8.	1.066	10.	1.154	12.	1.299	14.	1.508	16.	1.781	18.	2.116	20.	2.510
60							12.	1.080	15.	1.184	18.	1.355	21.	1.596	24.	1.908	27.	2.287	30.	2.729
80			8.	1.006	12.	1.030	16.	1.090	20.	1.207	24.	1.396	28.	1.661	32.	2.001	36.	2.410	40.	2.886
100							20.	1.098	25.	1.226	30.	1.430	35.	1.712	40.	2.073	45.	2.507	50.	3.009
150	7.5	1.0005	15.	1.008	22.5	1.038	30.	1.115	37.5	1.261	45.	1.492	52.5	1.809	60.	2.208	67.5	2.684	75.	3.233
200							40.	1.127	50.	1.287	60.	1.537	70.	1.877	80.	2.304	90.	2.810	100.	3.394
300			30.	1.010	45.	1.048	60.	1.145	75.	1.325	90.	1.603	105.	1.977	120.	2.442	135.	2.991	150.	3.621
400	20.	1.0007					80.	1.158	100.	1.353	120.	1.651	140.	2.048	160.	2.540	180.	3.120	200.	3.783
500			50.	1.011	75.	1.056	100.	1.169	125.	1.376	150.	1.689	175.	2.104	200.	2.617	225.	3.220	250.	3.909
600							120.	1.178	150.	1.394	180.	1.719	210.	2.150	240.	2.679	270.	3.302	300.	4.011
700	35.	1.0008	70.	1.013	105.	1.062	140.	1.185	175.	1.410	210.	1.746	245.	2.189	280.	2.734	315.	3.371	350.	4.099
800							160.	1.192	200.	1.423	240.	1.769	280.	2.224	320.	2.780	360.	3.432	400.	4.176
900			90.	1.014	135.	1.067	180.	1.198	225.	1.436	270.	1.790	315.	2.254	360.	2.832	405.	3.486	450.	4.243
1,000	50.	1.0009					200.	1.204	250.	1.447	300.	1.808	350.	2.281	400.	2.8[illegible]8	450.	3.533	500.	4.302
1,500			150.	1.016	225.	1.072	300.	1.225	375.	1.490	450.	1.880	525.	2.386	600.	3.001	675.	3.718	750.	4.533
2,000					300.	1.082	400.	1.241	500.	1.521	600.	1.931	700.	2.461	800.	3.101	900.	3.850	1000.	4.698
3,000	150.	1.0012	300.	1.019	450.	1.090	600.	1.263	750.	1.566	900.	2.004	1050.	2.568	1200.	3.246	1350.	4.035	1500.	4.929
4,000					600.	1.096	800.	1.280	1000.	1.599	1200.	2.057	1400.	2.643	1600.	3.348	1800.	4.167	2000.	5.093
5,000	250.	1.0013	500.	1.021	750.	1.101	1000.	1.293	1250.	1.624	1500.	2.098	1750.	2.703	2000.	3.428	2250.	4.270	2500.	5.221
10,000					1500.	1.118	2000.	1.337	2500.	1.709	3000.	2.233	3500.	2.805	4000.	3.686	4500.	4.601	5000.	5.635
15,000	750.	1.0017	1500.	1.026	2250.	1.1[illegible]7	3000.	1.361	3750.	1.754	4500.	2.304	5250.	2.997	6000.	3.824	6750.	4.777	7500.	5.852
20,000	1000.	1.0018	2000.	1.028	3000.	1.134	4000.	1.379	5000.	1.789	6000.	2.359	7000.	3.075	8000.	3.9[illegible]7	9000.	4.910	10000.	6.018

safety; on adjacent pins at the same side of the pole, rather than on opposite sides. Not only is their impedance thus reduced, but their inductive disturbing influence upon parallel telegraph or telephone circuits is reduced also. A still better means of reducing the impedance is to subdivide the lines, and employ several loops of small wire rather than one loop of large wire.

Curve sheets I to VIII, corresponding to Tables I to VIII



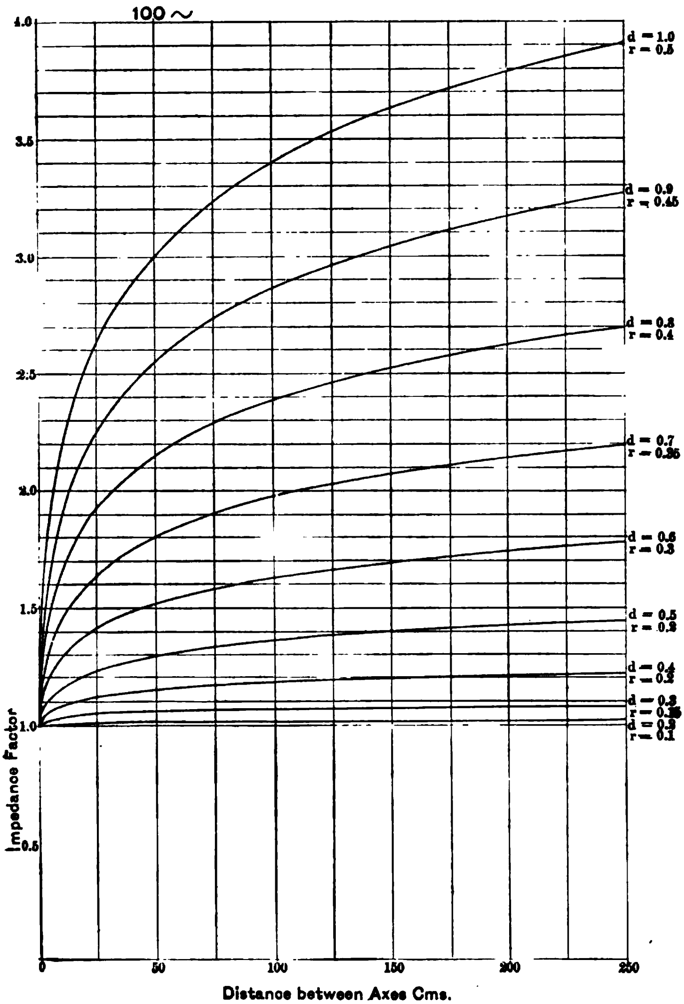
Curve-Plate III.

are arranged to indicate by direct inspection the impedance factors of such pairs of overhead copper wires. The covering of the wire, if free from iron, of course plays no part, or at least no practically appreciable part, in this matter. The tables are based upon Matthiessen's standard conductivity, and a temperature of 15° C. (59° F.), representing perhaps the mean annual temperature of wires in the temperate zone of climate. The factors are

TABLE IV. Impedance factors for pairs of parallel copper wires carrying simple harmonic currents of 80 ~ at 15° C.

$\frac{D.}{r}$	Diam. 0.1 cm. Rad. 0.05 cm. D.	factor.	Diam. 0.2 cm. Rad. 0.1 cm. D.	factor.	Diam. 0.3 cm. Rad. 0.15 cm. D.	factor.	Diam. 0.4 cm. Rad. 0.2 cm. D.	factor.	Diam. 0.5 cm. Rad. 0.25 cm. D.	factor.	Diam. 0.6 cm. Rad. 0.3 cm. D.	factor.	Diam. 0.7 cm. Rad. 0.35 cm. D.	factor.	Diam. 0.8 cm. Rad. 0.4 cm. D.	factor.	Diam. 0.9 cm. Rad. 0.45 cm. D.	factor.	Diam. 1. cm. Rad. 0.5 cm. D.	factor.
2			0.2	1.0002	0.3	1.0008	0.4	1.0028	0.5	1.0061	0.6	1.0126	0.7	1.023	0.8	1.039	0.9	1.062	1.	1.093
3							0.6	1.0050	0.75	1.0124	0.9	1.026	1.05	1.047	1.2	1.078	1.35	1.123	1.5	1.182
4					0.6	1.0024	0.8	1.0075	1.	1.018	1.2	1.037	1.4	1.068	1.6	1.114	1.8	1.177	2.	1.259
5			0.5	1.0006			1.	1.0097	1.25	1.024	1.5	1.048	1.75	1.087	2.	1.144	2.25	1.223	2.5	1.325
6					0.9	1.0037	1.2	1.0117	1.5	1.027	1.8	1.058	2.1	1.104	2.4	1.172	2.7	1.264	3.	1.383
8							1.6	1.015	2.	1.037	2.4	1.074	2.8	1.134	3.2	1.219	3.6	1.334	4.	1.479
10			1.	1.0011	1.5	1.0057	2.	1.018	2.5	1.044	3.	1.089	3.5	1.158	4.	1.259	4.5	1.391	5.	1.557
12							2.4	1.021	3.	1.050	3.6	1.101	4.2	1.180	4.8	1.292	5.4	1.440	6.	1.623
16							3.2	1.025	4.	1.061	4.8	1.122	5.6	1.216	6.4	1.348	7.2	1.520	8.	1.731
20			2.	1.0018	3	1.008	4.	1.029	5.	1.070	6	1.140	7.	1.246	8.	1.394	9.	1.585	10.	1.817
25							5.	1.033	6.25	1.079	7.5	1.158	8.75	1.278	10.	1.441	11.25	1.651	12.	1.905
30					4.5	1.012	6.	1.037	7.5	1.087	9.	1.174	10.5	1.304	12.	1.481	13.5	1.706	15.	1.979
40			4.	1.0027			8.	1.042	10.	1.101	12.	1.20	14.	1.347	16.	1.546	18.	1.796	20.	2.096
60					9.	1.017	12.	1.051	15.	1.121	18.	1.239	21.	1.411	24.	1.640	27.	1.925	30.	2.264
80							16.	1.058	20.	1.137	24.	1.268	28.	1.458	32.	1.709	36.	2.0915	40.	2.385
100			10.	1.0041	15.	1.021	20.	1.064	25.	1.150	30.	1.291	35.	1.496	40.	1.764	45.	2.093	50.	2.481
150	7.5	1.0003					30.	1.075	37.5	1.174	45.	1.336	52.5	1.566	60.	1.865	67.5	2.229	75.	2.655
200					30.	1.027	40.	1.083	50.	1.192	60.	1.368	70.	1.617	80.	1.938	90.	2.354	100.	2.780
300			30.	1.0062			60.	1.095	75.	1.218	90.	1.416	105.	1.692	120.	2.043	135.	2.467	150.	2.959
400	20.	1.0004			60.	1.034	80.	1.104	100.	1.238	120.	1.451	140.	1.745	160.	2.119	180.	2.567	200.	3.085
500							100.	1.111	125.	1.253	150.	1.478	175.	1.787	200.	2.178	225.	2.645	250.	3.184
600			60.	1.0077	90.	1.039	120.	1.117	150.	1.266	180.	1.501	210.	1.822	240.	2.226	270.	2.708	300.	3.265
700	35.	1.0005					140.	1.122	175.	1.277	210.	1.520	245.	1.851	280.	2.268	315.	2.763	350.	3.333
800					120.	1.042	160.	1.127	200.	1.287	240.	1.537	280.	1.877	320.	2.304	360.	2.811	400.	3.394
900			90.	1.0087			180.	1.131	225.	1.296	270.	1.552	315.	1.900	360.	2.336	405.	2.853	450.	3.447
1,000	50.	1.0006			150.	1.044	200.	1.134	250.	1.304	300.	1.566	350.	1.920	400.	2.364	450.	2.890	500.	3.493
1,500							300.	1.149	375.	1.335	450.	1.619	525.	2.000	600.	2.474	675.	3.034	750.	3.676
2,000			200.	1.011	300.	1.053	400.	1.160	500.	1.357	600.	1.657	700.	2.058	800.	2.554	900.	3.138	1000.	3.805
3,000	150.	1.0007	300.	1.012	450.	1.059	600.	1.175	750.	1.390	900.	1.712	1050.	2.140	1200.	2.666	1350.	3.284	1500.	3.989
4,000			400.	1.013	600.	1.063	800.	1.187	1000.	1.413	1200.	1.751	1400.	2.198	1600.	2.745	1800.	3.387	2000.	4.118
5,000	250.	1.0008	500.	1.013	750.	1.066	1000.	1.196	1250.	1.431	1500.	1.783	1750.	2.243	2000.	2.808	2250.	3.467	2500.	4.219
10,000			1000.	1.016	1500.	1.077	2000.	1.226	2500.	1.493	3000.	1.885	3500.	2.393	4000.	3.010	4500.	3.730	5000.	4.547
15,000			1500.	1.017	2250.	1.083	3000.	1.243	3750.	1.526	4500.	1.938	5250.	2.471	6000.	3.117	6750.	3.868	7500.	4.720
20,000	1000.	1.0012	2000.	1.018	3000.	1.088	4000.	1.256	5000.	1.552	6000.	1.980	7000.	2.532	8000.	3.198	9000.	3.973	10000.	4.852

strictly speaking only correct at this temperature, but if applied to the resistance of a wire at its actual temperature, the error is usually small. Thus the impedance factor for a No. 5 A. W. G. wire at 9.1 in. (23.1 cm) interaxial distance, is seen to be 1.311



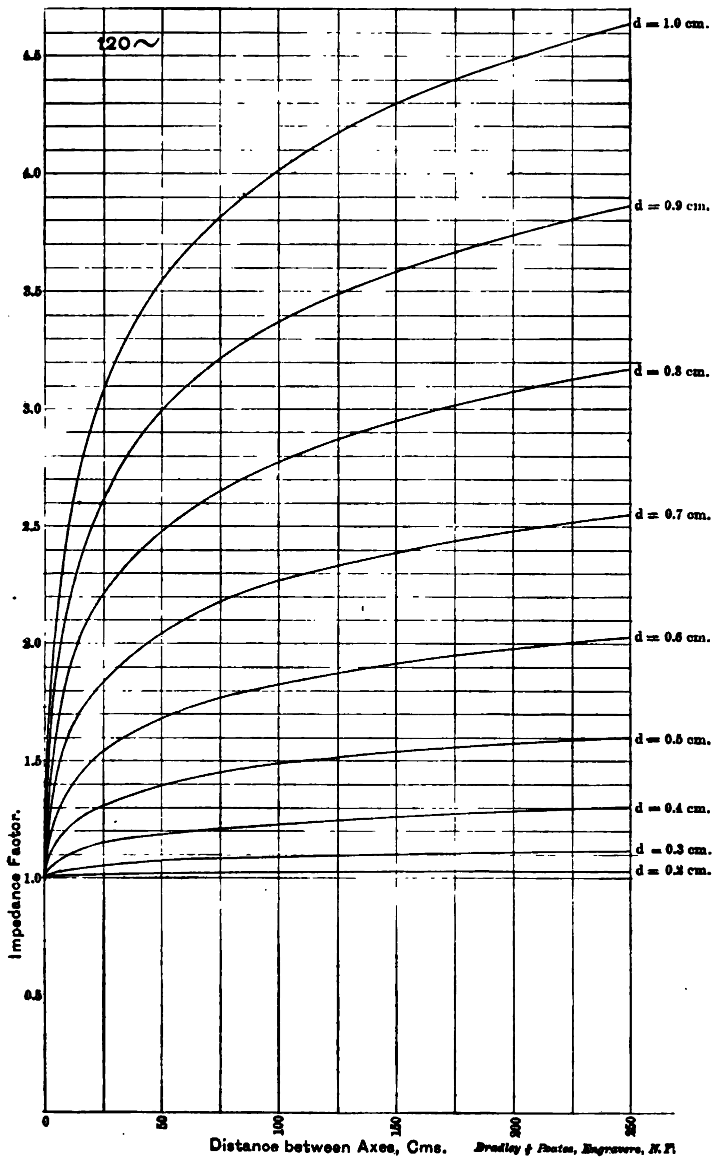
Curve-Plate IV.

at 140 ∼, so that its resistance at standard conductivity and 15° C of 0.0003069 ohms per foot becomes an impedance under these conditions of 0.0004024 ohms per foot. But if the wires were at the freezing point of water, their resistance per foot at that tem-

TABLE V. Impedance factors for pairs of parallel copper wires carrying simple harmonic currents of 60 ~ at 15° C.

$\frac{D.}{r}$	Diam. 0.1 cm. Rad. 0.05 cm.		Diam. 0.2 cm. Rad. 0.1 cm.		Diam. 0.3 cm. Rad. 0.15 cm.		Diam. 0.4 cm. Rad. 0.2 cm.		Diam. 0.5 cm. Rad. 0.25 cm.		Diam. 0.6 cm. Rad. 0.3 cm.		Diam. 0.7 cm. Rad. 0.35 cm.		Diam. 0.8 cm. Rad. 0.4 cm.		Diam. 0.9 cm. Rad. 0.45 cm.		Diam. 1. cm. Rad. 0.5 cm.	
	D.	factor.	*D.*	factor.	*D.*	factor.	*D.*	factor.	*D.*	factor.	*D.*	factor.	*D.*	factor.	*D.*	factor.	*D.*	factor.	*D.*	factor.
2			0.2	1.0001	0.3	1.0004	0.4	1.0014	0.5	1.0034	0.6	1.007	0.7	1.0313	0.8	1.0224	0.9	1.036	1.	1.046
3									0.75	1.007	0.9	1.015	1.05	1.0265	1.2	1.045	1.35	1.071	1.5	1.106
4							0.8	1.0042	1.	1.010	1.2	1.021	1.4	1.039	1.6	1.066	1.8	1.103	2.	1.153
5					0.75	1.0017			1.25	1.013	1.5	1.028	1.75	1.050	2.	1.084	2.25	1.131	2.5	1.194
6							1.2	1.0066	1.5	1.016	1.8	1.033	2.1	1.064	2.4	1.100	2.7	1.156	3.	1.230
8									2.	1.021	2.4	1.043	2.8	1.077	3.2	1.129	3.6	1.199	4.	1.291
10			1.0	1.0006	1.5	1.0033	2.	1.0103	2.5	1.025	3.	1.051	3.5	1.092	4.	1.153	4.5	1.235	5.	1.342
12									3.	1.029	3.6	1.058	4.2	1.105	4.8	1.173	5.4	1.266	6.	1.386
16							3.2	1.0144	4.	1.035	4.8	1.070	5.6	1.127	6.4	1.209	7.2	1.318	8.	1.458
20					3.	1.0053			5.	1.040	6.	1.081	7.	1.145	8.	1.238	9.	1.360	10.	1.515
25							5.	1.019	6.25	1.045	7.5	1.092	8.75	1.164	10.	1.268	11.25	1.404	12.	1.575
30									7.5	1.050	9.	1.101	10.5	1.181	12.	1.293	13.5	1.441	15.	1.625
40			4.	1.0015	6.	1.0077	8.	1.024	10.	1.059	12.	1.117	14.	1.207	16.	1.335	18.	1.501	20.	1.705
60									15.	1.070	18.	1.140	21.	1.248	24.	1.397	27.	1.588	30.	1.823
80							16.	1.034	20.	1.079	24.	1.158	28.	1.278	32.	1.443	36.	1.653	40.	1.908
100					15.	1.012			25.	1.087	30.	1.173	35.	1.302	40.	1.479	45.	1.704	50.	1.975
150							30.	1.043	37.5	1.101	45.	1.201	52.5	1.348	60.	1.547	67.5	1.798	75.	2.099
200									50.	1.112	60.	1.221	70.	1.381	80.	1.597	90.	1.866	100.	2.187
300			30.	1.004	45.	1.018	60.	1.057	75.	1.128	90.	1.251	105.	1.431	120.	1.669	135.5	1.965	150.	2.315
400									100.	1.140	120.	1.273	140.	1.466	160.	1.721	180.	2.036	200.	2.406
500							100.	1.064	125.	1.149	150.	1.291	175.	1.495	200.	1.763	225.	2.091	250.	2.478
600					90.	1.022			150.	1.157	180.	1.305	210.	1.518	240.	1.796	270.	2.136	300.	2.537
700							140.	1.070	175.	1.164	210.	1.318	245.	1.538	280.	1.825	315.	2.175	350.	2.586
800									200.	1.170	240.	1.329	280.	1.556	320.	1.850	360.	2.210	400.	2.630
900			90.	1.005	135.	1.025	180.	1.076	225.	1.176	270.	1.339	315.	1.571	360.	1.873	405.	2.239	450.	2.668
1,000							200.	1.078	250.	1.181	300.	1.348	350.	1.585	400.	1.893	450.	2.265	500.	2.702
1,500							300.	1.086	375.	1.199	450.	1.383	525.	1.640	600.	1.971	675.	2.369	750.	2.835
2,000					300.	1.031	400.	1.093	500.	1.214	600.	1.408	700.	1.679	800.	2.027	900.	2.444	1000.	2.930
3,000			300.	1.007			600.	1.102	750.	1.234	900.	1.444	1050.	1.736	1200.	2.106	1350.	2.549	1500.	3.064
4,000							800.	1.109	1000.	1.249	1200.	1.471	1400.	1.776	1600.	2.163	1800.	2.625	2000.	3.159
5,000	250.	1.0005			750.	1.037	1000.	1.114	1250.	1.261	1500.	1.492	1750.	1.808	2000.	2.207	2250.	2.683	2500.	3.233
10,000			1000.	1.009			2000.	1.133	2500.	1.300	3000.	1.560	3500.	1.912	4000.	2.353	4500.	2.874	5000.	3.476
15,000							3000.	1.143	3750.	1.322	4500.	1.597	5250.	1.968	6000.	2.430	6750.	2.975	7500.	3.601
20,000	1000.	1.0006	2000.	1.010	3000.	1.050	4000.	1.151	5000.	1.339	6000.	1.626	7000.	2.011	8000.	2.489	9000.	3.052	10000.	3.698

perature would be 0.0002897 ohms, an impedance with the same factor of 0.0003798, while the correct value for temperature 0° C

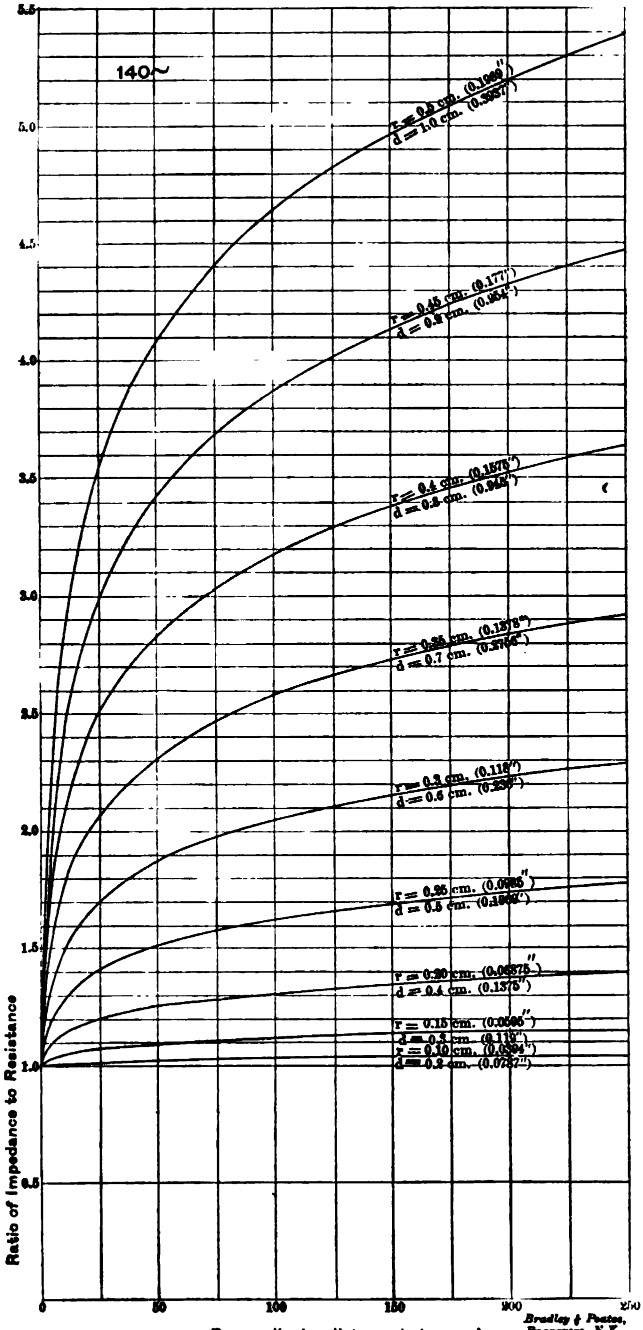


Curve-Plate V.

would be 0.0003895 and the corrected factor 1.344, a discrepancy of about 2.5 per cent. for a change of temperature amounting to

TAB .E VI. Impedance factors for pairs of parallel copper wire carrying simple harmonic currents of 40 ~ at 15° C.

$\frac{D}{r}$	Diam 0.1 cm. Rad. 0.05 cm.		Diam. 0.2 cm. Rad. 0.1 cm.		Diam. 0.3 cm. Rad. 0.15 cm.		Diam. 0.4 cm. Rad. 0.2 cm.		Diam. 0.5 cm. Rad. 0.25 cm.		Diam. 0.6 cm. Rad. 0.3 cm.		Diam. 0.7 cm. Rad. 0.35 cm.		Diam. 0.8 cm. Rad. 0.4 cm.		Diam. 0.9 cm. Rad. 0.45 cm		Diam. 1. cm. Rad. 0.5 cm.	
	D. cms.	factor.	D. cms.	factor.	D. cms.	factor.	D. cms.	factor.	D. cms.	factor.	D. cms.	factor.	D. cms.	factor.	D. cms.	factor.	D. cms.	factor.	D. cms.	factor.
2			0.2	1.00004	0.3	1.0002	0.4	1.0006	0.5	1.0015	0.6	1.0032	0.7	1.0058	0.8	1.010	0.9	1.016	1.	1.024
3									0.75	1.0031	0.9	1.0064	1.05	1.012	1.2	1.020	1.35	1.032	1.5	1.048
4							0.8	1.0019	1.	1.0047	1.2	1.0095	1.4	1.018	1.6	1.030	1.8	1.047	2.0	1.071
5					0.75	1.0008			1.25	1.0059	1.5	1.012	1.75	1.023	2.	1.038	2.25	1.060	2.5	1.090
6							1.2	1.0029	1.5	1.0071	1.8	1.015	2.1	1.027	2.4	1.045	2.7	1.072	3.	1.108
8									2.	1.0092	2.4	1.019	2.8	1.035	3.2	1.059	3.6	1.093	4.	1.138
10			1.	1.0003	1.5	1.0014	2.	1.0046	2.5	1.011	3.	1.023	3.5	1.042	4.	1.070	4.5	1.111	5.	1.165
12									3.	1.013	3.6	1.026	4.2	1.048	4.8	1.081	5.4	1.126	6.	1.187
16							3.2	1.0064	4.	1.016	4.8	1.032	5.6	1.058	6.4	1.098	7.2	1.152	8.	1.225
20					3.	1.0023			5.	1.018	6.	1.037	7.	1.067	8.	1.111	9.	1.174	10.	1.256
25							5.	1.0084	6.25	1.021	7.5	1.042	8.75	1.076	10.	1.127	11.25	1.197	12.5	1.288
30									7.5	1.022	9.	1.046	10.5	1.084	12.	1.140	13.5	1.216	15.	1.315
40			4.	1.0007	6.	1.0034	8.	1.011	10.	1.026	12.	1.053	14.	1.097	16.	1.161	18.	1.248	20.	1.360
60									15.	1.032	18.	1.065	21.	1.117	24.	1.193	27.	1.295	30.	1.426
80							16.	1.015	20.	1.036	24.	1.073	28.	1.132	32.	1.217	36.	1.330	40.	1.474
100					15.	1.005			25.	1.039	30.	1.080	35.	1.144	40.	1.236	45.	1.358	50.	1.513
150							30.	1.019	37.5	1.046	45.	1.094	52.5	1.168	60.	1.273	67.5	1.412	75.	1.585
200									50.	1.051	60.	1.104	70.	1.185	80.	1.300	90.	1.450	100.	1.638
300			30.	1.0016	45.	1.008	60.	1.025	75.	1.059	90.	1.119	105.	1.211	120.	1.339	135.	1.507	150.	1.714
400									100.	1.064	120.	1.130	140.	1.230	160	1.368	180.	1.548	200.	1.769
500							100.	1.029	125.	1.069	150.	1.138	175.	1.244	200.	1.391	225.	1.581	250.	1.812
600					90.	1.010			150.	1.073	180.	1.146	210.	1.257	240.	1.411	270.	1.608	300.	1.848
700							140.	1.032	175.	1.076	210.	1.152	245.	1.268	280.	1.427	315.	1.630	350	1.878
800									180.	1.079	220.	1.158	260.	1.277	320.	1.441	360.	1.651	400.	1.905
900			90.	1.0022	135.	1.011	180.	1.034	225.	1.082	270.	1.163	305.	1.286	350.	1.454	395.	1.669	450.	1.929
1,000									250.	1.084	300.	1.167	350.	1.293	400.	1.465	450.	1.685	500.	1.949
1,500							275.	1.039	350.	1.093	425.	1.185	500.	1.323	575.	1.510	650.	1.747	725.	2.031
2,000					300.	1.014	400.	1.042	500.	1.100	600.	1.198	700.	1.345	800.	1.543	900.	1.792	1000.	2.090
3,000			300.	1.003	450.	1.015	600.	1.047	750.	1.110	900.	1.218	1050.	1.360	1200.	1.589	1350.	1.856	1500.	2.174
4,000					600.	1.016	800.	1.050	1000.	1.118	1200.	1.232	1400.	1.399	1600.	1.623	1800.	1.902	2000.	2.234
5,000	250.	1.0002			750.	1.017	1000.	1.052	1250.	1.124	1500.	1.243	1750.	1.417	2000.	1.649	2250.	1.938	2500.	2.280
10,000					1500.	1.019	2000.	1.061	2500.	1.143	3000.	1.280	3500.	1.477	4000.	1.736	4500.	2.058	5000.	2.433
15,000			1500.	1.0042	2250.	1.021	3000.	1.066	3750.	1.154	4500.	1.299	5250.	1.509	6000.	1.783	6750.	2.119	7500.	2.514
20,000	1000.	1.0003	2000.	1.0045	3000.	1.022	4000.	1.070	5000.	1.163	6000.	1.315	7000.	1.534	8000.	1.819	9000.	2.167	10000.	2.576



Curve-Plate VI.

15° C. A correction formula, for a greater degree of accuracy, is, however, given in Appendix I.

Tables and curves I to VI inclusive are of general application, dealing with wires from 1 mm. up to 1 cm. in diameter, and also all interaxial distances up to 250 cms., for the successive frequencies of 40, 60, 80, 100, 120 and 140 ∼. Tables and curves VII and VIII apply more particularly to American practice, dealing with all sizes of A. W. G. wires from No. 000 to No. 11 inclusive, and all interaxial distances up to 100 inches for the two frequencies 120 ∼ and 140 ∼. Factors for intermediate sizes or frequencies can readily be found by interpolation. For instance, suppose that the impedance factor is required for a pair of No. 6 A. W. G. wires suspended at an interaxial distance of 40 inches at 15° C, and at a simply harmonic frequency of 130 ∼. Curves VII and VIII give

No. 6 at 140 ∼ = 1.34 at 40 in.
" " 120 ∼ = 1.26 " "

Taking the mean value for the midway frequency, we have No. 6 at 130 ∼ = 1.30. Direct calculation yields in fact 1.300 as the factor at 130 ∼.

The evidence upon which these tables and curves are based is not merely theoretical. Observations made upon voltage drops on overhead wires in actual alternating current systems have corroborated the results. A particular set of observations is also added in Appendix I, together with a simple development of the formula from which the tables have been computed to the fourth significant digit.

The same tables and curves will of course apply to secondary circuits or interior wiring, and even if the pairs of wires are twisted together into double cords, without serious error, the only exception being to wires laid in, or close to iron pipes.

Single copper wires suspended overhead at a mean elevation h, with ground return, have an impedance as though that ground return were replaced by an insulated copper wire of equal size vertically beneath, and at a distance h below the surface. An elevation h, thus represents a loop interaxial distance of $2h$, and the imaginary wire below ground is the "image" of the suspended wire, so called from the optical analogy with respect to the surface midway between. Heaviside has pointed out[1] that this result is based upon the assumption that the return current

1. "Reprint of Electrical Papers," vol. i. p. 101.

through the ground spreads uniformly through a thin film over the surface, and does not penetrate the soil. As a fact, the current cannot be confined to this superficial layer, and the real equivalent loop must be considerably wider than the hypothetical image requires. However, as the real limits can scarcely be assigned, the image impedance factors may be justly regarded as minima, while the increase in the factor is usually very slow with moderate elevations, even for large changes in the interaxial distance, so that the error is not so great as might be deemed on first apprehension. The minimum impedance factor for a wire say 20 feet above ground, is therefore, its loop impedance factor at 40 feet between axes. The curves indeed could not conveniently be extended, at their existing scale of construction, to include overhead wires, but the tables have probably sufficient range for practical purposes in this respect.

We may next examine how far these tabulated impedance factors are liable to be affected by waiving the restrictions hitherto maintained concerning the type of current waves, and the non-ferric nature of the inductances.

Impedance factors for conductors remote from iron, while independent of the current strength, depend immediately upon the current wave character. In practice, alternating current circuits contain motors or transformers, in which iron plays a prominent part; and owing to the hysteresis in this iron, the current waves deviate from simple sinusoids, particularly at light loads, even when the E. M. F. supplied by the generator is simply harmonic. Of all possible current wave types, however, the simply harmonic or sinusoidal has the lowest impedance factor, and the entries in the tables are consequently minimum values; but there appears to be no limit to the possible augmentation of these factors under appropriate conditions, as an example will indicate.

Let the current waves be of the saw-tooth type [Fig. 7], the rising and falling lines intersecting the zero line z o at equal angles but in opposite directions. Here the time occupied in ascent from, or descent to, the zero line, is just one quarter period. The impedance factor for such currents is

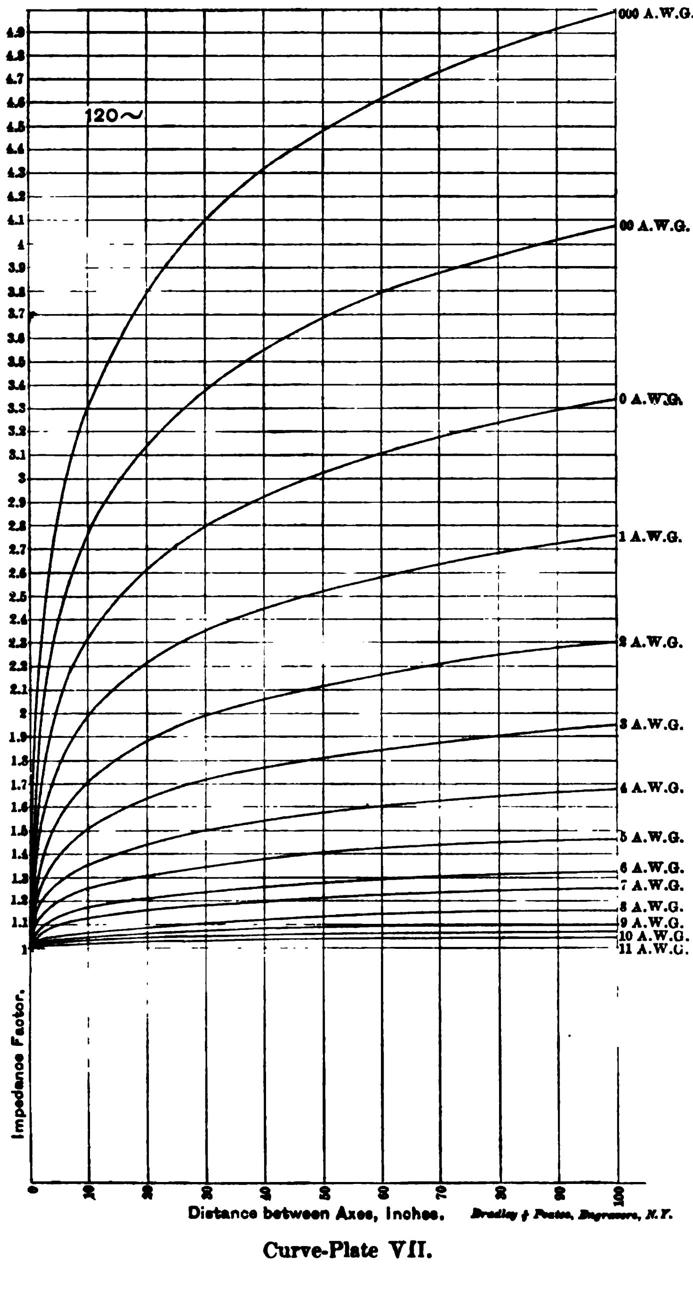
$$\sqrt{1 + \frac{48\, n^2\, l^2}{r^2}}$$

where n is the frequency and $\frac{l}{r}$ the ratio of inductance to resis-

TABLE VII. Impedance factors for pairs of parallel copper wires carrying simple harmonic currents at 120 and 140 ~ 15° C.

D/r	No. 11, A. W. Gauge. Dia. 0.09074 in. 0.23045 cm Rad. 0.04537 in. 0.1152 cm			No. 10, A. W. Gauge. Dia. 0.10190 in. 0.2588 cm Rad. 0.05095 in. 0.1294 cm			No. 9, A. W. Gauge. Dia 0.1144 in 0.2906 cm Rad. 0.0572 in. 0.1453 cm			No. 8, A. W. Gauge. Dia. 0.1285 in. 0.3263 cm Rad. 0.0643 in. 0.1632 cm		
	D. (inches.)	factor. 120~	factor. 140~	D. (inches)	factor. 120~	factor. 140~	D (inches.)	factor. 120~	factor. 140~	D. (inches.)	factor. 120~	factor. 140~
2	0.0907	1.0006	1.0008	0.102	1.001	1.0013	0.114	1.0016	1.0021	0.1285	1.0025	1.0037
3	0.136	1.0013	1.0017	0.153	1.002	1.003	0.172	1.0032	1.0043	0.193	1.005	1.007
4	0.181	1.019	1.0025	0.204	1.003	1.006	0.229	1.0047	1.0063	0.257	1.0075	1.010
5	0.227	1.0024	1.0033	0.255	1.004	1.005	0.286	1.0065	1.0080	0.321	1.0096	1.013
6	0.272	1.0029	1.0039	0.306	1.005	1.006	0.343	1.0073	1.010	0.386	1.212	1.015
8	0.363	1.0038	1.0051	0.408	1.006	1.008	0.458	1.0095	1.013	0.514	1.015	1.021
10	0.454	1.0045	1.0062	0.510	1.007	1.010	0.572	1.011	1.016	0.643	1.018	1.024
12	0.544	1.0052	1.0071	0.611	1.008	1.011	0.687	1.013	1.018	0.771	1.021	1.028
16	0.726	1.0063	1.0086	0.815	1.011	1.014	0.915	1.016	1.022	1.028	1.025	1.034
20	0.907	1.0073	1.0099	1.019	1.012	1.016	1.144	1.018	1.025	1.285	1. 29	1.039
25	1.134	1.008	1.011	1.274	1.013	1.018	1.431	1.021	1.028	1.606	1.033	1.045
30	1.361	1.009	1.012	1.529	1.015	1.020	1.717	1.023	1.031	1.928	1.037	1.050
40	1.815	1.011	1.014	2.038	1.017	1.023	2.288	1.027	1.036	2.570	1.043	1.057
60	2.723	1.013	1.018	3.057	1.021	1.028	3.433	1.033	1.044	3.855	1.052	1.070
80	3.63	1.015	1.020	4.076	1.024	1.032	4.577	1.037	1.050	5.141	1.058	1.078
100	4.537	1.016	1.022	5.095	1.026	1.035	5.72	1.041	1.055	6.425	1.064	1.086
150	6.806	1.019	1.026	7.642	1.030	1.041	8.58	1.048	1.064	9.638	1.075	1.100
200	9.074	1.021	1.029	10.19	1.033	1.045	11.44	1.053	1.071	12.85	1.083	1.111
300	13.61	1.025	1.033	15.29	1.039	1.052	17.17	1.060	1.081	19.28	1.094	1.127
400	18.15	1.027	1.036	20.38	1.042	1.057	22.88	1.066	1.089	25.7	1.103	1.139
500	22.69	1.029	1.039	25.48	1.045	1.061	28.61	1.071	1.095	32.13	1.111	1.148
600	27.23	1.030	1.041	30.57	1.048	1.064	34.33	1.075	1.100	38.55	1.116	1.156
700	31.76	1.031	1.043	35.66	1.050	1.067	40.05	1.078	1.105	44.98	1.122	1.162
800	36.3	1.032	1.045	40.76	1.052	1.070	45.77	1.081	1.109	51.41	1.126	1.168
900	40.83	1.034	1.046	45.85	1.054	1.072	51.49	1.084	1.112	57.82	1.130	1.174
1,000	45.37	1.035	1.047	50.95	1.055	1.074	57.21	1.086	1.115	64.25	1.134	1.178
1,500	68.06	1.039	1.053	76.42	1.061	1.082	85.82	1.096	1.128	96.38	1.149	1.198
2,000	90.74	1.042	1.056	101.9	1.066	1.089	114.4	1.103	1.138	128.5	1.159	1.212
3,000	136.1	1.046	1.063	152.9	1.073	1.098	171.7	1.113	1.151	192.8	1.175	1.231
4,000	181.5	1.049	1.066	203.8	1.078	1.104	228.8	1.121	1.161	257.1	1.186	1.246
5,000	226.8	1.052	1.070	254.7	1.081	1.109	286.1	1.127	1.169	321.3	1.195	1.258
10,000	453.7	1.060	1.082	509.5	1.095	1.127	572.1	1.147	1.196	642.8	1.225	1.297

D/r	No. 7, A. W. Gauge. Dia 0.1443 in 0.3665 cm Rad. 0.07214 in. 0.1832 cm			No. 6. A. W. Gauge. Dia. 0.1620 in. 0.4115 cm Rad. 0.0810 in. 0.2058 cm			No. 5, A. W. Gauge. Dia. 0.1819 in. 0.4621 cm Rad. 0.0910 in. 0.231 cm		
	D. (inches.)	factor. 120~	factor. 140~	D. (inches.)	factor. 120~	factor. 140~	D (inches)	factor. 120~	factor. 140~
2	0.1443	1.0039	1.0053	0.162	1.0063	1.009	0.1819	1.010	1.014
3	0.2164	1.008	1.011	0.243	1.0128	1.017	0.273	1.020	1.027
4	0.2886	1.012	1.016	0.324	1.019	1.025	0.364	1.030	1.040
5	0.3608	1.018	1.021	0.4051	1.024	1.032	0.455	1.038	1.051
6	0.4329	1.0207	1.025	0.4861	1.029	1.040	0.546	1.046	1.062
8	0.5772	1.024	1.030	0.6482	1.038	1.051	0.728	1.059	1.080
10	0.7214	1.029	1.038	0.8101	1.045	1.061	0.9095	1.071	1.095
12	0.8658	1.033	1.044	0.9721	1.052	1.069	1.091	1.081	1.108
16	1.154	1.040	1.054	1.296	1.063	1.084	1.455	1.098	1.131
20	1.443	1.046	1.062	1.621	1.072	1.096	1.819	1.112	1.149
25	1.804	1.052	1.070	2.025	1.082	1.109	2.274	1.127	1.169
30	2.164	1.058	1.077	2.430	1.090	1.120	2.728	1.140	1.186
40	2.886	1.067	1.090	3.241	1.104	1.139	3.638	1.061	1.214
60	4.329	1.080	1.1.8	4.861	1.125	1.167	5.457	1.193	1.255
80	5.772	1.091	1.122	6.482	1.141	1.188	7.276	1.217	1.286
100	7.214	1.099	1.133	8.101	1.154	1.205	9.095	1.236	1.311
150	10.82	1.116	1.155	12.15	1.179	1.237	13.64	1.273	1.358
200	14.43	1.128	1.171	16.21	1.197	1.261	18.19	1.300	1.393
300	21.64	1.146	1.195	24.30	1.225	1.296	27.28	1.340	1.443
400	28.86	1.160	1.212	32.41	1.245	1.322	36.38	1.369	1.480
500	36.08	1.171	1.226	40.51	1.261	1.342	45.48	1.392	1.508
600	43.29	1.180	1.238	48.61	1.274	1.359	54.57	1.411	1.512
700	50.50	1.187	1.248	56.71	1.285	1.374	63.67	1.427	1.553
800	57.72	1.194	1.257	64.82	1.295	1.387	72.76	1.442	1.571
900	64.93	1.200	1.265	72.91	1.304	1.398	81.86	1.454	1.587
1,000	72.14	1.206	1.272	81.01	1.312	1.408	90.95	1.466	1.602
1,500	108.2	1.228	1.300	121.5	1.344	1.448	136.4	1.510	1.657
2,000	144.3	1.244	1.321	162.0	1.367	1.477	181.9	1.543	1.698
3,000	216.5	1.267	1.350	243.0	1.400	1.519	272.9	1.590	1.755
4,000	288.6	1.283	1.372	324.0	1.424	1.550	363.8	1.624	1.797
5,000	360.7	1.297	1.388	405.1	1.443	1.573	454.8	1.650	1.829
10,000	721.4	1.341	1.444	810.2	1.506	1.652	909.5	1.737	1.936



Curve-Plate VII.

tance in any length of conductor, or in other words its time constant. The tabulated factors for simple harmonic currents we know to be

$$\sqrt{1 + \frac{p^2 l^2}{r^2}},$$

$$\text{or } \sqrt{1 + 39.48 \frac{n^2 l^2}{r^2}},$$

so that the saw-tooth factors are the greater. If we file down the points of the teeth as shown in Fig. 8, the current will rise from zero to a maximum, along the straight line O A in less than $\frac{1}{4}$ period, and the factor diminishes, until this time of ascent is reduced to $\frac{3}{16}$ of a period [indicated in Fig. 8], when the factor has its minimum value for this character of wave, viz.:

$$\sqrt{1 + 43.2 \frac{l^2 n^2}{r^2}}.$$

After this, as we shorten the period of ascent, the factor rises. Neither wave amplitude nor strength of current can of course affect this consideration.

When asc. and des. each occ. $\frac{1}{8}$ period, factor $= \sqrt{1 + 48 \frac{l^2 n^2}{r^2}}$;

" " " " " " $\frac{1}{10}$ " " $= \sqrt{1 + 54.5 \frac{l^2 n^2}{r^2}}$;

" " " " " " $\frac{1}{12}$ " " $= \sqrt{1 + 62 \frac{l^2 n^2}{r^2}}$;

" " " " " " $\frac{1}{100}$ " " $= \sqrt{1 + 411 \frac{l^2 n^2}{r^2}}$,

and finally, when the ascending time is reduced to zero, so that the rise is absolutely perpendicular, as shown in Fig. 9, the factor becomes indefinitely great, and the drop on such a wire would no longer be finite with a finite current, which is equivalent to the statement that it is practically impossible to create currents of this strictly rectangular type.

Since theory assigns no limits, we are compelled to accept the terms that existing practice may impose. Fig. 10 shows a wave form taken from Professor Ryan's paper on Transformers[1] read before this Institute in January, 1890. It was the wave of primary current supplied to an idle transformer by an alternator

1. TRANSACTIONS, vol. vii, p. 1.

whose E. M. F. was nearly sinusoidal. This wave has been analyzed into its harmonic components, given in Appendix II. We know from Fourier's beautiful theorem, that any periodic, non-re-entrant wave, however complex in outline, can be compounded by, or analyzed into, simple sine waves whose frequencies are all some multiple (integral and not fractional) of the resultant or complex wave frequency. It is evident that any wave, however irregular in form, can be constructed out of a sine wave of the same length, with sine ripples of sufficient number and diminutiveness added to or subtracted from this fundamental sinusoid at suitable points, but it is wonderful that the process can always be completed with sine ripples, each belonging to an endless succession of its own type, and each ripple-chain having a wave length some submultiple of the fundamental. The impedance factor for any complex harmonic current wave is always determinable from the formula of Appendix II, when its dissection into component ripples, that is to say its harmonic analysis, has been determined. In this instance the harmonics and fundamental wave that approximately compound into the current wave considered, are indicated by the dotted lines of Fig. 10, and the impedance factor is

$$\sqrt{1 + 1.605 \frac{p^2 l^2}{r^2}} \quad \text{or} \quad \sqrt{1 + 63.35 \frac{n^2 l^2}{r^2}}$$

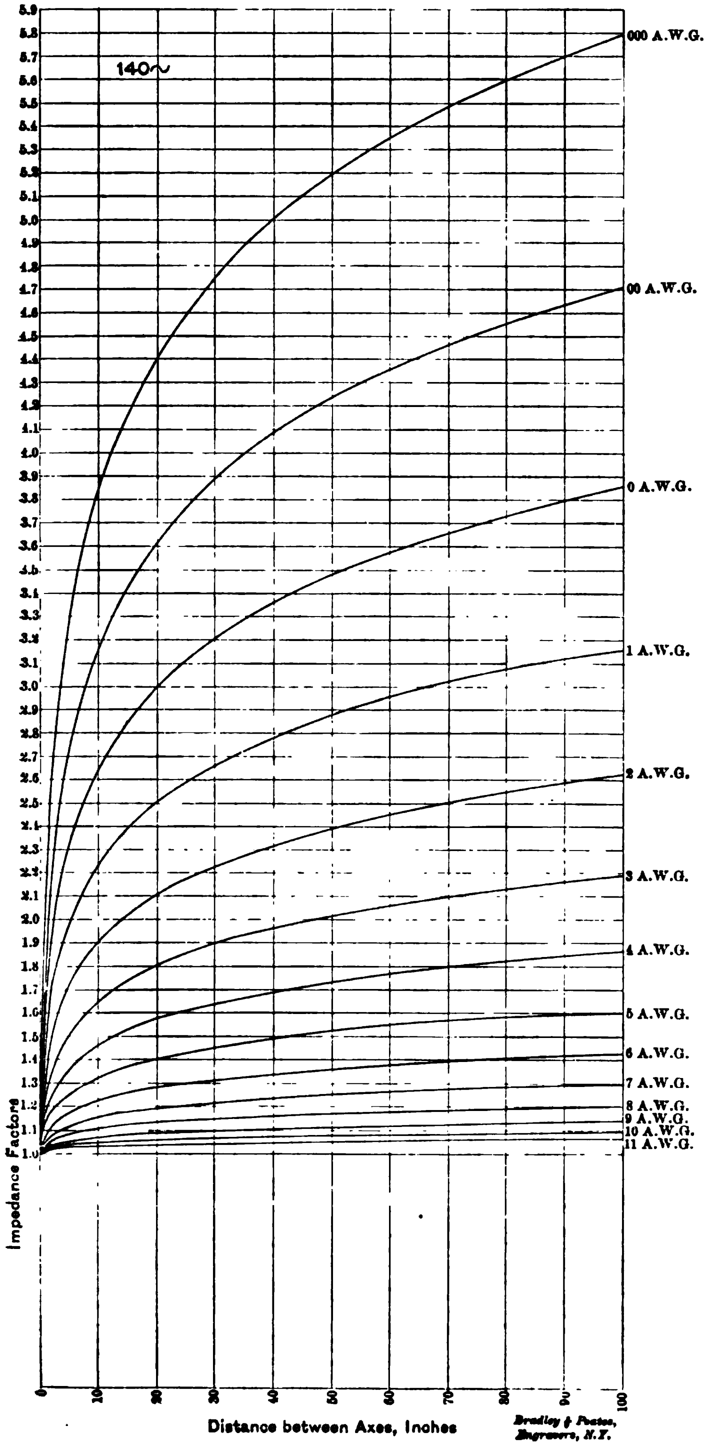
so that a pair of No. 2 A. W. G. wires interaxially distant 30 in. would offer an impedance factor to this current wave at 140 $\sim$ of 2.708, an increase of nearly 22 per cent. above the tabular value for simple sinusoids. It is evident that the drop in voltage over such wires, or over any suitable coil of known resistance and non-ferric inductance, would furnish evidence as to whether the current wave type differs sensibly from the simply harmonic. The sensitiveness of the test increases with the frequency, and with the time constant of the coil. Thus the wave in Fig. 10 which adds 22 per cent to a normal factor of 2.22, adds only 8 per cent. to a normal factor of 1.18 at 140 $\sim$, such as would be furnished by two No. 8 A. W. G. wires 66 in. apart.

Professor Ryan's paper shows, however, that this distortion in the current wave through the primary circuit of the transformer, gradually disappeared as the load was added to the secondary circuit, and in fact at full load, the current wave was nearly sinusoidal, with an impedance factor close to the normal or tabular

TABLE VIII. Impedance factors for pairs of parallel copper wires carrying simple harmonic currents of 120 ~ and 140 ~ at 15° C.

$\frac{D}{r}$	No. 4, A. W. Gauge. Dia. 0.2043 in. 0.5189 cm Rad. 0.1022 in. 0.2595 cm D (inches.)	factor. 120~	factor. 140~	No. 3, A. W. Gauge. Dia. 0.2294 in. 0.5827 cm Rad. 0.1147 in. 0.2914 cm D. (inches.)	factor. 120~	factor. 140~	No. 2, A. W. Gauge. Dia. 0.2576 in. 0.6543 cm Rad. 0.1288 in. 0.3272 cm D. (inches.)	factor. 120~	factor. 140~	No. 1, A. W. Gauge. Dia. 0.2893 in. 0.7344 cm Rad. 0.1447 in. 0.3672 cm D. (inches.)	factor. 120~	factor. 140~
2	0.2043	1.016	1.022	0.2294	1.025	1.034	0.2576	1.039	1.053	0.2893	1.062	1.083
3	0.3065	1.032	1.043	0.3441	1.051	1.068	0.3864	1.079	1.106	0.4339	1.123	1.164
4	0.4086	1.047	1.063	0.4588	1.073	1.098	0.5153	1.114	1.153	0.5785	1.176	1.234
5	0.5108	1.060	1.080	0.5735	1.094	1.1255	0.6441	1.145	1.185	0.7232	1.223	1.294
6	0.613	1.072	1.096	0.6882	1.112	1.150	0.7730	1.173	1.230	0.8678	1.264	1.347
8	0.8172	1.092	1.124	0.9177	1.143	1.191	1.030	1.220	1.291	1.157	1.333	1.435
10	1.0215	1.110	1.147	1.147	1.170	1.226	1.288	1.260	1.338	1.447	1.391	1.507
12	1.226	1.1255	1.167	1.376	1.193	1.256	1.546	1.294	1.385	1.736	1.439	1.568
16	1.634	1.151	1.201	1.835	1.232	1.306	2.061	1.351	1.457	2.314	1.520	1.668
20	2.043	1.173	1.229	2.294	1.264	1.347	2.576	1.396	1.514	2.893	1.584	1.748
25	2.554	1.195	1.259	2.868	1.297	1.389	3.221	1.444	1.574	3.616	1.651	1.826
30	3.065	1.214	1.283	3.441	1.325	1.424	3.864	1.484	1.624	4.339	1.706	1.898
40	4.086	1.246	1.324	4.588	1.371	1.482	5.153	1.549	1.704	5.785	1.796	2.007
60	1.13	1.294	1.384	6.882	1.439	1.567	7.730	1.644	1.821	8.678	1.925	2.164
80	8.172	1.328	1.419	9.177	1.480	1.630	10.30	1.713	1.906	11.57	2.019	2.277
100	10.215	1.357	1.464	11.471	1.528	1.679	12.88	1.768	1.973	14.47	2.093	2.366
150	15.32	1.409	1.531	17.21	1.603	1.771	19.32	1.870	2.097	21.70	2.229	2.530
200	20.43	1.448	1.579	22.94	1.657	1.837	25.76	1.943	2.185	28.93	2.326	2.647
300	30.65	1.505	1.649	34.41	1.735	1.933	38.64	2.049	2.313	43.39	2.466	2.813
400	40.86	1.546	1.700	45.88	1.791	2.001	51.53	2.124	2.414	57.85	2.566	2.932
500	51.08	1.578	1.740	57.35	1.836	2.055	64.41	2.184	2.476	72.32	2.644	3.026
600	61.30	1.605	1.773	68.82	1.872	2.099	77.30	2.232	2.534	86.78	2.708	3.101
700	71.50	1.627	1.801	80.30	1.903	2.138	90.16	2.274	2.584	101.22	2.762	3.166
800	81.72	1.648	1.826	91.77	1.931	2.171	103.0	2.310	2.627	115.7	2.810	3.222
900	91.92	1.666	1.848	103.2	1.955	2.200	115.9	2.342	2.666	130.1	2.852	3.272
1000	102.15	1.681	1.867	114.7	1.976	2.226	128.8	2.370	2.700	144.7	2.889	3.316
1500	153.3	1.743	1.943	172.1	2.060	2.327	193.2	2.482	2.832	217.0	3.034	3.487
2000	204.3	1.788	1.998	229.4	2.120	2.400	257.6	2.561	2.927	289.2	3.131	3.610
3000	306.5	1.852	2.076	344.2	2.206	2.502	386.5	2.674	3.061	434.0	3.283	3.782
4000	408.6	1.898	2.132	458.9	2.267	2.575	515.3	2.754	3.156	578.5	3.385	3.904
5000	510.8	1.934	2.175	573.5	2.315	2.633	644.2	2.816	3.230	723.3	3.467	3.999
10000	1021.5	2.051	2.317	1147.1	2.471	2.819	1288.	3.019	3.471	1447.	3.729	4.308

$\frac{D}{r}$	No. 0, A. W. Gauge. Dia. 0.3249 in. 0.8252 cm Rad. 0.1624 in. 0.4126 cm D. (inches.)	factor. 120~	factor. 140~	No. 00, A. W. Gauge. Dia. 0.3648 in. 0.9265 cm Rad. 0.1824 in. 0.4628 cm D (inches)	factor. 120~	factor. 140~	No. 000, A. W. Gauge. Dia. 0.4096 in. 1.040 cm Rad. 0.2048 in. 0.520 cm D. (inches.)	factor. 120~	factor. 140~
2	0.3249	1.096	1.130	0.3648	1.150	1.199	0.4096	1.230	1.303
3	0.4873	1.189	1.251	0.5473	1.288	1.377	0.6144	1.431	1.558
4	0.6498	1.269	1.353	0.7296	1.404	1.523	0.8192	1.595	1.761
5	0.8122	1.337	1.440	0.912	1.501	1.645	1.024	1.730	1.927
6	0.9747	1.397	1.515	1.094	1.585	1.748	1.229	1.845	2.066
8	1.300	1.496	1.638	1.459	1.722	1.917	1.638	2.032	2.293
10	1.6243	1.576	1.739	1.824	1.833	2.053	2.048	2.181	2.472
12	1.950	1.645	1.823	2.189	1.926	2.166	2.458	2.305	2.621
16	2.599	1.755	1.958	2.919	2.076	2.346	3.276	2.503	2.857
20	3.249	1.844	2.066	3.648	2.194	2.489	4.096	2.659	3.043
25	4.061	1.935	2.176	4.560	2.315	2.633	5.120	2.817	3.231
30	4.873	2.010	2.267	5.473	2.415	2.756	6.144	2.947	3.385
40	6.498	2.130	2.411	7.296	2.574	2.941	8.192	3.153	3.629
60	9.747	2.303	2.619	10.94	2.80	3.211	12.29	3.447	3.976
80	13.00	2.427	2.767	14.59	2.962	3.403	16.38	3.655	4.222
100	16.24	2.524	2.884	18.24	3.089	3.553	20.48	3.818	4.415
150	24.37	2.703	3.096	27.36	3.320	3.828	30.72	4.117	4.764
200	32.49	2.831	3.248	36.48	3.486	4.023	40.96	4.329	5.014
300	48.73	3.014	3.464	54.73	3.722	4.299	61.44	4.629	5.368
400	64.98	3.143	3.618	72.96	3.888	4.496	81.92	4.842	5.619
500	81.22	3.243	3.737	91.20	4.019	4.649	102.4	5.009	5.812
600	97.47	3.327	3.835	109.4	4.124	4.773	122.9	5.144	5.971
700	113.7	3.397	3.918	127.7	4.214	4.880	143.3	5.258	6.106
800	130.0	3.458	3.991	145.9	4.294	4.972	163.8	5.359	6.203
900	146.2	3.513	4.055	164.1	4.362	5.054	184.3	5.446	6.327
1000	162.4	3.561	4.111	182.4	4.424	5.125	204.8	5.525	6.418
1500	243.7	3.747	4.330	273.6	4.662	5.405	307.2	5.828	6.773
2000	324.9	3.879	4.487	364.8	4.832	5.604	409.6	6.044	7.027
3000	487.4	4.067	4.708	547.3	5.071	5.884	614.4	6.348	7.381
4000	649.8	4.200	4.863	729.6	5.240	6.140	819.2	6.563	7.634
5000	812.2	4.304	4.985	912.0	5.371	6.237	1024.	6.730	7.828
10000	1624.	4.638	5.379	1824.	5.798	6.737	2048.	7.271	8.462



Curve-Plate VIII.

value. This condition is the more fortunate, since in arranging supply circuits, it is only the full load drop that is usually of consequence, and for this it would seem that the tabular impedance factors are closely applicable, provided that the wave of E. M. F. generated by the alternator can be regarded as sinusoidal.

The impedance factor for an overhead line wire of copper is the same, whether the current it carries is returned through one or through many similar wires, provided that in the latter case the return wires are all equally distant from it. In other words, if with any single outgoing wire as axis, we trace a cylinder in space, the impedance factor of that wire will be the same whether its return circuit be completed through one or many wires of its own size, provided that all such return wires have their axes located on the cylindrical surface, and independent of the proportions of return current that these may individually carry. In the extreme case the return wires will all be in contact side by side, and form a continuous shell or cylinder round the outgoing conductor at the centre, whose inductance and consequent impedance remains the same as when all the returns are removed save one. It follows, therefore, that the inductance of an insulated copper wire of radius r, concentrically surrounded, (or even, it happens, eccentrically), by a very thin conducting cylindrical sheath of radius R has the impedance factor of the same wire when forming one side of a looped pair at interaxial distance R. The inductances and impedance factors of the return conductors in these cases when separated or dispersed, are always much reduced, and in this last example, the inductance of the exterior conducting shell diminishes indefinitely with the thinness of the cylinder, its impedance factor tending coincidently to the limit unity.

For the same reasons the tables and curves also apply to three parallel copper wires carrying triphase sine currents of equal effective strength, provided that the three wires are equidistant. The distance between any one of the three pairs will then correspond to the ordinary loop distance.

All the results thus far considered and tabulated have been based upon the assumption that the current density in the conductors remains uniform. It is well known that with alternating currents, as the size of the wire and the frequency increase, the current waves cease to penetrate with full amplitude to the axis, but tend to desert the interior for the more superficial layers, thus increasing the apparent resistance of the wire while diminishing its inductance. The basis of this phenomenon of incomplete penetration involves the theory of the transmission of electric energy through space, as well as the relative share of the conductor and its environing dielectric in this activity; but without exploring fundamental causes, the facts are readily explained by causes more proximate and familiar. If we cause magnetic flux to oscillate round a straight conductor, an induced alternating E. M. F. will be set up along that conductor, as in any alternating transformer. But an alternating current sent through the wire would itself set up an oscillating field of this nature about the wire's axis, and the current would be opposed by the induced E. M. F. it thus establishes in the substance of the conductor. This E. M. F. would be greatest at the axis, for at the surface only the flux sweeping round the wire externally, generates voltage, while as we descend towards the centre, the flux within the wire adds to the effect, particularly if the wire is of iron or nickel. At the axis, all the flux in and beyond the wire unites to generate E. M. F.. in oscillation. Under the resultant influence of this, and the impressed voltage at the terminals of the wire, the current will be strongest where the induced counter E. M. F. is weakest, namely at the exterior surface, and its distribution must be such that the "drop" will just sustain a balance between this induced and the impressed E. M. F's. at every point and instant.

This extra virtual resistance by imperfect penetration is generally negligible in ordinary sizes of copper wires at the frequencies employed in practice. Thus with No. 000 A. W. G., the largest wire in the tables, this extra resistance is 1.6 per cent. at 140 ~, 1.2 per cent. at 120 ~, and 0.8 per cent at 100 ~; while the impedance factors, combining this with the inductance are affected in a still lesser degree. Again for No. 00 A. W. G., the extra resistance is 1.0 per cent. at 140 ~, 0.75 per cent. at 120 ~ and 0.5 per cent. at 100 ~, so that for nearly all purposes we may neglect this influence and accept the results embodied in the curves and tables, wherein perfect penetration is assumed.

But in iron wires this virtual extra resistance is known to attain large proportions within practical limits of size and frequency. Figs. 11 and 12 represent a series of observations upon iron wires carrying current waves approximately sinusoidal,[1]

1. The analysis of the wave of E. M. F., generated by the alternator in these and other experiments described, was made by a wiping contact and static voltmeter, over 100 subdivisions of the cycle, and the plotted observations nowhere differed from a sinusoid by more than 3%.

at 140 ~. These iron wires were stretched in a long loop upon the testing room floor, the interaxial loop distance being 10 cms. The impedance factors of the wires in copper under these circumstances would have been 1.42 for No. 5 B. W. G. and 1.1 for No. 9 B. W. G. The impedance factors commenced at values not far above these, but increased with the current to a maximum about ten amperes for the larger wire, and 7.5 amperes with the smaller, while beyond those currents the factors diminished. These impedances were compared with that of a stout german silver wire in the form of a long loop, and with the aid of a differential dynamometer,[1] each of the two separate dynamometers in the complete differential instrument being connected by "pressure wires" with either the standard loop of german silver or the measured loop of iron wire, the connections being occasionally interchanged as a check. As the current in the loops varied, the electrodes on the iron wire loop had to be shifted, in order to restore the differential balance. In this way reliable results could be obtained with 55 metres of stout iron wire, and so long as the temperature of the iron is not sensibly elevated, the results are definite, and retain, within the limits of observation, the same values in ascending or descending series of current strengths. By welding short lengths of the two samples of wire into circular loops, and wrapping these with wire, their permeability and reluctivity were determined by ballistic galvanometer according to the curves shown.

A similar impedance test of a loop of No. 14 A. W. G. iron wire (diameter 0.16 cm.) showed that the impedance did not sensibly alter between current strengths of 0.2 and 2.0 amperes, the latter being the limit imposed by elevation of temperature. In this case with an interaxial distance of 19 cms. and 140 ~, the observed factor was 1.03.

The influence of imperfect penetration on the impedance of iron wires has been calculated by Raleigh and Heaviside on the basis of a uniform permeability through the wire. In reality the results are still more complex, owing to the variation of the permeability at different distances from the centre.

A number of measurements upon impedance in iron wires of the quality in ordinary use for telegraphic and telephonic use, seem to show that with comparatively feeble currents, the mean permeability of the iron is about 150. This means that the in-

1. This instrument has been lately fully described in the *Electrical Engineer*.

ductance of an iron wire is 12 millihenrys per mile greater than the inductance would be if the wire were of copper. On page 10 of Vol. VIII. of the TRANSACTIONS of this Institute, a table is given of the inductance of suspended single wires of copper with ground return. The average value is about 3 millihenrys per mile. If these are increased by 12 millihenrys, the entries should correspond to the average inductances of iron wires under similar conditions (*i.e.*, about 15 millihenrys per mile), except that for wires of more than 1 mm. radius or 2 mms. diameter—say No. 12 A. W. G.—the influence of imperfect penetration commences to be serious, and impedances calculated from these inductances are only reliable for iron wires of No. 12 A. W. G. or smaller.

Continuing, Mr. Kennelly added orally:

In conclusion, to sum up briefly in inverted order:—First the inductance of a telephone iron wire is about five times as great as the inductance of that wire if it were copper. From that inductance, the impedance of the wire for various telephonic frequencies can be determined.

Secondly, the impedance of a larger iron wire, wherein imperfect current penetration plays a considerable part, is such that it increases to a maximum with a certain current, and then diminishes as that current is increased.

Thirdly, the impedance of copper wires such as are used in the distribution of electric light and power on alternating current systems can be determined, if the current waves are known to be practically sinusoidal, when the distance of these wires apart, and their sizes are known. Take the drop that would be established by the current if continuous, and multiply by the factor as shown in these tables and curves.

And, fourthly, and with most emphasis of all, the paper has been written with the endeavor to point out that the difficulties which at present enshroud the use and the working theory of alternating currents are largely fictitious. An alternating current circuit is certainly a very complex thing; but so is a dynamo, and we can predetermine dynamos with a degree of accuracy sufficient for all practical purposes. We know when a machine is designed, within reasonable limits, what the voltage will be at its terminals when driven at a certain speed with a certain excitation and load, although the exact calculation would transcend all the capabilities we possess at this day. Similarly, working theories which are sufficiently good for most practical purposes run through all the kindred sciences, whereas the exact theories are beyond present human capabilities; and although alternating currents are so difficult when studied accurately and absolutely, the working theory of alternating currents can be made as simple as the working theory of continuous currents. I am firmly impressed with the belief—a belief which I trust I may be able to communicate—that the most convincing way of proving that this difficulty which has hitherto surrounded the alternating current and its distribution, can be eliminated and removed is, by the development of the notion of impedance.

APPENDIX I.

Determination of the inductance of a loop formed by two indefinitely long parallel copper wires each of radius r cms. fixed at an interaxial distance d in a medium of inductivity μ. Let the substance of the wires have a resistivity ρ and inductivity μ_0. Let the two wires be denoted by the symbols A and D.

The inductance L (cms.) of a given length l (cms.) in either of these wires, say A, will be the total flux through the loop linked with unit current ($\gamma = 1$) flowing through that length. This flux is partly external to the wire A and partly within its substance.

Considering first the external flux, the intensity B at any point

$$P \text{ is } B = \frac{2\,\gamma\,\mu}{\rho}$$

due to uniform current of γ units in A where ρ is the radial distance OP. from the axis O of A. In a rectangle of length l and breadth $d\,\rho$ drawn in the plane through the wire axis, the total flux will be

$$F = B\,l\,d\,\rho = 2\,l\,\gamma\,\mu\,\frac{d\,\rho}{\rho}\,.$$

The total flux through the loop from the surface of A to the centre of D, so far as depends on A's current is the integral of this expression between the limits $\rho = r$ and $\rho = d$ or

$$F = 2\,l\,\gamma\,\mu\,\log\varepsilon\,\frac{d}{r}$$

and all of this flux encircles and is linked with the current γ making the current flux

$$2\,\mu\,l\,\gamma^2\,\log\varepsilon\,\frac{d}{r}\,.$$

Within the substance of A, and at radius ρ, the intensity will be

$$B = \mu_0\,\gamma\,\frac{\pi\,\rho^2}{\pi\,r^2}\cdot\frac{2}{\rho} = \frac{2\,\gamma\,\rho}{r^2}\,\mu_0\,.$$

In a similar rectangle of length l and breadth $d\,\rho$ the penetrating flux is

$$F = \frac{2\,l\,\gamma}{r^2}\,\mu_0\,\rho\,d\,\rho$$

and this flux is not linked with all the current γ, but with that portion only which is within a cylinder of radius ρ namely with

$$\gamma\,\frac{\pi\,\rho^2}{\pi\,r^2} = \gamma\,\frac{\rho^2}{r^2}$$

so that the current flux linkage is

$$\frac{2\,l\,\gamma^2}{r^4}\,\mu_0\,\rho^3\,d\,\rho$$

for the elementary rectangle. The total flux linkage within the wire will be the integral of this expression from

$$\rho = o \text{ to } \rho = r \text{ or } \frac{l\gamma^2}{2}\mu_0.$$

The total current flux linkage for the wire A is

$$\gamma^2 l\left(\frac{\mu_0}{2} + 2\,\mu \log \varepsilon \frac{d}{r}\right).$$

This is the inductance L of the length of l when $\gamma = 1$ so that

$$L = l\left(\frac{\mu_0}{2} + 2.\mu \log \varepsilon \frac{d}{r}\right)$$

and the inductance per cm.

$$\lambda = \frac{L}{l} = \frac{\mu_0}{2} + 2\,\mu \log \varepsilon \frac{d}{r}.$$

By symmetry it will be the same for either A or D.

For wires in air μ is practically unity

$$\text{and } \lambda = \frac{\mu_0}{2} + 2 \log \varepsilon \frac{d}{r}.$$

For iron wire in air μ_0 is practically about 150 for currents of a few milliamperes from a number of measurements, so that

$$\lambda_{Fe} = \left(75 + 2 \log \varepsilon \frac{d}{r}\right) \quad \text{centimetres.}$$

For copper wire $\mu_0 = 1$ practically, and

$$\lambda_{Cu} = \left(\frac{1}{2} + 2 \log \varepsilon \frac{d}{r}\right) \quad \text{centimetres.}$$

The harmonic impedance factor for copper wires is

$$f = \sqrt{1 + \frac{p^2 \lambda^2}{r^2}}$$

where $p = 2\,\pi\,n$

$r =$ the resistance of the wire per linear cm.

$$= \frac{\rho}{\pi\, r^2}$$

$$\therefore f = \sqrt{1 + \frac{p^2\,\pi^2\,r^4\left(0.5 + 2 \log \varepsilon \frac{d}{r}\right)^2}{\rho^2}}$$

This is the formula from which the Tables I. to VIII. have been computed, ρ being taken for 15° C as 1688.6 units C. G. S.

For any small change $d\,\rho$ in ρ, such as might be due to a change in assumed temperature or conductivity, the corresponding discrepancy introduced into the tubular value of the factor f is

$$df = -\frac{d\,\rho}{\rho}\left(\frac{f^2 - 1}{f}\right)$$

and the fractional error

$$\frac{df}{f} = -\frac{d\rho}{\rho} \cdot \left(\frac{f^2 - 1}{f^2}\right)$$

The coefficient

$$\frac{f^2 - 1}{f^2} \text{ or } \left(1 - \frac{1}{f^2}\right)$$

is the coefficient of reduction from a small percentage variation in the resistance to the corresponding percentage variation produced in the impedance factor.

Thus for

$$f = 2, \quad \left(1 - \frac{1}{f^2}\right) = \tfrac{3}{4},$$

and a change of 4% additional resistivity in ρ will reduce the impedance factor by $4 \times \frac{3}{4}$ or 3%.

PARTICULARS OF THE MEASURED VALUES OF IMPEDANCE FACTOR ON THREE PARALLEL OVERHEAD COPPER WIRES.

These parallel copper wires each 4,000 feet long and No. 6 B. W. G. 0.203″ (0.516 cm.) in diameter, suspended on poles at an elevation of about 18 feet. The mean distance of these wires from one another is approximately one foot (30 5 cms.)

The mean resistance of each wire was 1.03 ω and the mean inductance of each wire when tested in looped pairs 1.264 millihenrys, the temperature of the air in the shade being 29° C. Insulation of each wire approximately 30,000 ω to ground.

A 1,000-volt alternator of 141.6 $\sim$ whose E. M. F. is practically sinusoidal, was connected to a transformer reducing to 50 volts and this secondary pressure through volt and ammeters to successive looped pairs of these overhead wires. The mean impedance of each wire by volt ampere reading was 1.513 ω. Reproducing with the same instruments the same readings of current and voltage from a continuous current dynamo, the mean resistance was 1.029 ω per wire, making the observed impedance factor 1.47. With the mean measured inductance of 1.264 millihenrys the calculated impedance factor is 1.48 by the formula

$$f = \sqrt{1 + \frac{p^2 l^2}{r^2}}.$$

For No. 6 B. W. G. wires, the factor for an interaxial distance of 12 inches is in fact 1.485 by interpolation from the curves and tables.

APPENDIX II.

If the periodic current of c amperes (effective) flowing through a wire at $n \sim$ be analyzed into harmonic components:—

$$c = C \sin pt + D \sin(2pt + \theta_2) + E \sin(3pt + \theta_3) + F \sin(4pt + \theta_4) +$$

$$= C\{\sin pt + \alpha \sin(2pt + \theta_2) + \beta \sin(3pt + \theta_3) + \gamma \sin(4pt + \theta_4) +$$

The impedance factor for the wire will be

$$f = \sqrt{\frac{\left(1 + \frac{p^2 l^2}{r^2}\right) + \alpha_2\left(1 + \frac{2^2 p^2 l^2}{r^2}\right) + \beta^2\left(1 + \frac{3^2 p^2 l^2}{r^2}\right) + \gamma^2\left(1 + \frac{4^2 p^2 l^2}{r^2}\right) +}{1 + \alpha_2 + \beta^2 + \gamma^2 +}}$$

or if $\frac{p^2 l^2}{r^2} = J$

$$f = \sqrt{\frac{(1 + J) + \alpha_2 (1 + 4J) + \beta^2 (1 + 9J) + \gamma^2 (1 + 16J) +}{1 + \alpha_2 + \beta^2 + \gamma^2 +}}$$

The primary current wave observed by Messrs. Ryan and Merritt as supplied to an unloaded transformer is approximately compounded as follows. See also p. 454 Fleming's "Transformer," vol. ii.

$$i = 0.1963 \sin(pt - 49°.\ 20') + 0.04827 \sin(3pt - 76°.\ 50') + 0.01627 \sin(5pt - 90°).$$

$$\text{Here } \beta = \frac{0.04827}{0.1963} = 0.2463, \text{ and } \delta = \frac{0.01627}{0.1963} = 0.08156.$$

$$\therefore f = \sqrt{\frac{\left(1 + \frac{p^2 l^2}{r^2}\right) + 0.06066\left(1 + 9\frac{p^2 l^2}{r^2}\right) + 0.006652\left(1 + 25\frac{p^2 l^2}{r^2}\right)}{1 + 0.06066 + 0.006652}}$$

$$= \sqrt{1 + 1.605 \frac{p^2 l^2}{r^2}}$$

$$= \sqrt{1 + 63.35 \frac{n^2 l^2}{r^2}}.$$

Discussion.

The Chairman:—I think every one here will agree with me that the Institute is to be congratulated upon this magnificent paper of Mr. Kennelly's. It is indicative of a very large amount of painstaking care. There are quite a large number of gentlemen here in the meeting to-night who have made considerable study of this subject, and the Institute will be very glad to hear from them. Before, however, asking for remarks, the Chair would like to refer to one point in Mr. Kennelly's paper that he made inquiry about in London during the last year. Mr. Kennelly referred to the "well-known Ferranti effect." As I understand, shortly after the station started—the sub-station, I think it is in Grosvenor Gallery, some miles distant from the main station, was supplied with electricity from the main station, and when tests were made, it was found, to the surprise of a great many people, that the potential, instead of being lower at that point, was higher than it was at the original source.

Last year while in London, I visited the Ferranti station at Deptford a number of times and inquired particularly of Mr. d'Alton, the manager and electrician of the company, whether this so-called Ferranti effect existed upon the Ferranti mains, and whether it was due to an electrostatic effect or not. He informed me that it did not exist, and that many prominent scientific men had rushed into print to explain theoretically the causes of this effect; that a number of papers had been written about it, but that it was found afterwards to be an effect which did not exist. I asked if he could show me the difference of potential between the generating station and the sub-station, and he took me to the switchboard and stated that the difference of potential was several volts lower at the sub-station than at the generating station. Periodically a signal was sent from the latter to the former, indicating that the potential had fallen below normal. "If you will stand by the instrument," he said, "you will see that the difference of potential is exactly what the drop is calculated to be between the sub-station and the main-station, and it is higher at the original generating source than at the other. Accompanying me at the time was Mr. C. H. W. Biggs, the editor of the *Electrical Engineer* of London. Mr. Biggs and another editor of a paper in London stated to me that this Ferranti effect did not exist, and that a great deal had been written about it that should not have been written. Mr. Biggs said that at the time this appeared, he stated editorially that it was based upon wrong premises. This is all that I can say personally about it, but I doubt not that Mr. Kennelly can give further information on the subject and I should be pleased to hear from him upon the subject.

Mr. Douglass Burnett:—Mr. Chairman, let us for a moment consider a little more carefully two of Mr. Kennelly's equations.

First, the one on p. 178. Let us put it equal to c; *i.e.*,

$$c = e\,p\,k = 2\,\pi\,e\,k\,n, \qquad \text{(I.)}$$

and regard c and n as the variables. It is evident that the curve is a straight line passing through the origin. In other words, for zero frequency, we get zero current through the condenser, and thereafter current is directly proportional to frequency. For infinite frequency, infinite current passes, at any finite voltage e.

Second, observe the formula of p. 179, for impedance. We may put it equal to I, and hence

$$c = \frac{e}{I} = \frac{e}{\sqrt{r^2 + \left(\frac{1}{p\,k}\right)^2}} = \frac{e}{\sqrt{r^2 + \left(\frac{1}{2\,\pi\,k\,n}\right)^2}}. \qquad \text{(II.)}$$

This may be transformed into

$$n = \frac{1}{2\,\pi\,k + \sqrt{\frac{e^2}{c^2} - r^2}}. \qquad \text{(III.)}$$

We see directly that:

(*a*) For the same frequency, current is directly proportional to E. M. F., which was to be expected; that

(*b*) For the same E. M. F., resistance, and capacity, current is a complicated function of frequency; and that

(*c*) For the same current, necessary capacity is inversely proportional to frequency. Substituting, as in the paper,

$$\varepsilon = 1000\,;\ r = 500\,;\ k = 5 \times 10^{-6}\,;$$

in either II. or III., we get the curve of frequencies and currents. [See Fig. 13, p. 232.] For direct currents,

$$n = 0\ \&\ c = o. \quad \text{For } n = \infty\,,\ c = \frac{e}{r} = \frac{1000}{500} = 2\,.$$

For frequencies above 200, c lies between 1.906 and 2.0. This last result is the same as if the condenser were quite short-circuited, and only the resistance existed in circuit.

Equations II. and III. probably apply to (1) the leakage current of a condenser, and to (2) an electrolytic cell used as a condenser, especially when current-density on electrodes is smaller than that value at which decomposition takes place.

If we consider as the best in practice, a diacritical point on the frequency-current curve (see Fig. 13), as with H — B curves, at which the tangent to the curve makes equal angles with both axes, we should have (from III.)

$$\frac{d\,c}{d\,n} = 1\,. \qquad \text{(IV.)}$$

From which we find that

$$\varepsilon^2 = 2\,\pi\,k\,(\varepsilon^2 - c^2\,r^2)^{\frac{3}{2}}. \qquad \text{(V.)}$$

is the best condition. That is, in designing apparatus for a circuit consisting of condenser and resistance in series, such a frequency should be chosen as makes the different quantities have the relation given by V.

It is to be noticed that the laws given by Mr. Kennelly are strictly applicable to Wheatstone's Bridge.

While on the relations between frequency and other quantities, one effect may be worth mentioning. We consider, with Fleming,[1] a circuit of resistance R and inductance L, carrying current x, in parallel with a similar one of resistance S and inductance N, carrying current y; then when $S\,L > R\,N$, the current y leads ahead of the main current by an angle φ, determined by

$$\tan\varphi = \frac{(L\,S - R\,N)\,p}{R\,(R+S) + L\,(L+N)\,p^2}.$$

Here for both $p = 0$ and $p = \infty$, φ becomes 0. That is, for continuous and for infinitely rapid currents, there is no lead. Using the ordinary rule of calculus for determining the maximum, we find that for

$$n = \frac{1}{2\,\pi}\sqrt{\frac{R\,(R+S)}{L\,(L+N)}} \qquad \text{(VI.)}$$

the function is a maximum. Or,

$$\tan\varphi = \frac{S\,L - R\,N}{2\,\sqrt{R\,L\,(R+S)\,(L+N)}}. \qquad \text{(VII.)}$$

Therefore, for a certain determinable frequency, dependent on the resistances and inductances, the lead is a maximum.

These points are mentioned as showing the importance of remembering that in discussions like Mr. Kennelly's, we are dealing with a factor, often subordinated, but which sometimes exerts great influence—namely, frequency.

Mr. T. D. Lockwood:—I presume that each member present had been profoundly interested in Mr. Kennelly's paper, and that each one of us can also say, and say it sincerely, that this evening we have added something to our stock of knowledge—not so much, perhaps, that we can all assimilate at once everything we have heard, because the paper, though beautifully plain, and made still more plain by the admirable extemporaneous exposition which accompanied it, is still far too copious and exhaustive to be readily digested at one sitting; and this I conceive, Mr. Chairman, is the real value of the paper, that we have had a store

1. Fleming; "Alternate Current Transformer," vol. i, p. 133.

of knowledge, as it were, brought right to us, and so placed that it can be embodied in our proceedings in order that at any time we may refresh our memory and replenish our minds by going to it, as we now go to a text-book or an encyclopædia.

It is now, I think, three years since at the Boston meeting of this Institute,[1] I pointed out that impedances were capable of, and had already been employed in many useful applications to electric work, although we did not at that time often make use of the word "impedance." It seems to me that one of the great beauties of practical electrical work is that functions, magnitudes, and apparently disadvantageous phenomena, which seem at first to be prejudicial to good work, can often be utilized and made subservient to good work in other directions. I may instance the polarization, which to the early battery inventors and constructors was such an obstacle, frequently opposing their best attempts at success; yet at the present time the phenomena of polarization having been more fully worked out, we find that it can be made really advantageous, and that by prosecuting researches in such phenomena, the storage battery has been reached; so, taking my own special line of work, it was found in the very early years of telephony that an electro-magnet in a telephonic circuit was a very strong bar to the passage of the telephonic impulses; so much so that one of our members, Mr. F. W. Jones, in devising a remedy for it, said that electro-magnets were *opaque* to rhythmical or rapidly alternating currents. But with the greater advance of the science in its many industrial applications, and our own improved knowledge and experience, it is found that impedances can actually be made useful, in that two wires can be made use of to transmit three messages at the same time, using the two wires as the direct and return wires of the metallic telephone circuit; while on the Van Rysselberghe plan, each one of the two wires composing that circuit may be severally used for the transmission of telegraphic messages; and this simply by placing impedances, made by properly winding covered wire around the iron, and interposing them between the telephonic circuit and the telegraph circuit proper. Such contrivances are much older however in telegraphy. I believe Mr. Cromwell Fleetwood Varley (now gone from us, but whom England and America delighted to honor,) used impedances in 1870—he called them echocymes—for a similar purpose, that is for working Morse telegraphy and harmonic telegraphy on one and the same wire. In the matter of electric lighting we find that Professor John Hopkinson devised impedances by winding wires, the amount of which could be made adjustable, on closed magnetic circuit laminated iron cores, in order that arc lights might be regulated in multiple arc; and I need scarcely call the attention of any one here to the wonderful application in the automatic regulation of lighting by transform-

1. "The Industrial Utilization of the Counter Electromotive Force of Self-Induction" TRANSACTIONS, vol. vii., p. 236.

ers, as suggested, I believe, by Mr. Kennedy, but worked out by his successors, Messrs. Zipernowski, Déri and Blathy.

I have mentioned that "impedance" is an extremely modern term, it having been devised and given to us by Mr. Oliver Heaviside, within the last seven years; it is, however, a very graphic and highly suitable term, and I for one am much obliged to Mr. Oliver Heaviside for this favor, and others of the same kind.

Mr. Chairman, I have heard or read, I cannot tell which, at the present time, that the late Lord Derby in one of his addresses to students, defined knowledge as that which a man has so secured that he can produce it at any moment from his own head. Referring once more to the remarks I first made, I would like to say that I agree with Mr. John T. Sprague entirely, in his view that while that definition is no doubt a first class definition when cramming for an examination, it is not such a good definition when applied to the man of science who must use his science to earn his daily bread; and that real knowledge, the knowledge which the practical man carries in his head, is not perhaps the most valuable part of his knowledge. What really constitutes the best part of his outfit, is the knowing where to find what he wants when he wants it; and it seems to me that the paper we have had this evening will be a great aid to many of us in that kind of knowledge. Hereafter when we want to know anything about impedance, so far at least as it has up to the present time been investigated, we shall come to Mr. Kennelly's paper.

The knowledge which we can carry in our heads is, it seems to me, very much like money we carry in our pockets—very useful for making small change at the moment; but not so useful to carry on business with. But the knowledge which we can get hold of when we need it, when we are asked a question about it, or when a question comes up in our business—the knowledge that we can have recourse to, and find out what we want—such knowledge is more like money that we have in our bank accounts, that is to say, if we have bank accounts, and any money in them.

Dr. Pupin:—Mr. Chairman, I intended to make several remarks on the paper, but since it is so late I feel that I must cut them down and be very brief.

In the first place, it strikes me that Mr. Kennelly has done very good service to practical electrical engineers in making tables to which the electrical engineer can always refer, just as he refers to tables for his resistances, and finds out what will be the impedance of such and such a circuit. It is a very useful thing and it is done in the way in which only Mr. Kennelly can do it. He is well known for his neatness, and for his patience in working out such problems, so that any eulogistic remarks from me would be superfluous.

The other point which struck me as a very good point indeed, is Mr. Kennelly's remark regarding the simplicity of the alternating current theory. I pointed out in a paper read three years ago at the Boston meeting of this Institute,[1] that the alternating current theory is just as simple as the continuous current theory, it is based on one single law, namely, Ohm's law. But, of course; we have to limit ourselves to instantaneous values of things, and just as we say in a continuous current circuit, that the impressed electromotive force plus the total drop is equal to zero, so we can say in the alternating current circuit that the sum of all the electromotive forces taken with their proper sign plus the total drop is equal to zero. We have therefore one and the same law for both kinds of current. That gives us our fundamental equation or fundamental relation between the quantities which are involved. But since that relation is true for infinitely short intervals of time, its symbolical expression gives us what is called in mathematics a differential equation. The integral relations which will hold true for any interval of time are obtained by the process of integration. Now in finding the integral relation between the current and the other quantities which define the circuit, like self-induction, capacity, resistance, etc., we find that if we want to obtain the current, we have to divide the impressed E. M. F., not by the resistance alone, but by the resistance plus something else, and that something else is composed with the resistance, just the same way as two forces, namely by the parallelogram of forces—two forces wich are at right angles to each other. We have therefore the simple rule that the resistance can be composed with the inductance speed, as Mr. Kennelly calls it, just the same as one force can be composed with another which is at right angles to it. In other words the parallelogram of forces is applicable to this case. If there is a condenser, there is capacity in the circuit, and in finding the integral relation between the current and the time, we find that the behavior of the condenser can be represented graphically in a very simple way by introducing into the parallelogram of forces just mentioned, a third component equal to the capacity speed, this third component to be always subtracted from the component representing inductance speed.

I call this graphic method of representing impedance the application of the parallelogram of forces to the alternating current circuits. Mr. Kennelly prefers to speak of vector quantities and the addition of vectors. But, of course, these graphical methods are incidental results of considerable practical importance. The primary law is Ohm's law in its generalized form, and its applicability to variable currents, variable but stationary. If the flow is not stationary, then Ohm's law even in this generalized form will be applicable to infinitely short lengths only of linear conductors as, for instance, in the case of very long wires and high frequency.

1 "Practical Aspects of the Alternating Current Theory": TRANSACTIONS, vol. vii. p. 204, 1890.

The next point which I wish to discuss very briefly is the so called Ferranti effect. I am, however, somewhat timid about it after the remarks of our Chairman, saying that a great many people had rushed into print about this Ferranti effect.

The Chairman:—It was merely the statement that was given to me.

Dr. Pupin:—Well, the statement then that was given to our Chairman makes me hesitate. I also rushed into print about something like the Ferranti effect. I published a paper in the *American Journal of Science*[1] and the next paper is now in print about this very thing. The Ferranti effect is a real, existing effect and can be made very strong indeed. Most people have no idea how strong this effect can be made. It is very simple. The Ferranti effect is only a special case of the more general effect which I call the resonance. It could have been foreseen twenty-five years ago from Maxwell's equations deduced at about that period. Maxwell, I think, was the first to show the effect of introducing a condenser capacity into an alternating current circuit, and it is very interesting to observe this circumstance. Maxwell was spending an evening with Sir William Grove who was then engaged in experiments on vacuum tube discharges. He used an induction coil for this purpose, and found that if he put a condenser in parallel with the primary circuit of his induction coil, that he could get very much larger sparks, which meant, of course, that he got a very much larger current through his primary coil, an alternating current generator being used to feed the primary. He could not see why. Maxwell, at that time, was a young man. That was about 1865, if I do not err. Grove knew that Maxwell was a splendid mathematician, and that he also had mastered the science of electricity as very few men had, especially the theoretical part of it, and so he thought he would ask this young man how it was possible to obtain such powerful currents in the primary circuit by adding a condenser. Maxwell who had not had very much experience in experimental electricity at that time, was at a loss. But he spent that night in working over his problem, and the next morning he wrote a letter to Sir William Grove explaining the whole theory of the condenser in multiple connection with a coil. It is wonderful what a genius can do in one night! He pointed out the exact relations between the condenser, the self-induction and the frequency which would give the largest current, and he was the first to do this, so far as I know.

We must always remember that as soon as we add capacity to a circuit we are giving it elasticity. Without capacity the circuit has no elasticity. Take a stiff wire and suspend a weight, say a cylindrical bar, by it. Suppose that this wire has no elasticity. In order to twist this weight, in order to deflect it from its posi-

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1 April, 1893.

tion of equilibrium, we have to use a force that will twist the weight out of shape. The moment of inertia of the weight, when twisted around, is being changed. That is just what happens in an alternating current circuit which has no appreciable capacity. The electromotive force working on such a circuit produces forced vibrations. But suppose that the stiff wire has elasticity. Then if you give an impulse to the weight, it will swing, and the period of the swing will depend on the moment of inertia of the weight, and on the elasticity of the wire and on nothing else, provided of course that the frictional resistances are not too large. We can make that weight swing rapidly by making the moment of inertia small or the elasticity large, one of the two, or both. Now the coefficient of self-induction in the circuit, corresponds to the moment of inertia of the swinging weight, and the capacity in the circuit corresponds to the elasticity of the wire, and just as the elasticity of the wire and the moment of inertia of the weight determine the period of the circuit, so the capacity in the circuit, and the self-induction determine the electrical period of the circuit. That is to say if you create a disturbance in the circuit, say by pulling quickly a permanent magnet away from the coil, you will start an electrical disturbance there, and the electricity will swing back and forth, just as a pendulum swings back and forth, the period of that swing depending on the coefficient of self-induction and the capacity of the circuit. The electricity in the circuit will swing back and forth till it is reduced to rest by the ohmic resistance. If you now apply a periodically varying impulse—not a single impulse, but a periodically varying impulse to the torsional pendulum just mentioned, you will make it oscillate, but the oscillation will be forced, if the period of the acting force is different from the natural period of the pendulum. But if the two periods are exactly the same, then the oscillation of the pendulum is a free oscillation, and the force and pendulum are in resonance. Now what is the effect of free oscillation? The effect of free oscillation is to reach a larger swing than the forced oscillation under the action of a force of the same mean intensity. The swing will continually increase, until the work done against the frictional resistances during one half of the swing is exactly equal to the work which the moving force does in that time. If, therefore, the resistance is very small, you see that the swing will increase indefinitely. But what happens to the wire? Resonant swinging means simply this:—The kinetic energy of the swinging weight is periodically reduced to zero, that is, transformed entirely into the potential energy of the elastic forces of the wire and *vice versa*; so that the larger the swing, the larger will be the maximum elastic force with which the wire reacts; so that the smaller the frictional resistances the larger the elastic reaction. But we must remember that the elastic force in the wire corresponds to our potential difference in the condenser, so that by acting upon the circuit by means of an electromotive force whose period is exactly the same as the natural period of the circuit, we produce an electrical flow which continually increases until it has obtained a maximum value and at that time, of course, the potential difference in the condenser may be many times higher than the voltage of the impressed electromotive force. I have produced by very simple means a rise of potential from 60 volts to 900 volts just that way, and from 80 volts to nearly 1,200 volts by putting a condenser in series with a coil, and a proper adjustment of the two. I took the secondary of a transformer and placed with it a large coil, large self-induction, in series, and then, in series with this, a condenser; and then I adjusted my capacity to the self-induction in such a way that I brought the period of the circuit in resonance with the frequency of my alternator, and the consequence was, that the difference of potential (indicated by a Thomson electrostatic voltmeter) went from 80 volts to 1,200 volts.

If you create too much rise of potential your condenser goes. am working at that problem yet, and I expect to be able very soon, in fact I have already promised, to read a paper before the Institute on this very thing. I have no doubt that it is quite possible to transform 50 or 60 volts into 10,000 or 15,000 or any number of volts in that way.

On page 178 of the paper it is mentioned that the reason for the non-equality of the voltage on the series of the two impedances with the arithmetical sum of the voltages measured on each impedance in turn, is due to the fact that the currents in the two coils are usually out of step. I should like to point out that this is misleading. The current must have at any moment the same phase at every point of an alternating current circuit, otherwise there would be an accumulation or absorption of electricity at the points where a change of phase existed. There is evidently a *lapsus linguae* in the paragraph referred to, and I have no doubt that Mr. Kennelly will easily change the paragraph.

MR. KENNELLY:—Dr. Pupin is right concerning the paragraph on page 178, which by an oversight is misleading. The sentence should read "due to the fact that 'otherwise the currents in i_1 and i_2 while of equal strength develop in these counter E. M. F's that are out of step."

DR. GEYER:—Mr. Kennelly has in those plates given us very instructive information tending to show that in the alternating system of distribution there is a somewhat greater drop than in the continuous current system, and he has also called attention to the interesting fact that on account of the distortion of the sinusoidal wave by the transformer under light load, this extra drop is to some extent increased, so that the distribution is still less uniform than it would be without this action of the transformer. I believe all this applies to the primary part of the circuit. I should like to consider for a moment the secondary part of the circuit—say the house wiring. I do not at this moment recollect whether there is a corresponding distortion of the sinusoidal wave in the secondary part of the circuit, and I should like to ask Mr. Kennelly if there is, or if there is not, a corresponding tendency there to increase the extra drop.

MR. KENNELLY:—As Dr. Geyer has remarked, the impedance factor increases above the tabular values when the current waves are not simply sinusoids, and in such cases the drop in the primary feeders is further augmented. But if the alternator supplying these feeders generates a wave of E. M. F. that is sinusoidal, the transformer must also generate E. M. F's that are very nearly sinusoidal, both in its primary and secondary coils, even although the primary current wave may be severely distorted under the influence of hysteresis in the core. Consequently, with only incandescent lamps, and no ferric inductances, in the secondary circuit, the secondary current waves will be very nearly sinusoidal, making the impedance factors of the secondary conductors practically tabular.

THE CHAIRMAN:—I want to say just a word in reference to Dr. Pupin's remark. The remarks that I made upon the Ferranti effect were based entirely upon the statements made to me by parties on the other side of the water, and referred particularly to the effect claimed to be produced upon the Ferranti cable, and not alone the peculiarity of a difference of potential which might be caused by electrostatic phenomena, and I should like to ask Mr. Kennelly's opinion as to the reliability of the reports respecting the Ferranti effect as produced upon his underground cable in London.

MR. KENNELLY:—Not having had the pleasure of being present at the measurements or observations that were made on the Ferranti cable, I am unable to give any direct evidence whatever, but having carefully read Dr. Fleming's very interesting paper, in which a large number of experiments were described upon the pressure as generated and supplied, I think there is no escape from the conclusion that under certain conditions of load and transmission, an effect was produced which bears the name of the Ferranti effect; but that possibly under conditions which were described by the Chairman that effect did not exist. It is entirely, of course, a matter of the resonance in the circuit, as Dr. Pupin has described it, and if you do not have the right load, that effect may disappear. So it is quite easy to understand that the cable, under certain conditions of load or transmission, might be resonant and might exalt the pressure; while under normal load this effect might cease to exist.

MR. C. S. BRADLEY:—Mr. Chairman, I think the paper is entirely beyond my scope. I should like to thoroughly understand it. It has given me a great many points and it is very clear indeed. I am very glad to have heard it, and I think it will prove of great value as a reference. I have not digested it yet, to borrow Mr. Lockwood's phrase, and therefore I am not prepared to say very much about it.

MR. LOCKWOOD:—As no one else seems inclined to discuss the paper, I should like to move that the Institute present to Mr. Kennelly a very hearty and cordial vote of thanks, for the intellectual treat which he has provided for us.

MR. SPRAGUE:—I should like to second the motion of Mr. Lockwood. I am prevented by an impedance in my own head from taking any part in the discussion. But I want to thank Mr. Kennelly for placing on the record, where it can be easily obtained, so much solid information, so clearly expressed, about a subject that is so little understood. While great knowledge is always of value if carried in one's own head; if it is placed where one can get at it with little trouble, and so that when gotten it can be easily digested, there is perhaps more benefit derived by the general run of electrical engineers than if they attempt to acquire too much personally. So I heartily second the motion that is made that a vote of thanks be tendered to Mr. Kennelly for his most interesting and valuable paper.

[The motion was carried.]

Adjourned.

[COMMUNICATED AFTER ADJOURNMENT BY MR. CHARLES P. STEINMETZ.]

I have read Mr. Kennelly's valuable paper on "Impedance" with the greatest interest, and am only sorry that I was not able to be present at its presentation.

One statement, however, I would like to emphasize somewhat more strongly, since in the paper it is enclosed between so many remarks of the highest practical value as to be liable to escape due notice, viz.: that

"Any circuit whatever, consisting of a combination of resistances, non-ferric inductances and capacities, carrying harmonically alternating currents, may be treated by the rules of unvarying currents, if the impedances are expressed by complex numbers:

$$a + b j = r (\cos \varphi + j \sin \varphi),$$

the algebraical operations being then performed according to the laws controlling complex quantities."

It is well known that the points of a plane can be represented by complex quantities in their rectangular representation

$$a + b j \ (*),$$

(*) Where j denotes $\sqrt{-1}$

or their polar representation

$$r (\cos \varphi + j \sin \varphi),$$

and use has been made hereof repeatedly in the mathematical treatment of vector quantities. It is, however, the first instance here, so far as I know, that attention is drawn by Mr. Kennelly to the correspondence between the electrical term "impedance" and the complex numbers.

The importance hereof lies in the following:—The analysis of the complex plane is very well worked out, hence by reducing the electrical problems to the analysis of complex quantities they are brought within the scope of a known and well understood science.

Let us consider an instance hereof which will bring us to another point I intend to dwell upon.

In the alternating current line mentioned on page 187, of Mr. Kennelly's paper, the impedance of the loop can be expressed by the complex quantity,

$$I_1 = 2.57 + 2.87 j = 3.854 (\cos 48^\circ + j \sin 48^\circ),$$

$$\text{or, } \frac{I}{2} = 1.285 + 1.135 j = 1.927 (\cos 48^\circ + j \sin 48^\circ) \text{ per wire,}$$

that means, at the line current $C = 10$ amperes, between the ends of each line a difference of potential will exist of

$$\frac{C I_1}{2} = 19.27 \text{ volts.}$$

Let, now, the impedance of the generator be

$$I_0 = 1.5 + 10 j = 10.11 (\cos 81^\circ + j \sin 81^\circ),$$

then, if the consumer's circuit has an impedance

$$I_2 = 25.0 + 27.8 j = 37.4 (\cos 48^\circ + j \sin 48^\circ)$$

(containing for instance, motors, etc.)
then the total impedance of the circuit is

$$I = I_0 + I_1 + I_2 = 29.07 + 40.67 j = 50 (\cos 54.5^\circ + j \sin 54.5^\circ)$$

In this case a generator potential of $E = 500$ volts will send a current of

$$C = \frac{E}{I} = 10 \text{ amperes}$$

through the circuit.

The difference of potential at the receiver end of the line will be

$$E_1 = C I_2 = 374 \text{ volts.}$$

At the generator end of the line

$$E_0 = C(I_1 + I_2) = 412.5 \text{ volts},$$

and the drop of potential in the line,

$$E_0 - E_1 = 412.54 - 374 = 38.54 \text{ volts},$$

or 19.27 volts per line, $= \frac{C I_1}{2}$, *i.e.*, equal to the difference of potential between the ends of the line, or the true ohmic drop, times the impedance factor.

If, however, the receiving circuit has an impedance

$$I_2 = 442$$

(incandescent lamp load),
the total impedance of the circuit is

$$I = 48.27 + 12.87\,j = 50\,(\cos 15^\circ + j \sin 15^\circ).$$

At the generator potential of 500 volts, 10 amperes will flow through the circuit again, but the difference of potential at the receiver end of the line is

$$E_1 = C\,I_2 = 44.2 \text{ volts}.$$

At the generator end,

$$E_0 = C(I_1 + I_2) = 468.6 \text{ volts},$$

since

$$I_1 + I_2 = 46.77 + 2.87\,j = 46.86\,(\cos 4^\circ + j \sin 4^\circ)$$

so that the drop of potential in the line is 468.6 — 442 = 26.6 ohms, or 13.3 volts per line. That is considerably less than the difference of potential between ends of the line, which still is 19.27 volts, so that in this case of the non-inductive, or lamp load, in spite of the impedance factor 1.5 of the line, the drop of potential of the line is practically only the true resistance drop, and the inductance of the line does not count at all.

Still more remarkable are the conditions if the receiver circuit has an impedance of a form similar to

$$I_2 = 15 - 60\,j = 61.95\,(\cos 76^\circ - j \sin 76^\circ),$$

as is the case if the line is feeding into a condenser of the capacity $K = 19$ mf. or running a synchronous motor (which under certain conditions of load and excitation acts like a condenser of large capacity).

In this case the total impedance of the line is

$$I = 19.07 - 46.22\,j = 50\,(\cos 67.7^\circ - j \sin 67.7^\circ).$$

Here again 500 volts generator E. M. F. will establish 10 amperes of current in the circuit.

But the difference of potential at the receiver end of the line is

$$E_1 = C\,I_2 = 619.5 \text{ volts.}$$

At the generator end,

$$E_0 = C\,(I_1 + I_2) = 598.7 \text{ volts}$$

that is; the drop of potential in the line will be

$$E_0 - E_1 = -20.80 \text{ volts,}$$

$$\text{or } -10.4 \text{ volts per line;}$$

that means that, while a continuous current will experience a loss of potential of 12.85 volts per line wire, and from the impedance of the line a difference of potential of 19.27 volts per line wire is calculated, we find in this particular case a *rise* of potential of 10.4 volts per line wire, so that the difference of potential is *rising* along the line instead of *decreasing*, and that by not less than 20.8 volts.

In this case at an E. M. F. of 500 volts produced in the generator, an E. M. F. of 598.7 volts will appear at the generator terminals, and 619.5 at the receiver terminals.[1]

The conclusion that we derive herefrom is, that it is not permissible to calculate the drop of potential in an alternating circuit by multiplying the resistance drop by the impedance factor. The difference of potential between the ends of the line will indeed be equal to the current times impedance. The losses of potential in the line, however, will usually be less than the difference of potential between the ends of the line, and may even be negative; that is, the potential may rise in the line.

This is due to the difference of phase between the line impedance and the impedances of the other part of the circuit, as explained in the paper. I considered it advisable, however, to dwell upon this more particularly, since the mistake is made frequently, to calculate the drop of potential in an alternating current line from resistance, impedance factor and current. In reality the *drop of potential in one part of an alternating circuit, depends not only upon the constants of this part of the circuit, but upon the conditions of the other parts of the circuit also*, and may, therefore, at the same current strength vary enormously with different conditions of load.

Now a few words more on a question of terminology. Perusing the literature of the last years on this subject, we find everywhere the endeavor to establish a suitable name for quantities like

$$2\,\pi\,n\,L \quad \text{or} \quad \frac{1}{2\,\pi\,n\,K}.$$

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1. It needs not to be remarked that if the generator contains iron, due to the variable permeability of iron the numerical values will, in practice, be found more or less modified.

It is of the dimension "speed" or "resistance," is expressed in ohms, and defined by

$$\frac{\text{E. M. F. at right angles to current}}{\text{current}}.$$

This quantity and the true or ohmic resistance are the catheti of a rectangle with the impedance as hypothenuse.

Kennelly names it "*inductance-speed*" (the factor $2 \pi n$ being an angular velocity).

This name, however, does not apply well to

$$\frac{1}{2 \pi n K}.$$

About two years ago I proposed the name "*inductance*" for this quantity, so that impedance should be the resultant of resistance and inductance, as components. Unfortunately the name "*inductance*" has now been applied to what we called before the "*coefficient of self-induction.*" S. P. Thompson in the new edition of his book has adopted the name "*inductance*" for $2 \pi n L$ also.

The name "*inductive resistance*" has been used by Fleming and others. This name, however, has been applied also as synonymous with "*impedance.*" "*Ohmic inductance*" has been proposed, but all these double names are too inconvenient to be of much practical value. Decidedly the best name would be "*inductance,*" the more so as the constant L is so little used in practice that the ponderous name "*coefficient of self-induction*" will hardly cause any inconvenience.

Lynn, Mass., April, 1893.

[Reply to Mr. Steinmetz, communicated by the Author.]

Concerning Mr. Steinmetz's remarks, one in particular is of such practical importance that it will not probably suffer by emphasis or repetition. Mr. Steinmetz points out that when a circuit is composed of an alternator, supply conductor, and load, each having its individual impedance, their vector sum will generally be less than their arithmetical sum. Consequently if the generator delivers 1,100 volts at its terminals, and the drop in the supply wires allowing for the impedance factor is 100 volts (50 on each conductor, as might be indicated by a voltmeter connected between the far and near ends of one wire), the voltage at the transformer terminals would be never less, but usually more than 1,000 volts, particularly if condensers are in circuit at the receiving end, and its exact value would depend upon all the impedances in the circuit. It is therefore essential to observe that all the tabular impedance factors given in the paper as applying to sinusoidal currents, or augmented impedance formulas applying to non-sinusoidal currents, accurately yield the drop in the conductors so long as the static capacity in those conductors is negligible. The voltage of delivery at the distant end will always be greater than the difference between this drop and the voltage of supply at the near end, unless it happens that the impedances of the load and of the conductor have the same vector, or time constant. But unless condensers are added to the receiving end of the line, this variation of the actual voltage of delivery from the difference between line drop and voltage of supply, is generally small at full load on practical alternating transformer circuits. In other words the time constant of the ordinary supply wires does not differ materially from the apparent time constant of the transformers when loaded, although obviously this statement cannot be depended upon too far.

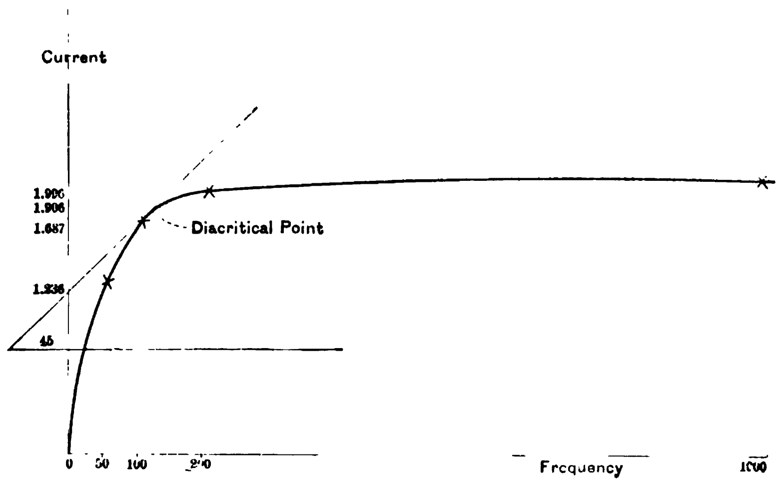

FIG. 13. (See Burnett's Remarks, p. 218.)

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

ANNUAL MEETING.

NEW YORK CITY, May 16th, 1893.

The meeting was called to order at 4.20 P. M. by the President, Mr. Frank J. Sprague, who announced that the first business would be the appointment of tellers to count the ballots for officers.

THE PRESIDENT:—I will appoint Messrs. T. C. Martin and George A. Hamilton, tellers, and they will proceed at once to the counting of the ballots which are in the possession of the Secretary.

MR. THOMAS D. LOCKWOOD: —In view of the fact that the procedure of election is a new one upon this occasion, and also that the rule governing our procedure is rather long and easily forgotten, I suggest that the Secretary be instructed to read the new rule before the tellers proceed to their work, or, as an alternative, that a copy of the rule be given to the tellers that they may refer to the same while proceeding with their work. I will make that as a motion.

[The motion was seconded.]

MR. PHELPS:—I hope Mr. Lockwood will be content with having a copy of the rule given to the tellers.

THE PRESIDENT:—It is hardly necessary to put the motion. A copy of the rule will be handed to the tellers.

MR. JAMES HAMBLET:—In view of the great work which will be required of the tellers, I would make a motion, if it is in order, that the number of tellers be increased to five in order to facilitate the work.

THE PRESIDENT:—The rule states that there shall be only two tellers.

THE SECRETARY:—I should say it would be perfectly in order for those tellers to have clerks to assist them.

MR. HAMBLET :—I was not aware of that rule. I merely made the suggestion in view of the great work entailed.

THE SECRETARY :—I have the ballots here with a list of the membership corrected to the first of May, and in order to facilitate this work, the ballots have been arranged in packages which are numbered from one up—about fifteen packages in all. The ballots will be found in alphabetical order to correspond with the list. The ballots are at the disposal of the tellers.

THE PRESIDENT :—The tellers will at once assume their duties.

THE SECRETARY :—I would like to suggest, in the first place, that the tellers are to count the ballots in the meeting room and we shall have to pass on the point whether the place they are going to is the meeting room or not. They have disappeared from view.

MR. PHELPS :—Let us call them back.

THE PRESIDENT :—I appoint Mr. Phelps a committee of one to recall the tellers for instructions.

[Mr. Phelps went after the tellers, who returned.]

THE PRESIDENT :—I take it that the front room might be used by the tellers.

[The tellers proceeded with the counting of the ballots in the front room.]

THE SECRETARY :—I have the report of the Council and the Treasurer printed.

MR. PHELPS :—Are these reports to be read? Or are they to be considered as read? It has been the invariable custom, on financial reports being presented, to appoint a committee, usually of two, to audit the Treasurer's report, and I hope that that procedure will be taken to-day.

The Secretary read the following reports:

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

REPORT OF COUNCIL FOR THE YEAR ENDING APRIL 30TH, 1893.

In compliance with the requirements of the Rules, the Council submits to the Annual Meeting a report of the work of the Institute during the past year.

Ten meetings of the Council have been held at which the average attendance has been 8, the highest number present at a meeting having been 14, and the lowest 5, which was at the June meeting held in Chicago.

At the meeting held May 17th, 1892, upon recommendation of a Special Committee, Lord Kelvin, Dr. Werner von Siemens, and Cyrus W. Field were elected to honorary membership.

In accordance with the announcement at the last Annual Meeting, the 9th General Meeting of the Institute was held at Chicago on June 6th, 7th and 8th. The meeting was very fully attended, especially by Western members, while the Eastern States were also well represented. The general result was entirely satisfactory. Eight other meetings have been held in New York City. At these various meetings 30 papers and reports were read and discussed, and have since been printed and distributed to the members. The volume of the TRANSACTIONS for 1892 contained 858 pages and was one-third larger than that of the previous year.

In response to an inquiry made by direction of Council, the authorities in charge of the World's Columbian Exposition assigned two rooms in the Electricity Building to be occupied during the World's Fair season as the headquarters of the Institute. A special committee was appointed to make arrangements for its proper equipment, and prepare a plan for raising the necessary funds for that purpose. Satisfactory progress has been made and it is hoped that the result will be beneficial to the Institute.

In pursuance of its general policy toward assisting in the preparation for the World's Electrical Congress of 1893, the General Committee of the Institute and its Sub-Committee on a Provisional Programme have been actively engaged during the year, and their work, which has been published through the TRANSACTIONS and the electrical press, has elicited much favorable comment.

An important change has been made in the plan of issuing the TRANSACTIONS, by which the various papers and discussions are now published at an earlier date than was possible under the previous arrangement. In order to provide for the extra expense involved, suitable advertisements are accepted for publication, and there is every reason to believe that the plan will be found so satisfactory as to justify its continuation hereafter. The revenue from advertisements will cover more than the additional cost.

Inquiries having been made by many members regarding the issuance of a badge and certificate of membership, the Council has from time to time considered designs for official acceptance. Designs have recently been approved, and arrangements for their supply are now being carried out.

The total membership of the Institute at the close of last year's report was as follows:

Honorary Members	3
Members	179
Associate Members	433
Total	615
Honorary Members elected during the past year	3
Associate Members elected	98
Restored to membership	1
Making a total of	717

The following have resigned during the year:

WM. LEE CHURCH, A. F. MASON.
C. J. BRINER, WM. M. SHEEHAN,
WM. C. BENBOW, D. H. BATES,
ALDEN M. YOUNG, J. NORMAN BULKLEY,
ADOLPH G. GREENBERG.

Total 9.

Our loss by death has been unusually large, the list being as follows:

CHARLES M. DAVIS, EDW. T. MIDDLETON,
CYRUS W. FIELD, GEORGE J. SPENCER,
DR. WERNER VON SIEMENS, DR. NORVIN GREEN,
GEORGE B. PRESCOTT, JR., J. P. ABERNETHY,
D. HERBERT JEFFERY.

Total	9.
Elections cancelled by reason of failure to qualify	6
Dropped from list for non-payment of dues	20
Loss of membership	44

Deducting the loss of membership as stated leaves a remainder of 673 (a net gain of 58) classified as follows:

Honorary Members	3
Members	206
Associate Members	464
	673

Since the report of membership was closed, two additional deaths have occurred—Grosvenor Porter Lowrey, Esq., and James Bowstead Williams, M.D.

SECRETARY'S BALANCE SHEET.

For the Year Ending April 30th, 1893.

Dr.		*Cr.*	
To balance from 1892	$ 25 56	By Cash to Treasurer	$7,552 67
Sundry receipts	45 55		
Entrance fees	480 00		
Life Members	200 00		
Past dues	245 82		
Current dues	5,297 13		
Advance dues	80 17		
Electrotypes sold	516 83		
Typewriting and stenography	139 58		
Transactions sold	364 87		
Subscriptions to Transactions	142 00		
Received for binding	38 50	Secretary's Balance on hand	23 34
	$7,576 01		$7,576 01

DISBURSEMENTS DURING THE YEAR.

The Treasurer has disbursed upon warrants drawn by the Secretary for bills approved by Council and Finance Committee, the amount of $7,707.30, classified as follows:

Stenography and typewriting	$ 428 90
Stationery and miscellaneous printing	426 60
Postage	265 95
Messenger service	82 34
Salary account	1,844 43
Engraving and electrotyping	689 84
Meeting expenses	386 20
Rent	660 00
Printing Transactions	2,650 27
Binding	113 83
Office fittings	20 00
" expenses	138 94
	$7,707 30

This amount should be credited with the following items:

Electrotypes sold	$ 516 83	
Collected for stenography and typewriting	139 58	
Received for binding	38 50	
Sundry receipts	45 55	
	$740 46	740 46
Showing the net expenses of the year to have been		$6,966 84
And the net receipts for fees, dues, sales and subscriptions of Transactions		6,809 99
Showing an excess of net expenses over net receipts for the year of		156 85
The outstanding current bills against the Institute April 30th, were		617 45
Amount of uncollected accounts outside of membership		250 06
Due from members		710 80
Balance of general fund in Treasury		90 90
Secretary's balance May 1st, 1893		23 34

Judging from past experience, about half the dues in arrears will be paid during the year.

Respectfully submitted by direction of Council,

RALPH W. POPE,

Secretary.

New York, May 1, 1893.

TREASURER'S REPORT.

FROM MAY 1, 1892, TO APRIL 30, 1893.

GEORGE M. PHELPS, TREASURER, in account with
AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

Dr.

Balance from previous year	$ 245 53	
Received from Secretary, May 1, 1892, to May 1, 1893	7,552 67	
		$7,798 20

Cr.

Payments, from May 1, 1892, to April 30, 1893, on warrants from Secretary, Nos. 353 to 428 inclusive	$7,707 30	
Balance to new account	90 90	$7,798 20
Balance on hand, General Fund, May 1, 1893	$ 90 90	

BUILDING FUND.

Balance as per last report, plus interest at 3 per cent. from July 1, 1891, on certificate of deposit in Mercantile Trust Company, New York	$ 850 00

Respectfully submitted,

GEO. M. PHELPS,
Treasurer

THE PRESIDENT:—While the reports are being distributed I will make this announcement. Dr. Pupin, of Columbia College, whose paper was on the list to be read first at to-morrow afternoon's session, wishes to read it at Columbia College where he can have the advantage, both for himself and the members of the INSTITUTE, of conducting certain experimental work in illustration of the facts stated in the paper. It has been decided that perhaps the best way to accommodate both Dr. Pupin and the INSTITUTE members generally, is to change the order of the papers so that Dr. Pupin's paper will be the last read to-morrow afternoon. The first papers will be read and disposed of and the INSTITUTE will then adjourn to meet at four o'clock at the lecture room at Columbia College. The opportunity of listening to this lecture illustrated by the experiments which will be conducted by Dr. Pupin will more than offset any disadvantages connected with the transfer of the place of meeting.

THE SECRETARY:—The following Associate Members were elected at the regular meeting of Council this afternoon:

Name.	Address.	Endorsed by
BARBOUR, FRED. FISKE	Manager, Power and Mining Department, Pacific District, General Electric Co., 15 First St., San Francisco, Cal.	O. T. Crosby. Louis Bell. F. A. C. Perrine.
BARTH-BARTOSHEVITCH, A.	Mechanical and Electrical Engineer, Westinghouse Electric and Mfg. Co., 160 Arch St., St., Allegheny City, Pa.	Albert Schmid. F. S. Smith. Henry Floy.

Name.	Address.	Endorsed by
BRYANT, WALDO C.	Manager and Treasurer, The Bryant Electric Co., Bridgeport, Conn.	Wm. J. Hammer. H. A. Foster. James Hamblet.
BURKE, JAMES	Electrical Engineer, General Electric Co., 24 Front St , Schenectady, N. Y.	A. L. Rohrer. H. N. Marvin. Arthur Churchill.
CABOT, JOHN ALFRED	City Electrician, 123 Garfield Place, Cincinnati, Ohio.	H. B. Slater. C. O. Mailloux. F. G. Waterhouse.
CHRISTMAS, ADOLPH FREDERICK	Manager, Electrical Department, Johnson Co., Johnstown, Pa.	Philip A. Lange. Hermann Lemp, Jr. Ralph W. Pope.
CRAIG, J. HALLY	Cumner, Craig & Co., John Hancock Building, Boston, Mass.	T. C. Martin. Geo. M. Phelps. T. D. Lockwood.
DA CUNHA, MANOEL IGNACIO	Manager of the Electrical Section, Empresa Industrial Gram-Para, Para, United States of Brazil.	G A. Hamilton. T. C. Martin. Ralph W. Pope
DOOLITTLE, THOMAS B.	American Bell Telephone Co., 125 Milk St., Boston, Mass.	Thos. D. Lockwood. F. A. Pickernell. F. W. Dunbar.
FRYE, HENRY W.	Associate Editor, The *Electrical World*, Times Building, New York City.	Carl Hering. W. D. Weaver. Louis Duncan.
GIFFORD, CLARENCE E.	Assistant Electrical Engineer, The Buffalo Railway Co., 860 Prospect Ave., Buffalo, N. Y.	Geo. H. Stockbridge. Ralph W. Pope W. A. Anthony.
GUY, GEORGE HELI	Secretary. The New York Electrical Society, 131 West 34th St., New York City.	Frank J. Sprague. T. C. Martin. Nikola Tesla.
LIEB, CHARLES A.	General Electric Co., 44 Broad St., New York City.	Wm. J. Hammer. H. A. Foster. H. Ward Leonard.
LORRAIN. JAMES GRIEVE	Norfolk House, Norfolk St., London, W. C., England.	Ralph W. Pope. G. H. Stockbridge. T. C. Martin.
MATTHEWS, CHARLES P.	Instructor in Physics, Cornell University, 15 Heustis St., Ithaca, N. Y.	Ernest Merritt. E. L. Nichols. Frederick Bedell.
OLAN, THEODOR J. W.	Civil and Electrical Engineer, New York City.	James Hamblet. Wm. J. Hammer. Horatio A. Foster.
PETTY, WALTER M	W. M. Petty & Co., General Electrical Supplies, Rutherford, N. J. and New York City.	James Hamblet. Chas. E. Emery. H. A. Sinclair.
SCHEFFLER, FRED.	Agent, Stirling Boiler Co., 74 Cortlandt St., New York City.	Wm. J. Hammer. T. C. Martin. Ralph W. Pope.
SONN, GEORGE C.	Instructor in Physics, Newark High School, 285 Bellevue Ave., Newark, N. J.	Wm. J. Hammer. H. A. Foster. James Hamblet.
WIENER, ALFRED E.	Electrical and Mechanical Engineer, General Electric Co., 24 Yates St., Schenectady, N. Y.	A. L. Rohrer. H. N. Marvin. Arthur Churchill.
WRIGHT, PETER	Inspector of Electrical Works, United Gas Improvement Co., Drexel Building, Philadelphia, Pa.	Alex. J. Wurts. C. J. R. Humphreys. E. G. Willyoung.

Name	Address.	Endorsed by
YARNALL, V. H.	Superintendent of Construction, J. G. White Co., 29 Broadway, New York City.	Wm. J. Hammer. C. G. Young. E. T. Birdsall.

Total, 22.

The following Associate Members were transferred to Membership.

Approved by Board of Examiners, March 15, 1893.

HUNTING, FRED. S.	Electrical Engineer, Fort Wayne Electric Co., Fort Wayne, Ind.
GALE, HORACE B.	Consulting Electrical and Mechanical Engineer, 40 California St., San Francisco, Cal.
KINSMAN, FRANK E.	Electrical Engineer, Plainfield, N. J.
STEARNS, CHARLES K.	Superintendent of Construction, N. W. Thomson-Houston Electric Co., St. Paul, Minn.
DAVIS, MINOR M.,	Assistant Electrician, Postal Telegraph-Cable Company, New York City..
RODGERS, HOWARD S.	Electrical Engineer, Eddy Electric Mfg. Co., Windsor, Conn.
WHITE, ANTHONY C.	Electrical Engineer, American Bell Telephone Co., Boston, Mass.
DUNBAR, F. W.	Assistant Electrician, American Telephone and Telegraph Co., 153 Cedar St., New York City.

Total, 8.

The certificates of membership which were authorized have now been delivered by the engraver, and copies may be had upon application to the Secretary. These are issued to members only, not to associate members, and the fee, covering the price of engraving, printing and engrossing will be $2.00.

The Council found it necessary to change the price on account of the cost of engrossing.

THE PRESIDENT:—Dr. Hutchinson and Mr. Delany are appointed a committee to pass upon the accounts of the Secretary and Treasurer. While the tellers are at work, informal remarks would perhaps be in order.

DR. FREDERIC A. C. PERRINE:—I have an informal remark to make. The members may know that I am going out to teach electrical engineering on the Pacific Coast. I have talked to Mr. Phelps and Mr. Martin a little bit about the possibility of starting some sort of branch of the INSTITUTE there. I do not know how many men we have, or how many men are available, in fact I do not know anything about the country, but I shall know after I get there, and I was wondering whether it would be in accordance with the wishes of the association to have an attempt made on the Pacific Coast to start a branch of the INSTITUTE. I would like to hear some opinion from the members present.

THE PRESIDENT:—Do you mean a branch of the INSTITUTE that will hold meetings there?

Dr. Perrine:—Yes. That would be all that the branch would consist in—some authorized headquartersof the Institute where meetings could be held and papers read, as Pacific Coast men cannot get to New York meetings.

The President:—There is a social branch in Chicago—the Chicago Electric Club, which ought to be a centre for electrical men.

Dr. Perrine:—My idea was to have it done, if at all, under the auspices of the Institute, giving it a little more authority than it would have as an electric club.

Mr. Phelps:—The suggestion of Dr. Perrine is an interesting one. The Chicago Electric Club and other similar clubs are unrelated to this body, being mainly social organizations. The region to which Dr. Perrine is going is quite alone. There is no other region quite like it. I suppose none of us know much better than Dr. Perrine how many persons might be found on the Pacific Coast within a convenient distance of San Francisco sufficiently interested in electrical engineering to give attention to a sub-organization under the auspices of this Institute. But it is a matter well worth looking into. It might be referred to some committee. We have no committee which could properly deal with the subject; but a committee might be appointed to consider it during the summer. I suppose Dr. Perrine will scarcely begin his new duties before the autumn. We could get our ideas in shape meanwhile. A great deal of electrical work is being done in that region.

The President:—I question whether there is anything in the rules to permit of the establishment of branches of the Institute.

Mr. Lockwood:—It has been for some two or three years a project of our worthy Secretary to establish what he has called "chapters" of the Institute in different cities. He had spoken about establishing one in Boston, but I discouraged him, because I make a monthly trip to New York out of it. I think if one is started in San Francisco we should see less of Dr. Perrine. My experience with California men of all classes shows me that if they think there is anything in it they will take it up, but unless they can see the prospect of immediate profit, they will not interest themselves in it. It seems to me it would be an excellent plan for Dr. Perrine to communicate with the Institute after he gets to California and let us know what the prospect is for the establishment of a chapter. I suggest, therefore, that the matter be left to him for the present, and that he be requested to communicate with the Secretary on that matter.

The Secretary:—I presume there are gentlemen here—I can see at least one—who will bear witness that I have had this project on my mind the last two years. In fact, we went so far as to have a committee appointed by Council to consider this question of holding meetings in other cities. It is one that has quite recently been taken up the American Society of Civil Engineers, for the reason that they found that in various cities in the United States civil engineers, and in some cases mechanical and electrical engineers, were forming local bodies, issuing in some cases printed transactions and in others simply holding meetings, and a proposition was made to reorganize them in chapters, under the auspices of the American Society of Civil Engineers, but in the case of the civil engineers the movement had been delayed too long. It had lain dormant until some of these local bodies had attained considerable importance, and for that reason they hesitated when it came to a question of amalgamation, as it appeared to them to involve a sinking of their identity in the national society. That is about the status of the case, as I understand it. The American Institute of Architects has chapters in the different cities, and the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS with which, of course, I am most familiar, has held meetings in other cities, although it is considered sometimes, or has been considered, a kind of a New York institution. There is, however, barely a quarter of our membership in New York and vicinity. Our membership extends all over the United States, and in a certain sense all over the world. But as a matter of convenience the meetings have been held in this city, because it is always possible to get together here a sufficient number of members actively interested to make good meetings. That was one of the difficulties encountered in the early stages of the INSTITUTE--getting enough people to make a good meeting and bringing forth discussion; and if we undertake to establish chapters or branches in other cities, of course it is necessary to avoid doing anything which would possibly prove a failure. That is, we must not make the attempt in cities which are too small to give the necessary membership. When this question was discussed in Council, one of the stumbling blocks was in regard to the publication of the papers—what papers should be printed, and if they were printed in the series of transactions should the INSTITUTE at large bear the expense, or should the local members assume it? It was felt that perhaps the TRANSACTIONS might become too voluminous, and that it would be too much of a financial burden for the INSTITUTE to carry, as it is well known that the largest item of expense in running the INSTITUTE to-day is the publications of the TRANSACTIONS, and if that was increased fifty per cent., unless we had a corresponding increase of membership, it would lead to a deficit. This might be covered to a certain degree by the income from advertisements. But what I rose to say was, that this is a question that will confront us sooner or later. It appears to me that it should be taken up before the state of affairs existing in the civil engineers is reached; that is, before the local societies attain such strength that they would refuse to affiliate with us on account of losing their identity. Further, I wish to add, that in a great many of these local societies valuable papers have been read and discussed, but they have not had the means to publish them or report their discussion, and consequently whatever has been done has been practically lost. All who have been benefited by it have been those who happened to be present at the meetings, and that is one reason why I believe many of these societies would be willing to assume closer relations to the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS. It is not quite true that this is the only society, because we have on the Pacific Coast the California Electrical Society, we have the Buffalo Electrical Society, we have the New York Electrical Society. We have the National Electric Light Association, which from a purely electric light association has developed into a tramway association and an electric power association, and deals with almost every branch of electrical work, excepting perhaps telegraphy and telephony.

MR. PHELPS :—It seems to me that the suggestion of my friend, Mr. Lockwood, indicates about the probable disposition of this matter and I am very glad that the Secretary has told us so fully what the situation is. We have but one more Council meeting this summer, and if Dr. Perrine will look over the field on the Pacific Coast during the ensuing few months and write the Secretary, I think that when meetings are resumed we shall be able to deal with the subject not only on the Pacific Coast but elsewhere.

PROF. HOUSTON :—I think that Dr. Perrine's suggestion is one which the INSTITUTE should consider very carefully before we take any decided steps in the matter. It is a case in which it would be well to make haste slowly. What shall be the status of the members of these chapters in the event of the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS considering it advisable to establish them? Shall they be associate members or full members? Does the INSTITUTE feel that it is advisable to scatter its influence rather than to concentrate it? Is it not true that the great value of the meetings of the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS is their cosmopolitan character? As soon as they become mere local meetings then it seems to me they would be shorn of much of their real value, which consists in bringing gentlemen interested in our particular line of research from all over the country to one central place. I think that the proposition ought to be considered very carefully.

MR. FRANKLAND JANNUS :—It seems to me, Mr. President, that we have adopted a title that is far more impressive than any other —the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS. It sounds very different from the New York Electrical Society or the San Francisco Electrical Society. Such a name might indicate a social club with a co-operative bar-room, or something of the sort. This is an institute of electrical engineers. Now we have gone so far as to maintain a solemn title of that sort for all these years, and I do not see why we should not go farther and insist that that is the representative title of all electrical bodies in the United States.

Dr. Edward L. Nichols:—It seems to me that there are two ways in which this extension could be brought about, one of which would be admirable; the other very doubtful. If it was proposed to delegate to parties on the Pacific Coast or elsewhere the right to elect members, associate or otherwise, I should regard that as a doubtful policy. The Institute has members, and doubtless will have more members, on the Pacific Coast. Those men, as I understand it, are capable now, under the present constitution, of recommending new members who could be elected by the Council here. Now, when that membership gets to be sufficient on the Pacific Coast so that there is an interested body of men, why cannot they hold meetings? I do not know of anything in the constitution of this society to prevent their meeting and producing as much good material as they can. Then the question of official recognition and affiliation will naturally come. It seems to me the conservative plan is to see what increase of membership through the present channels can be reached in California, and then when there is enough of such an element to constitute a body which would naturally hold meetings, which has life enough to keep itself together, the question of affiilia-tion to the main body might well be brought up and some plan adopted.

The President:—I doubt the wisdom of assigning an identity to a body of men who as members are already affiliated with the Institute. If they have as much pride as they should have in the Institute they will get together and bring together other members. I agree certainly with Professor Houston that we want to make haste very slowly in a question of doing anything which tends to create a separate organization or separate interests in any part of the United States. I understand that Dr. Perrine is going to San Francisco. While he is there he will get in touch with the electrical people. He will be in a position to know what are the indications, whether there is any likelihood of an independent electrical organization existing on the Pacific Slope and if members of this Institute who are resident on the Pacific Coast will become members of it and separate themselves from the Institute. In that event a warning note should be sounded which will give this Institute an opportunity to investigate the matter and determine what policy to adopt. I doubt the power of this Institute to give anybody authority to establish a branch. We have no such division of our organization authorized, and I do not see that anybody could at present do more than simply get the opinions of other people as to whether it is advisable to have local meetings of the Institute in any particular locality.

Mr. Phelps:—I suppose it would be premature altogether at this time to consider just what sort of organization such a body on the Pacific Coast might have. I have assumed, in anything I have said, that what was meant was that persons interested in electrical engineering, eligible to membership in this Institute and residing on the Pacific Coast, having joined this INSTITUTE, becoming either associate members or members, might hold local meetings. I have not for a moment thought that it was contemplated that a distinct and different organization might be affiliated with this body.

DR. PERRINE:—The idea that I threw out has grown very much in this discussion. I had no idea that an association on the Pacific Coast would be allowed to elect members—simply that those members of the INSTITUTE at present, and members of the INSTITUTE elected hereafter, who reside on the Pacific Coast should be allowed to select a chairman and hold meetings under the auspices of the association. It would amount to nothing more than their coming together of their own accord, but there would be a little more encouragement to them, and they would feel more in touch with the general body if they knew that their meetings were under the auspices of the association. As regards printing their papers, I think that should be a burden upon themselves entirely, except so far as their papers which would come here would be considered worthy to be incorporated into the transactions of the INSTITUTE, and that would only be done where papers were sent on from San Francisco to be judged by the INSTITUTE whether they would print them or not.

THE SECRETARY:—I might add for the information of the meeting that we have elected, I think, from California about a dozen members this last year, several of those being in San Francisco. There are also others on the Pacific Coast. The idea that has been broached in regard to holding meetings elsewhere has always been, that members have authority to hold meetings, and there is nothing to prevent their assembling. If this idea was carried out according to the plans which have been considered, a local secretary would be appointed, as we have local secretaries now, who would look after the details, and of course the meeting could appoint its chairman, and under the old system of elections, they might have secured a Vice-President or Manager in the Council who would preside. But under the new system this might not be possible, unless the Council should assume that that was the proper thing to do. If the Council should assume that out of the nominations it was proper to have one representative Manager or Vice-President in California—if such a man was named in the nominations I presume he might be selected as a "Council Nominee," and in that way, a regular representative might be elected to Council.

THE PRESIDENT:—It is common with some societies to have corresponding secretaries in different parts of the country. The United States Naval Institute has such in different parts of the country. The new Institute of Naval Architects has no separate branches.

DR. CHAS. E. EMERY:—As a member of the Am. Soc. of Civil Engineers, I was at the time fairly familiar with the difficulties met by that society in the consideration of the subject of affiliation with local societies. My recollection is that the most important difficulties were those outlined by Prof. Houston. In that case the local organizations were already formed and had attained considerable strength. In a number of the Western cities they held regular meetings under the control of officers selected by themselves, and a number of the same were so associated that they published their papers jointly under the title of the "Journal of the Association of Engineering Societies," and it frequently contained very good papers. When the subject of consolidation or affiliation of some kind was urged by a large number of members of the Civil Engineers, questions immediately arose of the kind suggested by Prof. Houston as to the qualifications of the members of the local societies, and how the standard of the American Society of Civil Engineers in regard to membership could be maintained in a way satisfactory to such local societies. I will say, to illustrate the difficulties, that I heard remarks in this connection somewhat as follows: "I know So-and-So; he is not fit to be a member of the Civil Engineers; he was only a rodman, but had an eye to business, bought Western land in the right place, and having become well-off wishes to put M. Am. Soc. C. E. after his name." Such pointed, illustrations did not come out in the meetings that I recollect, but there were references in the discussions sufficiently pointed so that all understood the nature of the difficulty. The membership of the local societies had already become established. In order to gain strength, all classes of engineers had in some cases affiliated together, civil, mechanical and electrical, including in some cases practical workingmen of intelligence, and, doubtless, in some instances, persons entirely engaged in trade, who made a specialty of furnishing supplies to engineers of various kinds. Many reputable persons, therefore, in good and regular standing in the local societies would not even be qualified as associates of the Civil Engineers, yet such societies were doing good work in their own way in the particular locations. It was suggested that the organization could be turned over to the Civil Engineers and chapters or local societies formed, which, as local societies, would have a membership independent of that of the Civil Engineers, but the members of the American Society felt that the others would in some way be entitled to use the name. Various plans were proposed. The subject was discussed in the engineering periodicals, and was finally made an issue in the election of officers. As a result, the conservative element which opposed affiliation prevailed at the election, and the subject has only been referred to incidentally since. I may add that it was urged that the American Society of Civil Engineers, being the oldest engineering society, should be at the head, and that it should concentrate rather than disintegrate, and the history and practice of the British Institution of Civil Engineers was urged as a precedent

—every engineer prominent in any branch of engineering in Great Britain must be a member of the Inst. C. E., and this great society holds meetings only in London. As that city can, however, be reached in a day from the principal parts of England and Ireland, the conditions are entirely different from what they are in this country, except in respect to members of the Institution residing in Canada and other British colonies. Here we have a great country with cities scattered through it, separated altogether too far to make it possible for any large proportion of the members to attend meetings regularly in New York. The local societies seem, therefore, to be a necessity. I have always thought there is some common ground upon which a parent society could affiliate with local societies, and am happy to hear that the American Institute of Architects has been able to establish chapters which operate satisfactorily. It seems to me that the AM. INST. OF ELECTRICAL ENGINEERS can do something of the kind without all the embarrassment experienced by the Civil Engineers. They can at least insist that the sub-organizations be conducted entirely by members of the INSTITUTE, that all questions of membership be settled at the general meetings, and this will leave only the collateral but important questions as to what steps it is best to take in particular cases to permit others engaged in kindred pursuits to take part in the proceedings of the local organizations, and add that strength which is absolutely necessary to secure success. I think I have said enough to show the nature of the difficulties, and although I believe they are not insuperable I am not at the moment prepared to suggest any definite course of action. I trust that all the members will give the subject careful attention, and believe that in due time a satisfactory solution will be reached.

THE SECRETARY:—I believe the solution of this problem will come when the society gains such financial strength that it can undertake to print such transactions of the meetings in other towns and cities as will be acceptable to the INSTITUTE. It appears to me that if this is done, all the members in these cities will see that there is not only an opportunity for them to hold their meetings, but also to have their proceedings placed on record, and bring them into one set of transactions which would add just so much to their value, as there is talent in these various cities, and it is simply a question of there being a sufficient number of members of the INSTITUTE in any district to get together and make what we may call a respectable meeting and discussion. I might add to the other cities that I have mentioned, Schenectady and Lynn, where societies have grown up. There is the Thomson Scientific Society in Lynn and the Schenectady Technical Association. Both these societies have meetings, and I presume they are of a character similar to those which we hold. Further, there is as yet no tendency, as the President has suggested, on the part of the members of these local societies to sever their connection with the INSTITUTE, but the tendency is rather for members of such local societies to affiliate with the INSTITUTE. George P. Low, President of the California Electrical Society, became a member of the INSTITUTE a short time ago. Mr. Guy, Secretary of the New York Electrical Society, was elected an associate member of the INSTITUTE at to-day's Council meeting, and two members of the Schenectady Association have joined within a month or two.

THE PRESIDENT:—This question has not, to my knowledge, come up in the city of New York between ourselves and the New York Electrical Society. It seems to me that the most effective work that can be done by the INSTITUTE towards any consolidation of electrical interests is for every member to get as many available and suitable members into the INSTITUTE as possible, and thereby strengthen it both in numbers and from a financial standpoint.

MR. PHELPS:—I think our friend Dr. Perrine has heard sufficient from the members to satisfy him of the sort of welcome that will be extended by this body to any organization that he may choose to recommend in California; and, without any formal action on the part of this meeting, I hope he will be pleased to feel that he is requested to look over the ground and write freely to the Secretary as to the condition of electrical affairs and the number of persons he may meet there who would be likely to become members of such a sub-organization as he contemplates.

THE PRESIDENT:—I think I can say on behalf of the INSTITUTE, that we should be only too glad if Dr. Perrine would bring to the attention of the electrical engineers of the Pacific Coast, the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS, and also to keep the INSTITUTE and its officers posted as to anything likely to affect its welfare.

DR. PERRINE:—I am very much obliged to the President and members for discussing this question as they have. The discussion is practically all that I wanted. I wanted to know how the members felt. With Mr. Phelps, I now feel that a welcome would be accorded to any association that may be established among the members of the INSTITUTE out there. When I reach there, I will write the Secretary what I have done, whom I have met and how they feel on the subject, and then he can tell better whether it would be necessary for any formal action to be taken on the part of the association. I do not think any such action is necessary at the present time.

THE PRESIDENT:—Is the Auditing Committee ready to report?

DR. HUTCHINSON:—We have to report that we have examined the accounts and have found them correct.

DR. EMERY:—I move that the report be accepted.

[The motion was carried.]

MR. LOCKWOOD:—I move now that the report of the Council and Treasurer be accepted and adopted.

THE PRESIDENT:—I would add that the thanks of the INSTITUTE be extended to them.

DR. EMERY:—I second that.

[The motion was carried.]

PROF. HOUSTON:—Will a sufficient number of the members of the INSTITUTE be present at the dinner to constitute a quorum?

THE PRESIDENT:—I believe a majority of whoever is present constitutes a quorum. There will not be a majority of the members of the INSTITUTE present at 6.45.

[The tellers not having finished the counting of the ballots a recess was taken until 6.45 P. M.]

The meeting was again called to order at 6.45 P. M.

THE PRESIDENT:—The tellers will please announce the result of the election.

MR. T. C. MARTIN:—Mr. President and gentlemen, it is utterly impossible to canvass the whole of the ballot in all its details. The members have shown their objection to a straight ballot anyhow, and have illustrated how great the vagaries of the intelligent voter can be. Casting aside all irregularities and doubtful votes the net return is as follows:

For President, Prof. E. J. Houston, 235; Mr. T. D. Lockwood, 146. The rest of the ticket is of course elected overwhelmingly.

The nominations for Vice-Presidents and for Managers and for Treasurer were the same on all the ballots that were sent in, with a few variations.

H. Ward Leonard, P. B. Delany and William Wallace have been elected Vice-Presidents. Harris J. Ryan, Charles Hewitt, W. J. Hammer and J. J. Carty have been elected Managers, and George M. Phelps has been elected Treasurer.

There is a possibility that the pluralities of one or two of those may be changed by the application of the principle of the cumulative vote, which has been very freely applied. But the proportion to overcome is so overwhelming that the general result can not be changed.

THE PRESIDENT:—Gentlemen, you have heard the result of the balloting.

MR. LOCKWOOD:—I move that the election of Prof. E. J. Houston to the Presidency of this INSTITUTE be made unanimous so far as that can be done by such proportion of the members as are present.

The motion was carried.

MR. SPRAGUE:—I appoint Mr. Delany and Mr. Hammer a Committee to escort the President to the Chair.

President Houston on taking the Chair said: Gentlemen of the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS. I can only thank you for the very great honor you have conferred on me. I recognize it as an exceedingly high honor to be called to preside over what I know to be the representative electrical engineering society in the country, if not in the world. While I am your presiding officer I shall endeavor to make my rulings such as will conduce to the welfare of the Institute of Electrical Engineers. [Applause.]

The regular business having been fully carried out, a motion to adjourn is in order.

[A motion was here made to give a vote of thanks to the retiring President.]

I beg pardon, it is but proper to permit the gentleman who has so ably represented the electrical engineers in the past make some remarks.

MR. SPRAGUE:—Gentlemen:—There is little that I can say beyond extending my personal welcome and the welcome of every individual member of the INSTITUTE to our new presiding officer. He needs no introduction from me; his name is too well known, his personality is too familiar to you all. As an inventor, as a teacher, as an author, as a compiler, as a scientist, in fact in all respects, I think we have embodied in our new President a man of whom we can be most proud.

As far as I personally am concerned, I have to thank the members of the INSTITUTE for the courtesy they have always shown me, and express my belief that there will be an even more active and healthy growth of the INSTITUTE in the future than in the past. I must express regret at my own shortcomings, and the lack of attention I have been guilty of during my tenure of office, but I look forward to the future of the INSTITUTE with more interest than when I took the office, with perhaps more ambition to be of aid to it. I think the Columbian Exposition in Chicago, with all the evidences of what electrical engineers have performed in this country and what they promise to perform, will do much to accomplish that which we all desire—the building up of what we hope will be the greatest engineering society in this country. In leaving the office to the newly elected President, I leave it in the hands of one who will do very much in making the advance towards that goal.

[Applause.]

MR. LOCKWOOD:—There is still one more item of business, it seems to me, before we adjourn. I think it would be unbecoming in the highest degree if we should part with our late President and relegate him to the honorable but rather effete chair of the Past Presidents, were we to fail to take some notice of the manner in which he has presided over our deliberations in the past year. His inaugural speech, which some of us had the privilege of hearing in Chicago, has to a certain extent set the pace for the papers that have been read before the INSTITUTE and discussed from time to time, and those who have had the opportunity of being associated with him in Council will gladly bear witness to the zeal and strength with which he has presided at our meetings. I for one have been very favorably impressed with him, and shall miss those monthly meetings of the Council very much indeed. I will not further take the time of the INSTITUTE but will content myself, Mr. Chairman, by moving that this INSTITUTE offer a rising vote of thanks to our Past President for his able services in the past year.

The motion was seconded by Mr. Phelps and carried unanimously.

MR. SPRAGUE:—I wish to extend my personal thanks to Mr. Lockwood, who has really done so much more than the President has, in actively carrying on the administration of the society and I really feel almost personally embarrassed by the very cordial compliment he has paid me. I do not feel that I merit it. But since the society has so affirmed it, I thank the members as heartily as if I did.

[The meeting then adjourned, and the members reassembled at "The Arena," No. 41 West 31st Street, and participated in the Annual Dinner of the INSTITUTE.]

GENERAL MEETING.

NEW YORK CITY, May 17th, 1893.

President Houston called the meeting to order at 10 A. M.

THE PRESIDENT:—I take great pleasure in introducing to you a new member, Mr. Charles P. Matthews, of Cornell University, who will read a paper on "Fuse Metals in Direct and Alternating Current Circuits."

Mr. Matthews read the following paper:

A paper presented at the Tenth General Meeting of the American Institute of Electrical Engineers, New York, May 17, 1893, President Houston in the Chair.

THE ACTION OF CONTINUOUS AND ALTERNATING CURRENTS ON FUSE METALS.[1]

BY C. P. MATTHEWS.

For the protection of electric circuits from the passage and consequent heating effects of currents of abnormal intensities, no device is in more general use than the so-called safety-fuse. Commercially the fuse appears in a variety of connecting devices and inclosing boxes, classed as thermal "cut-outs," and familiar to all having to deal with the distribution of electric energy. The protective element in all such, consists of a wire or strip of some easily fusible metal of such current carrying capacity that it will melt under the heat developed by an excessive current. In points of simplicity, cheapness, ease of adjustment and low resistance, the safety-fuse possesses advantages over most, if not all, electro magnetic safety devices. Experience shows, however, that unless certain conditions for successful working are carefully observed, the safety-fuse may fall far short of furnishing satisfactory and reliable protection.

With a view of studying these conditions, and of obtaining at the same time reliable data on the fusion of conductors under circumstances closely approximating those of actual practice, a series of experiments, some of the results of which appear in this paper, were recently made in the Physical Laboratories at Cornell University.

This experimental work, which was undertaken by several persons, may with propriety be divided as follows:

1. An abstract of two theses, viz: "Safety-fuses for Electric Circuits," by J. S. Peck and C. P. Matthews; and "The Action of Alternating Currents on Fuses," by W. S. Rugg and C. R. Sturdevant.

Part I. On the behavior of various alloys under the action of direct currents, with special reference to their use in thermal cut-outs.

Part II. On the *disintegrating* effect of the alternate current on fuse metals.

PART I.

The first experimental work of any extent on the electric fusion of wires was performed by W. H. Preece, *F. R. S.*, in 1884.[1] He showed that the relation between diameter and fusing current is parabolic,[2] being

$$C = a\, d^{\frac{3}{2}},$$

where a is a constant for any given metal or alloy. This relation is obtained as follows:—The rate at which heat is developed by a current C flowing in a wire of resistance R is

$$H = \frac{C^2 R}{J}$$

in thermal measure. The resistance of the wire may be expressed in terms of its dimensions and the specific resistance ρ of the material thus:

$$R = \rho\, \frac{l}{\frac{1}{4}\pi\, d^2},$$

By substitution,

$$H = C^2 \frac{4\,\rho\, l}{J\,\pi\, d^2} = K\, l\left(\frac{C}{d}\right)^2. \qquad (1)$$

Now as the temperature rises, there are heat losses by radiation and by convection into the surrounding medium, and also, to a greater or less extent, by conduction into the terminals. Neglecting this last, and assuming the radiation and convection to be proportional to surface (to what extent this is justifiable will be presently considered) we may write for the rate at which heat is thrown off

$$H' = \pi\, d\, l\, \varepsilon\,(T - T^1), \qquad (2)$$

where $T - T^1$ is the excess of temperature of the wire above that of the envelope, and ε is the emissivity of the surface, or

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1. "On the Heating Effects of Electric Currents," Proc. Roy. Soc., April 8, 1884.

2. This law had been previously enunciated by Prof. Forbes before the British Association in 1882. (See Rept B. A.) See also paper by same author, Jour. Soc. Telegraph Engineers, vol. xiii, p. 236.

the number of water-gram-degrees emitted per second, per square centimetre of surface, per degree rise of temperature. Equation (2) may be written

$$H = K' d l$$

for any permanent temperature. With a constant current a condition of affairs is soon reached when heat is lost at the same rate as that at which it is generated by the current, and a constant temperature obtains. We then have

$$H = H', \text{ or}$$

$$K l \left(\frac{C}{d}\right)^2 = K' l d,$$

from which

$$C = a d^{\frac{3}{2}},$$

where

$$a = \sqrt{\frac{K}{K'}} = \sqrt{\frac{\pi^2 J \varepsilon T}{4 \rho}}.$$

This result shows the fusing current to be independent of the length of the conductor. Evidently a is the current necessary to fuse a wire of unit diameter. The emissivity is here assumed to be a constant quantity for a given surface. The valuable experiences of Kennelly[1] on the "Heating of Conductors by Electric Currents," in which the radiation and convection losses were separated, and the results of Ayrton and Kilgour[2], indicate that such is not strictly the case. These latter investigators found that for any temperature the emissivity is the higher, the finer the wire. The rapidity with which fine wires heat and cool is a matter of common observation. Mr. Kennelly's results indicate that the convection is proportional to the temperature elevation for small differences of temperature, and that moreover losses occuring in this way increase slightly with the diameter, and are dependent on the form of the conductor. For the purposes of calculation of the fusing currents of wires, however, it is sufficient—as both the results of these investigations and those of Mr. Preece show—to consider only the total heat loss, and to assume the emissivity a quantity independent of the diameter and temperature elevation.

Mr. Preece's first experiments with platinum wires of small

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1. London Electrician, vol. xxiv: pp. 142, 169, 194.

2. "The Thermal Emissivity of Thin Wires in Air." Abstract B. A. paper London Electrician, vol. xxviii: p. 119.

diameter do not appear to confirm the foregoing relation. He says :—"Platinum wires—especially those of small diameter—are liable to flaws which practically reduce their effective diameter to a great extent; and also the larger wires from their greater weight necessarily tend to part asunder at a lower temperature than those which are lighter, and in which the strain is less."

In 1887[1] Mr. Preece presented the results of an elaborate series of experiments on wires of different metals, showing that the theoretical relation is quite strictly borne out in experiment except for wires of extremely small diameters. The following year "final and corrected" values of *a* were announced for the following metals:

Copper, - -	10244
Aluminium, - -	7585
Platinum, - -	5172
German Silver, -	5230
Platinoid, - -	4750
Iron, - - -	3148
Tin, - - -	1642
Alloy (Tin 1, Lead 2),	1318
Lead, - - -	1379

In determining these constants, Preece used six-inch lengths of wire, while to ascertain the value of the same metals as cut-outs 1¼ inch lengths were used. It will be noticed that a value of *a* is given for only one alloy of lead and tin. In the course of the experiments of which this paper treats, a redetermination was made for lead and for tin, and at the same time values of the constants were determined for various alloys of these metals. These latter are, it is believed, now announced for the first time. These metals were dealt with especially, because of their extended use in the manufacture of fuse-wires.

To determine to what extent the fusing current is influenced by the cooling effect of the terminals, pieces of wire of given diameter and composition, but of varying lengths were successively tested. The marked variation in current is shown graphically in Fig. 1, Curve A. It will be noted that while an 8 in. piece fused at 6.6 amperes, a ½ in. piece of the same wire carried 12.5 amperes, or an excess of nearly 100% before yielding. For lengths of this wire greater than 6 in. the conduction losses are

1. Proc. Roy. Soc., Dec. 22, 1887.

practically nil in their influence on the value of the fusing current. The reader is referred to a formula deduced by Lord Kelvin[1] for the loss of heat occurring in this way. The practical point to be observed at this juncture would seem to be this: manufacturers cannot accurately rate their fuse-wire unless the length of the specimen to be used is specified. In the fuse-blocks in common use it is impracticable to use extreme lengths, yet if 3 in. pieces were used, the cooling effect would not be serious. Fuse-blocks are on the market employing from ½-in. to 3 in. lengths of wire and all the variation indicated by the curve may be expected.

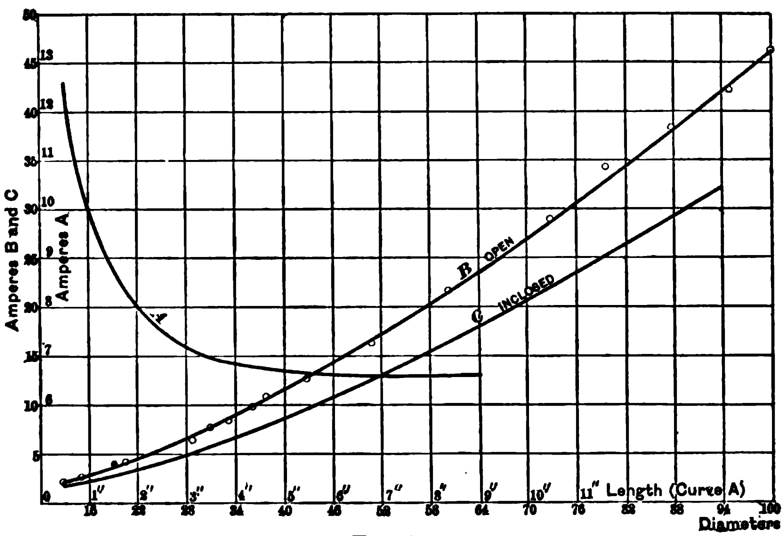


Fig. 1.

In the experiments to determine *a* for alloys of lead and tin, ordinary commercial metals were used, for, although the fusing current is affected by the presence of impurities, yet it was considered better to experiment with such grades of metal as would be used in practice. The alloys were cast in the form of cylindrical slugs, and these were drawn into wires varying in diameter from 10 to 100 mils. These were tested with great care. A Thomson graded ammeter was used to measure the current, the instrument being frequently calibrated during the work. For

1. In J. T. Bottomley's paper "On the Permanent Conductors Through which an Electric Current is Passing." Proc. Roy. Soc., June 20, 1884.

varying the current strength, a liquid resistance was used. The box containing the electrolyte was provided with a sliding contact, such that the current could be increased or decreased continuously without break or undue fluctuation.

There is an important *time element* to be considered in connection with the phenomena of fusion. A certain interval elapses before the wire reaches the permanent temperature corresponding to a definite current. The duration of the current in this portion of the work was 60 seconds, experience showing that if a wire of moderate diameter did not fuse in that time under the action of a certain current it would not fuse at all. Curves A′

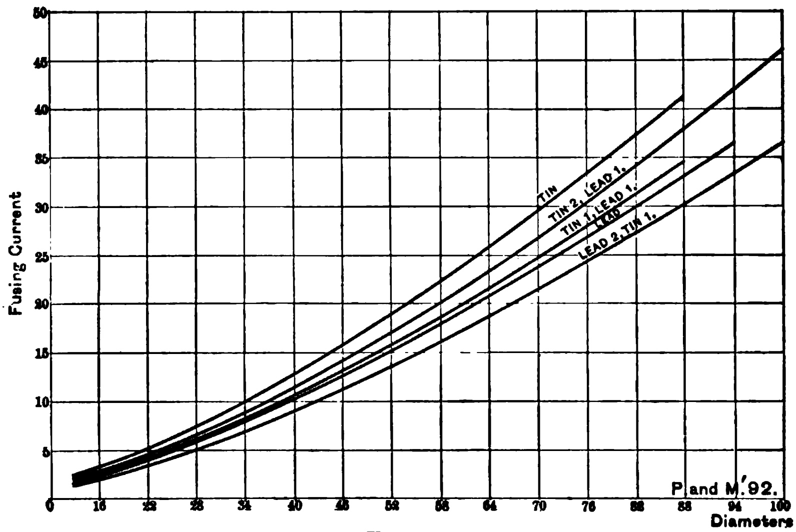


FIG. 2

and B′ of Fig. 5 illustrate the magnitude of this effect in a particular case, the time intervals being as 1 : 2. The variation in fusing current exceeds 5 amperes for the larger sizes. It should be noted in this connection, that unless massive terminals are used, they may themselves become heated, thereby checking the flow of heat through the ends of the wire, and tending to promote ultimate fusion.

The limits of this paper do not allow the insertion of the complete data on these alloys. The curves of Fig. 1 (B and C), however, may be taken as representative, and indicate that the "three-halves power law" was quite closely followed, a result which was obtained in all the other cases. From each observation of current and diameter, a value of the quantity a was computed; the mean of these values was taken, and a curve of calculated fusing currents drawn for each alloy. These curves appear in Fig. 2. It will be noted that while the temperature of fusion of tin is considerably lower than that of lead, its *fusing current* is about 24% higher. This is undoubtedly due to the difference in

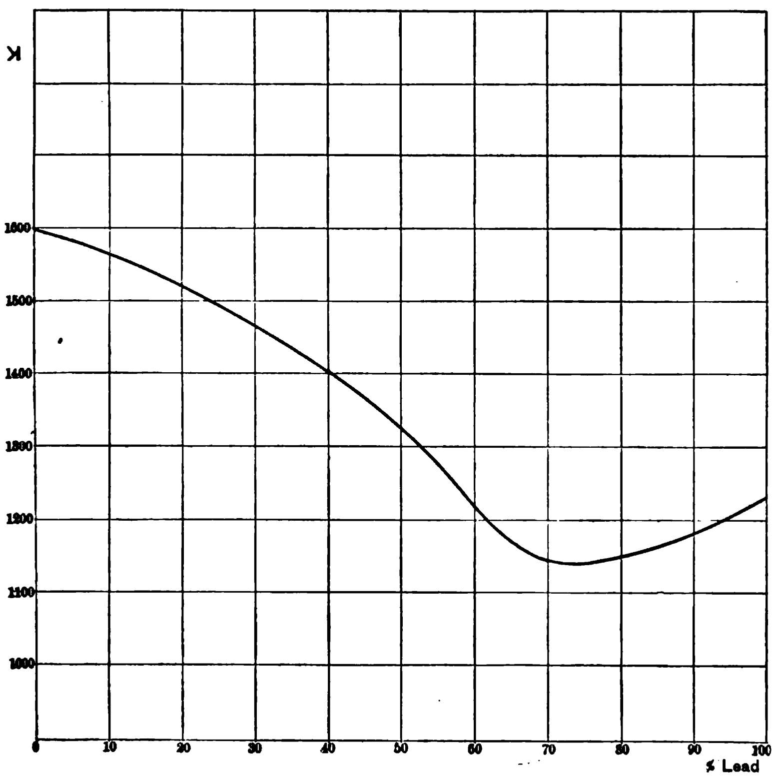


FIG. 3.

the resistance of the two metals. The specific resistance of tin is 13,360; that of lead 19,487 (C. G. S.). There is possibly also some difference in the emissivities of the two surfaces. Curve c, Fig. 1, illustrates the effect of enclosing the fusible wire in glass tubes. As would be expected, the convection is thereby checked, and the fusing current very appreciably lowered. This is a point which should not be overlooked in practice, that is to say, the nature of the fuse-block, whether closed or open, should be considered.

The constants obtained were:

Alloys.	Per Cent. Lead.	a	Preece's Constant.
Tin	0	1592	1642
Tin 2, Lead 1	33.3	1441	
Tin 1, Lead 1...........	50	1328	
Tin 1, Lead 2	66.6	1156	1318
Tin 1, Lead 4	80	1131	
Lead.....................	100	1283	1379

Plotting these constants as ordinates, with per cent. of lead as abscissæ the curve shown in Fig. 3 was obtained. From this curve the constant for any alloy of lead and tin whatsoever may

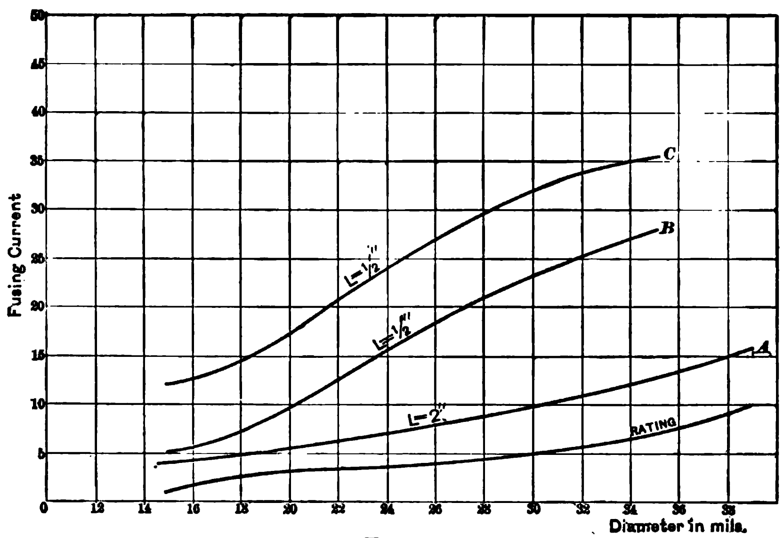


FIG. 4.

TABLE A.

DATA FOR FIG. 4.

Rating.	Diameter.	2 in. piece Current.	½ in. piec Current.	½ in piece in Fuse-Block.
1	.0149	4	5.37	12.03
3	.020	5.72	9.9	17.68
5	.0305	9.65	23.4	32.16
7	.0351	12.76	27.5	35.37
10	.0395	15.78		

be taken, and the current necessary to fuse a wire of any diameter readily calculated.

Attention is called to the fact that those values of a, which may be considered as redeterminations, are all lower than the values announced by Mr. Preece in 1888.

Comment on this fact is made in the original report in the following words:—"Had they been higher, we should have ascribed the difference to the fact of our using wires of shorter lengths. As it is, we can only say that Mr. Preece doubtless experimented with chemically pure metals, while our wire was drawn from the commercial article. He gradually increased the current until fusion occurred; we endeavored to find *what current, when it had plenty of time to bring about a permanent state*

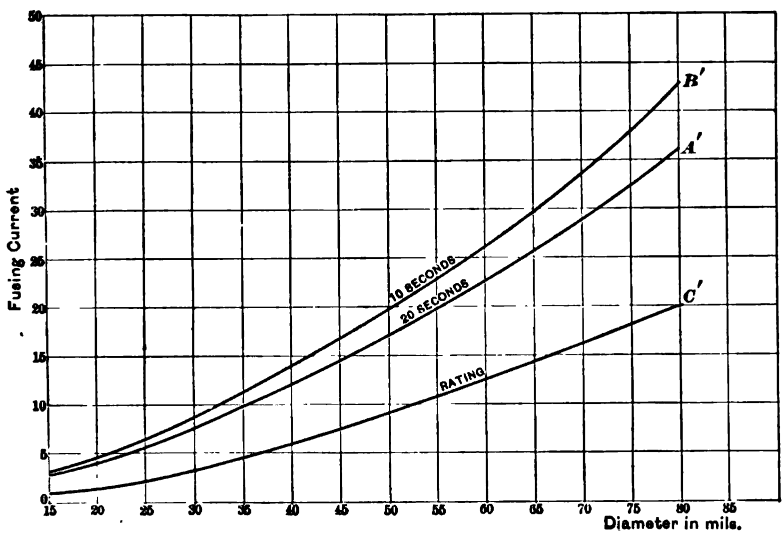


FIG. 5.

TABLE B.

DATA FOR FIG. 5.

Rating.	Diameter.	Current. 10 seconds.	Current. 20 seconds.
1	.0155	3.08	2.93
2	.0248	6.20	5.64
5	.0374	12.6	10.81
10	.0533	21.24	18.29
20	.0804	42.61	36.18

would fuse the wire. In increasing the current continuously from zero to that which melts the wire, there is danger of getting too high current readings. Mr. Preece's first determinations were entirely too high, as his later investigations show."

In order to ascertain the status of marketable fuse-wire, a number of samples were obtained from different manufacturers and tested under conditions similar to those already described. The results were represented graphically by curves, only two sets of which are here reproduced. In Fig. 4 curve A is that obtained where 2 in. specimens were tested, and, as will be seen, is not far from a $\frac{3}{2}$ power curve. When, however, $\frac{1}{2}$ in. pieces of the same wire were tested, curve B, much higher and distorted, was obtained. The marked cooling effect of the terminals is here apparent. Curve C is the most remarkable of all. One-half inch lengths of the same sizes of wire were successively fused in a porcelain ceiling cut-out block. This block was so constructed that the wire came in contact throughout its length with the cooling surface of the porcelain. The heat was conducted away so rapidly that the current reached the abnormal values shown, before rupture occurred. For example, a 7-ampere fuse did not "blow" until more than 35 amperes were flowing in the circuit. Curves A′, B′ and C′ in Fig. 5 represent the behavior of the best sample of wire tested. This wire was quite uniform in diameter, and gave evidence of careful rating. The curve of rating should be of the same general equation as that of fusion, since both represent isothermal conditions.

The requirements for an efficient safety-fuse may be summed up as follows:

1. Promptness of action.
2. Low fusing point.
3. Permanency under normal conditions.
4. Moderately low resistence.
5. Firm and lasting contact.

Promptness of action requires that the temperature of fusion should not be too far removed from that attained when the wire is being worked at its rated capacity. Moreover, it is important that the metal should not undergo chemical changes produced by the action of the heat, which may appreciably alter its melting point. The formation of oxides may or may not promote prompt fusion. In the case of pure tin and some of its alloys, a coating of oxide is often formed which retains the molten metal some time after the temperature of fusion has been passed, ultimate rupture usually occurring with considerable violence. On the contrary, the rapid oxidation of iron and copper wires seems to favor prompt fusion.

The question of low fusing point is one with which the fire underwriters are concerned, since wires fusing at high temperatures introduce an element of danger from fire. With fuses enclosed in properly constructed blocks, the danger from this source is not great, and the necessity of employing a metal of low melting point is not so important as might be imagined. Experience shows that fuses made of nearly all the metals available for such purpose, will melt without dangerous scattering of particles or liberation of hot gases, when the current is gradually increased until the break occurs. This, however, is a condition which rarely obtains in practice. Mr. Preece experimented with various metals to determine their behavior on short-circuit He made for each metal a verbal description of the character of its fusion, such as :—"Scintillating particles flew in all directions to a great distance." His results are unique and comprehensive as regards the variety of metals experimented with, but suffer from the inadequacy of the English language to express with proper shading, the nature and degree of the disturbance in each case. The author of this memoir hit upon the plan of surrounding the fuse with a paper cylinder about 3½ in. in diameter and 10 in. in length, and then exploding the fuse by an excess of current. In this way a series of autographic records were obtained, showing with considerable accuracy what happened in each case. These records are perhaps more curious than valuable, but they are undeniably interesting, and it is to be regretted that some of them can not be reproduced here. Owing to the cylindrical form of the paper, the particles of molten metal striking it, in any way but normally, traced out sinusoidal curves of continually decreasing amplitude. The vapors liberated, colored the paper quite beautifully in many cases, and usually in characteristic tints; while an idea of the temperature attained was given by the degree to which the paper was burned. In most cases when ½ in. pieces were used, an arc was established across the terminals, the intense heat of which was far more destructive than anything resulting from the temperature of the metal. The reader is referred to the original thesis[1] for these curious records, as well

1. In Cornell University Library T 1893. 216.

as for much other matter pertaining to a subject which, it is believed, is too liable to be regarded of trivial importance.

Part II.

What has been said in Part I. will doubless apply equally well to fuses on either direct or alternating current circuits, but in the latter case a new and annoying phenomenon, well worthy a special investigation, confronts the electrician. Reference is had to the subtle effect of the alternating current on fuse metals by reason of which they often yield when carrying currents well within their rated capacities. This phenomenon was mentioned in the editorial columns of the New York *Electrical Engineer* of Sept. 16, 1891. Referring to a paper previously read before the Providence convention of the Electric Light Association, the following words were used:

"Perhaps the most important (point) was the nature of the fuses employed in the alternating current system, and the difficulties which accompany their use as described by several members. It would thus seem that the action of the alternating current has a disintegrating effect upon the fuses, which causes them to 'blow' after comparatively short use, and thus necessitating a constant renewal and supervision of the circuits."

The discussion following the paper above mentioned, showed that this trouble had been experienced by many, and the hope was expressed that a careful investigation of the matter should be made. To cite a particular case:—In the lighting system of the Cornell University Library eight 50-volt circuits were supplied with fresh fuses and a record kept of their behavior for a period of three months. During this time it was found necessary to furnish six of the circuits with new fuses, one circuit requiring three such renewals. In this last case the fuse was badly corroded, owing to water dripping from the floor above. The others were in dry and airy places, the contact being bright and firm. One of the fuses had the appearance of being simply cut, the opening being so narrow as to require close examination to detect it.

In their attempts to study this peculiar effect of the alternating current, the chief difficulty encountered by the experimenters was the fact that their reports had to be submitted in a comparatively short time. The subject is peculiarly one requiring long periods in which the action, whatever its nature, may proceed far enough to yield results of definite and quantitative value.

For the purpose of systematic investigation it was assumed that the effects of the current might be one or more of the following:

(*a*) A change in the resistance of the metal. It was thought that this would likely be a rise, with a result of lowering the value of the fusing current.

(*b*) A change in the physical properties of the metal. It was thought possible that a molecular change, such as crystallization, might take place, caused by alternate contraction and expansion from the heating effects of a varying current.

(*c*) Poor contact. This might be due to the loosening of the binding screws or other holding device, because of the frequent alternations. Imperfect contact due to electrolytic action would be more likely to occur with direct, than with alternating currents.

(*d*) The effect of the current upon various metals and alloys was thought likely to be different, and consequently some alloys might be of just the right composition to overcome most of the difficulties mentioned.

The following results deal particularly with, and tend to corroborate, the first and second assumptions:

Samples of wire of various rated capacities were obtained from four different manufacturers (hereafter designated by i, ii, iii and iv), and of this wire, fourteen pieces were chosen for use in the tests. The sizes and disposition of these several pieces were as tabulated.

No.	Make.	Rated Capac.	(Cms.) Length.	Diameter.	(Sq. cms.) Area.	(Legal ohms) Resist.	(Microhms.) Specific Resist.
1	i	2	188	.0635	.00316	1.276	21.4
2	iv	2	105	.0476	.00178	.8727	14.76
3	i	3	299.5	.0797	.005	1.3271	22
4	iv	3	113.3	.053	.00213	.7188	13.5
5	iv	5	98.1	.0824	.00534	.2420	13.2
6	iii	10	196.5	.1224	.01172	.3326	19.29
7	i	18	178	.1585	.0197	.1153	12.76
8	i	36	70.4	.1585	.0197	.0229	12.76
9	ii	18	113	.1585	.0197	.0756	12.76
10	iv	10	109	.1063	.00886	.1674	13.59
11	i	18	129.2	.1585	.0197	.0872	12.76
12	ii	18	126	.1585	.0197	.0786	12.76
13	ii	25	198	.1913	.0287	.0866	12.55
14	ii	25	240	.1913	.0287	.0916	12.55

There is some difficulty in accurately measuring the diameter of a wire of soft material. The foregoing values were obtained from a large number of measurements, the variation being never greater than 2%, and this very seldom. Pieces of wire from each sample, varying in length from 2 to 10 feet, were wound into helices and mounted on wooden blocks. The ends were soldered to copper terminals of negligible resistance, the metal itself being used as a solder and resin, as a flux. The joints were allowed to stand at least twenty-four hours before any measurements of resistance were taken. For the determinations of resistance, a "fall of potential" method was chosen, as being best adapted to these small values. The standards used were a one-ohm coil standardized at the Cavendish Laboratory in 1885, and a smaller compensated resistance composed of copper and carbon, accurately tested and compared with the standard coil.

After the fuses were mounted as above described their resistances were found, and they were placed on the circuits available in the laboratory as follows:

Nos. 1 to 6 inclusive.	Alternating	$\sim$	= 250.
" 7 " 9 "	"	$\sim$	= 132.
" 10 " 14 "	Direct	——	——

These fuses were subjected to the action of the current for periods ranging from 83 to 487 hours—this variation being due to the fact that the circuits were used intermittently for lighting and for general laboratory purposes. From time to time additional resistance measurements were made. In the original report the change in resistance is exhibited graphically by means of curves. These curves are so widely different in form that their interpretation becomes a difficult matter, and the determination of any law impossible. They indicate, however, that a distinct rise in resistance occurred in all of the fuses on alternating circuits except No. 6, which shows a somewhat anomalous behavior. The maximum rise occurred in the case of No. 8, amounting to 29.6% in 105 hours. Of the fuses on direct current circuits, all but one showed a slight decrease in resistance, the maximum change being only 3.5%. The figures for each specimen appear in a succeeding table.

That some molecular change actually took place was shown in the following way:—It being practically impossible to test wires of so soft material for tensile strength, it was considered best to subject them to an alternate torsional stress of a given amount, continuing the alternations and noting the number until the specimen gave away. A simple piece of apparatus was constructed for this purpose, and it did its work very well. Twenty pieces of each of the fourteen specimens were tested in this way. Ten of these had been subjected to current action, and ten had not. The change in brittleness in the case of the fuses which had been on alternating circuits was quite marked, being in no case less than 7%; while the change in those which had been on direct circuits was, with one exception, negligible. These results together with some data on the fusing points before and after the passage of the currents are given in the subjoined table:

No.	Rated capac.	Fusing Current.			Torsion Test.			Hours Run.	Per Cent. Change Resist.
		Before.	After.	Per Cent. Change.	Before.	After.	Per Cent.		
1	2	8.83	8.56	3.06	14.4	10.9	24	487	0.2
2	2	7.96	7.86	1.26	21.6	19.9	7.9	169	0.5
3	3	12.6	12.15	3.56	14.9	13.2	11.14	487	15
4	3	8.46	8.24	2.6	28.8	22.4	21.4	169	0.4
5	5	17.96	17.46	2.55	21.7	18.5	14.8	170	2.2
6	10	27.4	25.3	8.3	13.3	11.7	12	487	2.0
7	18	52	46.4	10.8	14.8	11	26.4	209	11.3
8	36	52	45.4	11 9	15.2	13.6	10.5	105	29.6
9	18	52	49.5	4.8	16.9	13.9	17.8	83	3.5
10	10	26.8	26.8	.3	24 1	23.9	.8	170	— 2.9
11	18	52	51.5	.9	13.8	13.7	.7	200	— 3.5
12	18	52	51.4	1.1	13.8	12.3	10	296	— 0.1
13	25	65.06	65.02	.061	11.1	11.0	.9	300	— 2.3
14	25	65.06	65	.092	11.4	11.3	.8	264	— 1.5

While these results may be considered as only a beginning along a line of work which is of both theoretical interest and practical value, yet they would seem to warrant the following conclusions:

(1) There is a distinct although irregular rise in the resistance of a fuse on an alternating circuit. (2) This is perhaps to be accounted for by a change in the molecular structure of the metal. (3) This results in a decided lowering of the fusing current, in some cases at least, easily sufficient to affect the reliability of the fuse.

Discussion.

The President:—Gentlemen, you have heard the interesting paper of Mr. Matthews.

I understood you to say, Mr. Matthews, that on Figure 1, page 255, the influence of the lower current required to blow the fuse in the enclosed tube was attributed to conducting power. I should think in that case it would be rather due to the preventing of convection and radiation.

Mr. Matthews:—Yes, sir. That was what I intended to say —that the convection losses were checked.

The President:—Did you make any experiment in the alternating fuses on the influence of the corrugation or the increase of the surface? I should think that the surface action of the alternating current in a fuse wire would rather suggest the introduction of a fuse, the cross-section of which would be a corrugation, so that you would have the same weight of metal per lineal foot.

Mr. Matthews:—No experiments were made to determine that. But we noticed that in those samples which were rough, the radiation, evidently, was thereby greatly increased. Consequently they had a higher fusing current than those of the same diameter which were smooth. That is as far as we went.

The President:—I was alluding to the surface action with an alternating current.

Has any gentleman any remarks to make on this paper?

Mr. Elmer G. Willyoung:—I was very much interested by Mr. Matthews paper, as it bears on certain experiments I had occasion to make a couple of years ago upon the action of direct currents on fuses, and I think there are certain factors in the action of such currents on fuses that Mr. Matthews has not touched upon. For instance, a very important consideration which I found made a great difference in the fusing point, was whether the fuse was arranged in a horizontal position or in a vertical position. The vertical fuse, if I remember rightly, fused at a much lower fusing point than the horizontal fuse.

Another point—in the matter of length of fuse has a relation not only to the conductivity of the terminal blocks, but it is important in this respect—that when the current is passing through the fuse, and the latter gets somewhere near the fusing point, the weight of the fuse itself is an element. I have found that when you are still some distance from the fusing point, and the fuse is pretty well warmed up, that it begins to drag on the terminals and thereby lengthen out the fuse, and consequently diminish the cross-section. If the current is now reduced and then comes up at some later period, the fuse will blow at a much lower point than it would have done if it had been fused by a steadily rising current in the first place.

A good many fuses that I experimented with were obtained from those manufacturers recognized as making the best fuse wires. I found that crystallization took place in a great many of the fuses—a majority of them I think—which had the result of raising the fusing point. If the current were brought up to within 75 per cent. of the fusing point and retained there for a considerable length of time; then lowered and again raised, the fusing point had gone right up. An examination of the broken fuses showed crystallization, as I thought, in the fuses.

THE PRESIDENT:—Mr. Edison noticed that fact in condensing platinum, subjecting it to the action of prolonged heat.

MR. WILLYOUNG:—I found, too, that while the law ($C = a\, d^{\frac{3}{2}}$) placed upon the blackboard was followed in a general way, the way in which it was followed was *so general* and was *modified* by so many circumstances in practice that it really had no value whatever. It may have a theoretical value and be theoretically correct. But practically I have never been able to find that it applied, and the question of the manner in which the fuse is to be connected up in the circuit is such a tremendously important one, that I think it is just as important to investigate the question of uniformity of terminals as it is to investigate the fuses. I do not believe that we will ever be able to use fuses in any satisfactory manner until there is some uniformity in the method of connecting the fuse in with the circuit. You can buy on the market all sorts of cut-outs with all sorts of terminals and surfaces, the fuses being enclosed in all sorts of boxes of all sorts of cubic capacities, and the result in a given fuse will be vastly different in different cases, differing by thirty, forty or fifty per cent., as I have found actually to be the case. I think it is one of the things that ought to be urged on manufacturers, to endeavor to come to some uniform method in manufacturing fuses and fuse boxes, not necessarily in external appearance, but in internal dimensions. The fuses should be of a certain length, should be used in a certain way, either horizontally or vertically, and should have terminals exposed in a certain manner and of certain sizes, according to the maximum currents which are to be used. On alternating current work I have no experience at all.

The curves that Mr. Matthews shows, and of which I have been able to get a dim comprehension, are certainly very curious and very interesting. I have had a great many people ask me what I knew about the subject of fuses that were used with alternating currents, and have always been obliged to say that I did not know anything. I am very glad to see that Mr. Matthews has taken hold of this subject and is endeavoring to get some information upon it.

DR. NICHOLS:—I am rather sorry that Mr. Matthews has not attempted to incorporate in his paper, in published form, some example of the use of the method which he described, of obtaining a record by means of a paper cylinder. It was, I presume, omitted from the feeling that it is not very easy to print properly the record thus obtained. But it seems to me that that method is well worth perpetuating. It is an ingenious and very simple one and the automatic indications, although not very easy to translate in detail, yet are indications of the performance of different metals when fused, which it seems to me would be very useful to those who have to deal with fuses. The fact that they form these sine curves—that is purely incidental, and has no bearing; but the way in which the paper is treated under the action of the flying bits of molten metal in the different cases is to my mind very instructive. I think probably many members would be glad to get a nearer glimpse of some of the curves which Mr. Matthews showed us from a distance.

MR. MILTON C. CANFIELD:—I would like to ask the author a question with regard to the use of copper for fuses. It is frequently used in railroad work. I would like to know how copper compares with other fuse metals in regularity of fusing point, and whether the fusing point changes with age, and also whether it shows any different action when used with the alternating current.

MR. MATTHEWS:—My opinion is that copper furnishes one of the best materials for a fuse metal, when the fuse is entirely surrounded, so that there is no danger of fire resulting from it. I think it one of the most reliable and most accurate metals that can be used for that purpose.

I can say this in response to something that Mr. Willyoung says, that the formation of oxides in the cases of the tin, and the tin and lead alloys, may delay the fusion almost indefinitely, the current being carried to an abnormal value before the fusion occurs, fusion then occuring with considerable violence; because you have there a sac of molten metal which finally ruptures and the particles are sent to a considerable distance. That never occurs, so far as my experience goes, in the case of iron and copper. On the contrary, oxidation of copper and iron seems to favor prompt fusion rather than to delay it.

So far as the effect of alternating currents on copper is concerned, I have not experimented at all.

One word in regard to that law. I would like to ask what law you are going to adopt if you do not adhere to that which is the most probable? Certainly there is no law which expresses the state of things better than that law, and by what rule is a manufacturer going to rate his fuses if he does not adopt some such law as that? The curve of rating should be a $\frac{3}{2}$-power curve in the same sense as the curve of fusion, because it represents an isothermal condition.

MR. WILLYOUNG:—I do not mean to discount the value of that law in certain senses of the word. The law is *generally* true; I found that to be the case. But what I mean is, that the law is so modified in the fuses which are put upon the market—I am not referring to copper and iron fuses, because they are not on the market, so far as I know; they are not offered to the public by the manufacturers—but I am referring to the fuses which are actually put on the market at the present time, the most of which are probably alloys of lead and tin, and in those fuses the action of the current is so modified by the attendant circumstances, such as length and position, shape of the enclosing area, shape of conductors and so on, that I should say that the law which the manufacturers ought to follow is not the law of Preece, but the law of actual tests. A manufacturer who makes fuses does not change his manner of making those fuses every day, and consequently it is a very simple matter for him to make a determination of the quality of his fuses—to make those determinations often enough to make sure that the standard he has decided on is not being varied from, rather than to have him attempt to make fuses from the theoretical law without considering how the fuses actually behave in practice.

THE PRESIDENT:—Are there any further remarks?

If not, paper No. 2 is in order on "A Modified D'Arsonval Galvanometer," by Lieutenant Parkhurst. The Secretary informs me that Lieutenant Parkhurst is not here. The Secretary will therefore read the paper.

The Secretary read the following paper:

A paper presented at the Tenth General Meeting of the American Institute of Electrical Engineers, New York, May 17, 1893, President Houston in the Chair.

A MODIFIED DEPREZ-D'ARSONVAL GALVANOMETER.

BY LIEUT. CHARLES D. PARKHURST.

So far as the writer has observed in all forms of this galvanometer in actual use, or as illustrated and described in various articles descriptive of its construction and use, and in catalogues advertising it for sale, it has always been arranged for use with a lamp stand and scale for reading the deflections by the varying position of the spot of light and image of cross-hair reflected from a concave mirror; the protective screen to cut off air currents has always been of glass, and even though a plain mirror be substituted for the concave, there is nothing to put the mirror in shadow; hence, as the writer knows from experience, no satisfactory results could be obtained with this form used in connection with a reading telescope.

Opinions may differ as to the relative merits of the two systems of reading reflecting galvanometer deflections. Certain it is, however, that the lamp stand and scale, form a clumsy and bulky outfit. To get a good, sharp and clear image of the spot of light and its cross-hair needs a very strong light or a dark closet in which to use the galvanometer. Independent of its bulkiness and want of portability, it is a hot and inconvenient method of reading, and one that, in the writer's opinion, bears no comparison with the ease and refinements of reading, obtainable with a reading telescope worthy of the name.

Since to show up well in a reading telescope the mirror must be in a heavy shadow, and the scale upon the telescope be well illuminated, the writer found that a modification of the ordinary D'Arsonval galvanometer in his possession was necessary, and this modification will now be described, reference being made to the drawings herewith drawn to scale one-quarter size.

The horse-shoe or rather U-shaped magnet was dropped down completely through the base, so as to project below its lower surface sufficiently to allow the lower tension spring to be mounted on the *under side* of the base, where it could be reached at any time to adjust the tension of the suspension. This became necessary owing to the fixed brass case which enclosed the instrument, as shown in the illustration, this fixed brass case being the necessary opaque case to shut off all light except from the front, and at the same time protect the interior of the instrument from all air currents. The leveling screws of the base were made long enough to allow the tension spring screws to be conveniently

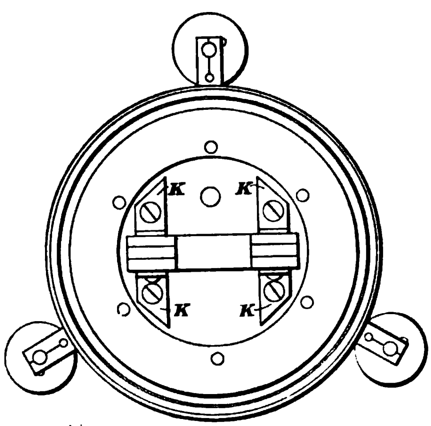


FIG. 1.

reached without disturbing the galvanometer when once set up and leveled. All adjustments necessary at any time could therefore be made without any undue disturbance of the galvanometer on its stand. This tension spring is shown at S, Figs. 2 and 3; the check nut and tension screw at C and T, same figures.

Although the construction of the ordinary D'Arsonval galvanometer is probably known to all, the following complete description is given of the modified construction for the benefit of any who may not be familiar with the instrument in any form, and also to enable the modification to be fully understood and be copied by any one desirous of so doing. Fig. 1 shows the plan of the base with the magnet in position; double brass knees are used to ensure the vertical and rigid position of the magnet.

(See K K, Figs. 1, 2 and 3.) In Figs. 2 and 3 it will be seen how the magnet is dropped down through the base, giving room between the bottom of the base and the bend of the magnet for the point of the tension spring to extend into the centre line, so as to receive the lower suspension wire. The spring itself is a flat strip of spring sheet brass about $\frac{1}{16}$ inch thick, $\frac{1}{2}$ inch wide at the securing end, and $\frac{1}{8}$ inch wide at the free end. It is mounted upon a block of brass $\frac{1}{2} \times \frac{3}{4} \times \frac{1}{8}$ at the securing end, and here rigidly secured to the base; this gives plenty of free play

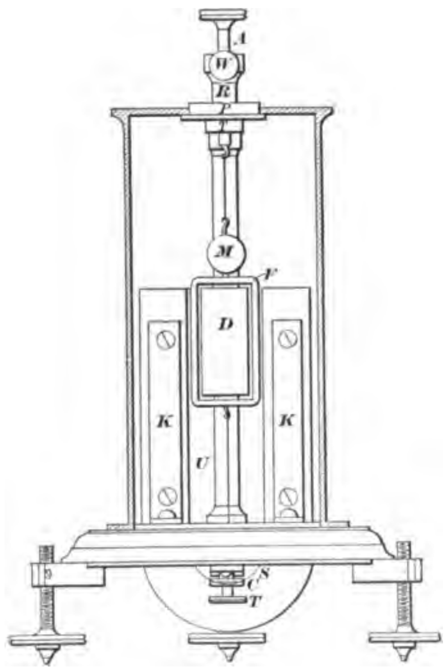


FIG. 2.

for the spring at its free end and it carries a small silver hook securely soldered into the spring.

The base itself is turned up from solid ebonite, with a raised central part that fits the bottom of the brass case snugly, thereby centering the latter upon the base; the brass case is of 4 inch brass tubing, $\frac{1}{16}$ inch thick, provided with the window as shown. The window is made large to allow the mirror and its reflected scale to be seen through a wide angle.

The frame of the instrument to provide suspensions for the coil is entirely independent of the case. It consists of the upright brass rod U and the upper bar T. The upright is securely fastened to the base in rear of the centre of the magnet as shown, and at right angles to the base.

The bar T projects over the top of the magnet, as shown, and carries upon it, as an integral part, the circular flanged plate P. This plate fits into a corresponding central hole in the top of the brass case, so as to close this latter hole snugly; this hole is necessary so that the case can be removed at any time without disturbing the suspension, or can be put on after any adjustment or

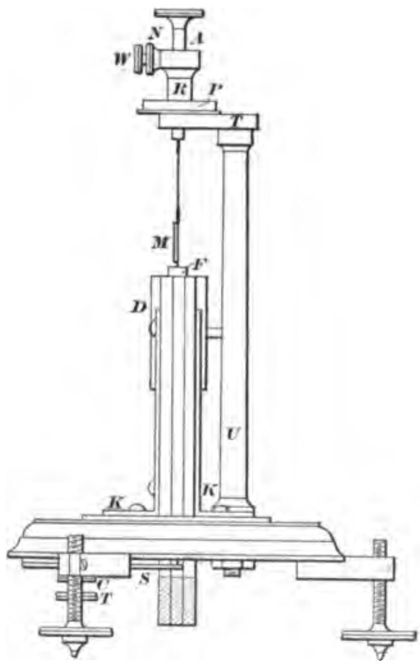


FIG. 3.

repairs that require the removal of the case, and yet leave the suspension untouched.

In the centre of the plate P, and so as to be *exactly* over the centre of the magnet, the tube R is securely fastened; this tube carries and centres the sliding rod A; this latter rod carries at its lower end the silver hook as shown, and is clamped in any position desired, by the screw W and check nut N.

The coil frame F (for details see Figs. 4, 5 and 6) is made from thin sheet copper, worked over a form into a light rectangular frame with flanged edges to form a groove into which the wire is wound. The thinnest kind of thin copper is used. It forms a thin, light and yet rigid frame, the flanged sides greatly stiffening it. Such a frame may be made from silver, or from aluminium, with probably as good results. This frame is wound full of No. 40 double silk covered copper magnet wire, and has, in this in-

FIG. 4.

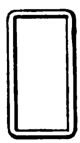


FIG. 5.

FIG. 6.

FIG. 7.

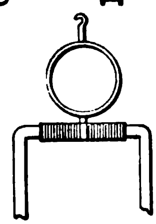


FIG. 8.

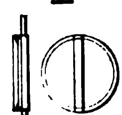
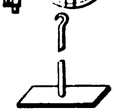


FIG. 9.



stance, a resistance of 275 ohms. Other windings can, of course, be used, depending upon the use for which the instrument is intended.

The supports for this coil frame are two small silver hooks, the upper one long enough to form a support for the mirror M. To the end of each of these hooks a light plate of copper is fastened (see Fig. 9), and by these plates the hooks are secured to the frame, as shown in Fig. 7, by a winding of waxed silk; a thin plate of mica insulates the plates from the windings of the coil.

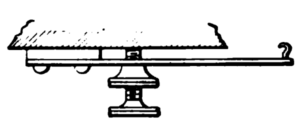


FIG. 10. (ONE-HALF ACTUAL SIZE.)

After the hooks are accurately adjusted so as to coincide with the true axis of the frame, and firmly secured, the ends of the coil wire are made fast, one end to each hook, so that the hooks form the true terminals of the coil.

The mirror is supported in a very light ring of brass, as shown in Figs. 7 and 8. This ring has a shoulder within it that holds the mirror from falling out at the front, while a piece of very light thin tubing is soldered carefully across the back, in fine notches filed out of the ring to receive it, and holds the mirror securely. The wire of the upper hook fits this tubing friction tight; before bending the wire to the hook form the mounted mirror is slipped upon it by its tube, and the hook is then formed. The friction of the tube on the wire holds the mirror frame se-

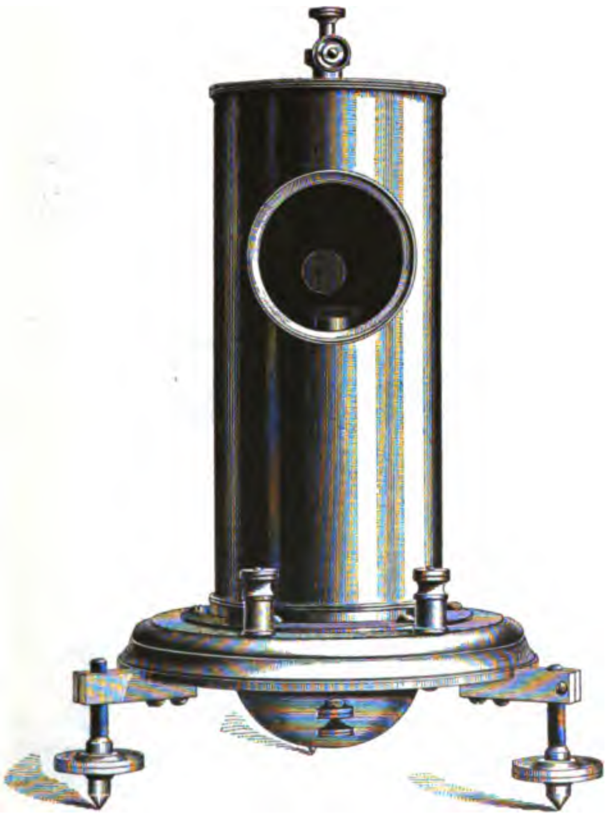


FIG. 11.

curely and yet allows it to be adjusted, if necessary, to any required angle, with or in the true plane of the coil frame.

This mounting was adopted after many others had been tested and rejected. Cementing the mirror upon the wire is a clumsy method that may result in the ruin of the mirror by pulling off the amalgam or by distortion due to the strains set up by the drying cement. These mirrors are so thin and flexible, and distortion is so easily done, that care must be exercised in keeping them free from strain, so as to preserve a plane mirror and a true image. This mounting is light and yet rigid, and holds the mirror perfectly true, with no distortion; the bright brass is made black with platinum chloride, so as to prevent any false image or glitter from the frame. This mounting also brings the mirror close in against the suspension, so that its deflection affords a very close approximation to the true deflection of the coil. By strict

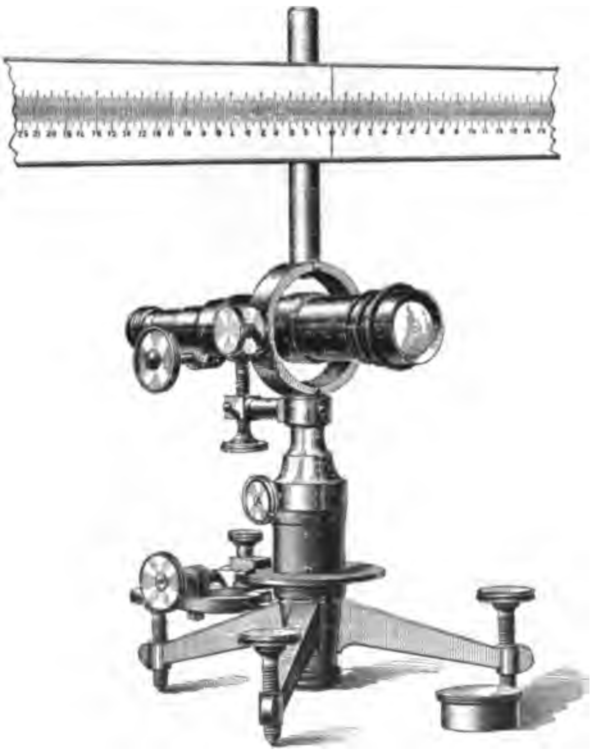


FIG. 12.

principles the axis of the suspension should pass through the mirror, and a suspension frame for the mirror can be, and has been made to secure this. But in this particular case, since the current has to flow through the suspension, the simpler and more direct the flow the better, and the error introduced is so insignificant that it can be neglected.

The cylinder D is of soft iron, and the softer and purer it is the better. It is permanently secured to the upright U by the small brass arm shown in Figure 3. It should be *solid* and *not a tube*, as in a good many instruments, as it thus has much greater permeance, more lines of force are drawn in, and the field of force between it and the limbs of the magnet becomes greatly strengthened. Since it is in this field of force that the coil is to turn, it naturally follows that it should be made as intense as possible.

As shown in Fig. 2, the coil frame is suspended between the top hook at T and the bottom hook upon the tension spring S. It should swing freely in the space between the magnet poles and the cylinder D, without rubbing or striking. The leveling screws are provided to permit of proper adjustment to secure this free swing, as the suspension wire is so light that it has not enough rigidity to hold the frame true if the instrument is out of level, and the frame will then sag to one side and strike or rub against the cylinder or magnet. If properly leveled the frame has a clear, free swing. When deflected far enough it will, of course, bring up on the support of the cylinder D; but this cannot be avoided, and such a deflection is away beyond the usual and *proper* use of the instrument.

The interior of the case is made a *dead* black, the same as the interior of telescope tubes; no light is reflected except by the mirror, and a clear, sharp, well-defined image of the scale is the consequent result.

The wire used for the suspension is of the utmost importance; upon this depends in a great measure the "dead beat" qualities of the instrument. For such a coil as that described, wire as fine as .003 in. was used, and a true "dead beat" galvanometer was the result. This wire is strong enough to hold the mirror and frame under proper tension, so that the torsion of the wire will bring and hold the mirror at zero, and yet it is so light that the feeblest kind of a current will produce an appreciable deflection. For coils of other windings, heavier wire may possibly be used, but as the writer wanted a very *delicate* and yet "dead beat" instrument, it was made as described, and after several sizes of wire for the suspension, down as fine even as No. 32, were tried and did not give good "dead beat" results, the finest wire ordinarily obtainable, *i. e.* .003 in. diameter was tried and all the troubles vanished. Now the mirror swings smoothly and truly up to its deflection and stops without a quiver, and, when a short-circuiting key is used to break the current from the b-at tery or other source, it swings back to zero and stops there dead, without quiver or oscillation.

The circuit through the instrument is from one binding post through a heavy wire to the foot of the upright U; thence through this to the upper hook and suspension wire to the coil, through the coil to the lower suspension wire to hook and tension spring, and thence by another wire to the other binding post. The foot of the upright and the tension spring, are therefore the true terminals of the instrument, and should be insulated from each other perfectly. The binding posts can be anywhere convenient, so long as they are also insulated from each other.

It should be plain that in this instrument we have the ordinary galvanometer reversed. The needle is replaced by the powerful fixed magnet, while the coil is movable. Since the field of force of the magnet is so strong as to be practically independent of the earth's field of force, we can use the galvanometer in any position independent of the magnetic meridian. *For its most delicate use*, however, it should be set up so that the plane of the magnet lies in the magnetic meridian with the poles in proper relation to the north and south; the earth's field of force then reinforces that of the magnet, and adds to the power of the instrument.

Since this is an instrument by itself, none of the known laws will probably apply except the general law that for *small* deflections the current strengths may be taken to be proportional to the angles of deflection. We can determine its "constant" or deflecting current necessary to produce a deflection of one scale division, and then use this constant ever afterwards (for the *same distance of the scale from the mirror*) to determine the strengths of other currents producing other deflections.

For laboratory work the galvanometer and telescope would probably be permanently set up, with some convenient fixed distance between them; the constant once determined becomes then the true constant forever afterwards.

When used as a portable instrument, however, the distance from mirror to scale may not always be kept the same. In this case the constant must be determined for each position of the telescope and scale; but this is very simply done, and need cause no great delay or trouble.

As a specimen of the delicacy of the instrument *as used with a reading telescope*, the writer has frequently seen a deflection, when balancing resistances by the Wheatstone bridge method, *that did not uncover the cross-hair of the eye-piece completely from the zero line of the scale.* The cross-hair is a fine spider web, so fine that it can hardly be seen with the naked eye, and the lines upon the scale are as fine as they can be printed from an engraved plate, yet they did not completely separate from true coincidence; the slight movement of the scale was perceptible but of course could not be measured. This movement yet indicated that a true balance had not yet been obtained, for finally it would disappear and the mirror hang dead still upon further adjustment of the resistances. This shows how sensitive the instrument is, and *how closely we can see the slightest suspicion of deflection* when using a reading telescope. Resistances adjusted down to absolute zero of the mirror's movement may be taken as pretty closely measured, much closer than is possible with any ordinary galvanometer, and, as the writer believes, much closer than can possibly be done with lamp stand and scale, even with the best of dark closets.

The one great objection to this galvanometer is its want of ready portability; admirably adapted as it is for all kinds of measurements with the utmost delicacy, even in the dynamo room, in proximity to dynamos in action, it yet is not readily transported and set up, owing to the delicacy of the suspension; it will stand a considerable handling without injury, but yet there is much to be desired to render it absolutely portable and free from any liability to injury in transportation, and quickly available, without any tedious adjustment being necessary, when set up wherever required.

The Weston voltmeter and ammeter is such an adaptation, these instruments being the simple D'Arsonval galvanometer in another form, and specially constructed and graded for special work; though they are most admirable and accurate, and can be used for all kinds of measurements, they yet leave something to be desired, in that they read by index and not by mirror, and hence extreme refinement of reading becomes impossible. With a view to meeting this requirement the following described instrument has been devised and is now in process of construction: (See Figs. 13 to 17, which are drawn to scale half size.)

The magnet is made circular and compound with soft iron pole-pieces, similar to the arrangement in the Pitkin form of D'Arsonval instrument; but in place of the wire suspension of that instrument the steel pinion and pivoted centres of the Weston form, with the double hair springs, is adopted.

The coil may be the flat coil of the Pitkin instrument, or it may be the rectangular coil as generally used. If the flat coil is used, the mirror can be attached to the coil; with the rectangular coil the mirror frame is set within the rectangle as shown; in both cases, of course, the mirror turns with the coil. A window, as shown, is provided in the case, to allow the movements of the mirror to be observed. This window can have a brass cap or cover to slip on, to protect the glass in transportation.

The bottom and top of the case are of hard rubber, the side of the case is thin brass tubing; in the case, in the space within the bend of the magnet, a set of shunt coils may be inserted, with terminal blocks on the top of the case; the binding posts are upon the top, and a short-circuit may be provided as shown.

Where the rectangular coil is used, an iron core can be still put in inside the frame of the coil, being cut away as shown to allow the mirror to have room to swing. This really makes this iron core in two pieces, yet it contains a large mass of iron, and helps to strengthen the field of force in which the coil swings; the field of force is kept symmetrical by the shape given to the pole-pieces, so that the lines of force are evenly distributed from front to rear, and the coil remains in practically a constant field during its entire swing. The cutting away of the poles each side of the mirror is to allow the scale to be still reflected from the mirror after large deflection.

We then have a neat, compact and readily portable instrument, whose readings should be as fine and delicate as one may desire. Since the beam of light weighs nothing, and can be made almost as long as one chooses, within reasonable limits, it should be evident how greatly the scale is magnified, and how fine and close the readings may be. The writer may, perhaps, be pardoned if he illustrates this point a little more fully.

Galvanometers with indices must have a very finely graduated scale to read closer than one-fourth of a degree; the average will not read closer than one-half degree; fine readings are, therefore, impossible as a *certainty*, particularly when the tremble and quiver of a finely poised needle and index are considered. The Weston instruments are of this class, and, although they can be closely read *by estimation*, we are *not certain* of the reading, though perhaps we can read close enough for the general work for which they are used.

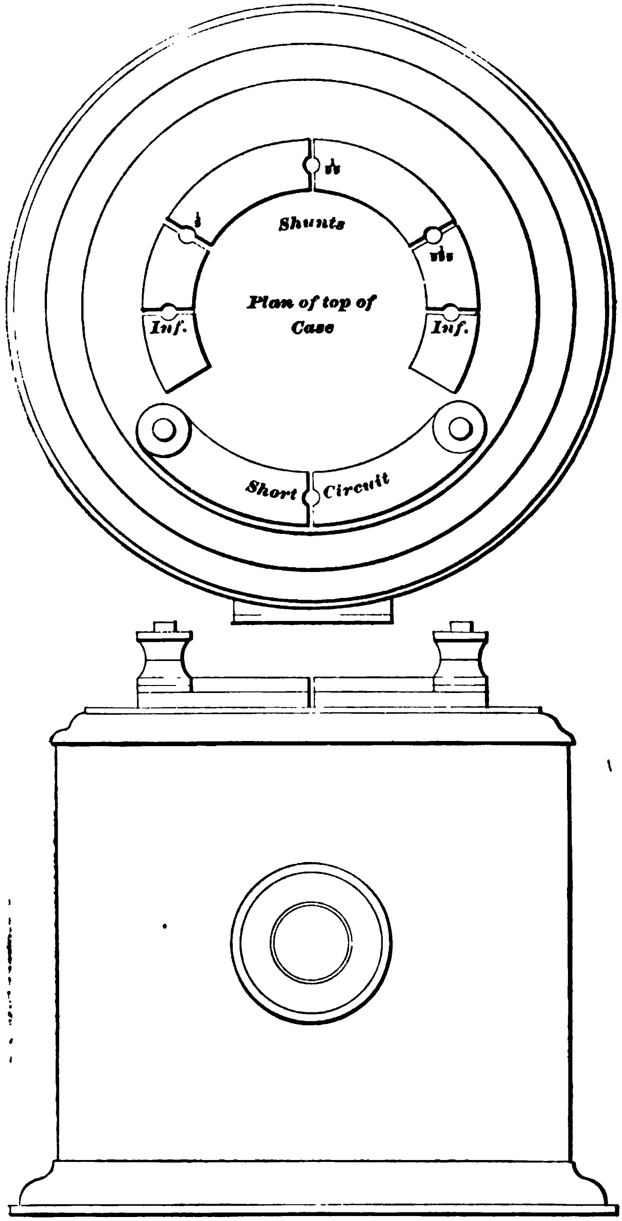
With a mirror, scale and fine reading telescope we have an index that is imponderable, and about as long as we choose within reasonable limits. The scale being graduated to millimetres is easily read at a distance of 3,000 scale divisions, a little over ten feet; the writer has frequently used a telescope at a distance of twelve feet, the distance being only limited by the size of the room, and not by any difficulties in reading the scale.

At this distance, then, a deflection of one millimetre, easily accurately read, means a tangent of $\frac{1}{3000}$ or .000333, corresponding to an angle of about *one minute of arc*, and, as this is the tangent of twice the actual angle of displacement of the mirror, we see that the mirror has actually moved about one-half minute only.

We can read this small angle *positively;* by estimation we can read one-half of a scale division, or our scale can be graduated to half millimetres, and we can read that deflection positively; hence, we see that we can read down much finer, and with more positive exactness, than can be done by any other method, a movement of the mirror being visible in the telescope that could hardly be detected with the unaided eye, and that would be imperceptible if an index had been used.

In conclusion, the writer would say that he hopes the modifications of the instrument as described may be found of value and use. So far as he knows they are original, and are not protected by any patents; any one should be, therefore, free to construct and use such instruments, and if any one so does, and gets as much satisfaction from their use as has been his good fortune, the writer will be fully satisfied.

While upon the subject of measuring instruments, though not exactly pertinent to the instrument described, the writer would beg leave to invite attention to some action being necessary to try and induce instrument makers to be more complete in the data that is furnished with their instruments, which data can only be accurately determined by the maker during process of manufacture, and which, if not then made of permanent record, forever after can only be arrived at approximately by experiment. Such data, carefully and permanently recorded, would oftentimes enhance the value of the instrument more than the extra cost from the extra trouble could possibly amount to.



Side Elevation

FIG. 13.

Reference is here made to the various makers of tangent galvanometers as requiring such attention to the data of their construction, etc., as is shown by the following:—

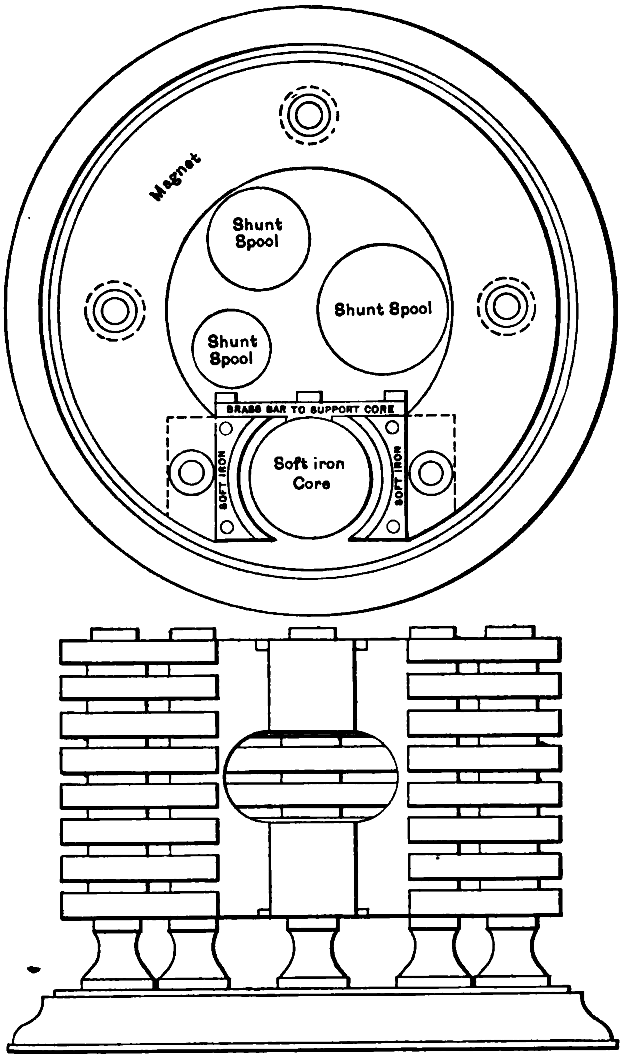
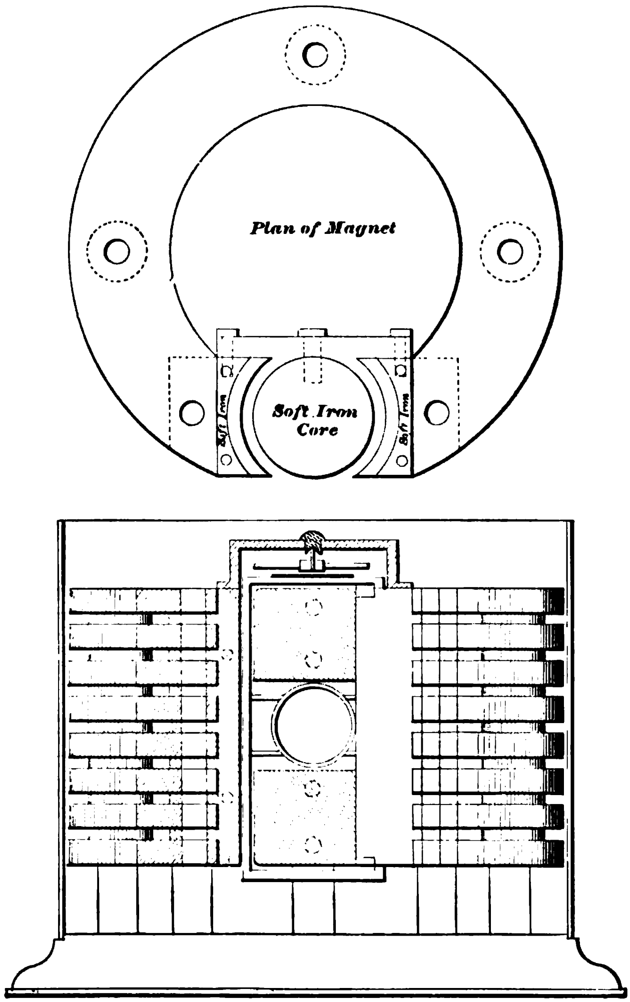


FIG. 14.

The writer has seen more than one fine standard tangent galvanometer of the Western Union pattern about which there was no information or data to be had as to the *number of turns of wire* *in each coil*, or their mean radii. Similarly, Helmholtz-Gaugain galvanometers have been seen and used by him that were lacking in this same important data. These instruments were all high



Elevation and Section, showing one cap in place, and poles not cut away

FIG. 15.

grade, costing from $125 to $175 each, and yet, for want of data that could have been recorded permanently by the maker at but slight additional expense, that could only be used *positively* and *absolutely* for the *relative* strengths of currents, except by calibrating them absolutely by reference to some other instrument that had an absolute calibration.

The writer well remembers a long, tedious and delicate series of experiments he was called upon to perform, to try and work backwards, and, from the absolute strengths as shown by a cop-

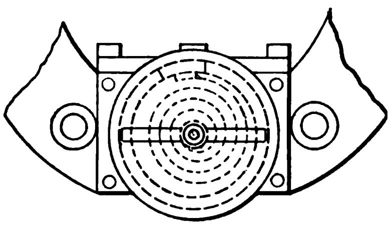


FIG. 16. PLAN OF CAP CARRYING JEWELED CENTRE.

per voltameter, to determine the turns of wire upon a coil, so that the instrument could afterwards be used in accordance with the formulæ for its use in measuring the absolute strength of currents. Necessarily the experiments were unsatisfactory, and, after its determination, the number of turns as deduced was unreliable.

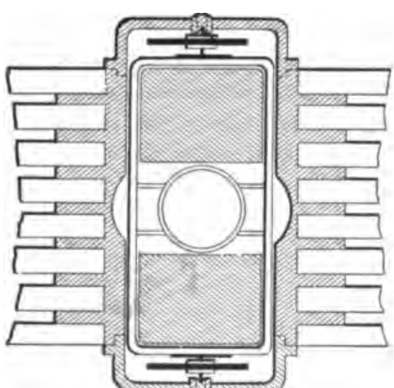


FIG. 17. SECTION AND ELEVATION SHOWING COIL FRAME, MIRROR AND CAP.

The formulæ for the use of tangent galvanometers in this manner contain the following quantities as necessary to know:
n = number of turns of wire in coil or coils in use.
r = means radius in *centimetres* of the coil or coils in use.
d = distance of centre of coil from centre of needle in *centime-*

tres (for use in sliding coil tangent instruments, such as the Edelman and others).

H = horizontal component of the earth's magnetism at the place of observation (to be determined by experiment and does not necessarily become data for any particular instrument except for a fixed locality).

It will be seen that *the resistance of the coils or of the* instrument is *not* required. Yet we find resistance carefully measured and recorded and much more important data completely omitted.

On foreign, and particularly German instruments, we generally find all this data given, and centimetre scales properly arranged to measure D when the coil is moved away from the needle to any position. But I have yet to see any uniformity in this respect with American made instruments, hence my comments as above.

We all know that *if* we know certain quantities, very hard to know or to keep constant, we can find the strength of current giving any deflection by the application of Ohm's law. But, though we may know the exact *external resistance* in circuit by careful measurements, do we know for a dead certainty what the battery or other generator's internal resistance may be from time to time as we use it? Can we be *positively sure* of the E. M. F. of our source of current? Until we can be *certain* of these two changeable and ever changing quantities we cannot be certain of the current strengths as given by Ohm's law; hence, the great desirability for use of the general formulæ, into which E. M. F. and battery or other resistance does not enter, and the great need for the furnishing of the proper data by instrument makers to enable us to use these formulæ.

DISCUSSION.

MR. WILLYOUNG:—The author has endeavored to cover a great many points in his paper. The first point he takes up seems to be with respect to the merits of the two systems of reading reflecting galvanometer deflections. As he says, that is a matter of opinion. It depends on the particular case. For many cases I think the lamp stand and scale to be vastly superior to the telescopic method. In cable testing work, a man wants to manipulate his instrument. If he is located behind the telescope it is not easy to do it. Not only that, but if a person is going to make a large number of observations, as happens to be a necessity in many commercial enterprises, the continued daily use of the telescope becomes exceedingly hard on the eye. I have a number of men who are doing that right along, and almost without exception they have to wear glasses, and their right eye has become very different from their left eye. Of course, the question of the sensibility that is required has something to do with it. You can certainly read finer with the telescope and scale, than with the lantern and scale. But there are very few purposes for which galvanometers are used for which that exceeding fineness is required.

In one place here on page 279 Lieutenant Parkhurst says that he has frequently seen a deflection, when balancing resistances by the Wheatstone bridge method that did not uncover the cross-hair of the eye-piece completely from the zero line of the scale, and then he goes on to intimate that resistances can be more closely measured on account of that fact. That is a great mistake, as instruments are made, because there are other conditions which would prevent the adjustment of any resistance with any respectable galvanometer to the closeness indicated by a deflection of that size. A galvanometer which has a constant of 500 megohms, that is one which with one volt upon a scale at a metre's distance will give a deflection of one millimetre through 500 megohms, is not a galvanometer of *very* high sensibility. It is very easily obtained. If you are comparing coils of 1,000 ohms each with such a galvanometer you will get a swing of 20 to 25 centimetres for a variation of but $\frac{1}{10}$ of one per cent. You do not need, therefore, any such fine deflections as Lieut. Parkhurst speaks of.

This description of the D'Arsonval galvanometer starts off with the insinuation that manufacturers do not understand what a good D'Arsonval galvanometer is. The glass shade spoken of is still clung to by manufacturers for the simple reason that the galvanometer is a historical article, and there are a great many people who want exactly that instrument. That form of galvanometer is very much inferior to other forms of D'Arsonval galvanometers which are not worth half so much money. But, as I say, a great many manufacturers cling to it because of its historical value, and possibly because they have not posted themselves up as to what has been done since. I have an illustration here of a galvanometer made in that way [exhibiting a tube shown in Fig. 1]. You see that the mirror is as shaded as any one desires. The mirror lies right behind this hole.

I do not see that the author claims to have any particularly new modification of the D'Arsonval galvanometer. What he is endeavoring to do is to obtain a very sensitive galvanometer and

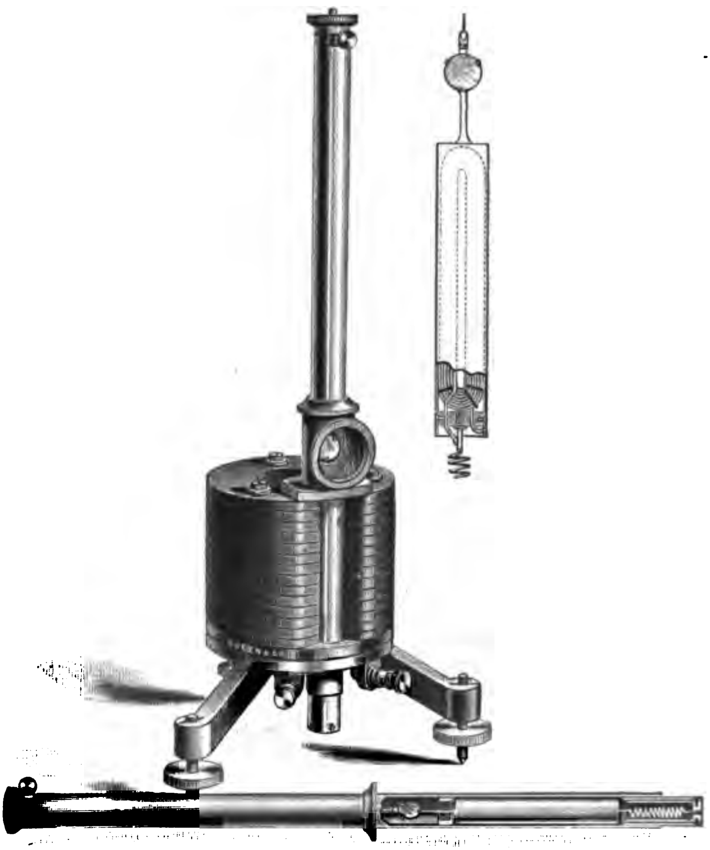


FIG. 1.

a very portable one, and he does it by using spiral springs. You cannot begin to get as sensitive a galvanometer by the use of a spiral spring as with a suspension. This galvanometer will not begin to give such good results as the galvanometer which the author has first described. A galvanometer suspended in pivots has a friction error that cannot be gotten rid of perfectly, and that would prevent it from being a *very* sensitive instrument, and in the second place the torsion of those spiral springs is very much greater than a suspension of the orthodox character. His coil is wound on a copper form. This copper form was patented by Mr. Weston in 1888 and is owned, I believe, by the Weston Electrical Instrument Co.—was originally used by Mr. Weston, I believe, and I have no doubt could be used for experimental purposes, but certainly is not new with Lieut. Parkhurst. In fact, this identical galvanometer, very similar to its present shape with the exception of the shunt coils not being inside, was made in 1884 by Carpentier, and is illustrated in an issue of *La Lumiere Electrique* for that year. I do not suppose Lieut. Parkhurst has ever seen it. Examples of independent invention are very common. I might say about this portable form that there is another defect about it, if he is endeavoring to obtain high sensibility, and that is his magnets are separated by spaces equal to the thickness of the magnets themselves. Of course, he gets half the strength of the magnetic field that he would get if the magnets were solid.

I have a print of a galvanometer here—the galvanometer with which this tube goes and I will just pass it around for inspection. (All these details can be studied in Fig. 1.) You will see the magnets are laminated, but they are laid right on top of one another. These magnets are punched from the best English steel, punched with a die. They are laid one on top of the other. We get the maximum strength of field. The shape of the coil also in that galvanometer has been shown by Ayrton and Mather and several others[1], to be a shape that is not adapted to the highest sensibility. The coil as used in that illustration is rectangular in form. Now this wire lying horizontally, top and bottom, is dead wire, of absolutely no use in producing deflection. The moment of inertia of that coil is greatly increased by making it in that shape. It has been completely shown mathematically (in the article just quoted) and practically, that the proper shape of the coil is more like this—a very narrow coil the cross-section of which should be two circles and I verified that practically.

Now with reference to making a very sensitive D'Arsonval galvanometer portable, the usual difficulty of course has been—and in the instrument first described that difficulty exists—that with the coil hanging by a suspension it is utterly impossible to hold the coil; it will break away and break the suspension. The galvanometer illustrated by the blue-print (shown in Fig. 1) is a perfectly portable instrument and yet it has maximum sensibility. A tube contains the suspension and the coil. The coil hangs in the lower portion of the tube, and there is a suspension from the top of the tube to the coil, and a very light elongated spiral spring at the bottom. Inside of the tube, above and below the coil are a couple of flanges (seen in the tube at the bottom of Fig. 1.)

1. *London Electrician*, April 11th, 1890.

At the bottom is another tube inside this outer tube held there by friction; the lower flange is on the inside tube. When you want to transport the instrument or change its position all that is necessary to do is simply to slide this inside tube up and the coil is raised, the tension is taken off the upper suspension and the coil is securely clamped for transportation. I might say that this idea of clamping the coil is not strictly original with me. It was done by Professor Ayrton in a slightly different manner. In Professor Ayrton's instrument there was a hole in the side of the tube and a plug went into the hole; the coil was not raised, however, but was simply *pushed* to one side leaving the tension still upon the suspension which was thus liable to be broken.

As regards suspensions, we have used as fine as one mil wire rolled down into strips, the width of the strip being five or six times its thickness making an exceedingly delicate instrument, which makes it very easy on the 500-ohm instrument to get a constant of 1,000 megohms.

The question of damping with this instrument, is a question that can be regulated to any extent you like, to satisfy yourself. The damping is produced by means of this tube, which is of aluminium, very light, in which the coil itself is contained. The whole tube is rigidly fixed to the coil, and any desired damping can be produced by merely cutting a slot in the side of the tube to prevent the induced currents; this one has been slotted as you see and the desired damping has been produced.

There is one very important point in connection with galvanometers of this type which I have not seen touched upon anywhere. The author says that the constant of such a galvanometer once set up is invariable. Of course everybody knows that that means—*if the magnet does not change.* The magnet *will* change. But that is not the point I was going to bring out. The *tension* of the suspension is a very important factor in determining the sensibility of the D'Arsonval galvanometer. You can vary the sensibility by 40% or 50% by merely changing the tension with which that coil is suspended in the magnetic field, and consequently the constant is liable to daily changes produced by the difference in the coefficient of expansion between the supports for the coil, and the suspension itself. There, by the way, is the form on which the coils are wound for ballistic purposes where no damping is desired. [Showing a boxwood cylinder grooved upon the sides to receive the wire.]

Just one further point with respect to the author's remarks about manufacturers not furnishing sufficient data with instruments. That is a very important thing, and as a representative of a manufacturer I cannot allow that to go unnoticed. I have probably seen as many imported instruments, if not more, than any other man in the United States. A large percentage of all the instruments imported into this country I have seen, and I cannot remember more than three or four cases in which a foreign instrument was accompanied by data. You usually have to write and get it if you want it. Often when you do get it, it is not the correct data. I remember a tangent galvanometer imported from Germany when I was at college. We had the data given, but it was utterly impossible to verify it. The only conclusion we could form was, that the data were erroneous by 20 or 25 per cent. However, instruments of precision in this country, so far as I know, are accompanied by all the necessary data. I should hardly call the Western Union type of galvanometer an instrument of precision. It never was intended for an instrument of precision, but was merely intended for testing in telegraph work and work of a similar character, and nobody expects to get an instrument of precision from it. It would be impossible to construct an instrument of precision at that price. It requires special skill, and men to do that character of work, and they are expensive. But *real* instruments of precision are accompanied by all the necessary data. In the manufacture of tangent galvanometers, with which I have had to do, there are a certain number of turns on each coil. The form is very carefully measured in the first place by a steel tape which is very accurately divided and its errors known. The thickness of the tape is allowed for, and from the circumference is figured out the radius. Then the radius of each layer is figured in the same way, and from all the radii the mean radius is obtained. In the making of resistance coils, standard instruments of precision of all kinds, all the data are furnished, so far as I know. I have here a number of certificates that I will just pass around which illustrates what I am referring to. [Passing certificates around.] For instance here is a certificate that accompanies a high-grade resistance-box. This certified that the box marked as stated has been adjusted with a certain percentage error at a certain temperature centigrade. Wire used, so and so, having a coefficient so and so, between so and so, and so and so degrees centigrade. That a greater potential than so and so must not be used. It says the connecter running from the bridge to the rheostat plus the plug contacts across the rheostat measures so and so. A good deal *more* is given than is *necessary*. I think it is only justice to American manufacturers in general to say this. As far as I know they are all striving in every possible manner to make instruments in the best possible way and to furnish all the data that are necessary in connection with instruments and I think it is pretty thoroughly done.

MR. EDWARD WESTON:—The attention that has been called within the last few years to this type of galvanometer, commonly known as the D'Arsonval galvanometer, has been quite marked. I think it has been the subject of six or eight papers during the last few years. It is somewhat remarkable that this type of galvanometer received very little attention from its first description by Maxwell in the first edition of his work, until comparatively within the last few years. It seems to me that Maxwell described it very fully and in considerable detail, and Deprez and D'Arsonval took up the matter and seem to have been the first ones to construct an instrument embodying the moving coil and adapted for use as a galvanometer. That was some time about the year 1881, I believe; at least that is the date of the French patent. Later on they made some attempts to make a more generally useful instrument out of the galvanometer, and I think that is clearly shown in two French patents which were granted to them, one in 1881 and one in 1884. In the 1881 instrument of the patent, they had the form of field described by Lieutenant Parkhurst, namely, a series of permanent magnets fastened together in a vertical position with a moving coil between them, just as shown here, but with a soft iron tube in the centre instead of a solid core. Such a type of instrument is of very little value, excepting for use with a mirror and scale, or as a simple indicator of the difference of potential. As an absolute measuring instrument it is of very little value indeed, unless it is calibrated throughout a very large length of movement of the coil. Of course if the coil moves a certain distance, it changes its position in the field and the strength of the field naturally varies materially, so that such an instrument as that would have to be calibrated for a great many different positions. Later on D'Arsonval took out another French patent in which he placed pole-pieces, making a practically uniform field. That was, I think, in December, 1884; still the tubular form of core, and now the coil, in the same position in relation to the field. That of course is a very great improvement. I think Mr. D'Arsonval was quite some time behind me on that work. I have never been in the habit of saying very much about my work; I go on with it quietly; but I can say that long before the date of the patent, I had such an instrument in use and knew of its advantages and disadvantages. Later on the form of instrument which is commonly known as the Weston voltmeter and Weston ammeter was brought out by myself. That was put on the market in 1888. Of course it was not put on the market immediately when it was made. In that type, this same kind of field is employed, and I believe that that form of field, and that kind of arrangement was original with myself. I am not quite sure about it. I think so. But instead of the suspension, a pair of spiral springs were used to carry the current into and out of the coil, and to supply the devices which measure the forms of the current passing through the coil. After the attention of the world at large seemed to have been called to the utility of this moving coil, more especially in the little portable instruments that were used and distributed throughout the world, a great deal of attention was bestowed upon this moving coil type of instrument, and Mather and half a dozen others, and probably still more, have taken up the matter and discussed it from various standpoints, and we have had such an instrument as Mr. Willyoung has described here, brought out by them on the other side. I can scarcely agree with the remarks of Mr. Willyoung in regard to the advantage of that instrument. I think if he makes careful experiments and careful tests of different forms of coil, he will find that Messrs. Ayrton and Perry have made a fundamental mistake on that. That type of coil is not as good as the other type of coil. It has some advantages and it has some disadvantages. I read the paper twice or three times over, because it dealt with a branch of work to which I had given a great deal of attention, and it seems to me that the author has overlooked a good many important matters. In practical work an index is accurate enough for an instrument that is to be used to measure electromotive force or current strength. In making that statement I make it because I know it to be a fact, that with a very slight increase in the length of the index over our regular type of instrument it is perfectly possible to get an instrument that will work within $\frac{1}{10}$ of one per cent. If a man can standardize an instrument within that limit he is doing remarkably good work; I think a good deal better work than it is possible to do generally.

The little voltmeters and ammeters which he mentions are not the ones I refer to. Those are capable of being used to indicate correctly to within one-tenth of one per cent. I think they will be found to be, if not abused, correct to within about one-fifth of one per cent., and where they are called for we can make them correct within one-tenth of one per cent. and a man with a little experience can read to one-tenth of those divisions with accuracy. I find no difficulty in it. I have compared them with very delicate instruments and I found no serious error in estimating the tenths at all. It must not be overlooked that with a mirror and scale, the working angle is generally very small. A mirror and scale instrument is unquestionably an admirable thing for the adjustment of resistance. But for the measurement of current strength and electromotive force I think it is wholly unnecessary. With the mirror and scale as usually employed the working angle is very small. On the other hand, in a portable instrument or instrument with indexes, the working angles are as large as 90° and can be made even more if necessary. So if you take a mirror and scale you have either got to use a curve scale and increase the working angle to get very much greater sensibility or accuracy, and that never can be quite equal to the index, due to the fact that you cannot move over one-half the angle with the mirror that you can with the index.

I agree with what Mr. Willyoung says in regard to the portable instrument shown in this case. I think the author has a little difficulty about how much travel he has with the type of instrument he shows here. The coil is about three inches long and very nearly two inches in diameter. How to carry such a coil as that upon a pair of pivots and to expect to use it with a pair of springs which would ensure extreme sensibility and at the same time also use it with a mirror, I tell you it requires a good deal of courage to take that kind of a job up. If any try it they will find that they have got to throw it away and start all over again.

One of the serious difficulties in making these portable instruments is to get the weight on the pivot just as minute as possible. In our earlier models we did the best we could in this direction. We made the coil just as light and of as fine wire as we could, considering the mechanical skill we had at our command and the means of disposal in an instrument of that character, for those portable instruments require workmanship of an exceedingly high degree of accuracy. In the original coil the copper weighs somewhere, as I remember now, about 45 grains. The coil on the instrument made to-day weighs four grains; two-thousanths of an inch wire. We tried to get it to one-thousandth, but could not cover it. We got down to two-thousandths and succeeded very well there. The moving coil, pivot and all, without the index, weigh about 10 grains, or two-thirds of a gramme. The moving coil and frame alone weigh about seven grains. The needle weighs about three grains. It is made of aluminium and made in the manner described in this paper and has been made so for many years. I think there are vastly better instruments obtainable than the one shown here.

In regard to the use of the mirror, I would like to say that we have employed the mirror on one of our instruments and I have brought one here. It was made, I think, some time in 1879 or 1880. The scheme of supporting the mirror is a great deal better one than that described in the paper. The core is not cut away in the manner described, which certainly is a very detrimental thing. A better form is one which was made about a year or a year and a half ago, shown in the other model. There is no necessity for cutting away or removing such a large portion of the core as is shown in the form described in the paper.

Dr. Duncan:—Mr. Willyoung and Mr. Weston seem to differ as to the best shape of core. Mr. Willyoung favors having it very long and narrow. Mr. Weston thinks that it should be square. There is the question, of course, if the induction is the same in both cases, that the square coil is much better. The turning moment depends on the change in the number of lines of force for a given angle of deflection, and with a square coil that change would be greater for a given length of wire than with the other. If you have a long coil and no iron core, I imagine you can get more induction through a long coil, but if the induction is the same, as I say, the square coil should be better. If the core is used, then I think Mr. Willyoung is unquestionably wrong and that Mr. Weston's contention is true—that you have the greater turning moment with the square coil.

Mr. Willyoung:—I am willing to agree with Dr. Duncan in that respect. My remarks on the galvanometer, of course, were applied to the galvanometer as used as an instrument of precision, that is to say, in work which requires high sensibility. There is no question at all that for Mr. Weston's purposes the square coil is very much better in every respect, mechanically, electrically and every other way. But where extreme sensibility is required I think Mr. Weston will admit that it cannot be obtained. I mean the kind of sensibility that is required in very exact work, with the Wheatstone bridge and measurements of that kind. It cannot be obtained by coils suspended on pivots, and torsion produced by springs. It can only be obtained, as far as I know, by the use of a suspension, and it cannot be obtained perfectly enough even with that. It is not possible at present to produce a D'Arsonval galvanometer which is suitable for testing the insulation of cables unless the deflection method is abandoned for the condenser method. In the orthodox method of testing cables you require a very sensitive instrument which has a constant of a good many megohms and you get small scale divisions, of course, because you require a sensitive instrument and you could not *get* large scale divisions. You have not got the *sensibility* for a large angle. However, at the present time, I do not know of any D'Arsonval galvanometer that has ever been made that is sensitive enough for the application of the deflection method to the testing of cables or that begins to compare in sensibility with the Thomson galvanometer. Professor Ayrton says it does, and he has some mathematical arguments with which to support his views. But in actual practice it does not. I have never seen one that does. I have never *seen* any one that has seen one that does. I have not been able to come anywhere near to it myself. What I have to say about the galvanometer applies entirely to its use as a very delicate instrument. For commercial purposes, certainly, it would be impossible to improve on Mr. Weston's instrument. I have a great admiration for Mr. Weston's instruments, the manner in which they have been worked out, and the beautiful way in which they work. Undoubtedly the square form of coil, and his method of construction are the best in that form of instrument. But for an instrument requiring great sensibility I have found by actual experience with different instruments that I can get vastly more sensibility out of a form of galvanometer which has an elongated coil.

Dr. Duncan:—My contention is that it would be more positive with a square coil provided the induction were the same in all cases.

Mr. Willyoung:—Precisely.

Prof. R. C. Carpenter:—There is just one remark I would like to make regarding some light metals which might be of interest. During the last year we have been investigating under the direction of Dr. Thurston the physical properties of a new light metal. It is not new in any general sense, but it is new to us, and I do not think its properties are usually known. I refer to the metal magnesium. We find that it is in many respects very much superior to aluminium. Its specific gravity is about two-thirds that of aluminium. Its strength runs from 18,000 to 22,000 pounds per square inch. We have broken a great many pieces of aluminium and never yet have found one stronger than 12,000 pounds to the square inch. Its usual strength is 8,000 to 11,000 pounds. Magnesium is very much more ductile than aluminium, and so far as we can determine, it is not oxidized to any great extent by the atmosphere, certainly not at temperatures under 125 degrees. It seems to me that in looking for light metals it is well worthy of attention. At present, of course, the price is five or six times that of aluminium.

MR. WESTON:—In connection with that I would like to ask if it is possible to work it satisfactorily.

PROF. CARPENTER:—It apparently works very nicely, although we have had a good deal of difficulty in producing alloys. We can alloy it with one per cent. of copper very readily. But when we pass one per cent. we have difficulty. With aluminium we can alloy it readily, and the alloy of aluminium and magnesium has double the strength of pure aluminium.

MR. WESTON:—I asked that question because some time ago I undertook to construct an electrostatic voltmeter using a magnesium needle instead of an aluminium needle. I obtained a sheet of comparatively thin magnesium, but it was not thin enough for my purposes. It seemed impossible to work it, except by actually driving it down, just rolling it.

PROF. CARPENTER:—Possibly we have had a different class of magnesium. Our magnesium has resembled lead in its ductility. It seemed to have no well defined elastic limit. It seemed to give gradually as the force is applied, yet its ultimate strength is very great. It has a silver lustre much more like silver than that of aluminium.

[Professor Carpenter then read the following paper:]

A paper presented at the Tenth General Meeting of the American Institute of Electrical Engineers, New York, May 17, 1893. President Houston in the Chair.

THE VARIATION IN ECONOMY OF THE STEAM ENGINE DUE TO VARIATION IN LOAD.

BY R. C. CARPENTER.

The writer recently made some tests on two classes of engines, subject to variations in load, the results of which were reported at the last meeting of the American Society of Mechanical Engineers. Since that time the matter has been given considerable attention, from a practical standpoint, and the following paper contains the results of considerations and a comparison of all the data at hand, with certain simple formulæ. The object is the production of a practical formula to express the economy of any class of engine, under any condition of loading.

In deducing the formulæ which are expressed later, certain fundamental principles were used as a basis for reasoning, which, so far as this paper is concerned, need be considered as resting only on an empirical basis, although the author has reason to suppose a broader foundation for the propositions which are stated.

The fundamental proposition will, I think, be generally admitted. It is as follows :—

I. The steam consumed by an engine may be considered as composed of two parts, one part being that which would be required to do the work in a *perfect engine*, which is defined here as an engine working without waste of any available heat, and whose efficiency is equal to the thermodynamical efficiency of an engine working within the same limits of temperature ; the other part being that required to overcome the losses due to cylinder condensation and various wastes of the engine.

The steam required by a perfect engine, which as defined above, is one not subject to wastes, except those due to the exhaust, will vary directly with the work done, or under every condition of loading, the weight of steam per indicated horse-power must remain the same.

The author is well aware that authorities may not agree in every particular respecting the essential qualities of a perfect engine, and it is to be understood that the use of the term in this place only applies to a standard with which to compare the results obtained on actual engine trials.

The perfect engine is generally understood to be one working in a Carnot or reversible cycle, in which case the amount of heat and also the steam required, is perfectly definite and readily computed. This quantity was used as the standard in the first comparisons made, but no satisfactory results could be obtained; it was found, however, that the formulæ for steam consumption which applied to the Carnot cycle, if slightly modified would give a standard, which agreed very closely with the results of the tests examined, consequently this latter quantity has been adopted as the standard.

The method of computing the amount of steam required for the standard engine is explained in the following paragraph:—

Amount of Steam Required by the Perfect Engine.

The amount of steam required by the perfect engine will vary with the range of temperature through which the engine works. Its efficiency being expressed in each case by the difference of absolute temperature of entering (T_1) and exhaust (T_2) steam divided by that of the entering steam.

The efficiency

$$e = \frac{T_1 - T_2}{T_1} \qquad (1)$$

This fraction expresses the greatest possible proportion of the entering heat that can be transformed into work in any heat engine.

To find the heat units required for the perfect engine, we have only to divide 2545, the equivalent of one horse-power for one hour in B. T. U., by the efficiency expressed as above.

That is, the heat units, required by one I. H. P. per hour by the perfect engine equals

$$\frac{2545}{e} \qquad (2)$$

To find the number of pounds of steam required, this number is to be divided by the available heat in one pound of the steam. Were the engine to work in a true Carnot's cycle, this available heat would be measured by the latent heat of the entering steam, since by that theory the amount equivalent to loss of temperature in expansion is restored by compression. The engine assumed to be perfect in this discussion, is somewhat different from the ideal engine of the Carnot cycle, although the required steam can be as readily calculated.

To obtain the steam consumption of an engine working in the Carnot cycle, the value given in equation (2) must be divided by the value of the latent heat (r) to obtain the steam consumption for the ideal engine adopted as standard, the value as given in equation (2) must be divided by the latent heat increased by a quantity equivalent to the loss of sensible heat of the steam in passing through the engine. That is, if r represents the latent heat in a pound of the entering steam, λ the total heat, and q_0 the heat of the liquid in the exhaust steam,

We should have

$$\left.\begin{array}{l}\text{Steam required by the ideal}\\ \text{engine of Carnot's cycle}\\ \text{per I. H. P. per hour.}\end{array}\right\} = \frac{2545}{\frac{T_1}{T_1 - T_2}\, r} \qquad (3)$$

$$\left.\begin{array}{l}\text{Steam required by the \textit{stan-}}\\ \text{\textit{dard engine} per I. H. P. per}\\ \text{hour.}\end{array}\right\} = \frac{2545}{\frac{T_1}{T_1 - T_2}\,(\lambda - q_0)} = p \qquad (4)$$

That is, the standard engine as defined here does not work in a reversible cycle, but does utilize all available heat within limits of the temperature of entering steam and exhaust, whether it approximates more nearly or not to the real engine, it at least is the only value that can be used in expressing the economy in the propositions which follow.

The following table gives the amount of steam required by the *standard engine* per I. H. P. per hour for various conditions of steam pressure and exhaust, computed by formulæ (4). This quantity is in the following discussion called the *water-rate* for the standard engine.

TABLE I.—POUNDS OF DRY STEAM AND B. T. U. PER I. H. P. REQUIRED BY THE STANDARD ENGINE.

Steam Pressure Absolute.	Temp. of Steam Fah.	Back Pressure 14.7 lbs. [212°]			Back Pressure 5 lbs. [162°.3]			Back Pressure 2 lbs. [126.3°]		
		Thermal Efficiency Per Cent.	B. T. U. per I. H. P. per Minute.	Dry Steam per I. H. P. per Hour.	Thermal Efficiency Per Cent.	B. T. U. per I. H. P. per Minute.	Dry Steam per I. H. P. per Hour.	Thermal Efficiency Per Cent.	B. T. U. per I. H. P. per Minute.	Dry Steam per I. H. P. per Hour.
25	240	2.6	1620	100.05	10.9	388	22.8	16.9	251	14.2
30	250.3	5.35	740	48.8	12.4	340	20.0	17.4	244	13.7
40	267.1	7.55	560	34.2	14.7	287	16.8	19.3	220	12.4
50	280.9	9.2	465	28.0	15.95	266	15.4	20.8	204	11.4
60	292.5	10.55	400	24.0	17.25	245	14.2	22.1	192	10.8
70	302.7	11.8	364	21.8	18.4	230	13.2	23.0	184	10.2
75	307.4	12.4	340	20.6	18.9	224	12.85	23.6	180	9.95
80	311.8	12.95	328	19.7	19.5	217	12.5	23.9	178	9.85
90	320.0	13.85	306	18.3	20.5	206	11.85	24.6	172	9.55
100	327.6	14.7	288	17.3	21.1	200	11.5	25.6	165	9.10
125	344.1	16.4	259	15.5	22.7	187	10.7	27.0	156	8.62
150	358.2	17.85	240	14.1	24.0	177	9.9	28.4	149	8.17
175	370.6	19.1	221	13.0	25.1	169	9.55	29.4	144	7.90
200	381.7	20.2	208	12.4	26.2	162	9.20	30.3	140	7.6
		e	b	s	e	b	s	e	b	s

AMOUNT OF STEAM REQUIRED TO OVERCOME WASTES.

It is well known that no adequate theory has been as yet produced which will give a definite expression for these wastes, consequently they must be stated in empirical formulæ, the accuracy of which can only be tested by comparison with results obtained by careful trial. These wastes are considered by Thurston[1] to vary with the square root of the number of expansions, and by Cotterill to vary with a logarithmic function of the number of expansions. For four expansions or less the two propositions do not differ greatly.

In this case I desire to produce a practical rule that can easily be applied, and instead of using the number of expansions, which are often difficult to obtain from the data at hand, I have expressed all the results in terms of the indicated horse-power, consequently, while the expression may not be scientific in form, it is one that can readily be applied and used by practical engineers.

Two formulæ will be employed, designated respectively A and B, both comparatively simple in their application, but giving best results when two cases, one less and one greater than most economical loads are considered.

Formula A is to be considered an application of the proposi-

1. See "Manual of the Steam Engine," Part I., page 517. N. Y.: John Wiley & Son.

tion stated by Thurston in a modified form, *i. e.*, that the wastes vary as the square roots of the ratio of variation in loading, instead of as the number of expansions. Formula B is a logarithmic function which was assumed by myself after a series of trials.

The two are used simply for the purpose of comparing two theories for variation in the wastes of an engine. In the discussion that follows, all the steam used by the real engine in excess of that required by the *standard* is considered as *waste*. All comparisons are made with the real engine working with its most economical load. The following formulæ are used :—

Let m = the indicated horse-power (I. H. P.) of the engine for most excellent results.

Let w = the steam actually used per I. H. P. per hour, when engine is developing m horse-power.

Let p = the steam required for the perfect, *i.e.*, the standard engine per I. H. P. per hour under same conditions.

Let b = least waste of the engine $= w - p$.

Let n = the horse-power of the engine for which the steam consumption is required.

Let $x = \frac{m}{n}$ for power less than m.

Let $x = \frac{n}{m}$ for power greater than m.

We shall have the following values for the steam consumption (y) per I. H. P. per hour :

First Case—n less than m.

$$y = p + b\sqrt{\frac{m}{n}}. \qquad \text{(A) or,}$$

$$y = p + b\sqrt{1 + \left(\log \frac{m}{n}\right)^{\frac{m}{n}}}. \qquad \text{(B)}$$

Second Case.—n greater than m.

$$y = p + b\sqrt{\frac{n}{m}}. \qquad \text{(A)}$$

$$y = p + b\sqrt{1 + \left(\log \frac{n}{m}\right)^{\frac{n}{m}}}. \qquad \text{(B)}$$

Both Cases.

$$y = p + b\sqrt{x}. \qquad \text{(A)}$$

$$y = p + b\sqrt{1 + (\log_e x)^x}. \qquad \text{(B)}$$

TABLE FOR FACILITATING THE USE OF FORMULÆ A AND B.

$$y = b \sqrt{x} + p. \qquad \text{(A)}$$

$$y = b \sqrt{1 + (\log_e x)^x} + p. \qquad \text{(B)}$$

Value of x.	Hyperbolic Logarithm of x.	Value of $1 + (\log x)^x$	Value of $\sqrt{1 + (\log x)^x}$	$\sqrt{x}$
1.00	0.0	1.00	1.00	1.0
1.05	0.05	1.049	1.024	1.025
1.10	0.0953	1.072	1.034	1.049
1.2	0.1823	1.129	1.063	1.095
1.25	0.2231	1.153	1.072	1.118
1.3	0.262	1.176	1.085	1.140
1.4	0.336	1.217	1.103	1.183
1.5	0.405	1.258	1.121	1.224
1.6	0.470	1.270	1.127	1.265
1.7	0.5306	1.340	1.157	1.304
1.8	0.588	1.384	1.175	1.342
1.9	0.642	1.430	1.196	1.378
2.0	0.693	1.480	1.216	1.411
2.25	0.8109	1.624	1.273	1.5
2.5	0.916	1.801	1.342	1.581
2.75	1.0116	2.028	1.424	1.658
3.00	1.0986	2.33	1.526	1.732
3.25	1.179	3.00	1.732	1.803
3.50	1.253	3.18	1.7832	1.871
4.0	1.386	4.68	2.163	2.0
4.5	1.504	7.98	2.825	2.121
5.0	1.609	11.77	3.431	2.236
6.0	1.79	33.9	5.82	2.449
7.0	1.95	108.15	10.39	2.646
8.0	2.08	351.5	18.75	2.828
9.0	2.197	1193.	34.53	3.000
10.0	2.30	4141.	64.38	3.163

In applying the formulæ, x is equal to the I. H. P. for most economical results divided by the actual I. H. P. until the quotient is equal to one; it is then taken as the reciprocal of the above ratio. The results obtained by application of the formulæ indicate that the effect, so far as changing the economy, is less for an overloaded than an underloaded engine.

A comparison with actual tests is given in the following pages.

APPLICATION OF FORMULÆ TO ACTUAL CASES AND COMPARISON WITH ACTUAL TESTS.

First. Triple expansion Corliss engine, made by E. P. Allis & Co., tested by C. H. Peabody, test reported at Am. Society Mech. Engineers ,November, 1892. Absolute steam pressure, 140.5 lbs., condensing engine; thermal efficiency, 30.2; steam required per I. H. P. per hour for standard engine, 7.7 lbs.; best actual consumption, 13.74 lbs., for 125.3 I. H. P.

We have as formulæ for this engine under the conditions of the test

$$y = 6.14 \sqrt{x} + 7.7. \qquad \text{(A)}$$

$$y = 6.14 \sqrt{1 + \log x^x} + 7.7. \qquad \text{(B)}$$

TABLE II.

Horse-power Developed.	Value of x.	Steam Consumption per I. H. P. per Hour.			Error Formula A Per Cent. Actual Results.	Error Formula B Per Cent. Actual Results.
		Formula A.	Formula B.	Actual by Test.		
140.79	1.125	14.1	14.0	13.82	+ 2.1	+ 1.45
125.37	1.	13.74	13.74	13.74	0	0
114.6	1.1	14.05	14.0	14.36	— 2.2	— 2.5
105.3	1.19	14.28	14.2	14.48	— 1.37	— 1.95
99.2	1.26	14.74	14.3	14.78	— 0.27	— 3.2
78.33	1.61	15.35	14.8	15.13	+ 1.62	— 2.0
67.45	1.86	15.95	15.0	16.0	— 0.3	— 6.2

Ratio of best performance to that of perfect engine 1.78.
General formulæ for this engine

$$y = p\,(1 + 0.78 \sqrt{x}), \qquad \text{(C) or,}$$

$$y = p\,(1 + 0.78 \sqrt{1 + \log x^x}). \qquad \text{(D)}$$

Second. Test of simple non-condensing engine, built by J. H. McEwen Mfg. Co., by R. C. Carpenter. Absolute steam pressure, 91.4; best consumption, 30.9 lbs. of water per I. H. P. at 75.8 horse-power. Perfect engine would require 18.4. (See table.)

We have then

$$b = 30.9 - 18.4 = 12.5.$$

$$m = 75.8.$$

$$p = 18.45.$$

$$y = 12.5 \sqrt{x} + 18.45. \qquad \text{(A)}$$

$$y = 12.5 \sqrt{1 + (\log_e x)^x} + 18.45. \qquad \text{(B)}$$

TABLE III.

Horse-Power Developed.	Value of x.	Steam Consumption Per I. H. P.			Error Formula A Per Cent. Actual.	Error Formula B Per Cent. Actual.
		Formula A.	Formula B.	Actual Test. 1		
18.9	4.0	43.4	45.4	45	— 3.5	+ 0.9
21.6	3.5	41.8	40.7	43	— 2.7	— 5.3
25.2	3.0	39.1	37.5	39.5	— 1.1,	— 5.1
30.3	2.5	37.1	35.2	35.5	+ 4.2	— 1.1
37.9	2.0	36.1	33.7	34.3	+ 5.3	— 1.7
42.0	1.8	35.1	33.15	33.2	+ 5.7	— 0.15
47.5	1.6	34.1	32.5	32.3	5.5	0.6
50	1.5	33.8	32.4	32.3	+ 4.4	+ 0.3
60.5	1.25	32.4	31.5	31.1	+ 4.2	+ 1.2
64	1.2	32.1	31.7	31.0	3.3	+ 2.1
69	1.1	31.5	31.3	31.0	1.5	0.9
75.8	1.0	30.9	30.9	30.9	0	0
83.4	1.1	31.5	31.3	31.0	1.5	0.9
91.0	1.2	34.1	31.7	32.0	0.3	0.9
94.7	1.25	32.4	31.6	32.0	+ 1.2	— 0.6

1. The results given in this column are derived from a curve drawn after the tests were made, and give the average results for each case.

General Formulæ for this Engine.

$$y = p\,(1 + 0.62\ \sqrt{x}\,), \qquad \text{(C) or,}$$

$$y = p\,(1 + 0.62\ \sqrt{1 + (\log x,)^{2}}. \qquad \text{(D)}$$

Test of engines of the "Bache," by Emery.

Steam pressure, constant, 80 pounds by gauge; back pressure, 3.2 lbs. absolute.

Required by perfect engine, 11.5 lbs. per I. H. P., or $p = 11.5$.

Best water rate, 20.3 lbs. at 91.2 I. H. P.

Equation.

$$y = 8.8\ \sqrt{x} + 11.5. \qquad \text{(A)}$$

$$y = 8.8\ \sqrt{1 + (\log x)^{2}} + 11.5. \qquad \text{(B)}$$

TABLE IV.

Horse-Power Developed.	Value of x.	Steam Consumption Per I. H. P.		
		Formula A.	Formula B.	Actual by Test.
40.50	2.25	24.7	22.7	25.18
70 33	1.26	20.9	20.9	20.71
91.21	1.0	20.3	20.33	20.33
94.23	1.04	20.5	20.5	22.38
97.70	1.08	20.7	20.6	21.97
102.07	1.12	20.8	20.7	20.36
125.75	1.36	21.6	21.0	21.169

General Equation.

$$y = p\,(1 + 0.78\ \sqrt{x}\,. \qquad \text{(C)}$$

$$y = p\,(1 + 0.78\ \sqrt{1 + (\log x)^x}. \qquad \text{(D)}$$

Test of engines of the "Gallatin." Simple condensing.

By C. H. Loring and Dr. Charles E. Emery.

Steam pressures vary from 37 to 44 by gauge, average 41.5; absolute back pressure, 3.2 lbs.

Required by perfect engine, 14. 2 lbs.; *i.e.*, $p = 14.2$.

Best rate, 24 lbs. at 182.2 horse-power.

$$y = 9.8\ \sqrt{x} + 14.2. \qquad \text{(A)}$$

$$y = 9.8\ \sqrt{1 + \log x^x} + 14.2. \qquad \text{(B)}$$

TABLE V.

Horse-Power.	Value of x.	Steam Consumption Per I. H. P.		Steam Pressure.
		Formula A.	Actual.	
122.8	1.49	26.1	26.0	44.8
127.2	1.43	25.9	26.7	40.9
182.2	1.00	24.0	24.0	43.3
182.4	1.01	24.01	26.3	39.4
236.9	1.31	25.4	24.5	39.5
236.5	1.31	28.4	28.1	37.6

General Formulæ.

$$y = p\,(1 + 0.69\ \sqrt{x}\,. \qquad \text{(C)}$$

$$y = p\,(1 + 0.69\ \sqrt{1 + (\log x)^x}. \qquad \text{(D)}$$

Test on the "Gleam." Roberts and Sayer. Engines compound condensing.

Steam pressure, 104 to 112 by gauge; average, 109.8.

Back pressure, 6.2 lbs. absolute.

Steam required by perfect engine, 11.5 lbs., *i.e.*, $p = 11.5$.

Best rate, 17.92 lbs. at 53.4 I. H. P.

We have

$$y = 6.4\ \sqrt{x} + 11.5. \qquad \text{(A)}$$

$$y = 6.4\ \sqrt{1 + \log x^x} + 11.5. \qquad \text{(B)}$$

TABLE VI.

Horse-Power.	Value of x.	Consumption Per I. H. P.		
		Formula A.	Formula B.	Actual.
28.5	1.89	20.3	19.1	19.83
29.0	1.84	20.1	19.0	18.86
53.4	1.0	17.92	17.92	17.92
55.9	1.05	18.0	18.1	18.24
70.1	1.31	18.8	18.4	18.88
76.0	1.42	19.1	18.6	17.86
83.2	1.56	19.5	18.7	18.89

General equation for this engine $p = 11.5$.

$$y = p\,(1 + 0.56\ \sqrt{x}). \qquad \text{(C)}$$

$$y = p\,(1 + 0.56\ \sqrt{1 + (\log x)^{1}}. \qquad \text{(D)}$$

TEST OF AN ENGINE BY HIRN.

Test of Hirn's [1]engine: 90 pounds steam pressure. Best consumption when developing 26.2 horse-power was 25 pounds per I. H. P. per hour, run non-condensing. In this case the waste per I. H. P. is 25 — 18.4 = 6.7, and the equation to be solved is

$$y = 6.7\ \sqrt{x} + 18.3. \qquad \text{(A)}$$

TABLE VII.

Indicated Horse-Power.	Value of Ratio x.	Consumption by Formula A.	Actual Results by Test by Hirn.
7.5	3.5	30.8	31.5
9.3	2.8	29.9	30.5
13.7	1.9	27.6	29.5
20.2	1.0	25	25
26.2	1.0	25	25
47.8	1.82	27.35	27
47.8	1.82	27.3	27

General formula $p = 18.3$.

$$y = p\,(1 + 0.37\ \sqrt{x}\,). \qquad \text{(A)}$$

COMPOUND CONDENSING ENGINE.

J. H. McEwen Mfg. Co. Tested by R. C. Carpenter.

Steam pressure, 125 absolute; back pressure, 5; I. H. P., 88.8; best water rate, 19.1.

Engine without steam jackets.

$$y = 8.5\ \sqrt{x} + 10.7. \qquad \text{(A)}$$

$$y = 8.5\ \sqrt{1 + \log x^{1}} + 10.7. \qquad \text{(B)}$$

1. Thurston's "Manual of the Steam Engine," vol. i.

TABLE VIII.

I. H. P.	Ratio x.	Results by Formula A.	Results by Formula B.	Actual by Trial.
56	1.58	21.4	20.7	22.1
70.3	1.26	20.2	19.8	21.3
88.8	1.00	19.2	19.2	19.2
97.8	1.105	19.7	19.4	19.8
119.3	1.32	20.5	19.9	20.1

General Formulæ.

$$y = p\,(1 + 0.79\ \sqrt{x}.$$

$$y = p\,(1 + 0.79\ \sqrt{1 + \log x^x}.$$

Same engine with jackets.

Best results, 18.9 lbs. at 80.5 I. H. P.

$$y = 8.3\ \sqrt{x} + 10.6. \qquad \text{(A)}$$

$$y = 8.3\ \sqrt{1 + \log x^x} + 10.6. \qquad \text{(B)}$$

$$p = 10.6.$$

TABLE IX.

27.17	2.95	24.8	23.2	23.11
44	1.84	21.8	20.4	20.7
61.03	1.32	20.1	19.6	19.69
80.5	1.00	18.9	18.9	18.9
95.1	1.18	19.6	19.3	19.1
100.6	1.32	20.1	19.6	21.7
118.4	1.46	20.6	19.9	20.3
126	1.58	21.4	20.5	21.2

General Formulæ.

$$y = p\,(1 + 0.78\ \sqrt{x}).$$

$$y = p\,(1 + 0.78\ \sqrt{1 + \log x^x}.$$

NON-CONDENSING AUTOMATIC ENGINE.

In this case the only data available to the author was the brake horse-power and corresponding water rate.

The total water consumed is obtained as the product of the number of brake horse-power by the corresponding water-rate. The steam consumed per I. H. P. per hour is obtained by dividing this quantity by the number of indicated horse-power, in each case assumed ten greater than the corresponding brake horse-power. The following are the results:

Steam pressure, 120 by gauge, 135 absolute.

Required per H. P. by perfect engine, 15 lbs. (See Table I.) Actual water rate per I. H. P., 21.75. Hence we have:

$$y = 7.75 \sqrt{x} + 15. \quad (A)$$

$$y = 7.75 \sqrt{1 + \log x^x} + 15. \quad (B)$$

$$p = 15.$$

TABLE X.

Brake Horse-Power.	Total Weight Steam.	I. H. P. Devel-oped.	Value of x.	Rate by Formula A.	Rate by Formula B.	Actual Results by Trial.
200	4788	210	1	22.75	22.75	22.75
160	4088	170	1.23	23.6	23.3	24.1
130	3161	140	1.5	24.5	23.7	22.6
100	2557	110	1.91	25.7	24.2	23.2
90	2386	100	2.1	26.2	24.6	23.9
70	2058	80	2.62	27.6	25.7	26.2
40	1602	50	4.2	30.8	31.7	32.0
20		20	10	39.3		

General Equations.

$$y = p\,(1 + 52 \sqrt{x}).$$

$$y = p\,(1 + 0.52 \sqrt{1 + \log x^x}.$$

Same engine, 100 pounds boiler pressure.

$$y = 7.2 \sqrt{x} + 16.6. \quad (A)$$

$$y = 7.2 \sqrt{1 + \log x^x} + 16.6. \quad (B)$$

$$p = 16.6.$$

TABLE XI.

160	4032	170	1	23.8	23.8	23.8
130	3440	140	1.21	24.3	24.3	24.4
100	2775	110	1.54	25.5	24.7	25.2
90	2547	100	1.7	26.0	24.9	25.4
70	2154	80	2.125	27.1	25.7	26.8
40	1572	50	3.4	29.9	29.5	31.4

General Formulæ.

$$y = p\,(1 + 0.44 \sqrt{x}).$$

$$y = p\,(1 + 0.44 \sqrt{1 + \log x^x}.$$

TESTS OF THE WESTINGHOUSE ENGINES WITH VARYING LOADS.

All the data regarding the economy of this engine that I have been able to obtain, is found in the advertisements of the makers. The following are the results of tests of the engine run non-condensing with varying loads and pressures, as given in a circular issued by the manufacturers :—

Table of actual steam consumed per I. H. P. Westinghouse compound non-condensing engine, cylinders 14 and 24 inches by 14 inches.

TABLE XII.

Horse-Power.	Boiler Pressures.			
	60 lbs.	80 lbs.	100 lbs.	120 lbs.
210				22.6
170			23.0	21.9
140		24.9	23.6	22.2
115		25.7	23.9	22.2
100	26.9	25.2	24.9	22.4
80	27.7	25.2	25.1	24.6
50	30.3	28.7	29.4	28.8

COMPARISON OF RESULTS WITH FORMULÆ,

Westinghouse engine, non-condensing.

80 pounds of steam.

Required by standard engine, 17.8 pounds.

Formula A.

$$y = 17.8 + 7.1 \sqrt{x}.$$

Formula B.

$$y = 17.8 + 7.1 \sqrt{1 + \log x^x}.$$

TABLE XIII.

Indicated Horse-Power.	Water Consumption by Trial.	Value of Ratio x.	Computed Water Rate.	
			Formula A.	Formula B.
140	24.9	1	24.9	24.9
115	25.7	1.22	25.6	25.4
100	25.2	1.4	26.2	25.6
80	25.2	1.75	27.2	26.1
50	28.7	2.8	29.6	28.2

Same engine, 60 pounds steam.

Formula A.

$$y = 20.6 + 6.3 \sqrt{x}.$$

TABLE XIV.

100	26.9	1	26.9	26.9
80	27.7	1.25	27.7	27.4
50	30 3	2.0	29.5	28.4

Westinghouse engine, non condensing, 120 pounds of steam.
Required by standard engine, 14.8.
Best actual consumption, 21.9.

Formulæ.

$$y = 7.1 \sqrt{x} + 14.8. \qquad \text{(A)}$$

$$y = 7.1 \sqrt{1 + \log x^x} + 14.8. \qquad \text{(B)}$$

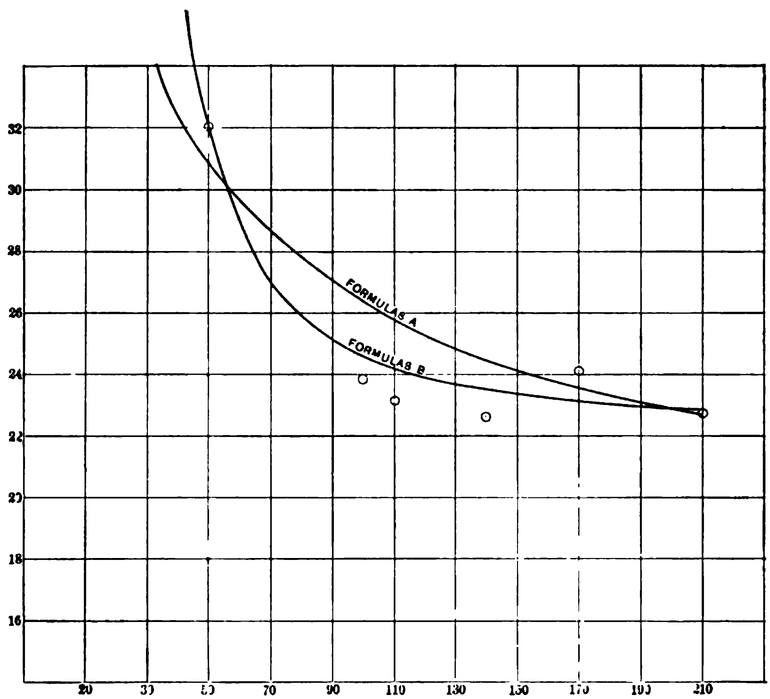


FIG. 1. WESTINGHOUSE NON-CONDENSING 14x24x14, 120 POUNDS STEAM PRESSURE ⊖ ACTUAL RESULTS.

TABLE XV.

Horse Power.	Water Consumption by Trial.	Value of Ratio x.	Computed Result.	
			Formula A.	Formula B.
210	22.6	1.24	22.7	22.4
170	21.9	1.0	21.9	21.9
140	22.2	1.21	22.6	22.3
115	22.2	1.52	23.5	22.8
100	22.4	1.7	24.0	23.0
80	24.6	2.12	25.4	23.6
50	28.8	3.4	27.9	27.5

Steam 100 pounds pressure, by same engine.

Equations for computed result.

$$y = 6.9 \sqrt{x} + 16.1. \qquad \text{(A)}$$

$$y = 6.9 \sqrt{1 + \log x^x} + 16.1. \qquad \text{(B)}$$

TABLE XVI.

170	23.0	1.0	23.0	23.0
140	23.6	1 21	23.8	23.4
115	23.9	1.52	24.6	23.9
100	24.9	1.7	25.1	24.1
80	25.1	2.12	26.1	24.7
50	29.4	3.4	28.7	28.4

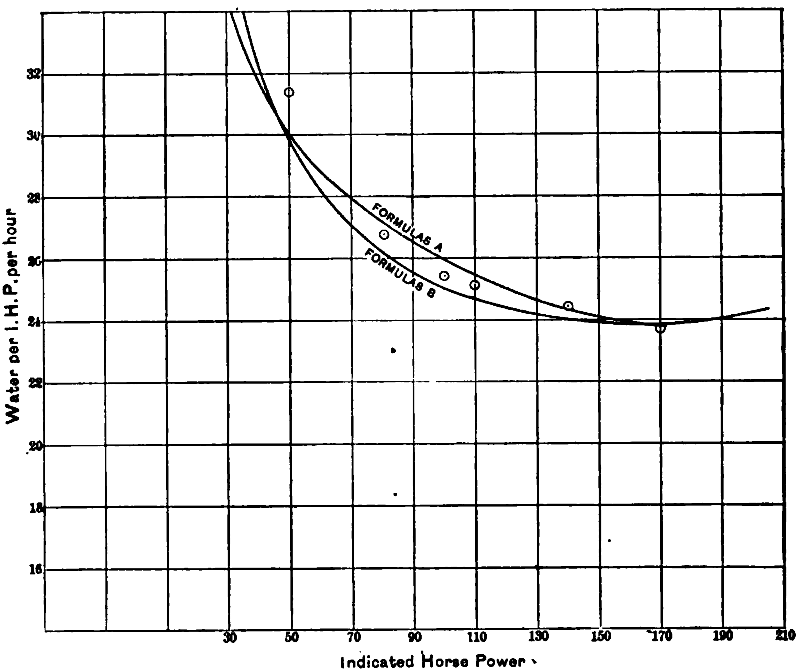


FIG. 2. WESTINGHOUSE COMPOUND NON-CONDENSING 100 POUNDS BY GAUGE ⊙ ACTUAL RESULTS.

A similar table giving the results of tests made on the same engine with varying loads when run non-condensing, is also printed in the catalogue of the builders. There is, however, no data given regarding the back pressure against which the engine worked. For this reason no comparison with the formulæ is possible, without an assumption, which might be very different from the actual facts.

PARSONS STEAM TURBINE.

The following test of the Parsons steam turbine, made by Prof. J. A Ewing, of Cambridge University, was reported in *Engineering News*, of January 12, 1893. In this case the results are expressed in terms of the output or delivered load, since no figures are given from which the indicated work could be obtained. In such a construction, friction is no doubt light, and indicated power must vary sensibly, the same as that delivered. The turbine worked with steam 115 absolute pressure to an absolute pressure of 1 lb. in condenser; the steam was superheated about 60°; turbine and attached dynamos made 4800 revolutions per minute; thermal efficiency for this case, 32.1%.

Water rate for standard engine, 6.72 lbs.

Best rate for real engine, 21.3 lbs.

Hence, as in preceding cases,

$$y = 14.6 \sqrt{x} + 6.7. \quad \text{(A)}$$

$$y = 14.6 \sqrt{1 + (\log x)^2} + 6.7. \quad \text{(B)}$$

General Formula.

$$y = p\,(1 + 2.18 \sqrt{x}).$$

Results of test and application of formula.

TABLE XVII.

Current Output.		Actual Water Rate.		Value of Ratio x.	Water Rate Computed.	
Kilo-Watts.	Elec. H. P.	Per K. W.	Per E. H. P.		Formula A.	Formula B.
20		48	35.8	4.5	37.7	47.7
30		39	29.1	3.00	31.9	28.9
40		34½	25.7	2.25	28.5	25.3
50		32	23.9	1.8	25.2	24.0
60		30½	22.7	1.50	24.6	23.0
70		29½	22.0	1.28	22.8	22.1
80		29	21.6	1.12	22.1	21.8
90		28½	21.3	1.00	21.3	21.3
100		28½	21.3	1.11	22.1	21.8

Steam turbine. Steam superheated 130°. Other conditions as before. In this case thermodynamic efficiency =

$$\frac{465 - 126}{925} = 37.0\%.$$

Water rate for perfect engine =

$$\frac{2545}{.37\,(1185 + 62 - 94)} = 5.95 \text{ lbs.} = 6 \text{ nearly.}$$

We have actual best result, 20.1. Hence,

$$y = 14.1\ \sqrt{x} + 6. \qquad \text{(A)}$$

$$y = 14.1\ \sqrt{1 + (\log x)^x} + 6. \qquad \text{(B)}$$

General Formula.

$$y = p\,(1 + 2.35\ \sqrt{x}).$$

The following is the result of the test:

TABLE XVIII.

Current Output.		Actual Water Rate.		Value of Ratio x.	Water Rate Computed.	
Kilo-Watts per hour.	Elec H. P.	Per K. W.	Per E. H. P.		Formula A.	Formula B.
20	--					
30	40	36½	27.2	3	30.2	27.5
40		32	23.9	2.25	27.0	24.0
50		29.5	22.0	1.8	24 9	22.6
60		28.5	21.3	1.5	23.3	21.7
70		28.0	20.9	1..8	22.0	21.1
80		27.5	20.5	1.12	20.9	20.6
90	116	27 0	20.1	1.00	20.1	20.1
100	129	27.0	20.1	1.11	20.8	20.0

These tests, which constitute all that are available at the present time to the writer, indicate that either Formula A or Formula B can be used to express the variation in economy in an engine of any class due to a change of load

As Formula A is much more simple in its application, and does not appear in any case which has come to the writer's notice to give results which differ greatly from the truth, its use is, in general, to be preferred.

It is to be noted that the use of this formula merely assumes that the wastes, reckoned from the standard adopted, vary as the square root of the change in the load.

PART II.

GENERAL FORMULÆ FOR WATER CONSUMPTION OF ANY ENGINE.

It may be considered that the first part of this paper, establishes the fact that the amount of steam used by an engine to supply the wastes, follows a law not essentially different from that expressed by either Formula A or Formula B. It now remains to see if any general expression can be found that will agree fairly with the results found by actual trial.

For this discussion it seems necessary only to use the more simple form of equation, as expressed in Formula A,

$$y = b\ \sqrt{x} + p, \tag{3}$$

in which y is the water-rate when the engine is developing n horse-power, $b + p$ is the water-rate at the best load of the engine, which is taken as m horse-power.

$x = \frac{m}{n}$, when n is less than m and $x = \frac{n}{m}$ for m less than n.

p is the water rate of the standard engine as given in the table for the same conditions of steam and back pressure.

b is the amount of steam required to overcome the wastes of the engine at its most favorable load.

This equation can be reduced to the following form: —

$$y = p\left(\frac{b}{p}\ \sqrt{x} + 1\right) = p\ (r\ \sqrt{x} + 1), \tag{4}$$

in which r is a constant coefficient and equal to the ratio of steam per I. H. P. per hour used to overcome wastes, when the engine has its most economical load, to that required by the standard engine under the same conditions.

To determine whether a coefficient which should be nearly constant, could be found for each class of engine, I have examined a large number of engine tests. The results seem to indicate that the coefficient r does not vary through wide limits, as will be seen by examination of the following compilation of a large number of tests, made with various classes of engines.

The water consumption per I. H. P. per hour as shown in the preceding table for the various classes of engines, differs greatly, yet when compared with that required for a perfect engine working between the same limits of temperatures, the variation is considerably reduced.

The ratio that the waste per I. H. P. per hour bears to the water-rate of a perfect engine, except for extreme cases varies from 0.5 to 0.8, and is in general, lower for the non-condensing than for the condensing engine.

TABLE XIX.—TESTS OF COMPOUND AND TRIPLE EXPANSION ENGINES.

Steam Pressure by Gauge.	Back Pressure Absolute.	Diameter of Cylinder.	Stroke.	Revolutions.	I. H. P.	Water Rate. By Trial. Actual.	Water Rate. For a Perfect Engine.	Waste per I. H. P. per Hour.	Ratio Actual to Perfect.	Ratio Waste to Perfect.	Kind of Engine—Authority.
Lbs.	Lbs.	Inches.	Inches.			a	p	$a - p = r$	$a \div p$	$f = r \div p$	
85	2.4	24 and 38	27	70.8	116	16.9	9.2	7.7	1.84	0.84	Geo. Mair.
111	5.0	22 " 24	60		716	19.36	10.7	8.7	1.81	0.81	Reynolds-Corliss. Barnes.
98	2	18 " 32				13.26	8 8	4.5	1.53	0.53	" " Henthorn (Cross Comp.)
96	2				999	14.5	8.9	5.6	1.63	0.63	" " Leavitt (Tandem).
02.8	2				270.5	16.1	9.0	7.1	1.78	0.78	Harris-Corliss. Hill.
124.8	2	19 " 38	48		325	14.07	8.4	5.6	1.67	0.67	Reynolds-Corliss (Ver'. Comp.)
114	2	22 " 44	60	68	631	14.3	8 5	5.8	1.67	0.67	Harris-Corliss (Twin Comp.) Barrus.
130	6	9½ " 21	20	160	95.4	18.77	10.5	8.87	1.80	0.80	Lansing Automatic. Albion. R. C. Carpenter.
100	2	14 " 21	20	172	227	16.57	10.7	5.87	1.67	0.67	" " Lansing. "
112	14.7	9 " 16	14	265	114	23.13	15.4	7 73	1.49	0.49	Automatic McEwen (Tandem), "
125	14.7	8 " 14	12	97	50	22.6	14.3	8.3	1.58	0.58	" " " "
80	15.0	14 " 21	20	150	69.6	32.8	17.8	15.0	1.83	0.83	Lansing Automatic. "
119.9	2	21 " 42	40	37.69	253.4	15.08	8.4	7.5	1.80	0.80	Blake Compound Pump. Prof. Peabody.
											Triple Expansion.
128	2	14, 25 " 33	48		515	12.94	8.2	4.74	1.57	0.57	Reynolds-Corliss. Leavitt and Henthorn.
192	2.2	5, 8 " 12	10, 10 and 15	278	55.15	12.68	7.6	5.08	1.62	0.62	Reynolds Eng. with jackets. Osb. Reynolds.
98	5	9½ 14 " 21	20	165	266	17.98	11.1	6.88	1.62	0.62	Lansing Eng. Auto. Tandem. R. C. Carpenter.
124.9	5	27, 46 " 70	60	16.6	787.8	12.675	8.3	4.37	1.52	0.52	Reynolds-Corliss Pumping. Tested by Mr. Fiend.

TEST OF SIMPLE ENGINES, NON-CONDENSING AND CONDENSING.

50	15	8	24	60.4	12.7	42.2	23.8	18.4	1.78	0.78	Corliss Non-Condensing. Dixwell.
95.8	15	18.02	48	75.3	137	25.6	16.8	9.1	1.54	0.54	Reynolds Corliss " Miller's Exposition.
96.1	15	18.03	48	75.8	134.3	23.9	16.7	7.2	1.46	0.46	Harris-Corliss " " "
96.3	15	18.26	48	76.1	140.0	24.9	16.6	8.3	1.50	0.50	Wheelock " " "
120	15	14.5	18	125.9	80.26	25.61	14.5	11.1	1.77	0.77	Hoadley s Portable. " "
77.4	15	12	14	245	75.8	30.9	18.1	12.8	1.66	0.66	Automatic McEwen. R. C. C.
90	15	17	30	83		26.6	17.0	9.6	1.56	0.56	" Denton and Jacobus.
70	16	14	20	171	45.4	30.3	19.0	10.7	1.6	0.60	Automatic Lansing. R. C. C.
											Condensing Engines.
95.8	4	18.02	48	75.4	162.3	20.6	10.8	9.8	1.83	0.83	Reynolds-Corliss. Millers' Exposition.
96.1	4	18.03	48	75.4	165.6	19.4	10.4	9.0	1.86	0.86	Harris-Corliss. " "
96.3	3.5	18.26	48	74.8	158.4	19.5	10.3	9.2	1.84	0.84	Wheelock.
95.8	4.5			75.4	162.3	20.6	11.0	9.6	1.87	0.87	Reynolds Corliss.
95.8	4.0	34	30	61.1	306.2	20.7	10.9	9.8	1.90	0.90	Gallatin Engine. C. E. Emery.

It may be noticed that in general the simple engine, when compared with the standard engine working under the same conditions, makes fully as good a showing as the compound or triple expansion engine. This indicates what is no doubt true, that a large portion of the gain in economy obtained by using the compound and triple-expansion engine, is due to increased steam pressure, but it is not to be understood from this that with high steam pressures the simple engine would be equally as economical as the compound and triple expansion engine.

While I have not been able to find a sufficient number of tests of these different classes of engines, working under the same conditions as to steam pressure and exhaust to draw any positive conclusions; still I am quite confident that a more extended investigation will show that as the steam pressure is increased, beyond a limit not yet definitely determined, the number of cylinders must be increased in order that the waste shall not increase unduly as compared with the steam usefully applied.

The results in the preceding table also show what is to be considered as best results for the classes of engines described, and no doubt show somewhat smaller coefficients than may be expected in ordinary practice.

The table which follows was made with the assistance of Dr. R. H. Thurston by compilation from a large number of tests, and from a paper read before this society by Dr. Chas. E. Emery,[1] it shows the results which may reasonably be expected for a given class of engine, in ordinary condition.

It will be noticed that the coefficients in the following table are considerably higher than those shown in the actual table of tests, they are however about the best results that makers would guarantee for the respective classes of engines, and presumably equal to the best results obtained in every day use.

In column IV is given the weight of steam per I. H. P. per hour used to overcome the wastes of the engine, this quantity corresponding to r in the general equation for steam consumption. Column III gives the water-rate for the standard engine and the values correspond to p in the general equation. Column IV is the difference between column II and III.

From the table which has already been given on page 300 for the water-rate of the standard engine, and the preceding table, column IV we can obtain the coefficients for predicting the probable

1. TRANSACTIONS, p. 119 *ante*.

economy of any engine working at any portion of its rated power.

TABLE XX.—USUAL WATER CONSUMPTION FOR BEST RATIO OF EXPANSION.

	I.	II.	III.	IV.	V.	VI.
Class of Engine.	Gauge Pressure.	Water-Rate. Actual Engine.	Water-Rate. For Standard Engine.	Waste per I. H. P. per Hour.	Ratio of Waste to that used by Standard Engine.	Average Ratio as in Column V.
	Lbs.	Lbs. per Hour.	Lbs. per Hour.	Lbs.		
Non-Condensing.		— w —	— p —	$w - p = b$	$b + p$	r
Small Throttling........	60	42.5	18.9	23.1	1.24	1.25
" "	80	40.0	17.8	22.2	1.26	
" Automatic	60	37.5	18.9	18.6	0.98	0.97
" "	80	35.0	17.8	17.2	0.96	
Medium "	80	32.0	17.8	14.2	0.79	0.79
" "	100	29.0	16.1	12.9	0.79	
Corliss Simple	80	30.0	17.8	12.2	0.68	0.68
" "	100	27.0	16.1	10.9	0.68	
Compound Automatic....	100	26.0	16.1	9.8	0.62	0.62
" "	125	23.0	14.4	8.6	0.62	
Condensing Engines, Vacuum 26 inches.						
Corliss Simple	60	25.0	10.	15.0	1.50	
" "	80	23 0	9.3	13.7	1.47	1.48
" "	100	22 0	8.8	13.2	1.50	
Compound Automatic....	80	20.0	9.3	10.7	1.16	
" "	100	19.0	8.8	10.2	1.15	1.14
" "	125	17.5	8.3	9.2	1.12	
Corliss Compound...... .	80	18.0	9.3	8.7	0.95	
" "	100	17.5	8.8	8.7	0.99	0.96
" "	125	16.0	8.3	7.7	0.93	
Triple Expansion.........	100	15.5	8.8	6.7	0.76	
" "	125	14.5	8.3	6.2	0.75	0.75
" "	150	13.75	7.9	5.85	0.75	

Thus the formula expressing the water consumption of any engine is expressed by formula (3).

$$y = b \sqrt{x} + p \qquad (3)$$

The value of p is given in table page 290, that of b in column IV of table on this page. As already explained x can safely be taken as unity, when the engine is working at its rated horse-power, unless the engine is known to be very much over or underrated; for it is the almost universal practice of American builders to design their engines to give the rated capacity at the most economical point of cut-off. For other loads, let n equal the required horse, m the rated horse-power, then $x = m \div n$ when n is less, then m; $= n \div m$ when n is greater than m.

The use of the formula will be rendered evident by an example as follow:—

Required the water-rate of a simple Corliss engine with 80 pounds steam by gauge, and working against 2 pounds absolute back pressure, that is with a vacuum of 10 pounds. Rated horse-power of the engine supposed to be 100.

We have for this case

$$p = 9.3 \qquad b = 13.7$$

So that

$$y = 9.3 + 13.7 \sqrt{x}$$

as the general equation.

The values of x would be taken as follows, when the engine is loaded to 10 per cent. of its rated capacity $x = 10$, at 25 per cent. $x = 4$, at 125 per cent $x = 1.25$.

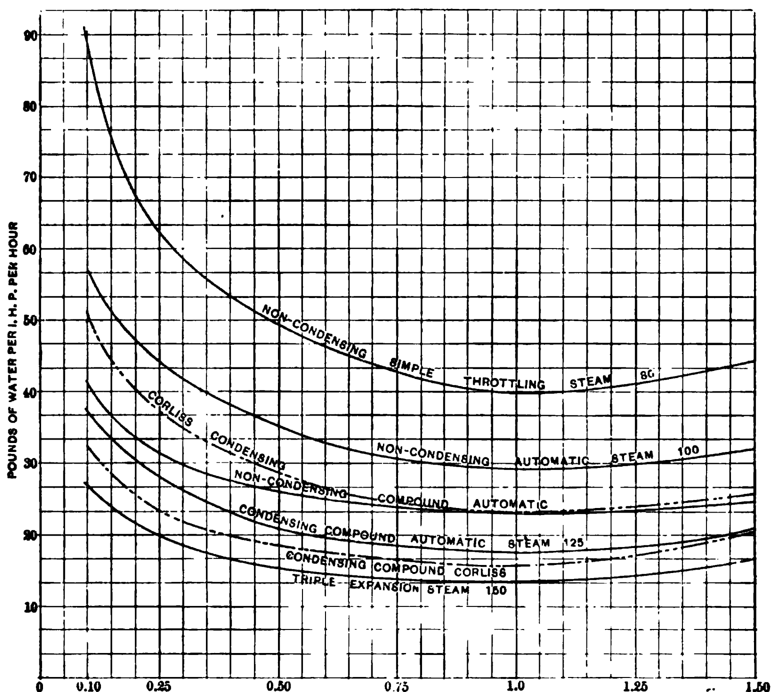
A computation made in this manner, gives the probable water consumption in pounds per I. H. P. for various loads, and classes of engines, which are given in the following table.

From the figures given, it will be noted that the increase in water consumption is small, when the extreme variation in load is confined between the limits of one half, and one and one half the load which gives the best results, and that the water consumption increases much more rapidly for light than heavy loads.

TABLE XXI.—PROBABLE WATER CONSUMPTION FOR VARIOUS CLASSES OF ENGINES WITH DIFFERENT LOADS.

Class of Engine.	Gauge Pressure.	General Equation of Steam Consumption.	Ratio of Load to Rated Capacity.						
			$\frac{1}{10}$	$\frac{1}{4}$	$\frac{1}{2}$	$\frac{3}{4}$	1	$1\frac{1}{4}$	$1\frac{1}{2}$
Value of x.			10	4	2	1.33	1	1.25	1.50
			Lbs. of Water per I. H. P. per Hour.						
Throttling... Non-Condensing	80	$22.2 \sqrt{x} + 17.8$	88.8	61.5	49.6	43.7	40.0	42.8	45.2
Automatic . . "	80	$14.2 \sqrt{x} + 17.8$	63.0	46.2	37.9	34.2	32.0	33.7	35.2
" .... "	100	$12.9 \sqrt{x} + 16\ 1$	56.8	41.9	34.4	31.0	29.0	30.5	31.9
Corliss Simple "	80	$12.2 \sqrt{x} + 17.8$	56.4	42.2	35.1	31.9	30.0	31.5	32.8
Compound Automatic "	100	$9.8 \sqrt{x} + 16.1$	47.1	35.7	30.0	27.5	25.9	27.1	28.1
" " "	125	$8.6 \sqrt{x} + 14.4$	41.6	31.6	26.6	24.3	23.0	24.0	24.9
Corliss Simple.....Condensing	80	$13.7 \sqrt{x} + 9.3$	51.5	36.7	28.7	25.1	23.0	24.6	2.61
Compound Automatic "	100	$10.2 \sqrt{x} + 8.8$	41.0	29.2	23.2	20.6	19.0	20.2	21.3
" " "	125	$9.2 \sqrt{x} + 8.3$	37.3	26.7	21.3	18.9	17.5	18.6	19.6
Corliss Compound "	100	$8.7 \sqrt{x} + 8.8$	36.4	26.3	21.1	18.8	17.5	18.5	19.5
" ' "	125	$7.7 \sqrt{x} + 8.3$	32.7	23.7	19.2	16.6	16.0	16.9	17.7
Triple Expansion.	125	$6.2 \sqrt{x} + 8.3$	27.9	20.7	17.1	15.5	14.5	15.2	15.9
" '	150	$5.85 \sqrt{x} + 7.9$	26.4	19.6	16.2	14.6	13.75	14.4	15.1

In conclusion the writer wishes to say that the paper which has been presented is to be considered simply as the result of a study of a large number of tests of actual engines working under various conditions. It differs from previous papers principally in the fact that the basis of all the results is a comparison of actual, with the theoretical steam consumption of an engine, which converts all heat received in the steam and not discharged with the liquid into the exhaust, into work.



Bradley & Poates, Engr's, N.Y.

FIG. 3. Curves of Probable Water Consumption for Various Classes of Engines with Different Loads.

The author is well aware that this base of comparison may be open to criticism, and at present is willing to say that the only reason for its use, is the fact which has already been shown, that it gives nearly a constant ratio when compared with the result of the actual engine, and this was far from being true, when the engine was considered as working in a reversible cycle.

As to the final computations, it may be said that their value depends upon agreement with actual tests. They are of course to be considered as only average results applying to well built engines, since they do not take into account differences due to poor or good construction, which in practice is of great importance, and may give results which differ 10 per cent., or even more, from those given.[1]

In addition to what has been already stated, it may be said that the general result of my study would indicate that *the more economical an engine, even when loaded at its best ratio of expansion, the less will be the change in economy, due to increase or decrease of load*, and I am fully satisfied that the results of actual tests provide a strong argument in favor of the most economical type of engine. even for conditions under which the load varies rapidly, and through wide limits.

The coefficients which I have been able to gather by compilation of tests are no doubt subject to modification and correction, still in the form presented I believe they may prove useful in pointing out the probable economy of an engine with its most economical load, and its modification due to change of load. It should be understood that the only claim for consideration that these values possess is due to agreement with the results of reliable and careful tests.

Friction of Engines, Economy on Basis of Delivered Horse-Power.

The results given in the paper are based on the work of the steam, as shown by the indicated horse-power developed, but in many cases it is essential to know the effect on the economy, caused by variation in delivered horse-power.

This result could easily be obtained from the preceding table, and the frictional work for various classes of engines known,

1. As illustrating the great difference due to excellent construction, the author would state that since writing the above he has conducted a test of 24 hours duration, of a Corliss triple expansion pumping engine, working between the limits of 186 pounds absolute steam pressure and 1.2 pounds back pressure. in which the actual water-rate was 11.79 pounds. The standard engine for this case (not given in table) would require 8 pounds, so that the equation is $8 + 3.76 \sqrt{x}$, and the ratio r is $3.79 \div 8 = .47$; a result very much below that of any triple expansion engine tabulated, and also probably the best result ever attained with any engine of any kind.

The probable water-rate for this engine for different loads would be as follows:

Part of Rated Load.	Water-Rate.	Part of Rated Load.	Water-Rate
0.10	17.9	1.00	11.79
0.25	15.6	1.25	12.25
0.75	12 37	1.50	12.65

because it is a thoroughly well established fact in steam engineering that the friction of an engine remains practically constant regardless of the work (see Thurston Manual Steam Engine, vol. i, art. 132-3), and is in every case essentially the same as that shown by the indicator diagram when the engine is working without load.

To determine the amount of friction that may reasonably be expected for various classes of engines, the author has gathered the following records of actual tests. These records are not as numerous as possibly might be desired, due, no doubt, to the great difficulty of obtaining an accurate record of the friction horse-power of large engines.

TEST OF SIMPLE ENGINES.

Total I. H. P.	Friction H. P.	Friction Per Cent. of Rated H. P.	Size of Engines.	No. Revolutions.	Kind of Engine.	Makers.	Test by
30	3	10	7 x 12	260	Automatic	C. U.	R. C. Carpenter 1888
36	2.6	7.2	8 x 12	220	Automatic	Lansing	" "
25	2.6	9.6	7 x 10	220	Throttling	Lansing	" "
33	1.8	5.5	7 x 7	600	Small Vertical	Lansing	" "
57	2.6	4.6	8 x 14	230	Automatic		Mitchell and Aldrich
32	2.8	8.8	6⅞ x 12	250	Strt. Line		
142	12.7	8.8	16 x 30		Porter-Allen		
84	10.[illegible]	11.8	12 x 11	300	West'house		Day and Riley
115	10.6	9.2					G. A. Hirn
144.8	12.3	8.5			Corliss	Bergre Andre	
45	6.1	13.5		350	West'house	West'house	G. H. Barrus
	Average	8.9					

TEST OF COMPOUND ENGINES.

CONDENSING ENGINES WITH AIR PUMP.

Total I. H. P.	Friction H. P.	Friction Per Cent.	Size of Engine.	No Rev.	Kind of Engine.	Makers.	Tested by
117.8	15.2	12.9	22 x 18 & 21 x 20	208	Automatic	Lansing	R. C. Carpenter.
191.4		10.4	?	?	Woolf	Koechlin	?
174.4		10.9	?	?	Compound	Alsatian Bitschwilde	?
248.9		13.7					?
77.8	10.3	12.8					Mair.
134.	14.0	10.4	18 & 36 by 26	36.4	High Duty Pump	Worthing'n	Wm. Rotch.
255.5		15.1	26 & 54 by 43	17.6	"	"	Prof. W. C. Unwin
370.		8.12	33 & 66 49.8	12.11	"	"	C. B. Brush. G. H. Benzenburgh

Average 12.3 per cent.

Deduct probable friction of air pump 3 per cent., gives net friction 9.3 per cent.

TEST OF TRIPLE EXPANSION ENGINES WITH AIR PUMP.

778.7	81.7	10.5	27, 46 & 70 by 60	16.6	Vertical Pump	E. P. Allis	B. H. Fiend, Chicago.
573.8	52.9	9.22	28, 48 & 74 by 60	20.3	"	"	R. C. Carpenter, Milwaukee.

Trials were made by Walther, Meniner and Ludwig to determine the friction of a compound engine run in various ways.

The results bring,

Compound engine with condenser and air pump.	248.97 I. H. P., 39.48 friction H. P. Per cent. of friction 13.7.
High-pressure cylinder with condenser and air pump.	153.1 I. H. P., 24.74 friction H. P. Per cent. of friction 16.0.
High-pressure cylinder without condenser.	128.4 I. H. P., 17.49 Friction H. P. Per cent. of friction 12.0.

It may perhaps be unsafe to draw conclusions from such a limited number of facts, but a study of the tests cited, show at once that there is no great difference as to the amount of friction in various classes of engines, the increased range of steam pressure and the greater return in work, being sufficient to make ample compensation for the extra work required to move the more complex mechanism of the compound or triple expansion engine.

The friction varies to a considerable extent in different engines; in general it is less in large than small engines.

It seems to be in every case between 5 and 9 per cent. of the rated load for non-condensing engines of 150 horse-power or over, whether simple, compound or triple, and to lie between 6 and 12 per cent. for condensing engines from 50 to 150 horse-power.

The condensing engines, to which an air pump is attached, usually have 2 to 3 per cent. greater friction than those without an air pump.

The average friction may be taken for engines of 50 to 150 horse-power as 10 per cent. non-condensing, and 13 per cent. condensing, those exceeding 150 H. P. as 7½ per cent. non-condensing, and 10 per cent. condensing, with very little error.

The following special test may be of interest as bearing on this subject:—

EXPERIMENT WITH SIBLEY COLLEGE EXPERIMENTAL ENGINE.

Sibley College has an experimental engine of the Corliss type, with three cylinders respectively, 9, 16 and 24 inches in diameter, and 36 inch stroke. The engine can be run triple expansion, compound or simple, as desired. The engine is provided with 3 brake wheels, and fitted with friction brakes of very excellent design and workmanship. The following trials show the friction of the engine when run in condition of simple, compound or triple expansion; that is with the high-pressure, high and intermediate, or with all the cylinders. The steam pressure in each case was essentially the same, and in no case was the friction of the air pump considered.

The results are as follows:

Simple engine, high-pressure cylinder only, 100 pounds steam pressure by gauge, 21 inches vacuum.

I. H. P.	Brake H. P.	Friction H. P.
62.4	55.5	6.9
62.45	55.5	6.95
73.4	66.0	7.4
82.8	76.7	6.1
	Average	6.8

COMPOUND.

High and intermediate cylinders, gauge 100 pounds.

I. H. P.	Brake H. P.	Friction H. P.
62 7	55.4	7.3
74.1	65.8	8.3
85.0	76.0	9.0
90.7	81.2	9.5
	Average	8.2

TRIPLE EXPANSION ENGINE.

I. H. P.	Brake H. P.	Friction H. P.
63.0	48.0	15.0
82.0	65.8	16.2
91.7	76.1	15.6
107.5	89.5	18.0
102.6	84.0	18.0
124.3	107.2	17.1
130.6	113.0	18.6
	Average	16.9

During these tests the simple engine was worked very much beyond its normal capacity, the triple expansion very much below.

As proved by subsequent tests, the limit of economical loading was with the simple engine at 62.4 horse-power, that of the compound at 85 horse-power, and that of the triple at 145 to 150 horse-power.

If the percentage of friction be reckoned on this basis, we shall have the following values of the friction: simple engine, 10.8 per cent.; compound, 9.65 per cent.; triple expansion, 11.2 per cent.

This particular engine has been run as a compound, using high and intermediate cylinders, a great many times; it has been run as a triple expansion engine only a few times, so that the excessive friction of the low-pressure engine can be readily explained as due to that fact.

Assuming what is probably true that this friction will be reduced by wear to 15 horse-power, we shall have in this case substantially uniform friction, whether the engine be operated as simple, triple or compound.

It seems quite certain that the friction of these various classes of engines, when developing the same power, is essentially constant. The following table is computed on the assumption that the friction is 10 per cent. of the rated load for non-condensing engines and 12½ per cent. for condensing engines.

TABLE XXII. PROBABLE WATER CONSUMPTION, PER DELIVERED HORSE POWER FOR VARIOUS CLASSES OF ENGINES WITH VARYING LOADS.

Class of Engine.	Gauge Reading.	[1] Ratio of Indicated Load to Rated Capacity.						
		1/10	¼	½	¾	1	1¼	1½
		Lbs. of Water per Delivered H. P per hour.						
Non-Condensing.								
Throttling Simple ……	80	Friction Load D. H. P. = O.	103.	74.5	50.5	44.5	46.6	48.5
Automatic " ……	80		77.0	47.5	39.6	35.6	36.6	37.8
" " ……	100		70.0	43.0	35.8	32.3	33.2	34.2
Corliss " ……	80		70.5	43.8	36.8	33.4	34.2	35.2
Automatic Compound.	100		59.5	37.7	31.8	28.8	29.4	30.1
" " ..	125		52.7	33.3	28.1	25.6	26.1	26.7
Condensing.								
Corliss Simple.. …….	80	Less than Friction Load.	73.4	38.3	30.1	26.3	27.4	28.5
Automatic Compound…	100		58.4	31.0	24.8	21.7	22.5	22.2
" " …	125		53.4	28.5	22.7	20.0	20.7	21.4
Corliss " …	100		52.6	27.2	22.6	20.0	20.6	21.2
" " …	125		47.4	25.6	19.9	18.3	18.8	19.3
Triple Expansion. ……	125		41.4	22.8	18.6	16.6	16.9	17.3
" " ……	150		39.2	21.6	17.5	15.7	16.0	16.5

1. The numbers express the proportion of the indicated horse-power developed to the rated indicated horse-power.

In this comparison, the general character of the curves which represent the variation in water consumption on this basis are somewhat changed, although the curves of each class, condensing and non-condensing, occupy the same relative positions as before. These curves for the condensing engines deviate more from a right line in this, than in the previous case, and all of them become vertical at a point when the friction of the engine is equal to the indicated horse-power.

The general form of these curves is shown in Fig. 4. It is noted that the simple Corliss engine shows wide variation in economy on the basis of delivered horse-power, and that its curve intersects and rises above that of the compound non-condensing engine for low loads.

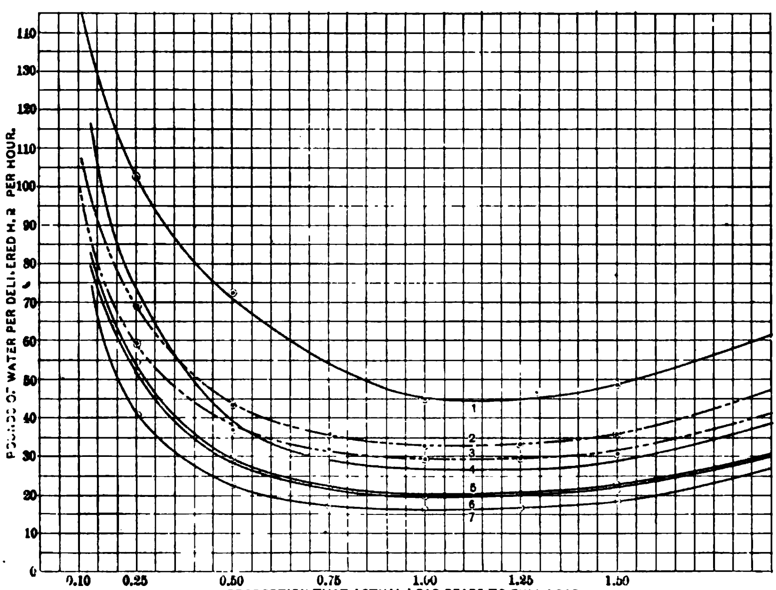


Fig. 4. Probable Water Consumption per Delivered Horse Power.

Curve No.	1.	Non-condensing simple throttling,	steam	80.
" "	2.	" " automatic	"	100.
" "	3.	" " compound automatic	"	125.
" "	4.	Condensing, Corliss	" "	80.
" "	5.	" compound	" "	125.
" "	6.	" " Corliss	"	100.
" "	7.	" Triple expansion	"	150.

Discussion.

Dr. Duncan :—These tests are made apparently for steady runs on even load. I would like to ask Prof. Carpenter if in the case of an actual engine where the load varies during the expansion these results would give a comparative rating under those conditions.

Prof. Carpenter:—I think that they would. I have incidentally had an opportunity to test several electric railroad plants. I remember one plant in particular, where we had on two different days quite a change in average load, and the difference in economy was very marked. On the first day—the engine being compound condensing—the test showed 25 pounds of water per indicated horse-power per hour. The next day the load averaged 25 per cent. higher although there was the same sudden variation. This had the effect of reducing the consumption to 21 pounds of water per indicated horse-power, showing a great increase in the economy of the engine as compared with the day previous, simply because of an improved load. I am quite well satisfied that the only way that we can get reliable data of engines employed on electric railroad work is to run these same engines on different, but steady loads. The results obtained from engine tests under the conditions which ordinarily exist on street railroads are very unsatisfactory indeed. You cannot possibly tell what conditions of loading correspond to average results since the economy varies with the load to a great extent, it is in some measure a function of the load, or in other words, the function of a variable and uncertain quantity. If only the average load, day by day, were constant the data would be more valuable.

Dr. Perrine:—I would like to ask if on these great variations of load it makes any difference if the engine—say it is a triple expansion engine—is governing the high pressure cylinder alone, or the low pressure cylinders also. I have seen a great many run on street railroad work with the governor stationary on the intermediate and low pressure cylinders, governing entirely from the high pressure, and I do not know whether that made any difference in the economy or not.

Prof. Carpenter:—The subject submitted by Dr. Perrine is one to which I have devoted considerable time, without at present being able to arrive at any definite conclusion. The question is a difficult one to decide experimentally, for the reason that it is nearly impossible to apply those diverse conditions to the same engine. Whether the low pressure valve should be operated by a fixed eccentric, or automatically by the governor, is a question regarding which, the best engineers of the country seem to differ, and I have been able to obtain very little data. I have compared several tests made with engines of each class and the difference has not been marked, nor has it always run in the same way. I think possibly it may depend on special conditions. In the latest engine test that I have made to date, we got very good results indeed, and the engine was governed entirely by the intermediate valve,—a case that I have never seen before—but it seemed to produce very good results indeed. The variation of load, however, in that case, was not excessive. I am of opinion that better results will, on the whole, be produced by having all the valves attached to the governor where there is a wide variation in the load. It is not, as often stated, likely to prevent that negative work which is sometimes done at the end of the stroke by the expansion line falling below the back pressure.

DR. CHARLES E. EMERY :—I would like to ask a question before making further remarks. Referring to the curves on page 319, the lower one is more nearly straight than the others, but was, as I understand it, derived from the same formula. Is not the less amount of variation shown by the lower curve due to the fact that smaller quantities were being dealt with so that the ordinates, though varying as the square root, show less difference than for the larger quantities?

PROF. CARPENTER :—Dr. Emery's explanation is correct. The change in economy is due simply to variation in the steam used to overcome the waste of the engine. If that quantity is small in amount, the variation is correspondingly reduced.

DR. EMERY :—This paper is valuable from the fact that it brings together in a series of tables the results of a large number of experiments made with different kinds of steam engines under different steam pressures and other variations of condition. The tables showing the probable variation in cost due to differences of load are also quite valuable, and the results appear to be well warranted by the references to experiments which are given. A hasty glance at some of the formulæ may, however, give the impression that they give the steam consumption per H. P. per hour, based upon general facts and dimensions. The running headings rather favor this view. The formulæ are, however, confined strictly to the subject expressed in the title, viz., "The variation in economy of the steam engine due to variation of load." To obtain the economy with these formulæ we must first know what the best economy is, then the formulæ simply increase the consumption in either direction for higher or lower powers according to the modification of the short empirical rule of Dr. Thurston, that the wastes increase substantially as the square root of the variation in loading (top of page 301), or the more elaborate function verbally credited by the author to Cotterill, though not so mentioned in the paper. That is, the minimum rate of consumption being known, either for that particular engine or another as nearly like it as may be found in the tables, the probable consumption for different relative powers may be computed from the formulæ. The criticism suggests itself that while the author gives fully the minimum cost for a large number of engines, he neither gives nor refers to rules by which the size or piston development of such an engine relative to the power may be obtained, he being simply satisfied with the statement (bottom page 317.). "As already explained x can safely be taken as unity when the engine is working at its rated power, unless the engine is known to be very much over or underrated, for it is almost the universal practice of American builders to design their engines to give the rated capacity at the most economical point of cut-off." It is true that the engine builders may get the proportions right in most cases, but engineers in general wish to know the average practice. The piston displacement can be determined directly from the mean pressure employed, which for compound engines is most conveniently stated in the form of the mean pressure in the large cylinder equivalent to the sum of the mean pressure in all the cylinders. A collation of the mean pressure in connection with the economical results will, I think, show that considerably higher mean pressure can be employed than it was formerly considered possible to use with economy. In other words, the engines, or rather the piston displacement per minute, can be smaller than generally supposed.

The method of arriving at the results is in some respects similar and in others different from that used by myself in 1888 in a paper on "The Cost of Power in Non-Condensing Steam Engines." [Vol. X Trans. Am. Soc. M. E.] Prof. Carpenter uses in connection with his actual minimum quantity the calculated minimum quantity which he bases on a modification of the ideal engine of Carnot's cycle, which I think very properly considers only the difference of temperature between the entering steam and the exhaust. This is really founded on one of the laws of thermodynamics expressing the general fact that as the steam engine is a heat engine the most that can be gotten from it is the mechanical equivalent of the thermal units included between limits, and I think practicable, not impossible limits, should be considered, so that his selection is not only warranted but rational. I may say, however, that in practice the way in which the heat operates upon the medium, the vapor of water for instance, makes differences which cannot be included in so simple an expression. The actual result in such case is really a function of the simpler expression, much as the magnetization is a function of the exciting force. In my investigation I therefore took the actual pressures and volumes derived by experiment as formulated and tabulated in connection with an ideal cylinder of unit capacity, and found the weight of steam required to fill such a cylinder under the conditions assumed. To this was added the weight of steam required to furnish the thermal units for the actual mechanical work performed in such ideal cylinder; next the cost due to cylinder condensation expressed in terms of the weight of steam condensed and a modification of the same due to variation in the relative size of the engines based on the amount of power developed. It will be seen that I considered that the relative power varied the absolute cost as respects this item only, whereas Prof. Carpenter has considered that it affects all the costs according to a certain rule. I next considered the costs, due to loss of pressure and other incidental losses, and finally the saving in cost due to expansion. The summation of these various items gave the total cost. I provided a separate formula for each one of the several items based necessarily upon experiment or calculation, or both, so that possible errors in any one of the several items could not have as great influence on the final result as if a mere empirical formula were used for the total losses. It is believed, therefore, that my formulæ and the tables calculated therefrom are from the methods adopted sufficiently accurate to ascertain the probable water consumption beyond present limits of experiment, and predictions were made at the time as to the economy of non-condensing engines for pressures as high as 500 pounds, which may be referred to by those interested in the subject.

An empirical formula is used in the paper of Prof. Carpenter to express the total losses. The formula does not purport to show absolute results, but the variations from a known result due to change of load. By this method an accurate starting point is provided for each particular engine considered, and the results may be applied to all kinds of engines. The comparison with the various experiments on different kinds of engines shows that the increase in cost due to varying the load in either direction, compared with that due to maximum economy is expressed within the limits of observation very satisfactorily by the formulæ.

In this connection I wish to call attention to the large number of valuable experiments which have been made by Professor Carpenter with steam engines of various kinds, and which have added largely to the general information on the subject. While the formulæ in this particular paper should be confined simply to the questions of variations of load, the tabulated results are of great value, independent of this special feature, on account of the large amount of valuable information brought together in condensed form.

Prof. Carpenter:—I am very much indebted, indeed, to Dr. Emery. This paper does not treat directly of the cost of operating or installing an engine, in any way whatever. I never felt competent to deal with that subject, and the remarks regarding the application of this paper to engine costs seems to have resulted from the fact that I have not made a clear statement of the object of this paper. The principal object is merely to express this fact; that the more economical the engine the less it seemed to be affected by the variation of power; and secondly, to give a simple expression showing the water consumption for any class of engine under any condition. I did not have data to go beyond that, and I did not desire it to be understood that the paper meant to give anything more than that. The paper is not intended to be used in computing the required sizes of engines, although I fully appreciate the value of Dr. Emery's remarks regarding its application in that direction. I had supposed that the paper would be useful to electrical engineers principally in pointing out the probable economy of any engine whose rated power was known. I desired also to point out that experiments show that the more economical type of engine was less affected by variations in power than the more wasteful type. That, I thought, would be the practical application of the results. I believe that the tabulated results—as indeed, Dr. Emery seems to think—on page 319, agree within fairly approximate limits of the actual consumption, say within one or two pounds of water per indicated horse-power per hour, from that obtained in practice.

The paper by Professor George S. Moler on "An Automatic Printing Speed-Counter for Dynamo Shafting," was then read by Dr. Nichols. Dr. Nichols prefaced the reading of the paper with the following remarks:

In the laboratories at Ithaca we have been compelled to use power from a variety of sources—water power, steam power from various engines, and even the two powers combined, and have been subjected thereby to perplexing changes of speed in our shafting. This fact made it desirable to have some means of knowing the range of flunctuation. It was this state of affairs which suggested to Prof. Moler the machine to be described in this paper. The only reason for presenting this account of it here is that the machine having been in operation for some months and having proved very useful to us, it seemed that it might be useful elsewhere.

A paper presented at the Tenth General Meeting of the American Institute of Electrical Engineers, New York, May 17, 1893, President Houston in the Chair.

AN AUTOMATIC PRINTING SPEED-COUNTER FOR DYNAMO SHAFTING.

BY GEO. S. MOLER.

The automatic printing speed-counter was designed and constructed by the writer, to supply a need felt of having a continuous record of the speed of the shafting which drives the dynamos and other apparatus being used by students in their laboratory experiments.

During part of the year the shafting of the dynamo laboratory at Cornell University is driven by water power, the wheels being situated at the bottom of a deep gorge, and several hundred feet distant. The power is transmitted by means of a wire cable running over large grooved sheaves, and the gate hoisting apparatus is operated by a wire rope passing around drums. No automatic regulator has yet been successfully applied to these wheels, so we have to rely upon hand regulation. On account of this, considerable variations of speed take place, although a tachometer is watched quite closely by the attendant in charge. The variations make it necessary in performing an experiment to continually take account of speed.

The printer (see Fig. 1) is essentially a speed-counter which prints the speed at the end of each minute upon a strip of paper, and does this continuously, requiring very little attention. It was built to give the speed of a shaft which runs at about 140 revolutions per minute, but it has a range of from 30 to 185 per minute.

It is connected to the main shaft of the dynamo laboratory by sprocket-wheels and chains, so that the shaft carrying the worm revolves at exactly the same rate as the main shaft. The type-wheel is about eight and one half inches in diameter, and is made by clamping printers type between a disk and a ring fastened with screws to one side of it. The disk has a series of small holes drilled in it at equal distances apart, and dowel pins passing through holes drilled in the type, hold the latter in place. If any of the type become worn, or accidentally bruised, the ring can easily be removed and new ones can be inserted. The type are spaced so as to correspond with the number of teeth of the large gear-wheel which engages with the worm. The printing is done upon a one-half inch tape of district telegraph paper. A typewriter inking ribbon is employed, and as it is fed along, it shifts sidewise back and forth so that the whole width of the ribbon is used. It takes several weeks to go once the length of the ribbon.

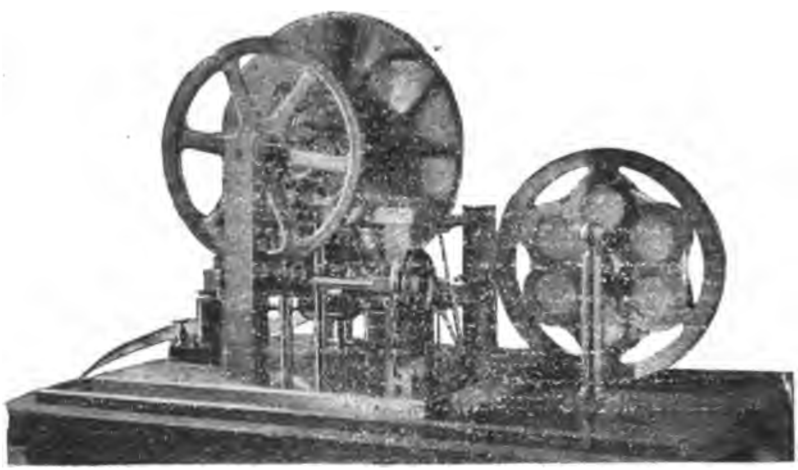


FIG. 1.

The type wheel practically starts from its zero reading at the beginning of each minute. It is necessary to have it do this, in order that the number printed at the end of the minute shall be the exact number of revolutions for that minute.

When a common speed-counter is used, its index is first set to zero, and it is then applied to the end of the shaft for just one minute, then is withdrawn and read. It is again set to zero, and the operation is repeated, but in doing this, a minute or more is lost each time while setting. Now if the speed were not too great, the index-wheel might be arranged so that it could be slipped backward or forward the proper amount, after reading, without stopping the counter. In this way a reading made from zero could be taken each minute. A similar method has been adopted in the printer. The type-wheel, which is held by friction on the shaft, and is carried with it, is at the beginning of the minute slipped backward or forward until it has the correct relative position, then for the remainder of the minute revolves at the same rate as the large gear-wheel on the same shaft. The method of slipping the type-wheel around is partly shown in Figure 2. The toothed wheel *a*, driven by the bevel gear, revolves once for every thirty revolutions of the main shaft. The plate or arm *b* revolves with it when the pawl *c* is allowed to catch in the teeth of the wheel. As the plate *b* goes around with the wheel, the pin *d* overtakes the arm *e*, which is attached to the type-wheel, and pushes it along slipping the type-wheel on the shaft. When *b* has made one complete revolution, the pin *f* on the pawl *c* comes opposite a notch, not shown in the figure, into which it is pushed by a spring, thus pulling the pawl from the wheel, and also holding *b* stationary. It takes thirty revolutions of the worm to carry the plate *b* around, it coming to rest just at the end of the thirtieth revolution. If at this instant the printing hammer were allowed to act, the number 30 would be printed, but if it does not strike till the end of the minute, the type-wheel will move with the large gear-wheel till that time. The striking of the hammer releases the pin *f*, and the pawl which has a spring of its own is again brought in contact with the teeth and so carries the plate *b* around again. If the type-wheel should complete its revolution before the pin *d* overtakes the arm *e*, then the arm *e* comes against another pin which stops it, until the pin *d* does overtake it. This is the slipping backward which has been mentioned. If the printer should be driven too fast, so that the type-wheel makes a revolution before the minute is up, or if the hammer is not caused to act for several minutes, then it will, the next time it acts, print a zero, which is the type under the hammer while the wheel is held stationary.

The hammer is released at the end of each minute by means of an electro-magnet operated by a clock, and when the hammer makes its stroke, it opens a switch in the electric circuit while the contact in the clock is still closed, so in this manner the spark on breaking the circuit is made at the switch, and not at the clock contact. The hammer is set again by a pin on the side of a wheel coming in contact with a lever, and this closes the switch again before the next contact of the clock. When the machinery is not running, the switch remains open, and so the battery materials are not consumed in useless work.

The numbers move past a stationary point which prints its position upon the paper when the hammer makes its blow. If the number printed is exactly opposite the point, then there has been a whole number of revolutions that minute, but, if the number is above or below it, the fraction indicated by its position can easily be determined. Fig. 3, shows how the fraction of a revolution appears on the record. An exact number, 145 is

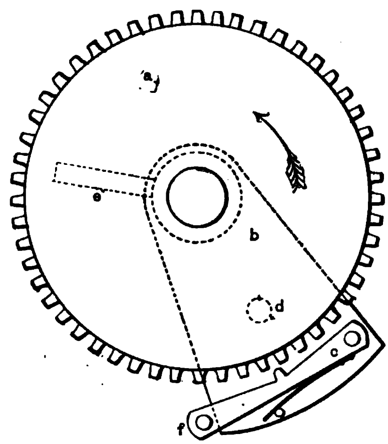


FIG. 2.

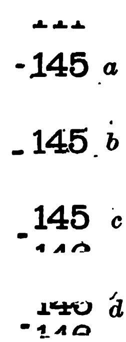


FIG. 3.

shown at (*a*), 145.25 at (*b*) 145.5 at (*c*) and 145.75 at (*d*). Usually enough of either the preceding or following number is printed to easily make it out, and to estimate the fraction, which can be read to one fifth of a revolution. In the original design of this instrument the question of adding a time printing device was considered, for it was thought to be necessary to know just the hour and minute that any part of the record was made. Such an attachment was planned but was not built, for the reason that in nearly all cases the record made will be referred to within the first few minutes after it is printed. If it is desirable to mark a certain point on the record to identify it, the paper can be displaced, and so leave a blank at that point. The paper is fed through at the rate of fifteen and one-fourth inches per hour, and so when necessary this distance can be taken as a unit of length to measure back, and mark off the hours.

One of the ways in which the record obtained is being used, is to find the ratio of its reading to that of the speed of the dynamo being experimented upon, then from the record calculate the speeds for the different parts of the experiment, but this will only answer when the belt is able to drive the machine without slipping.

There is now being built a spindle which is to be tight geared to the main shaft, and which will run ten times faster, or about 1,400 revolutions per minute. This will be for the purpose of testing a student's ability to obtain a correct reading of speed, for the true speed of the spindle will always be ten times the printed speed for that same minute.

The printer which has been described, has been running for about three months and has not failed in any respect to do the work that was expected of it.

THE SECRETARY:—I have blue prints of a chart prepared by Lieut. Parkhurst, of the Watervliet Arsenal, entitled "Diseases of Dynamos." As there was not time to have it put in type I had blue prints made, which are at the service of members who are interested in the subject.

A recess was then taken until 2 P. M.

Upon reassembling for the afternoon session, Mr. Willyoung presented a paper on "A New Method and Apparatus for Measuring Conductivity."

The following paper by A. H. and C. E. Timmerman on "The Heating of Armatures," was presented by Dr. Nichols.

A paper presented at the Tenth General Meeting of the American Institute of Electrical Engineers, New York, May 17, 1893, President Houston in the Chair.

HEATING OF ARMATURES.

BY A. H. AND C. E. TIMMERMAN.

Although much depends upon the heating of armatures, but little attention appears to have been given to the study of a subject of such importance to the engineer in the design of electrical machinery. The losses in an armature caused by the transformation of electrical energy into heat are by no means negligible quantities, as the total amount of energy that we can obtain from a dynamo depends directly upon the temperature of the armature.

What will be the temperature of an armature when a certain amount of electrical energy is transformed into heat within that armature? How much heat will be liberated per square inch per degree rise in temperature above the temperature of the room? Will this quantity be different for different temperatures? Does the field aid in the escape of heat, or does it prevent its radiation? Also, what effect has the peripheral velocity on the amount of heat radiated? etc. Such questions as these seem to have passed almost unnoticed. Professor Harris J. Ryan, of Cornell University, has done some work in this direction, and it was through him that the present series of experiments was undertaken. M. Rechniewski has also determined by experiments on machines, certain values for the heat radiated. In experimenting with dynamos or motors there is, however, some uncertainty as to the amount of heat generated. The loss in the coils of the armature can, of course be accurately calculated. The losses in the iron core can be calculated approximately, but not accurately; for we do not know that the hysteresis loss due to the rotation of a mass of iron in a magnetic field is the same as that due to a variation in the magnetizing force, nor is the eddy current loss easily estimated. Owing to this uncertainty in the actual amount of heat generated, it was decided to construct a special form of apparatus so as to be able to measure accurately the amount of heat generated. Following is a description of the apparatus designed and constructed and employed in this determination. (See Fig. 1.)

The machine consists of a hollow cylinder of brass, mounted on a shaft, so that it can be revolved between two pole-pieces, one end of the cylinder being detachable. In the interior of the cylinder is a coil of german silver wire. The coil is built

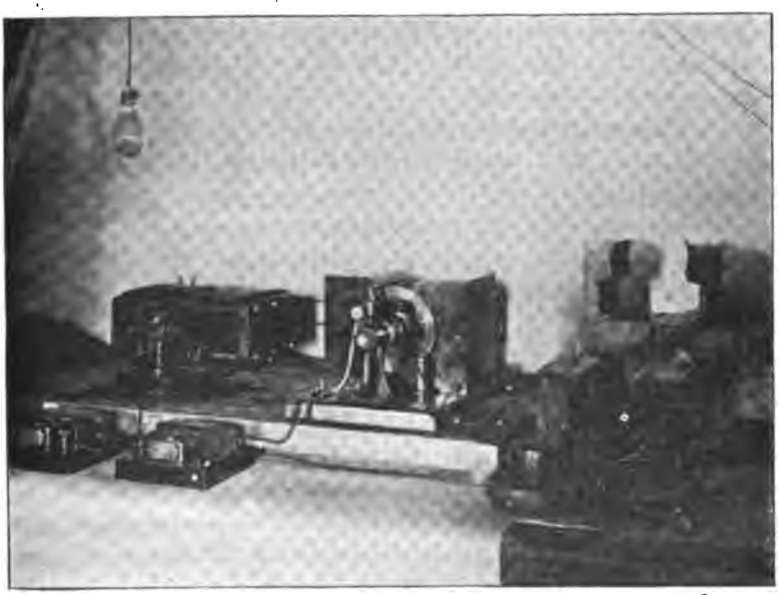


FIG. 1.

up in the following manner:—About 164 feet of german silver wire, No. 17, A. W. G., was covered its entire length with glass beads; the beads were about $\frac{1}{4}''$ in diameter and from $\frac{1}{8}''$ to $\frac{1}{4}''$ in length. The wire, thus insulated, was wound in a coil over a hollow core of glass; the completed coil being $5\frac{3}{8}''$ long and about $5''$ in external diameter. This glass core with the surrounding coil was then slipped over the steel shaft and into place within the cylinder, care being taken to pull one terminal of the coil through a hole in the closed end of the cylinder, this terminal being well insulated from the cylinder. The remaining portion of the interior of the cylinder, not occupied by the coil, was filled with sand. The other terminal of the coil, after being well insulated, was pulled through a hole in the detachable end of the cylinder and this end fastened in place. Mounted on the shaft, but well insulated from it and from the cylinder, are two brass rings, one at each end of the cylinder. To each of these rings is attached one terminal of the german silver wire coil. Brushes for supplying current to the coil within the cylinder bear upon these rings. On the exterior of the cylinder a layer of muslin was wrapped, the muslin being well soaked with shellac. A coil of copper wire (No. 18 A. W. G.) was then wound on the cylinder. Attached to each end of the cylinder, but insulated from it, is a brass ring. The ends of the copper wire coil (which is wound on the exterior of the cylinder) are attached to these rings. Brushes bear on these rings and supply current to the exterior coil. The object of this latter construction is to measure the temperature while the cylinder is revolving.

The temperature of the cylinder at any moment can be measured by finding the resistance of the copper wire coil. The temperature coefficient of the wire being obtained by previous experiment.

A current is sent through the interior coil, and heat is thereby generated; the energy thus transformed into heat being measured by the product of the resistance of the coil and the square of the current flowing through it. Or as the resistance is a variable quantity, the energy is better measured by the product of the current flowing through the coil and the difference of potential at its terminals. By placing an ammeter in the circuit and connecting a voltmeter to the terminals of the coil, we can measure the exact rate at which heat is developed, and thus the total quantity of heat developed during any period of time. The heat produced, raises the temperature of the wire, and thus that of the cylinder.

The method pursued was to make several series of runs; the amount of the cylindrical surface of the cylinder covered by the field being the same for each series, while the speed was varied from zero to about 2000 revolutions per minute, the speed being kept constant during each run. Four speeds were used, viz.—3000 ft. per minute, 2000 ft. per minute, 1000 ft. per minute and a fourth series with the cylinder at rest. Each of these series will show the effect of different peripheral velocities. To show the effect of the field in aiding or in preventing the radiation of heat, four sets of fields were used;—one covering 100% of the cylindrical surface of the armature, another covering 75%, a third covering 50% and a fourth run with no fields at all. A pair of 25% fields were also made but were not used because of the comparatively slight difference between the radiation with no fields and that with 50% fields. The object of the above method is to obtain curves showing the effect of speed, and another set showing the effect of the field in preventing or in aiding the radiation or conduction of heat from the armature surface. To find the relation of the amount of heat radiated to the temperature of the cylinder, the rate at which heat is developed in the cylinder was varied from 100 to 400 watts, the rate being kept as near constant as practicable during each run. An examination of the above will show that a series of runs was made with each pair of pole-pieces and that for each run in the series, a second series was made with different peripheral velocities, and again for each peripheral velocity a series of runs was made, each with a different amount of heat developed per second within the cylinder.

The method used in each run was as follows:—Being unable to obtain the requisite instruments we were compelled to abandon the proposed method of finding the temperature of the cylinder, and substitute a thermometer. While this method is not quite as accurate as that intended when the apparatus was built, nevertheless it gives very good results, as can be seen from the figures obtained, which agree very well when considered together. A current was sent through the coil within the cylinder and heat produced, thus raising the temperature of the cylinder. The rate at which heat was developed was kept constant during each run. To do this it was found necessary to place a resistance that could be easily varied in series with the coil, for, owing to the great range of temperature the resistance of the coil varied greatly. Runs were to be made long enough to allow the cylinder to reach constant temperature; for when the temperature becomes constant the amount of heat radiated must be exactly equal to the amount generated. Hence we have a very accurate method for determining the amount of heat radiated from the surface of the cylinder. However, one great difficulty was encountered; being unable to make use of the coil on the exterior of the cylinder, how were we to ascertain when the cylinder had reached constant temperature? A number of runs were made, and although continued for over three hours the results obtained did not agree as well as was expected. The reason for this disagreement it was thought was, that the runs were not long enough, that is the temperature of the cylinder had not become constant when the cylinder was stopped and the temperature taken. To ensure that the temperature had become constant, it was found best to adopt the following method:—The cylinder was allowed to remain at rest or revolved slowly, and a current sent through it until the temperature had become from 5% to 10% higher than the calculated temperature. The run was then started and continued for a length of time varying from one and a half to three hours, according to the conditions of the run. By the calculated

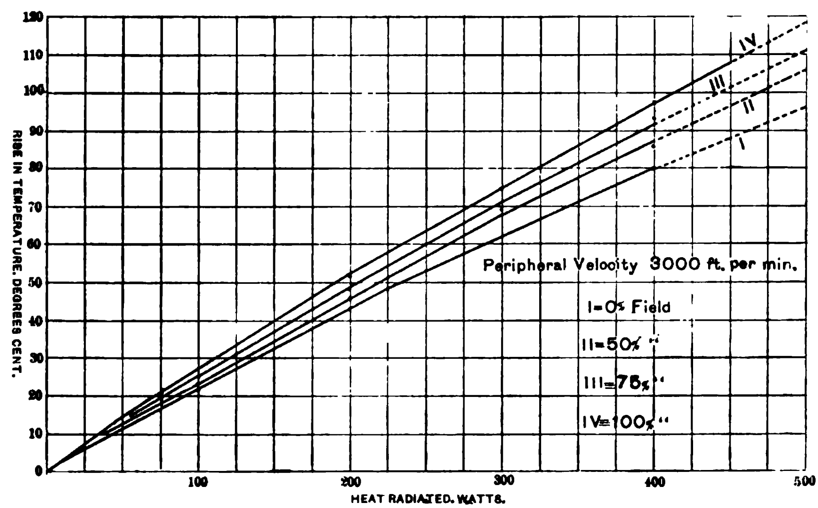


FIG. 2.

temperature is meant that calculated from runs which had been continued long enough to ensure constant temperature, in some cases over four hours. By this method greater accuracy was obtained and we were more certain of our results. To obtain the temperature of the cylinder a thermometer was placed on the cylinder and the bulb covered with a piece of cotton waste, the cylinder being first brought to rest. In order to obtain the temperature as quickly as possible after the cylinder had been brought to rest, it was found advantageous to heat the thermometer to a temperature higher than that at which the cylinder was calculated to be, and allow it to fall to the correct temperature. The temperature of the atmosphere was also taken. The difference between the two is the rise in temperature due to the heat generated within the cylinder.

Following is given a table of the data obtained by pursuing this method:—

		3000 ft. per min.		2000 ft. per min.		1000 ft. per min.		At rest.	
		Rise in Temp. Degrees Centigrade.		Rise in Temp. Degrees Centigrade.		Rise in Temp. Degrees Centigrade.		Rise in Temp. Degrees Centigrade.	
		Actual.	Rise per 100 Watts.	Actual.	Per 100 Watts.	Actual.	Per 100 Watts.	Actual.	Per 100 Watts.
100 per cent. Field.	100 Watts.							50.5	5c.5
	200 "	51.0	25.5	57.0	28.5	65.0	32.5	97.0	48 5
	300 "	75.0	25.0	80.5	27.0	99.0	33.0		
	400 "	97.0	24.25						
75 per cent. Field.	100 Watts.	26.5	26.5			35.5	35.5	55.0	55.0
	200 "	49.0	24.5	56.5	28.25	66.0	33.0	100.0	50.0
	300 "	69.0	23.0	84.0	28.0	96.0	32.0		
	400 "	92.0	23.0	111.0	27.75				
50 per cent. Field.	100 "	25.5	25.5	29.0	29.0	34.5	34.5	55.5	55.5
	200 "	47.0	23.5	52.0	26.0	63.0	31.5	95.0	47.5
	300 "	59.5	20.0	74.5	25.0	92.5	30.75		
	400 "	84 5	21.0	96.0	24.0				
								50 Watts.	
0 per cent. Field.	100 Watts.	23.5	23.5	26.0	26.0			26.0	52.
								93.0	46.5
	200 "	43.5	21.75	47.0	23.5	55.5	27.75	91.0	45 5
	300 "	[illegible]1.0	20.25						
	400 "	80.0	20.0	88.0	22.0	107.5	27.0		

[1]Two sets of curves were plotted from the above data. In each set the actual rise in temperature of the cylinder was plotted along the vertical. For one set Fig. 2. the total amount of heat radiated per second was plotted along the horizontal, while in the second set Fig. 3, the speeds were plotted as abscissæ. Smooth curves were drawn through the points or near to them; the two sets of curves forming a check upon each other aided greatly in drawing the curves correctly.

In the next table are given the corrected results as obtained from the curves, and also the amount of heat radiated per square inch per degree rise in temperature. The latter quantity was obtained by dividing the total amount of heat radiated (in watts) by the product of the rise in temperature and the area of the

1. One group in each set is shown.

surface of the cylinder (180 square inches.) By area of the surface is meant the total area exposed to the atmosphere.

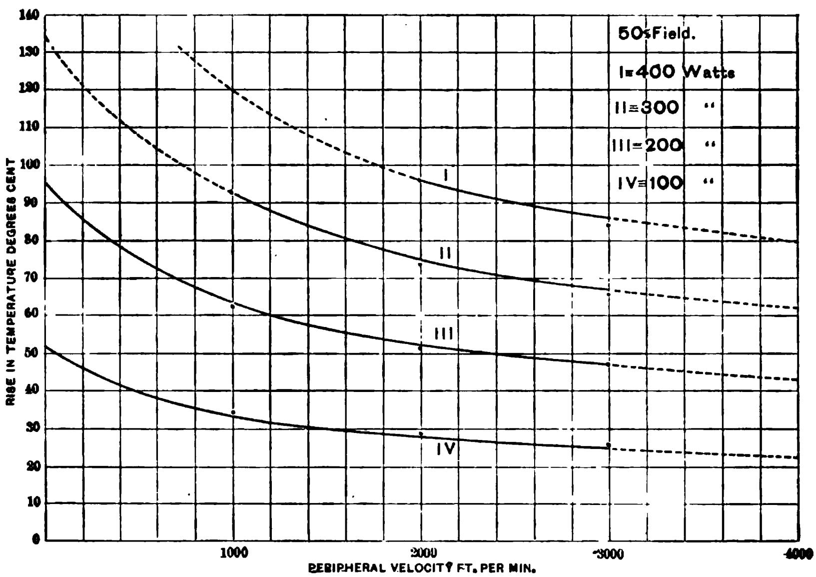


FIG. 3.

		3000 ft per min.		2000 ft. per min		1000 ft. per min.		At rest.	
		Rise in Temp. degrees centigrade.	Watts Radiated per sq. in. per degree rise.	Rise in Temp. degrees centimeter.	Watts Radiated per sq. in. per degree rise in temp.	Rise in Temp. degrees centigrade.	Watts Radiated per sq. in. per degree rise in temp.	Rise in Temp. degrees centigrade.	Watts Radiated per sq. in. per degree rise in temp.
100 per cent. Field.	100 Watts.	27.5	.02020	30.25	.01837	35.0	.01587	53.0	.01048
	200 "	52.0	.02137	57.0	.01947	65.0	.01709	97.0	.01145
	300 "	75.0	.02222	80.5	.02070	94.0	.01773	(136.0)	.01225
	400 "	97.0	.02291	(102.5)	.02168	(121.5)	.01829		
75 per cent. Field.	100 Watts.	26.5	.02096	29.5	.01883	35.5	.01593	55.0	.01010
	200 "	49.5	.02245	56.5	.01967	66.5	.01671	100.0	.01111
	300 "	70.75	.02356	81.5	.02045	96.0	.01736	(141.5)	.01178
	400 "	91.0	.02442	105.0	.02116	(124.5)	.01785		
50 per cent. Field.	100 Watts.	25.0	.02222	28.0	.01984	33.5	.01658	51.5	.01079
	200 "	47.0	.02364	52.5	.02116	63.5	.01750	95.0	.01170
	300 "	67.0	.02488	75.0	.02222	92.5	.01802	(134.0)	.01244
	400 "	86.0	.02584	96.0	.02315	(120.0)	.01852		
0 per cent. Field.	100 Watts.	23.5	.02374	25.0	.02224	29.5	.01883	49.25	.01128
	200 "	43.5	.02554	47.75	.02327	56.5	.01967	92.0	.01208
	300 "	62.25	.02677	69.0	.02415	82.5	.02020	(130.0)	.01282
	400 "	80.0	.02778	88.0	.02525	107.5	.02067		

( ) Indicates that the quantities within are beyond the limit of experiment.

From the data given in the last table three sets of curves were plotted. One set to show the variation due to the temperature, another the effect of speed in the amount of heat radiated, and the third showing the effect of the fields.

We come first to a consideration of the effect of temperature on the amount of heat liberated per square inch per degree rise in temperature. This is shown in Figs. 4, 5, 6 and 7. The curves plotted in these figures show the relation of the temperature (plotted along the vertical), to the amount of heat liberated per square inch per degree rise in temperature (plotted along the horizontal). In Fig 4 are shown four curves, I, II, III and IV obtained with the 100%, 75%, 50% and zero fields respectively. All

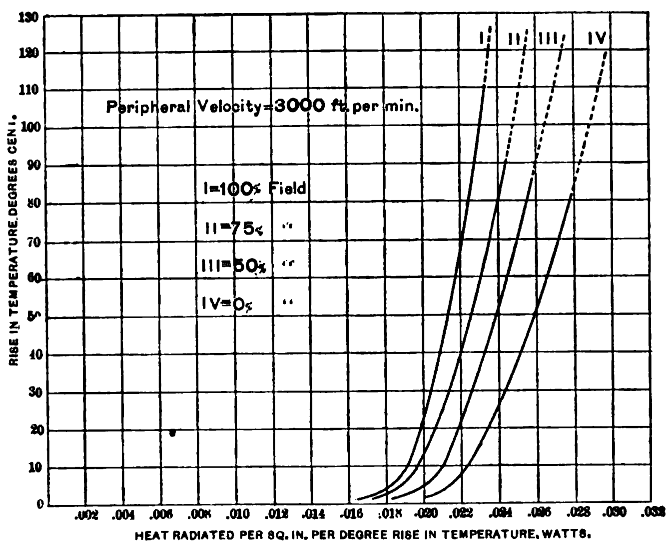


Fig. 4.

are with a peripheral velocity of 3,000 ft. per minute, or about 1895 revolutions per minute. All of these curves are of the same general form, curve I being the steepest, and the others less and less so in the order in which they run. On examination we can very readily see that as the rise in temperature increases, the amount of heat radiated per sq. inch per degree rise in temperature also increases, but the rate of increase diminishes. It seems probable from the curves obtained, that somewhere along them the rate of increase becomes zero; that is the amount of heat liberated per sq. inch per degree rise in temperature becomes constant, and the total amount of heat liberated will thus become proportioned, at the higher temperatures, to the rise in temperature. But this point is probably far beyond the range of temperatures allowable in armatures. In the experiments it was found that the hard rubber, used for insulation, became quite soft at about 140° centigrade.

A study of Figs, 5, 6 and 7 brings us to the same conclusion. In Fig. 5 are plotted curves similar to the preceding, but with a peripheral velocity of 2,000 ft. per minute, which is a speed of 1263 revolutions per minute. In these as in the previous set the amount of heat liberated per sq. inch per degree rise in temperature increases as the temperature, but the rate of increase gradu-

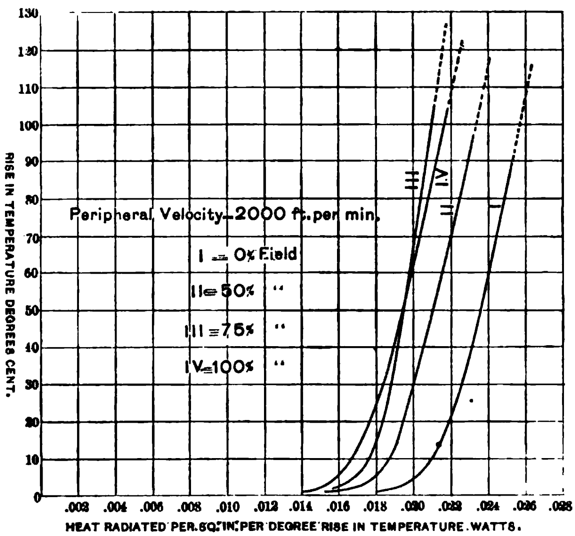


FIG. 5.

ally diminishes. In the previous case we found that the curves became less steep as the amount of the armature covered by the field became less. In this case, however, the curves become very nearly parallel, except in the case of curve IV, which seems to follow rather an opposite law (if it may be so called), that is, it becomes less steep than any of the other curves, less steep even than that plotted for no field.

We also ascertain from this set of curves a fact that at first seems rather remarkable, namely, that curve IV crosses curve III; which means that while at the lower temperatures the amount of heat radiated with the 100 per cent. field, is less than that radiated with the 75 per cent. field, at the same temperature, still as the rise in temperature increases, these two quantities gradually approach each other, become equal, and then the amount of heat liberated with the 100 per cent. field becomes greater than the amount liberated with the 75 per cent. field.

The curves plotted in Fig. 6 are for a peripheral velocity of 1,000 feet. per minute or a speed of 632 revolutions per minute. Here again we notice the same effect of temperature as in the previous case, and also the same peculiarity as regards the position of the curve for the 100 per cent. field. In this case, however, the curve for the 100 per cent. field lies entirely to the right of the curve for the 75 per cent. field, except for very small rises in temperature when it almost coincides with curve III. When we compare the curves shown in Figs. 4, 5 and 6, we find that as the speed is decreased, the difference between the curves for no field and for the 50 per cent. field becomes greater as compared with the difference between the 50 per cent. and 75 per cent. fields.

In Fig. 7 are shown curves similar to the above, but with the cylinder at rest, that is with a zero peripheral velocity. We see from these that the curve for the 100 per cent. field, lies entirely to the right of the curve for the 75 per cent. field, which means that the amount of heat liberated at any temperature, is greater with the former than with the 75 per cent., at the same temperature.

By comparing these four sets of curves (those given in Figs. 4, 5, 6 and 7) we find that as the speed is increased, the amount of heat liberated per square inch per degree rise in temperature is also increased; and that the rate of increase in the amount of heat liberated in any given case becomes smaller as the speed is increased. This is evident from the fact that with the lower peripheral velocities the curves are steeper, or are more nearly perpendicular to the horizontal, at the higher temperatures.

In every one of the preceding cases (16 in number), that is, with every speed and every field, we find that as the rise in temperature increases, the amount of heat liberated per degree rise in temperature also increases; and the rate of increase in the amount of heat liberated decreases as the rise in temperature increases.

Owing to the fact that the smallest amount of heat generated in the cylinder during any run was 100 watts, the curves are not accurate below a corresponding point. The above amount of heat gave under the most favorable condition a rise in temperature of 23.5° centigrade as a minimum. Of course in drawing the curves as shown in Fig. 2 all should go through the origin, but the exact curvatures between the points for 100 watts and the origin are somewhat uncertain. Nevertheless the portions of the curves, below the points mentioned above, must be approximately correct. All the points of the curves below 25.5° to 30° *C.* in Figs. 4, 5, 6 and 7 were plotted by calculating from points obtained from such curves as are shown in Fig. 2.

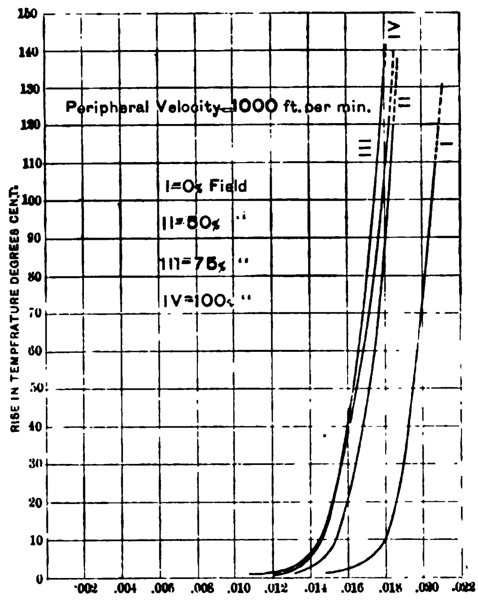


FIG. 6.

It is probable that all of the curves shown in Figs. 4, 5, 6 and 7 pass through the origin; the calculations indicate that they do. But if we calculate the point at which the curve cuts the horizontal axis, we find that it is indeterminate. Because the cylinder does not radiate any heat when its rise in temperature is zero, it does not follow that the amount of heat liberated per degree rise in temperature at that point is zero. The curve cannot cut the vertical axis either above or below the origin, nor the horizontal axis to the left of the origin. If the curve cut the axis above the origin, the cylinder would radiate no heat when its temperature was above that of the atmosphere. It cannot cut the vertical below the origin for a similar reason. And it cannot cut the horizontal to the left of the origin for in that case it must necessarily cut the vertical. There is a curve (perhaps similar in form to those shown) lying below the horizontal and to the right of the vertical axis, showing the effect of temperature on the amount of heat radiated or rather absorbed when the temperature of the cylinder is below that of the atmosphere. This curve must lie to the right of the vertical, because the rate at which heat is radiated must be a positive quantity. And it seems probable that the two branches of the curve form a cusp on the horizontal axis,

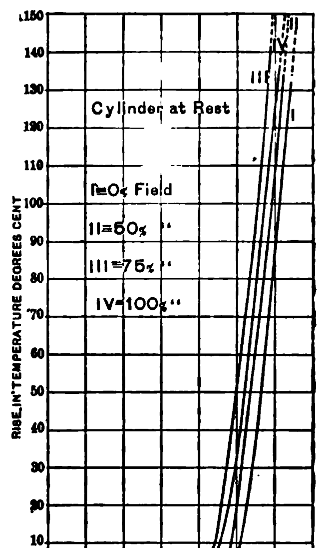


FIG. 7.

but whether this cusp is or is not at the origin we cannot say positively.

These curves also show that if the rise in temperature is small, the amount of heat liberated is also small, and most of the hea- generated, aids in raising the temperature of the cylinder or that of an armature. But as the temperature increases, the amount of heat liberated becomes very much greater, and but a small port tion aids in raising the temperature of the armature. This is one reason for the great length of time necessary to obtain constant temperature.

We come now to a consideration of the effect of the field in the amount of heat radiated or liberated from an armature surface. From the set of curves just discussed, points were taken at 25 degrees rise, at 50 degrees rise, at 75 degrees rise and at 100 degrees rise in temperature and another set of curves plotted from these points with the amount of heat liberated per sq. inch degrees rise in temperature as ordinates, and the per cent. of the cylindrical surface of the armature covered by the poles is plotted along the horizontal. In Fig. 12 are plotted curves for a peripheral velocity of 3000 ft. per minute. Curve I shows the relation between the amount of heat liberated per sq. inch per degree rise in temperature and per cent. of cylinder covered by the poles when the temperature of the cylinder is 100° centigrade above that of the atmosphere. Curves II, III and IV show similar relations when the rise in temperature is 75°, 50° and 25° respectively. All of these curves are similar in character and are very nearly straight lines. In fact for all practical purposes we would not be far wrong in calling them such. Curve II is perhaps of the most value of the four, as a rise of 75° *C.* above the temperature of the dynamo-room may be assumed as a safe rise in temperature of an armature. With no field, we find that the amount of heat liberated per sq. inch per degree rise in temperature is 0.0275 watts and with the 100 per cent. field the heat radiated is only 0.0222 watts per sq. inch per degree rise in temperature; which gives a variation due to the field of about 20% in the amount of heat liberated by the armature. That is, the amount of heat liberated with the 100% field is 20% less than the amount liberated when no field is used. This great variation should be of value to the engineer in the design of electrical machinery.

In Fig. 13 we have a set of curves similar to the last in construction, but entirely different in shape. The peripheral velocity in this case is 2000 feet per minute. We no longer find curves of single curvature, nor do they (except No. IV.) even approach straight lines. In curves III. and IV., plotted for a rise in temperature of 50 degrees and 25 degrees respectively, we find in IV. that the amount of heat liberated with the 100 per cent. field is less than that liberated with the 75 per cent. field, and in curve III. the amount liberated with the 100 per cent. field is just equal to that liberated with the 75 per cent field. And when we come to consider curve No. II., plotted for a rise of 75° C., we find that we have more heat liberated with the 100 per cent. field than with the 75 per cent. field; and similarly with curve I., for a rise of 100° centigrade. The above is apt to give rise to many questions as to the reason for the above results. Following we give what appears to us a satisfactory explanation. We will, however, leave others to judge whether it is or not, giving all the facts of the case to aid in the determination.

Part of the heat liberated from an armature leaves it by radiation, convection currents, etc., and part by radiation and conduc-

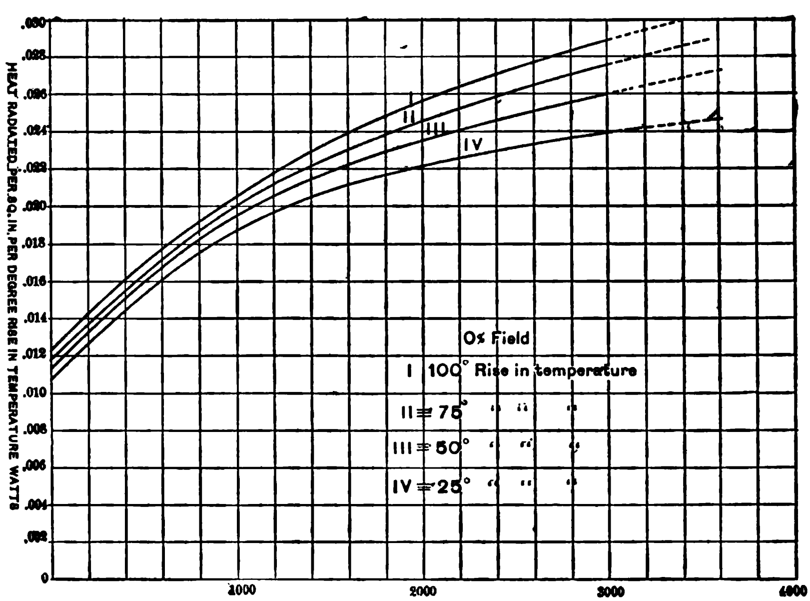


Fig. 8.

tion. The poles or fields play a double part. In the first place, they prevent the escape of heat from the armature surface by destroying convection currents, and by interposing a warm surface very near to the armature; and secondly, they aid in carrying away a certain amount of heat by radiation and conduction. During the first portion of a run, or in starting up a dynamo, the fields being at the temperature of the room, and good conductors of heat aid materially in keeping down the temperature of the armature, by carrying off the heat produced, but as they become warmed up there is no longer the same difference of temperature between the fields and the armature, and they rather prevent than aid in the liberation of heat. The fact that the fields become warm, and even hot, shows very plainly that they carry away a certain portion of heat from the armature. If we increase the external surface of the fields (that not next to the armature), why should they not carry away more heat from the armature? They would certainly radiate more heat and thus present a cooler surface to the armature, and the armature would in turn radiate more heat and its temperature be kept lower. Therefore if we have two pairs of fields, each pair covering an

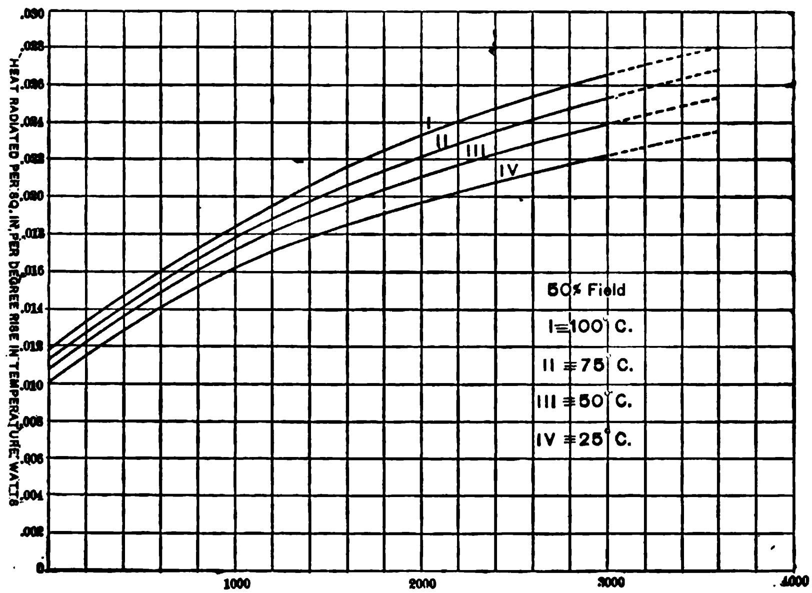


FIG. 9.

equal amount of the armature, but one having a greater radiating surface than the other, we should expect that the armature would radiate more heat when the field having the greater radiating surface was used. If the above is true, and it seems reasonable, there is no reason why we should not be able to construct a field with a radiating surface sufficiently great so that, although it covered more of the armature, it would still keep the armature at a lower temperature than some other field which covered a smaller percentage of the armature. In the experiments made, the external surface of the 100% fields was considerably greater than that of any of the other fields. And the above explanation will apply. It may be asked, why should the 100 per cent. field radiate more heat than the 75 per cent. field at 75° rise and 100° rise in temperature, when at 25° rise the amount of heat radiated with the 100 per cent. field is less than that radiated with the 75 per cent. field? The reason is that when the rise in temperature is greater, the amount of heat radiated per degree rise is greater, and the difference in the amount of heat radiated for a given rise in temperature is less with the 75 per cent. than with the 100 per cent. field.

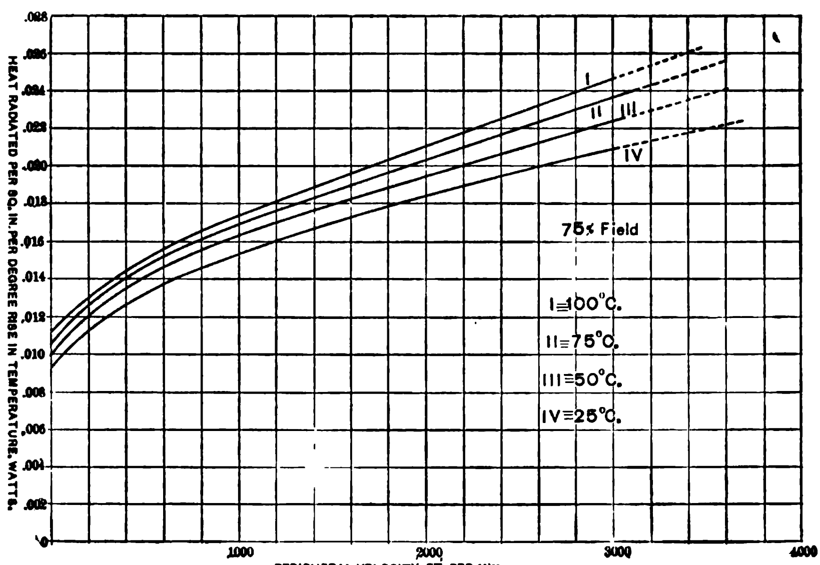


FIG. 10.

When we examine the curves in Fig. 14, plotted for a peripheral velocity of 1000 ft. per min., we again find the same effect of difference of temperature. But in this case all of the curves are of single curvature and concave upward. As in the preceding case the amount of heat liberated at higher rises in temperature is greater for the 100 per cent. than for the 75 per cent. field. The curves are all very nearly parallel to each other and the greatest difference between the amount of heat liberated with the 75 and the 100 per cent. fields is 0.0004 watts per sq. inch per degree rise in temperature.

By an examination of the curves shown in Fig. 15 we find results similar to the above, but the curves have a double curvature, being at first concave downward, and changing gradually until they become concave upward. There is some uncertainty as to the curves plotted in Fig. 15. It was found quite difficult to obtain constant temperature when the cylinder was at rest. The temperature of the atmosphere would sometimes fall several degrees, and the cylinder being as rest, the time necessary for it and the fields to fall to the proper temperature was considerable. For this reason the figures are perhaps somewhat inaccurate.

If we compare now the curves in Figs. 12, 13, 14 and 15 we find that with a peripheral velocity of 3,000 ft. per minute we

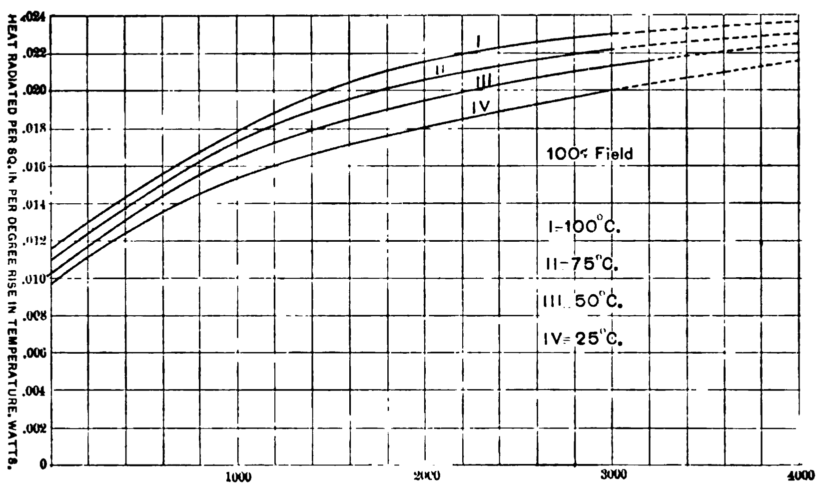


Fig. 11.

have more heat liberated with the 75 per cent. than with the 100 per cent. field, while in all the other cases, at the higher temperatures, the opposite is true. The reason for this is that at the higher peripheral velocities the convection currents are greater and the field plays a smaller part in the amount of heat liberated.

Whether the above explanation holds or not, there is of course the possibility of error in the experimental work. There must necessarily be some inaccuracy when the results depend upon the measurement of so many quantities. An error of two or three degrees would reverse many of the results obtained. If in obtaining the temperature of the cylinder for the runs with the 100 per cent. field an error was made, and if the temperature actu ally found was but a few degrees too low, this would account for the peculiarity observed

We come next to the discussion of the effect of speed on the amount of heat radiated. The points used in plotting the last set of curves were used to plot another set, in which the amount of heat liberated per sq. inch per degree rise in temperature is plotted along the vertical and the peripheral velocity in ft. per minute along the horizontal. These curves are shown in Figs. 8, 9, 10 and 11. In Fig. 8 are plotted four curves, all from data obtained with the zero field. Curve I shows the variation in the amount of heat liberated per degree rise in temperature, due to a variation of the speed, for a rise in temperature of 100 degrees *C.* Curve II, III and IV show similar variations for rises in temperature of 75°, 50° and 25° centigrade respectively. A study of these curves shows that at the lower peripheral velocities, the amount of heat liberated per sq. inch per degree rise in temperature varies considerably for a slight increase in speed, but this rate of increase in the amount of heat liberated per sq. inch per degree rise in temperature falls off rather rapidly as the speed increases. Taking curve No. I, for instance, which shows the amount of heat liberated when the rise in temperature is 100° *C.* we find that the amount of heat liberated per sq. inch per degree rise in temperature is 0.0122 watts when the speed is zero. At 1,000 feet per minute the amount radiated becomes 0.0205 watts, an increase of 68 per cent. on the amount liberated at zero speed. At two thousand ft. per minute this quantity becomes 0.0257 watts, an increase of 27 per cent. of that liberated at 1,000 ft., and an increase of nearly 111 per cent. above the amount liberated at zero speed. Again at 3,000 feet we find an increase of only 12 per cent. over the amount liberated at 2,000 ft. per minute. When we make a similar study of No. IV we find that the increase in the amount of heat liberated is nearly 75 per cent. from zero to 1,000 ft. per minute. But this falls to an increase of only about 19 per cent. between 1,000 and 2,000 ft. per minute and drops still lower, to an increase of less than 8 per cent. between 2,000 ft. and 3,000 ft. per minute. From the above we find the total *increase* in the amount of heat liberated to be in the first case 136 per cent., and in the second the total *increase* in the amount of heat liberated between zero speed and 3,000 feet per min. is 123 per cent. The intermediate cases, curves III and II, give values between these two. Making a similar calculation for the curves in Fig. 9, obtained with the 50 per cent. field, we find that the increase in the amount of heat liberated per degree rise in temperature when the temperature is 100° *C.*, is between zero and 1,000 ft. per min., 56 per cent. between 1,000 ft. and 2,000 ft. per min., 26 per cent. and between 2,000 and 3,000 ft. per min., the increase is only 14 per cent., giving a total *increase* between zero and 3,000 ft. per min. of 125 per cent. And when the rise in temperature is 25° centigrade the total *increase* in the amount of heat liberated is 120 per cent. (between zero

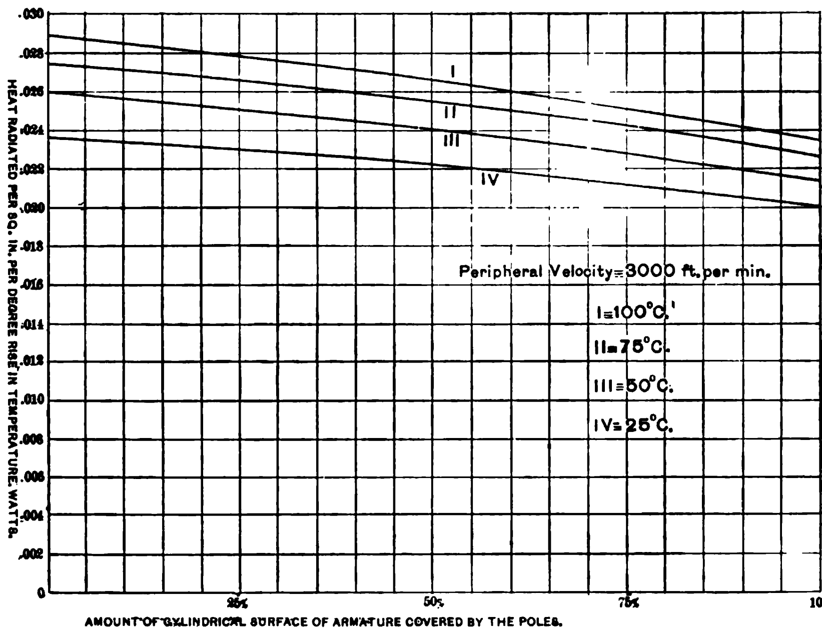


FIG. 12.

and 3,000 ft.) which is divided up as follows:—60 per cent. between zero and 1,000 ft., 22 per cent. between 1,000 and 2,000 ft., and 13 per cent. between 2,000 and 3,000 ft. per min. A similar study of the curves for the 75 per cent. field, shown in Fig. 10. gives us the following results:—When the rise in temperature is 100° *C.*, the total *increase* in the amount of heat liberated between zero speed and 3,000 ft. per minute per degree rise in temperature is 123 per cent., which increase is divided up in the following way:—Between zero and 1,000 ft. the increase is 59 per cent., between 1,000 ft. and 2,000 ft. per min. the increase is but 22 per cent., and between 2,000 ft. and 3,000 ft. the increase falls to 17 per cent. When the rise in temperature is 25 degrees centigrade the increase in the amount of heat radiated per sq. inch per degree rise in temperature is 126 per cent. between zero and 3,000 ft. per min. This increase is made up as follows:—From zero to 1,000 ft., an increase of 66 per cent.; from 1,000 ft. to 2.000 ft. per min., 20 per cent., and from 2,000 ft. to 3,000 ft. per min., an increase of but 14 per cent. in the amount of heat radiated per degree rise in temperature. We find similar results from the curves plotted for the 100 per cent. field

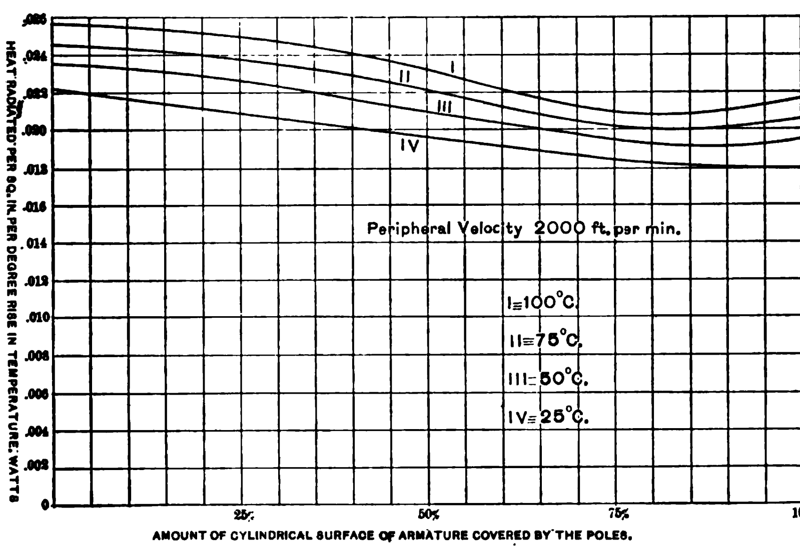


Fig. 13.

and shown in Fig. 11. When the rise in temperature is 100° *C.* we have an increase, between zero and 3,000 ft. per min. of 100 per cent. in the amount of heat liberated per sq. inch per degree rise in temperature; the increase at 3,000 ft. per min. is but 60 per cent. of the total amount liberated at 2,000 ft. per min. When the rise in temperature is 25° *C.* we find an increase of 106 per cent. between zero and 3,000 ft., the increase at 3,000 ft. being only 11 per cent. of the total amount per degree rise liberated at 2,000 ft. per min. In these curves also we notice the rapid decrease of the rate of increase in the amount of heat liberated per degree rise in temperature as the speed increases.

In the apparatus used, the area of the curved surface of the cylinder is 123 sq. inches, its radius being $3\frac{1}{32}$ inches and its length $6\frac{7}{16}$ inches. The area of the two ends is 57 sq. inches, with a mean radius of about two inches. When the peripheral velocity of the cylindrical surface of the cylinder is 3000 ft. per min., the mean velocity of the ends is about 2000 ft. per min. As the heat liberated at 3000 ft. is considerably greater than that liberated at 2000 ft., the peripheral velocities given in the curves are not the true peripheral velocities of the entire surface of the cylinder, or in other words, the amount of heat liberated would be greater if all portions of the cylinder revolved at the same peripheral velocity. We can, however, by a few trials, determine

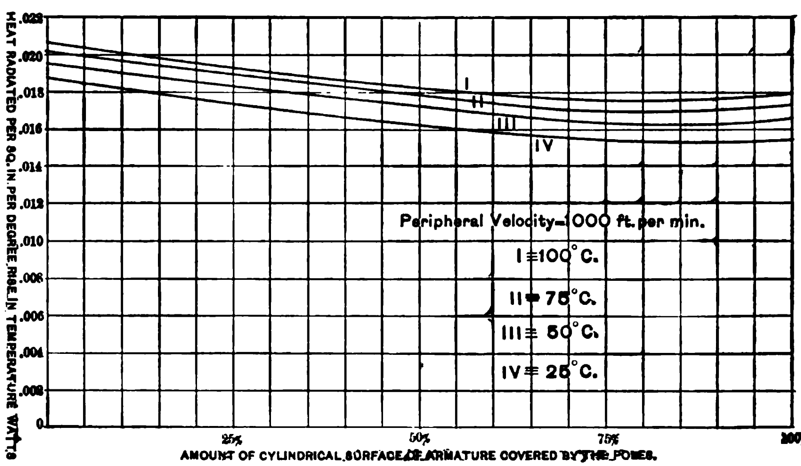


FIG. 14.

what would be the amount of heat liberated for any given case if every point of the cylinder revolved at the same peripheral velocity. We will make this determination for one case; that with no field when the rise in temperature is 100° *C.*

For this case the amount of heat liberated per sq. inch per degree rise in temperature, for 3000 ft. per min. is 0.0288 watts. and that liberated at 2000 ft. is 0.0257 watts, a difference of 12 per cent. on the latter quantity. If we multiply 0.0288 by 123. the area of the curved surface, the total amount of heat liberated by this portion of the cylinder per degree rise in temperature is 3.54 watts, and similarly that liberated by the ends whose peripheral velocity is 2000 ft. per min. is $0.257 \times 57 = 1.46$ watts, and the total heat liberated by the cylinder per degree rise in temperature is 5.00 watts. Dividing this quantity by 180 (the total area of the cylinder in sq. inches), we have for the heat radiated per sq. inch per degree rise, 0.02782 watts. This quantity, as was to be expected, is less than that actually found. Subtracting 0.02782 from 0.0288 we have 0.00098 watts. If we now increase the quantity 0.0288 by this amount, and the quantity 0.0257 by $\frac{100}{112}$ of the same amount, we ought to get two values very nearly correct for their respective speeds. The reason that the quantity 0.0257 is increased by $\frac{100}{112}$ of 0.00098 is that the amount liberated per degree rise in temperature is 12 per cent. greater at 3000 ft. per min. than at 2000 ft. Using the figures found above, we will now apply them to the cylinder

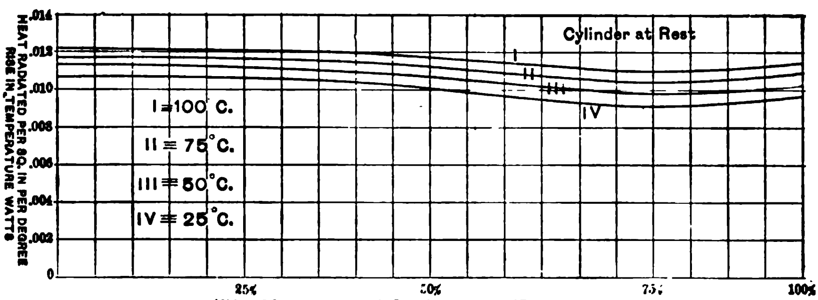


FIG. 15.

used, and we find that the amount of heat liberated at

3000 ft. is 0.02978 watts per sq. in. per degree rise,
and at 2000 " " 0.02668 " " " " " " "

The heat liberated from the curved surface =

$$0.02978 \times 123 = 3.663 \text{ watts},$$

and from the ends of cylinder =

$$0.02668 \times 57 = 1.521 \text{ watts},$$

or a total radiation from the cylinder of 5.184 watts per degree rise in temperature. And the radiation per sq. inch =

$$\frac{5.184}{180} = 0.0288,$$

the figure obtained by experiment.

Hence the amount of heat liberated per degree rise in temperature from a surface, every portion of which is revolving at

3000 ft. per min. is 0.02978 watts per sq. inch, and at
2000 " " " " 0.02668 " " " "

From this we see that under the most favorable conditions, the amount of heat liberated from a drum armature of a bipolar machine is 0.03 watts per sq. inch per degree rise in temperature. The increase at 3000 ft., due to the above correction, is about 3.5 per cent.

It was our intention to find if possible the effect of the air space between the armature surface and the pole faces but the great amount of time necessary for the determination prevented us from attempting it. The air space in the apparatus used was slightly more than $\frac{3}{32}$ of an inch. But it is probable that with an increased air space the effect of the field in preventing the radiation of heat would be less marked.

To sum up the results obtained we would state:

First—That an increase in the temperature of the armature causes an increased radiation of heat per degree rise in temperature, but that the rate of increase diminishes as the temperature increases, and that an increase in the amount of heat generated in an armature, increases the temperature of the armature, but less than proportionately.

Secondly—That the effect of the field is to prevent the radiation of heat. As the amount of the armature covered by the field is increased, the amount of heat radiated per degree rise in temperature becomes less. If not true for all speeds it is for the higher speeds at least. And it has been found to be true in *every* case where the rise in temperature was less than 50° *C.*

Thirdly—That, as the peripheral velocity is increased, the amount of heat liberated per degree rise in temperature is increased, but the rate of increase becomes less as we increase the peripheral velocity The effect of speed is very marked and the above law was found to hold in every case.

Fourthly—That the highest radiation that can be obtained from a drum armature of the ordinary construction ([1]and of the size used) is about 0.03 watts per sq. in. per degree rise in temperature, when that rise in temperature is not over 100° centigrade.

Physical Laboratory of Cornell University,
May 13, 1893.

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1. It is probable that in larger machines the amount of heat liberated would be greater in almost every case, than the figures obtained. The reason for this supposition is that each sq. in. of surface of the armature of a larger dynamo presents very much more than one sq. in. of radiating surface. This is due to the fact that the conductors are larger. Besides presenting a larger radiating surface the interstices between the wires cause greater convection currents, and thus give an additional means of escape for the heat generated.

Business Proceedings.

The President:—The final report of the tellers, I believe, is now ready.

Mr. T. C. Martin:—Mr. President and Fellow Members: The statistical return appended to our report gives the following results. I might say that we have the results inclusive of every vote counted, but I do not think it will be necessary to go into details, as some fifty or sixty members have been voted for and the votes run down in several cases as low as one vote, and I think it would be more amusing to read that in print than to have me read it.

Mr. Martin then read the following report:

Tellers' Report.

Of the ballots received there were counted	427
Rejected—	
Unidentified	24
Duplicates, including one change without stating what he wanted	14
Not on list	1
Total of ballots received	466

FOR PRESIDENT.

Edwin J. Houston	236	Thomas A. Edison	2
Thomas D. Lockwood	145	William E. Geyer	2
Elihu Thomson	9	G. A. Hamilton	2
Nikola Tesla	8	A. E. Kennelly	2
Edward L. Nichols	7	Frank J. Sprague	2
Louis Duncan	4	Charles F. Brush	1
William A. Anthony	3	Carl Hering	1
William J. Hammer	3		

FOR VICE-PRESIDENTS.

H. Ward Leonard	332	Park Benjamin	4
P. B. Delany	340	Leo Daft	4
William Wallace	220	Wm. E. Geyer	4
Edwin J. Houston	138	E. Wilbur Rice, Jr.	4
Nikola Tesla	27	J. H. Vail	4
A. E. Kennelly	25	Chas. E. Emery	3
G. A. Hamilton	15	F. B. Herzog	3
Louis Duncan	13	E. P. Roberts	3
C. O. Mailloux	11	Louis Bell	2
Harris J. Ryan	11	Jas. E. Denton	2
T. C. Martin	10	Charles Cuttriss	2
F. B. Crocker	9	C. T. Hutchinson	2
Jas. I. Ayer	8	A. L. Rohrer	2
Chas. R. Cross	8	F. R. Upton	2
Edward L. Nichols	8	C. F. Brackett	1
Townsend Wolcott	8	Alfred S. Brown	1
C. J. Field	6	J. J. Carty	1
Elisha Gray	6	J. C. Chamberlain	1
Franklin L. Pope	6	E. A. Colby	1
M. I. Pupin	6	S. D. Field	1
H. G. Reist	6	J. W. Howell	1
Joseph Wetzler	6	F. W. Jones	1
O. B. Shallenberger	5	L. Stieringer	1
C. P. Steinmetz	5		

FOR MANAGERS.

Harris J. Ryan	374	Albert Schmid	3
Charles Hewitt	365	C. A. Terry	3
J. J. Carty	358	W. D. Weaver	3
Wm. J. Hammer	358	Gilbert Wilkes	3
F. B. Crocker	15	Alex. J. Wurts	3
Carl Hering	12	Brown Ayres	2
H. S. Rodgers	12	E. M. Barton	2
Wm. Stanley, Jr.	10	Park Benjamin	2
E. E. Higgins	9	G. W. Blodgett	2
T. C. Martin	9	Alfred S. Brown	2
H. F. Parshall	9	J. B. Cahoon	2
J. W. Lattig	8	Charles Cuttriss	2
C. P. Steinmetz	8	C. E. Emery	2
Franklin L. Pope	7	F. W. Jones	2
Joseph Wetzler	7	H. Ward Leonard	2
S. S. Wheeler	7	E. L. Nichols	2
Wm. A. Anthony	6	Geo. M. Phelps	2
C. F. Chandler	6	Chas. D. Shain	2
C. T. Hutchinson	6	L. Stieringer	2
Edward Weston	6	E. P. Thompson	2
Dugald C. Jackson	5	Townsend Wolcott	2
G. W. Davenport	4	Jas. I. Ayer	1
G. A. Hamilton	4	E. Berliner	1
T. D. Lockwood	4	E. A. Colby	1
Wm. Maver, Jr.	4	George Cutter	1
Ernest Merritt	4	Richard O. Heinrich	1
E. F. Peck	4	C. O. Mailloux	1
J. H. Vail	4	Wm. D. Marks	1
C. J. Field	3	J. W. Howell	1
S. D. Field	3	Samuel Sheldon	1
Wm. E. Geyer	3	Geo. D. Shepardson	1
W. J. Jenks	3	Elmer A. Sperry	1
F. A. Pickernell	3	Edw. G. Waters	1
A. L. Rohrer	3		

FOR TREASURER.

George M. Phelps	405	F. B. Crocker	5
Edward Weston	10	T. C. Martin	5

REMARKS.

The irregularities observed were simply innumerable, but every allowance was made where the voter had given any kind of intelligible indication as to what he wanted.

A few of the more striking irregularities deserve note as a guidance to needed reforms in the present system, which has undergone its first trial triumphantly in calling out so large a vote out of a membership of 673.

Several ballots indicate their preference by stars, without cancelling the names not wished for, on the straight ticket. The stars were counted.

One voter transferred the whole straight ballot in writing to another sheet of paper, and enclosed both in the envelope

Many ballots had only one name left under each office, the idea being evidently that only one nominee could be chosen.

Per contra, one member voted 10 times for 3 managers; and another member voted for 6 vice-presidents and 10 managers.

In several cases the same nominee was voted for as vice-president and president, or vice-president and manager. In these cases the votes were counted for the higher office.

It certainly deserves notice that 13 members, among whom were found professors in colleges, voted twice; and that in one instance the two opposing ballots were enclosed in the one envelope. It will have been noted that these ballots were all of necessity rejected.

The tellers are glad that there were only two ballots in the field. The work would otherwise have involved much greater delay under the new system. They would recommend that the Election Rules be so revised as to limit the range of choice in the first nominations to the Council, so that mere complimentary votes by the opportunity now given of selection from so miscellaneous a list, may be avoided.

The tellers would add that the figures in the returns are fortunately so wide apart as to be unaffected by any of the decisions which they ventured to make.

T. C. MARTIN,
G. A. HAMILTON,
Tellers.

New York City May 17th, 1893.

On motion of Mr. Hammer the report of the tellers was accepted and the tellers were discharged with thanks.

MR. MARTIN:—Before we leave this matter I would like to offer a resolution that a committee be appointed on the revision of the rules applying to elections. Dr. Herzog informed me of his willingness to serve again in carrying out certain modifications of the present rule which I think will be necessary, although I think the rule has in general worked well.

THE PRESIDENT:—The Chairman would think it the part of wisdom to appoint the old Committee. Will the INSTITUTE take that question up?

THE SECRETARY:—I do not venture to speak for Mr. Upton, as he has been quite ill for the last ten days, but it appears to me there would be no question about his willingness to serve when he can be communicated with, and I think it would be entirely competent, if he should not be able to serve, that some person be substituted in his place. The Committee will have several months to work on the matter.

THE PRESIDENT:—What is your motion, Mr. Martin?

MR. MARTIN:—That a Committee be appointed on the revision of the rule pertaining to elections.

THE PRESIDENT:—Unless the INSTITUTE wish otherwise the chair will reappoint the old Committee. The old Committee is reappointed.

MR. T. C. MARTIN reported the following resolution:

RESOLVED. That the sincere thanks of the officers and members of the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS are hereby tendered to Mrs. Charles A. Seeley, of Mount Vernon, N. Y., for her kind remembrance of the organization, in the presentation to the society of an early dynamo electric machine as completed by her husband in the year 1867.

Signed, T. COMMERFORD MARTIN, Chairman.
A. E. KENNELLY,
E. T. BIRDSALL,
Committee.

THE SECRETARY:—I move that the report be accepted with thanks, and the Committee discharged.

THE PRESDENT:—If there is no objection it is so ordered.

Mr. Hammer has asked the chair for two or three minutes to make an announcement.

Mr. Hammer:—As representing the Committee of Ways and Means in charge of matters pertaining to the World's Fair and the Institute's official headquarters there, I wish to make a brief announcement. The Committee has already secured the assurance of our worthy President, of Prof. Thomson, Mr. Edison, Mr. Edward Weston, Mr. Tesla, Prof. Dolbear, Prof. Gray, Mr. Berliner and others, of their co-operation; and we expect to receive from these gentlemen, and have received from some of them already, interesting exhibits of a scientific and historical character. The Committee has also communicated with Prof. Ferraris with the object of securing the set of apparatus with which he demonstrated his discovery of the rotary field by alternating currents of different phase. My object in speaking of this at present is, to assure the Institute that the Committee is taking active measures to have the headquarters made as interesting as possible, and also to secure from any of the gentlemen who are present at this our Annual Meeting any objects of great scientific and historical interest which can be placed on exhibition.

The President:—We have now reached the final subject that is to be discussed in this room preparatory to adjournment to Columbia College. It is the discussion of the provisional programme of the World's Electrical Congress prepared by the Institute's Committee.

DISCUSSION OF ELECTRICAL CONGRESS PROGRAMME.

MR. A. E. KENNELLY:—Mr. President and Gentlemen: The Chairman of the Sub-Committe on Provisional Programme for the congress work being unavoidably absent, I may be permitted to offer a few remarks on the subject. There will be, as we all know, a large amount of work before the Electrical Congress this year when it meets in Chicago. It will be work of an unusually important character, for the reason that units are in demand which must either follow or depart from the course of all the practical units hitherto created, and any departure will mean a certain variation in the methods of dealing with electrical units and electricity as compared with the methods of dealing with magnetic units or magnetism. It is, therefore, with very considerable interest that we approach the problem which the Electrical Congress at Chicago will have in hand, and it is important that we should clearly understand among ourselves what the leading questions at issue may be, and how far the results that may be attained by the Congress will interest us individually, as well as the INSTITUTE as a body. It is unnecessary for me to recapitulate the work that was done so excellently and faithfully by the Committee of the British Association for the Advancement of Science, commencing its labors in 1861 and terminating them practically with the endorsement of the Congress of 1889. There can be little doubt that if that Committee were to undertake now the work that it undertook in its day, it would introduce certain modifications which would have the result of making a better and more completely coherent system of units than that we now possess. But inasmuch as we cannot change the past, and the past is extremely creditable, it is useless to speculate upon what might have been, and it is desirable to give our attention to what actually is. We have difficulties in the magnetic circuit of two kinds. One kind is that we cannot use the ampere-turn directly as the practical unit of M. M. F. without a numerical coefficient. We ought to be able to use ampere-turns on dynamos as the units of magneto-motive force, if the purposes of convenience in that unit alone were considered. We cannot consider the convenience of one unit alone, because we have to consider the whole system of units, and the Sub-Committee of this INSTITUTE has submitted the view, one I think in very general commendation, that we cannot employ ampere-turns directly, but we have to employ the awkward numerical factor of 12.57-4 π with it. There have been several schemes suggested, excellent ones in their way, for obviating this unfortunate necessity, and for giving us ampere-turns directly as the magneto-motive force and throwing this factor into some position in the system where it may, in the opinion of the parties promulgating the idea, be of less objection and disadvantage. Each of those questions must be considered impartially upon its own basis. But broadly, they all mean a departure from the existing system of practical units, the volt, ohm, ampere, etc.; and, therefore, if we are to have what may be for the magnetic circuit, what may be for dynamos and motors, a more convenient system of practical units than the one which is immediately suggested, it can only be at the expense of a departure from the old units, and that breach must necessarily be an awkward hiatus for all computations of a practical nature. The second difficulty is in the magnitude of the units. The practical system started out with a clearr field before it and made the ohm a practical unit, a reasonable quantity and a very practical one, and I think it meets with universal commendation and has served its purpose remarkably well. The volt was next issued, which was another arbitrary multiple of the fundamental C. G. S. unit approximating to the E. M. F. of one Daniell cell. The volt also has served its purpose well although the immediate combination of the volt and ohm, the ampere, at one-tenth of the C. G. S. unit of current, is a disadvantage. However, I do not think that any of us have suffered great inconvenience from the employment of the practical system at the present time. The employment of a concrete and coherent system, forces upon us practical units in the magnetic circuit that are of very awkward magnitudes. The unit of flux becomes one hundred million C. G. S. lines, and that is a very large quantity, as we do not have dynamos with so much flux in them. Also an intensity such as the earth's field, for example, became extremely difficult to designate on the suggested practical system. However, we have to bear in mind that all amendments of the practical system involve —while they may give us a certain convenience perhaps—a departure from the existing fundamental rules that are part and parcel of the absolute system of measurment, and that for this advantage of local convenience we have to assume an expense in work of memory. The four units—gilbert, weber, oersted and gauss—have been suggested by the Committee of this INSTITUTE as those best fitted to concur with the original ampere-volt-ohm system, and it is in the hope of being able to obtain a thoroughly practical system of units that we can look for the advantages which the forthcoming Electrical Congress will have in store for us.

THE SECRETARY:—It was one of the objects in preparing this provisional programme that it should be published in advance and given wide circulation in order to elicit certain criticisms which it might give to rise to, and there have been criticisms in regard to it, more especially in Europe, and a few communications in our own journals. I noticed one in an electrical paper last week by Professor Rosebrugh of Canada, and I understood that this Committee had undertaken to prepare at some time a recapitulation of these suggestions for presentation. I would like to inquire of Mr. Kennelly, who represents the Committee, whether he has taken note of those suggestions and whether he has any remarks to bring forward at this time in regard to them.

Mr. Kennelly :—I beg to say that the members of this Sub-Committee have kept account of all comments, criticisms, and suggestions that have appeared in the scientific and technical press in relation to the question of congress work. None of the criticisms which have been made up to the present time call, it seems to us, for immediate consideration. In regard to units, they all rest upon that distinction, in regard to the magnetic system of units, between the convenience of dynamo work, and the convenience of having a system which shall be a coherent one. It has been the intention of the Sub-Committee to draw up at a sufficiently mature period all the criticisms and discussion that have appeared—a recapitulation of all the considerations that have been offered, and the Committee has felt that it is premature at this time so to present them.

Mr. Lockwood :—I understood Mr. Kennelly to say that several criticisms offered dealt largely with the points which he mentions. Now there are one or two very minor points in the form of definitions which were reported to the Committee, to which I took the liberty to make exception verbally to the Committee, and later I sent a short communication to one of the electrical papers. I called attention to what seemed to me to be rather an odd definition of the north pole of the magnet, the Committee having proposed to define it as that which seeks the geographic north pole; and the south as that which seeks the geographic south pole. My criticism is, that the poles of a magnet, assuming it to be one which is pivoted as a needle poised to swing freely in a horizontal plane, does not point to or seek the geographic north pole, and does not lie in the geographic meridian, but seeks the magnetic pole which circles in a small orbit around the geographic north pole. It seems to me if we are going to define the polarity of magnets at all we should define them correctly. To that end I offered the following words as a substitution :

> "The north pole of a magnet to be defined as being that which in a magnet poised to swing freely in a horizontal plane seeks the magnetic pole in the direction of the terrestrial north, and the south pole that which seeks the magnetic pole, near the terrestrial south."

I do not know whether the Institute considers that a material point, but whether it be material or immaterial, it seems to me we should seek correctness, just as carefully as the magnet seeks the pole that it wants to find.

On the same page there was a proposal to substitute the term "continuous current" for "direct current." I took exception to that, for the reason that we may have continuous and discontinuous direct currents, and continuous and discontinuous alternating currents. I have known many currents which had one direction, but which were about as discontinuous as anything can well be. It seems to me, therefore, that the term would be decidedly misleading. We may say a current of one direction, or we may say a singly directed current, or we may say a direct current, but when we should say or assume that a direct current is necessarily continuous, I certainly fail to see. I think perhaps that it might be possible to find a better term than "direct current," but I should say "direct current" was a better term to describe what it is intended to describe, than "continuous current."

I object also on my own personal account to that part of the programme which states, "the value of the practical unit of illumination to be a bougie decimale at the distance of one metre. The bougie decimale is the unit of light and power already established, and is practically equal to one English standard candle." And on page 5: "For the practical unit of illumination defined above, the name 'bougie-metre.'" Now that is a pretty long term, and it is a hyphened term. As long as we can have words which are not hyphened, I think we should use them. As we have honored nearly every great inventor and discoverer, it would seem to me, as I suggested when I wrote the letter to which I have referred, that the first part of this being already established, that is to say the definition of the practical unit of illumination, ought properly to stand; but for the second part, that copied from page 5, I would substitute the following: "For the practical unit of illumination defined above, the name 'Davy.'" That name is, I conceive, much more appropriate than "bougie-metre," which is very clumsy, and would be a good deal of a bogey in the way of general use.

MR. KENNELLY:—Mr. Chairman and Gentlemen, Mr. Lockwood's criticisms are three. The first deals with a matter of fact, and the last two deal with matters of opinion. I think we must all admit that his substitution of the term "terrestrial" north pole for "geographic" north pole is felicitous, and I think that would be accepted in preference. As regards the second criticism, it is a matter of great dispute as to whether a "straightforward" current should be called "direct" or should be called "continuous." "Many men, many minds," and I think that perhaps neither of those terms, "continuous" and "direct," may be employed; but if Mr. Lockwood will only suggest to us some third or fourth term he may be giving us a great benefit.

In regard to the unit of light, there are so many new names to be introduced and brought forward this year, and there is so much objection to the creation of standards which are themselves barely admissible as definitions, that however advisable it might be to have the well remembered and honored name of Davy placed on the list of the illustrious founders of the science, still I think there would be considerable difficulty in introducing it at the present time. However, that is a matter of opinion, and I should certainly hope that Mr. Lockwood's suggestion might receive all the consideration that it merits.

MR. LOCKWOOD:—I think the word the gentleman used just now—straightforward—is about as good a name for the direct current as could well be devised, unless we should shorten it by simply using the word "straight," and the gentleman's suggestion appears to me a very appropriate one.

PROF. NICHOLS:—May I say a word as to the standard of illumination? It would seem to me unfortunate to assume that we are likely to have a standard, between now and the meeting of the Electrical Congress in Chicago, for illumination, that is worthy of the name of Davy or of the name of any one. I think the time is not probably very far distant when we shall be able to define illumination in terms of the C. G. S. system, in other words, to actually get within our grasp a candle-light equivalent which will express what we all desire, and which will be capable of interpretation in the sense that having defined it, we shall be able to make secondary standards which will produce the given amount of light, so that we may know what it is that we are measuring. That time has not yet come, but the indications are that it is not far distant. Would it not be better then, before establishing and fixing this matter, to simply regard it as tentative, to adopt temporarily whatever seems most desirable to the Congress, and to let the permanent naming of it wait until the time when we are able to define our light unit in a system which is a part of the great system which we are hoping to do something towards the completion of in Chicago this summer?

THE PRESIDENT:—I would like to ask Mr. Lockwood whether his suggestions of the north pole of a magnet would not bring about the very obscurity that I understood the Committee wished to avoid. If you define that to be the north pole of the magnet which points to the earth's magnetic north pole, then you make this pole of the magnet to be the south magnetic pole, I would not know then whether you meant the end which points to the earth's geographic north pole or its south pole. Whereas, if you say that the north pole of a magnet is that end which points to the terrestrial or nearly to the terrestrial north pole, as the Committee suggests; then it would be very simple. If the north pole of the magnet, out from which the assumed lines of force flow, points approximately to the earth's geographic north pole, then that pole must be the south magnetic pole. Now, frankly I would not know from your definition which you meant. I would rather suppose, if it were without any explanation, that you meant the true south pole of the needle.

MR. LOCKWOOD:—I would like it to be known to you and to the other gentlemen present, that the definition is not mine. I am only responsible for that amendment. I imagine that the Committee of this INSTITUTE has really considered the point concerning which you inquire. I believe you asked me whether it would not be productive of much ambiguity. In answer to that, I would say, that I think it would, and I also think that an absolutely correct statement would be productive of the same result, if we judge from the effect which our simple rules of election produced. I think no matter how we define anything, in very many minds it will tend to ambiguity. I admit very freely the force of your reasoning, and in my own mind I have gone through it all. The poles of the magnet which point to the terrestrial north or south, might be called—as attempts have been made to call them from almost time immemorial—the north seeking pole, or the south seeking pole, or the blue or red poles, or the austral and boreal poles, etc. I imagined that the Committee had taken those points into consideration, and had come to the conclusion that no matter what we officially name the magnet poles, we shall continue to call the north pole of the earth the north pole, no matter where the needle points, and although the majority of us know, or think we know, much better, we will persist in calling that point of the needle, which points northward, the north pole. My suggested amendment was simply to the Committee's definition, and in view of the fact that I supposed they had taken all these matters into consideration, I did not feel myself authorized to go any further into that, but simply to give one of the suggestions which had been called for.

THE PRESIDENT:—I may be wrong, but I think the general practice in this country is to call the end of the needle which points approximately to the earth's north, north, and to regard the magnetism of the Northern Hemisphere of the earth, as essentially south. That, I think, is the general idea that is held now.

MR. LOCKWOOD:—If that be the case, it is one of the instances where the smaller governs the greater. I do not suppose that any number of needles will govern the polarity of the earth. But I can imagine that the polarity of the earth will tend, in some degree at least, to direct the needle. I do not think that the terminology which we apply to the polarity of the earth should govern the terminology which we apply to the polarity of the needle. I should like to hear what the views of the Commitee were, if any of the members of the Committee will kindly give them.

MR. KENNELLY:—The Committee endeavored to ennunciate the very statement that the President has just given us, and the amendment that Mr. Lockwood has offered I take to be intended for the benefit of those unhappy creatures whose lot might be cast between the north geographic pole and the magnetic pole in its vicinity, and who, of course, contrary to the definition of the Institute's Committee, would find the magnetic pole geographically south, as they would live north of it. But if we eliminate that section of our planet, which is fortunately rather remote, the suggestion of the Committee will practically agree with the suggestion that I understood Mr. Lockwood's amendment to improve, namely, that the magnetic pole of the earth which is terrestrially north, or situated in the vicinity of the geographic north pole shall be the earth's south magnetic pole, and that the pole of a needle which points toward it when allowed to assume its natural direction, shall be the north pole of that needle.

THE SECRETARY:—In regard to this final closing up of the work of the Committee preparatory to the assembling of the Electrical Congress, it is probable that this will be the last meeting of the INSTITUTE before the convening of that Congress, and therefore it would be proper to suggest that this final summing up be published, either in the TRANSACTIONS or in the electrical journals or both, before the regular meetings of the INSTITUTE, in order to have it in general circulation before the Congress. As we are not prepared to close it up at present, and it appears to me that authority should be given for that to be done without further discussion of the matter before the INSTITUTE, I would make a motion to the effect that the Sub-Committee on Provisional Programme be authorized, at such time as they find convenient before the assembling of the Congress, to publish, through the electrical journals and the TRANSACTIONS, such a summing up as they may deem proper.

The motion was seconded by Mr. Hammer and carried.

A lecture delivered at the Tenth General Meeting of the American Institute of Electrical Engineers, Columbia College, New York, May 17, 1893. President Houston in the Chair.

PRACTICAL ASPECTS OF LOW FREQUENCY ELECTRICAL RESONANCE.

BY M. I. PUPIN, PH. D., COLUMBIA COLLEGE.

Mr. President and Gentlemen of the Institute:—A large part of the subject of the following discourse was discussed by me, but in a different way, in three papers. Two of these appeared in the April and May numbers of the *American Journal of Science*. The third will appear in the June number of the same journal. The method which I have adopted in the following discussion seemed preferable to the mathematical method which I followed in those papers. It is probably just as exact, and certainly a much clearer way of viewing the variable flow of electricity, especially those features of it, which have a more or less direct practical bearing.

1. On the Natural Period of an Electrical Circuit.

An electrical circuit possessing self-induction and capacity behaves in a great many respects as a body does in consequence of its inertia and elasticity. The fundamental reason for this analogy is simply this:—The electromagnetic energy of a coil through which a current flows, has all the characteristic properties of the kinetic energy of a moving body, whereas the energy of the static charge of a condenser has all the characteristic properties of the potential energy of a strained elastic body. If the neutral state of such an electrical circuit is disturbed, it will return to it again after performing a certain number of oscillations about the position of its neutral state. But a return to the neutral state is impossible until the energy which is spent upon the circuit to disturb its neutral state has left the circuit, or to use a more technical expression, until the energy has been dissipated or given off to some other circuit. The two principal causes which produce dissipation and compel the circuit to return to its neutral state again, are frictional resistances and radiation. Just as in the case of vibrating bodies, so also in the case of electrical oscillations, losses due to radiation, especially when no other electrical circuits are near, are exceedingly small when the oscillations are slow. In Herzian oscillations they are quite considerable. In oscillations of the Tesla frequency they are probably not negligible. My remarks refer to electrical oscillations of long period, therefore losses due to frictional resistances are the only losses which I shall consider. Consider now an electrical circuit consisting of a coil A and a condenser B (Fig. 1) in series with it. It is a circuit with localized self-induction and capacity. I trust that my discussion will lose as little in its generality as it will in its practical bearing if I confine it to such circuits only.

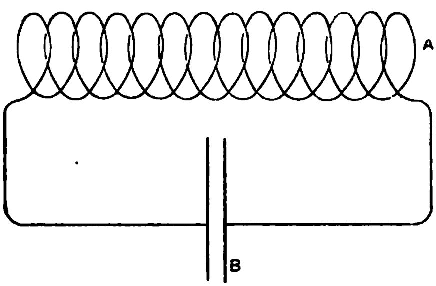

FIG. 1.

Let a sudden electrical impulse disturb the neutral state of this circuit; electrical oscillations will result. These oscillations follow laws practically identical with the laws of the motion of a slowly vibrating body. Their period is constant, as we all know, and it is in general completely determined by *the electromagnetic moment of inertia* and the *dielectric elasticity* of the circuit—that is, by its coefficient of self-induction and its capacity. When, however, frictional losses due to ohmic resistance, magnetic and dielectric hysteresis are large, then the period of this circuit is no longer defined by the self-induction and capacity alone, but it is also influenced by these frictional losses.

When ohmic resistance and hysteresis losses are small enough, then the natural period of the circuit is given by the well-known formula

$$T = \frac{2\pi}{10^3} \sqrt{L\,C}$$

Where T is the natural period of the circuit in seconds, L its coefficient of self-induction in henrys, and C its capacity in microfarads. For instance, a large Bell telephone whose coefficient of self-induction is 0.5 henrys when connected in series to a condenser of 1 microfarad capacity will have a natural period of very nearly $\frac{1}{225}$ seconds, that is to say, an electrical disturbance would set up oscillations in it, 225 of which would take place in one second. If a permanent magnet were brought into the vicinity of the telephone coil and then suddenly removed, the telephone would sing a note whose pitch would be a little below the well-known note C. But it would not sing it very long. For since the ohmic resistance is 100 ohms these oscillations would disappear almost entirely after

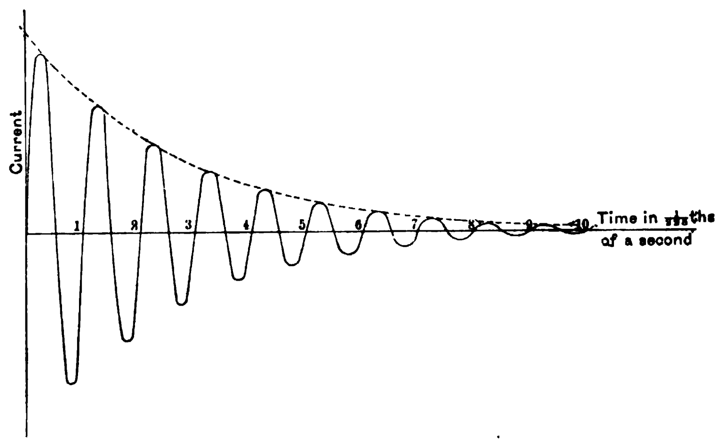

FIG. 2.

10 complete oscillations, somewhat in the manner represented in diagram Fig. 2, that is to say the telephone would sing only during about $\frac{1}{22}$ part of a second. By diminishing the resistance we could prolong its song. But diminish the resistance as much as you please, the pitch of the note of the telephone will remain the same, because, as I said, the natural period of the telephone circuit just described is within wide limits independent of the ohmic resistance.

2. On the Tuning of an Electrical Circuit.

To change the note, say to make it higher, it would be necessary to diminish the capacity of the condenser. When a piano tuner wishes to raise the pitch of a piano string he gives it more tension; so in *tuning an electrical circuit*, in order to change its pitch, it is necessary to change its *electrical elasticity* that is, its capacity. But there are other ways of tuning an electrical circuit, just as there are different ways of tuning musical instruments. Consider a reed pipe, say a clarionet. The musician places a little bit of wax on the reed. When the instrument is too low in pitch he takes off some of the wax, so as to diminish the moment of inertia of the reed, and when the pitch of the instrument is too high, he sticks on more wax so as to increase the moment of inertia. At any rate this used to be the method of old-fashioned country musicians. And so it is in tuning an electrical circuit. Instead of varying its *electrical elasticity*, that is, its capacity, we can vary its *electro-magnetic moment of inertia*, that is to say, its coefficient of self-induction. To show how this may be done in the telephone circuit just mentioned, insert into this circuit a small coil, *a*, [Fig. 3] an auxiliary coil, with a removable iron core *c* made up of very fine iron wire. In doing this

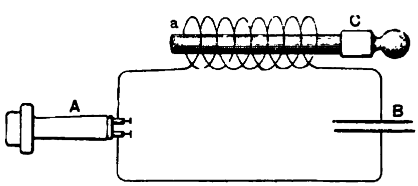

FIG. 3.

we do exactly what the country musician does when he puts wax on the reed of his clarionet. If the electrical pitch of the circuit does not suit, say it is too high, then simply put on more electro-magnetic wax, that is to say, insert the iron core and move it back and forth until the correct position is found which will give the correct electrical moment of inertia, that is to say, the correct coefficient of self-induction. This is, briefly stated, what I mean by the expression *tuning an electrical circuit.* From the simple expression given above for the natural period of an electrical circuit, it is evident that the tuning of an electrical circuit, if not simpler, is certainly quite as simple a process as the tuning of a musical instrument.

3. On the Determination of the Pitch of an Electrical Circuit.

If we wish to know the pitch of a musical instrument, say of a tuning fork, to choose a simple illustration, we simply give it an impulse, say a tap with the finger, and then listen to the vibrations, which in general will last for several minutes, and give us sufficient time to make up our minds as to what the vibrations sound like. In examining the pitch of an electrical circuit it is more convenient to adopt a different method. The reason is that as a rule electrical oscillations are, as pointed out in the example above, much more damped, so that the oscillations resulting from a single impulse would not last long enough to give us sufficient time to see what they look like, or to listen and hear what they sound like. The method suggested by the stroke of a violin bow over the string is preferable. The stroke of the bow produces a series of impulses which quickly succeed each other and maintain the string in uniform vibration. It is interesting now to observe that the same thing can be done with an electrical circuit. Consider the circuit A B C Fig. 4. The condenser B represents the tension on the violin string, coil A represents its inertia. The air-gap C stands for the point where the bow by its stroke excites

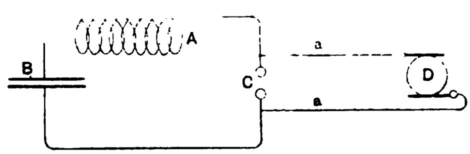

FIG. 4.

the string. If you now wish a musician who plays with a one-sided stroke, use a high potential direct current generator D, and for an alternating stroke substitute an alternator. The discharges at the air-gap C succeeding each other quickly enough, and at proper intervals will maintain in the circuit A B C practically uniform electrical oscillations. This is one of Tesla's favorite circuits, and I have no doubt but that he will accomplish great things with it yet.[1] Bring a few turns of wire of a telephone circuit into inductive influence of this circuit, and you will have in the telephone a musical note of exactly the same pitch as the pitch of the electrical oscillations in the circuit A B C. The note is not perfectly pure. It is marred by the noise of the spark discharge of the air-gap C. Neither is the note of the violin string pure; there is always more or less of the scraping noise of the bow. Just as it is necessary to keep the bow well rosined

1. It must be observed, however, that Hertz, in 1887, produced his oscillations by a circuit of this identical form.

so as to give it a good grip upon the string, so it is necessary to apply a strong current of air or the action of some other of Tesla's devices upon the air-gap C, otherwise an arc is formed and the generator D loses its grip upon the circuit A B C.[2] If we had a number of different coils with condensers like A and B alongside of each other, and arranged in such a way as to be able to place them at will, now the one and now the other, and now again a number of them in multiple under the action of the electrical bow which the generator D keeps moving over the air-gap C and at the same time vary the capacities, we should be able to change the electrical vibrations of our system with that ease, precision and grace which the violin player displays when with the one hand he guides his obedient bow, while the busy fingers of the other hand glide over the trembling strings, eliciting from them delightful notes which blend into pleasing harmonies. So with our system of properly tuned electrical circuits, we could produce harmonies, but they would be harmonies of silence, harmonious oscillations in the ether that affect neither eye, nor ear, nor taste, nor smell. But bring a part of a telephone circuit into inductive action of our harmonic system, and let a skilled experimentalist manipulate a properly constructed keyboard which controls the coils and condensers of the various circuits, and harmonies which before were as silent as the grave, will now agitate the responsive diaphragm of the telephone and produce music that could be made to re-echo in every telephone in the United States.

But after all, such an arrangement when used for such a purpose, would be a mere toy in comparison to the purpose for which our distinguished colleague, Mr. Tesla, employs it. To convert high-potential but small current electrical energy into low-potential big current energy, or *vice versa*, accompanied by all possible variations in the frequency of oscillations was the purpose for which Mr. Tesla constructed the device. To a physicist who delights, not less than the engineer, in neat, simple devices for the accomplishment of big and brilliant effects this device of Tesla naturally appeals more than all his other ingenious inventions. Many a delightful hour have I spent in watching experiments on a circuit like the one in Figure 4. The

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2. The importance of blowing out the arc for the production of powerful oscillations seems to have been first recognized by H. Classen, Wiedemann Annal. d. Physik und chemie, Band xixxx, p. 647, 1890.

coil A consisted of a short, stout copper wire; the condenser B consisted of a battery of Leyden jars, which my distinguished teacher, Professor Rood, of Columbia College, kindly lent me. The wires *aa* were thin copper wires connecting the air-gap C to the poles of an induction coil which is now in the hospital. It is a delightful sight to see the stout wire aglow under the powerful agitation of the rapid oscillations, whereas the thin wires *aa* adjoining them remained perfectly cool. Twist now the thick wire A into a few convolutions, say ten or twelve, and surround them by a few hundred turns of fine wire and you will have the now well-known oscillatory transformer with which Mr. Tesla and Professor Elihu Thomson produced some brilliant effects, a transformer that will give you any number of volts especially if,—and now I am going to touch a point which forms the central point of my discourse,—especially if the thin wire coil contains *capacity in series with it, so that the natural period of this circuit is the same as the period of the thick wire coil, that is if the two circuits are in resonance.*

4. On Resonance.

Here again I have borrowed a term employed in music. But a few simple considerations will show you that it is very natural that I should, for the phenomena of sound and those of oscillatory flow of electricity are governed by nearly the same laws. Very high frequency electrical oscillations would in all probability be identical with light, as first announced by immortal Maxwell. It is, therefore, not surprising to find that low frequency electrical oscillations should resemble so much the other group of oscillatory phenomena which next to light pleases our senses best, namely, the phenomena of sound, especially agreeable sound, that is music.

To gain a clear conception of what is meant by electrical resonance consider the following simple mechanical analogon—I call it the *torsonial pendulum*:—I used it often with my students in discussing alternating current phenomena and they liked it very much. A heavy bar A (Fig. 5) is suspended on a stiff elastic wire B, which is attached to a plate C whose weight may be neglected. This plate C slides in a groove *aa* which in consequence of friction acts like a brake. Suppose now that the friction between C and *aa* is such that when the angular velocity of C is ω the rate at which heat is generated by the friction between C and *aa* is equal to $\omega^2 R$ where R is independent of the angular velocity. This torsional pendulum resembles then very much an electrical circuit having localized self-induction, capacity and ohmic resistance. The moment of inertia of A, the elasticity of B, and the friction in C act exactly the same as the coefficient of self-induction of the coil, the capacity of the condenser, and the ohmic resistance of the circuit. Let $\frac{1}{B}$ stand for the elastic capacity of the wire, that is if the wire be twisted through an angle θ, then the moment of the elastic force which opposes this twisting is $\frac{\theta}{B}$. Let I stand for the moment of inertia of the weight A, then as long as the frictional resistance is within certain limits we shall have for the natural period of the pendulum

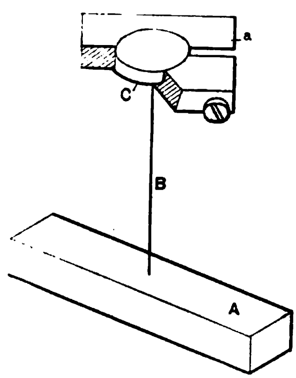

FIG. 5.

$$T = 2\pi \sqrt{I \times B}.$$

You see that this expression is exactly the same as the one which expresses the natural period of the circuit in terms of its coefficient of self-induction and capacity.

If this pendulum is set in motion by a single impulse it will oscillate with a constant period. The first elongation will be largest, and the successive elongations will be smaller and smaller, just as represented in Fig. 2, until the pendulum is reduced to rest again. This will happen when the energy of the impulse has been entirely dissipated into heat, which is the work done against frictional resistances in the break at *C*. The smaller this frictional resistance, the longer will the pendulum swing in consequence of the impulse, before it is reduced to rest. On the other hand we can increase the resistance in C to such an extent as to make the swing a-periodic or dead beat, as is for instance the case in the Weston voltmeters and ammeters.

Repeat the impulse at regular intervals, alternating them, the pendulum will keep on swinging; but you can easily see that if the intervals of the impulses are measured off in such a way that every time the pendulum passes through its position of equilibrium, the impulse strikes it, and strikes it in the direction in which the pendulum is moving, then the amplitude of the swing will be much larger than if the intervals of the impulse are not measured off in such a way. In other words when the period of the impulses is the same as the natural period of the pendulum then the swing is largest. The impulses are said then to *be in resonance* with the pendulum.

The same effect will be produced if for the impulses we substitute a periodically but gradually alternating force, say a simple harmonic force, having the same period as the natural period of the pendulum.

Such a force when acting upon the torsional pendulum just described will continually increase the amplitude of the swing, until the swing is so large that the work done during a swing against the frictional resistances, is just equal to the work of the moving force during that time. From that point on, the pendulum will swing with constant amplitude. It is evident, therefore, that the larger *the amplitude of the moving force and the smaller the frictional resistances, the larger will be the amplitude of the swing.* But a large amplitude implies two things:—1st, A *large torsional reaction* in the suspension wire, and secondly, a *large velocity* whenever the pendulum passes through its position of equilibrium.

The bearing of this mechanical analogon upon the electric circuit having self-induction and capacity is very direct, as I shall presently point out. Let a simple harmonic E. M. F. $E \sin pt$. act upon such a circuit. I shall presently consider a complex harmonic E. M. F., and also circuits possessing Foucault current losses, and losses due to magnetic and dielectric hysteresis. They will form the last, and in my opinion the most important part of my discourse. A simple harmonic E. M. F. acting upon an *elastic electrical circuit* in which the only frictional losses are those due to ohmic resistance, will when its period is the same as the natural period of the circuit, that is when *it is in resonance with the circuit*, continually increase the amplitude of the electric displacement, that is the amplitude of the condenser charge and therefore also of the current, until the work done against the ohmic resistance is exactly equal to the work of the impressed E. M. F. From this point on the amplitude of the current will remain constant and equal to the amplitude of the impressed E. M. F. divided by the ohmic resistance. Or, to state it in terms which are more generally employed: *Capacity and self-induction neutralize each other when the circuit is in resonance with the impressed* E. M. F.

It is evident also that in a resonant flow, there can be no difference in phase between the current and the impressed E. M. F. Since in this case the current at any moment depends on the E. M. F. and the resistance, and on nothing else.

It is clear, however, that if losses due to Foucault currents, magnetic and dielectric hysteresis are present, then the current cannot be made equal to the ratio between the E. M. F. *and the ohmic resistance by any combination of capacity and self-induction, although as a simple reflection will show, the difference in phase between the current and the* E. M. F. *may be reduced to zero in this case also,*[1] *in which case we should have no false current, as it is generally called.* I shall presently discuss this point a little more fully.

5. Resonance in Circuits with an Impressed Complex Harmonic Electromotive Force.

If the impressed E. M. F. is a complex harmonic, then the circuit may be brought into resonance with any of the component harmonics. I have an apparatus here which is capable of producing E. M. F.'s of almost any complexity. I call it an *electrodynamic current interruptor*, for want of a better name. It consists of a phosphor-bronze wire, the vibrator, *a*, *b*, *e*, (Fig. 6), stretched between the poles of a Weston horseshoe magnet *d*. A short, thin, amalgamated copper wire *b*, the dipper, is soldered on the vibrator. The vibrator is stretched like the wire of a monochord, over two hard rubber bridges *a e*, and its tension can be varied by a screw and lever *f*. The dipper *c* whenever it

1. Professor Duncan, of Johns Hopkins University informed me yesterday that he deduced the same result in the course of an investigation, a short account of which he read before this Institute at its General Meeting in Chicago in 1892, "Note on some Experiments with Alternating Currents." Transactions, vol. ix, p. 179.

dips into the mercury cup closes an electrical circuit *F f e b c g A*, and the vibrator is then repelled upward by the force of repulsion between the magnet and the current flowing through the vibrator. One gravity cell *f* is sufficient to work this apparatus. It is evident that the fundamental period of the interrupted current will be equal to the period of the vibrator. To change this period, say to make it equal to the period of this tuning fork, which is 256 complete vibrations per second, I simply strike the tuning fork, hold it then near my ear and by turning the screw *f* of the stretching lever I vary the tension of the vibrator until the rate of the vibrator and that of the tuning fork are in perfect unison, which can be easily recognized by watching the beats. This whole process of tuning takes up, as you see, only

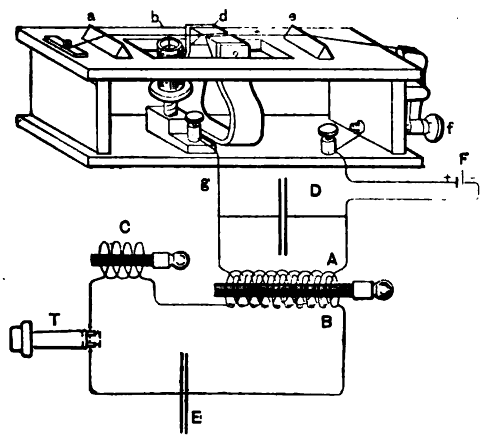

FIG. 6.

a few seconds. In series with the cell F and the vibrator, we have a small coil *A*, containing a well packed bundle of very thin iron wire, and in shunt with A is a condenser D. This condenser performs the function of bringing this primary circuit into partial resonance with the vibrator which is recognized by the circumstance that at the moment of resonance the spark at the dipper is a minimum.

A simple reflection will show you that the current generated by this interrupter is a complex harmonic. To analyze it into its component harmonics I shall adopt the method employed in acoustics.

A complex sound is analyzed by the well-known resonators of Helmholtz. These resonators are constructed in such a way that they will respond to one vibration only, and also in a measure to its upper harmonics. To any other vibration they are practically mute. By a number of such resonators we can find out, in the manner first pointed out by Helmholtz, all the vibrations which are contained in a complex sound.

The secondary circuit B C T E, given in the diagram of Fig. 6, performs the function of such a resonator, it is in fact *an adjustable electrical resonator;* C is a small auxiliary coil which you see here; E is a Marshall condenser, and T is a telephone whose note tells me to which of the component harmonics the electrical resonator responds. The telephone is silent now, at any rate as far as you can tell, because the condenser plugs are all out. Now, with this plug, I insert a small capacity. The note which you hear is evidently much higher than even the octave of the vibrator. The electrical flow in the resonating circuit is approximately the same as if we had in it a simple harmonic E. M. F. of the same frequency as this note. I insert another plug, so as to increase the capacity. The note which you now hear is deeper. The difference between the two notes is rendered very striking by making and breaking the contact of the second plug in rapid successions, as I do now. The sound of the two notes, succeeding each other at regular intervals, resembles very much the sound of bagpipes. I insert another plug, so as to increase the capacity still more. The note which you now hear is still deeper. It is the note of the fundamental electrical vibration, which is the same as the vibration of the vibrator. I know that, because, as I continue inserting more and more plugs, you do not perceive any further change in the sound. The capacities which I introduced by the three plugs are to each other very nearly in the ratio of 1 : 9 : 25, hence the frequencies of the three harmonics contained in the impressed E. M. F. and which I have detected by the resonating secondary circuit are to each other in the ratio of 1 : 3 : 5.

The impressed E. M. F. can therefore be written

$$E = E_1 \sin pt + E_3 \sin 3\,pt + E_5 \sin 5\,pt.$$

Still higher harmonics are present, but they are too weak to be heard all over this room.

I shall presently discuss a method by means of which we not only detect the presence of these harmonics, as I have just done by means of this telephone, but also determine the relative strength of each. The method imitates in a certain measure Prof. Langley's bolometric method of determining the distribution of energy in the spectrum of a given complex luminous vibration.

Before making the next step in my discourse I wish to call your attention to an application of resonant circuits (in connection with the electro-dynamic interrupter) to a method of producing simple harmonic currents of constant and easily determinable frequency.[1] I simply tune the vibrator by means of a standard tuning fork, and then weed out the upper harmonics by successive transformations. In this connection I ought to mention that Professor Duncan of Johns Hopkins University informed me several weeks ago that he was also weeding out harmonics. I now remember that I got the expression "weeding out" from him, but I have used it in my papers on "Low Frequency Resonance" without giving him credit for this beautifully selected expression.

6. On the Resonant Rise of Potential.

The question arises now, how are we to tell experimentally whether a circuit is in resonance to an impressed E. M. F. or not? Several methods suggested themselves to my mind in the course of my work. I selected the one which appeared to me to be the most sensitive and most interesting. *It also suggests a new way of transforming electrical energy which some day may perhaps be of some practical importance.* I wish to discuss this method now, but only very briefly.

As I observed before in the discussion of the motion of the torsional pendulum, represented in Fig. 5, when the moving force and the pendulum are in resonance then both the amplitude of oscillation and the amplitude of the velocity reach their maximum value. And so it is also with a circuit possessing self-induction and capacity. By varying gradually the one or the other, the current (which corresponds to the velocity of the pendulum) will continually increase. But the difference of potential in the condenser (which corresponds to the elastic reaction of the suspension wire) will also increase continually. When the point of resonance has been reached then evidently both the current and the difference of potential in the condenser have reached

1. Pupin, "Electrical Oscillations of Low Frequency and their Resonance," *American Journal of Science*, April, 1893.

their maximum values. This maximum difference of potential in the condenser can be many times greater than the impressed E. M. F., because just as in the case of the torsional pendulum the elastic reaction of the suspension wire is an accumulative effect of the moving force, so in the electrical circuit, the potential difference in the condenser is an accumulative effect of the impressed E. M. F. Now since a resonant electrical flow, which we have just considered, consists simply of a transformation of electrokinetic into electrostatic energy, and *vice versa*, during each half period accompanied by a frictional loss due to ohmic resistance, it is evident that the amplitudes of these two kinds of energies must be equal to each other, that is, we shall have in the usual notation

$$\tfrac{1}{2}\, P^2\, C = \tfrac{1}{2}\, L\, \frac{E^2}{R^2}.$$

But since owing to resonance we have $p^2\, L\, C = 1$, the preceding relation reduces to

$$P^2 = p^2\, L^2\, \frac{E^2}{R^2} \quad \text{or} \quad P = p\, L\, \frac{E}{R}.$$ [1]

It is evident, therefore, that with a little more than ordinary frequency, say 350 periods per second, and an inertia coil of considerable self-induction and small resistance high potentials can be obtained in the condenser by bringing the circuit in resonance with the impressed E. M. F.. *Connecting, therefore, the condenser to a voltmeter we can tell very accurately when in the course of our tuning of the circuit we have reached the point of resonance, because, as I shall presently show you, the voltage goes up very rapidly when the condenser capacity or the self-induction are near the point of resonance. It is this electrometric method which I have employed in my investigations on "Low Frequency Resonance."* [2]
The same method can be, and has been employed by several investigators in the study of Herzian oscillations.

To show you how strong this resonant rise of potential can be I have within the last few days wound these two inertia coils (see coil $a'\ b'$ in Fig. 8) and borrowed this most excellent mica condenser from our well-known electrician, Mr. Wm. Marshall, for which kindness I feel very grateful to him. The impressed E. M. F. is ob-

1. This relation was first obtained, I believe, by Prof. Oliver Lodge. (See a letter by Prof. O. Lodge, London *Electrician*, April 24th, 1891.)

2. *American Journal of Science*, April, May and June, 1893.

tained in the following way: A small 16-pole alternator running at about 2800 revolutions per minute and generating an E. M. F. of about 600 volts feeds the primary of this small transformer (*c d*, Fig. 8.) Diagram Fig. 7 gives the form of armature A and field B of

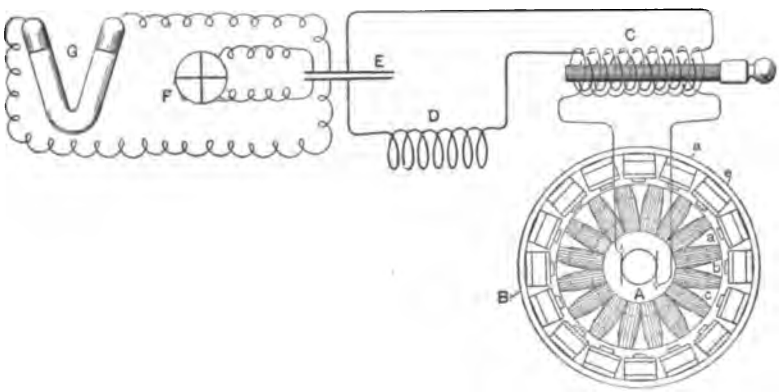

FIG. 7.

the machine. Coils *a b c d e* show how the machine is wound. The transformer consists of a hard rubber spool wound with about 3,000 turns (Fig. 8) of No. 20 A. W. G. wire for the primary. The secondary *h k* consists of about 500 turns of No. 16 wire. A well packed bundle *e* of very fine iron wire is the iron core.

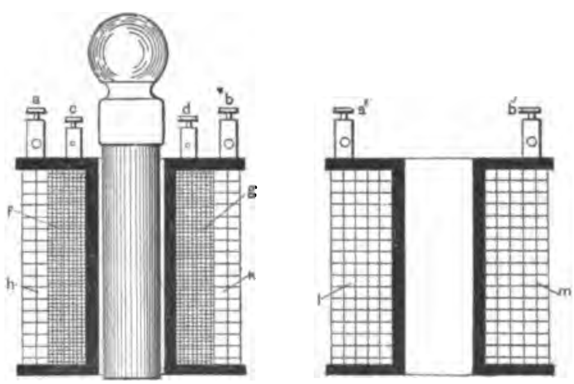

FIG. 8.

This transformer is indicated by C in Fig. 7. In series with the secondary we have inertia coils D and the above mentioned mica condenser E of 0.2 M. F. capacity. The inertia coils consist of hard rubber spools like *a' b'* Fig. 8 wound with No. 14 wire. The total self-induction of the secondary circuit is about 1 henry, *without iron in the inertia coils.* The poles of the condenser E are connected to a Sir William Thomson electrostatic voltmeter F and also to a vacuum tube G with condenser electrodes, that is to say tinfoil coatings on the outside of the tube. (Indicated by the shaded part in the diagram.) I now start the machine and by weakening the field of the driving motor, I gradually increase the speed of the machine. As you see the voltmeter needle is steadily advancing with the speed. To get the greatest deflection of the needle I ought to have about 350 periods per second. Now I shall advance to very near this speed. The voltmeter indicates now 2,000 volts. The impressed E. M. F. in this secondary resonating circuit is only about 100 volts. Instead of increasing the speed so as to reach the point of perfect resonance I prefer to leave the speed constant, and by gradually inserting these five iron wires to increase the co-efficient of self-induction until that value is reached which with the given speed and capacity brings the circuit in resonance with the impressed E. M. F. I am doing this now and watching the voltmeter needle at the same time. Now the voltmeter indicates 3,000 volts, which is 30 times the value of the original E. M. F. This is the point of resonance, because if I push the iron wire lower, the needle goes back. The point of resonance is very sharply defined, because the slightest motion of the iron wire one way or the other makes quite considerable difference in the reading of the voltmeter. The point of resonance can be shown to a large audience like this much more easily by connecting the poles of the condenser to the tinfoil electrodes of the vacuum tube G. A discharge starts in this tube at about 2,000 volts. Now as I insert these iron wires into the inertia coil you see the intensity of the discharge increases. [The room was darkened.] The point of maximum resonance is clearly marked by the intensity of the discharge, as you see. Now I insert the iron wire too far; as you see the discharge is stopped. I can start it again by raising the iron and stop it again by taking the iron wire entirely out, as you see.

By working with high frequencies and small ohmic resistance any voltage within practical limits can be obtained. Ohmic resistance, however, is not the only thing which limits the resonant rise of potential. *Dielectric hysteresis* and the *peculiar behavior of iron when under the inductive action of a rapidly alternating current,* modify this rise very much. The other day I employed this home-made condenser in parallel with that Marshall mica condenser. I obtained only 2,000 volts rise, whereas preliminary calculation led me to expect at least 4,000 volts. *But the smell of melting wax reminded me forcibly of the cause of this discrepancy. The home-made condenser got so hot in about two minutes that all the wax* (which when boiling hot was compressed between its mica plates to drive out the air and moisture) *between its plates was melted.* The energy which was thus used up in overcoming the dielectric hysteresis of the wax had prevented the resonant rise from reaching that value which it ought to have reached according to calculation. Taking this condenser out and using the mica condenser alone the rise went right up to a point considerably above 3,000 v. Paraffin and glass behave in a similar way *So that owing to dielectric hysteresis alone the resonant current* (which takes place when capacity and self-induction neutralize each other) *is never equal to the electromotive force divided by the ohmic resistance, and it can be very much less. The presence of iron is incomparably more powerful in destroying the resonant rise of current and potential, as will be seen presently. This is very unfortunate in view of the brilliant expectations which not a few electricians hoped to realize from the employment of condensers in the running of alternating current machinery.*

It is well to observe here that if we substitute a Weston voltmeter for the Thomson Electrostatic Voltmeter F Fig. 7, then we shall have practically the condenser in parallel with the inertia coils. It is easy to see from purely theoretical considerations that the resonant rise will be less in this case. Experiments show that the rise can be then very much less if the resistance of the voltmeter circuit is reduced. *The bearing of this observation on the so-called Ferranti effect with and without a load in the mains needs, I venture to suggest, no further discussion.*

7. Effect of Iron on the Resonant Rise of Potential.

I have already remarked that iron behaves in a peculiar manner when under the inductive action of a resonant flow of electricity. Allow me to call your attention to the diagram of Fig. 9. This curve was obtained with the machine and the circuits which are given in Fig. 7. Except that the inertia coil D had smaller self-induction. Keeping the speed of the machine and the self-induction of the circuits constant, the difference of potential in the condenser was varied by varying gradually the capacity of the condenser. Taking then the capacities for abscissæ and the corresponding voltmeter readings for ordinates, the curve in Fig. 9 was obtained. As you see the curve has two maxima. The corresponding capacities are to each other as 1 : 9. Hence the

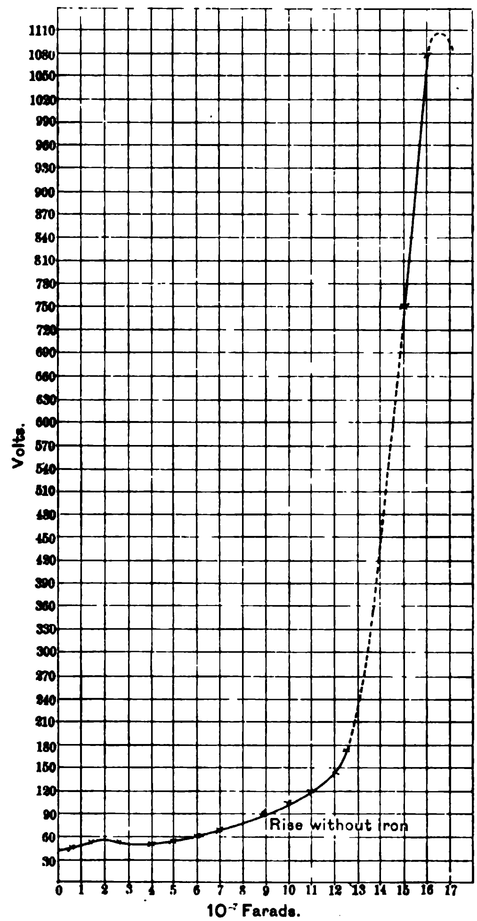

FIG. 9.

impressed E. M. F. can be represented by

$$E = E_1 \sin pt + E_3 \sin 3\,pt.$$

A comparison of the smaller maximum to the larger shows that E_3, the amplitude of the upper harmonic, is very small in comparison to E_1, the amplitude of the fundamental. This is surprising, because from the construction of the alternator one would expect a much stronger deviation of the impressed E. M. F. curve from the simple harmonic.

A similar *harmonic analysis* of the impressed E. M. F. obtained by means of the electrodynamic interruptor gave curves I, II and III Fig. 10. Curve I was obtained by inserting a certain capacity in shunt with the primary coil, and then varying the capacity in the secondary and recording the corresponding voltmeter reading. Capacity was measured off as abscissa and the corresponding voltmeter reading as ordinate. Curves II and III were obtained in the same way but with different capacities in the primary circuit. The impressed E. M. F. in each case can be represented by the following complex harmonic:

$$E = E_1 \sin pt + E_3 \sin 3pt + E_5 \sin 5pt.$$

But the amplitudes, E_1, E_3 and E_5 are different in each case.

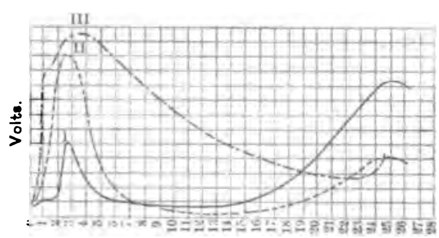

FIG. 10.

You see, therefore, that this method of harmonic analysis by the resonant rise of potential is very sensitive in detecting upper harmonics. Hence the natural conclusion that curve Fig. 9 indicates no strongly developed harmonics, simply because they were not present. Yet it is very difficult to suppose that my alternator generates practically a simple harmonic E. M. F. when one considers the shape of its armature core.[1] This point evidently deserved additional investigation.

I inserted a few iron wires into the inertia coil D, Fig. 7, and found that the upper harmonic, weak as it was, disappeared entirely. The iron wires became hot in a very short time. I suspected that iron when subjected to strong magnetization wipes out the upper harmonics in a resonant circuit. With the electrodynamic interruptor the magnetization of the iron cores is very weak, hence the persistence of upper harmonics. This suspicion was verified in subsequent experiments.

1. The armature core was a Crocker-Wheeler tooth armature.

When the inertia coil D was packed with iron wire then the resonant rise was very much diminished as is evident from the curve in diagram Fig. 11.

Various considerations led me to the conclusion that Foucault current and hysteresis losses were not the only causes of this remarkable diminution of the resonant rise due to the presence of iron. I will mention one only. In one of my experiments I used the primary coil of a small *closed magnet circuit transformer* for the inertia coil and found that as far as I could detect with my apparatus *no resonant rise* could be obtained with 100 volts of impressed E. M. F., no matter how low the frequency was which my machine could produce. About 75 periods per second was the lowest frequency which I employed. If, however, the

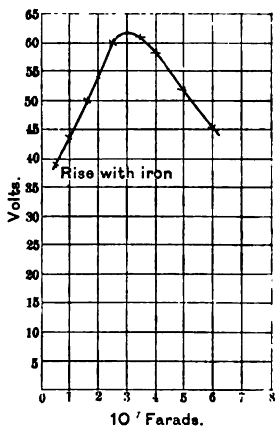

FIG. 11.

secondary coil of this transformer was short-circuited then the resonant rise appeared, but not nearly as strong as it ought to have been, even Foucault currents and hysteresis losses taken into account. The self-induction of the primary (with secondary short-circuited) calculated from the capacity which produced resonance was far too high. *There were unmistakable evidences in this, and in all my other experiments in which iron was present in the inertia coils, that the lower the frequency the stronger were the resonance effects.*

When an alternator giving 125 periods per second was substituted for the alternator in Figure 7, (which gave about 350 to 375 periods per second,) then the resonant rise with iron wire core in the inertia coil D was one-half of that when no core was used, the impressed E. M. F. being 45 volts. If, however, the impressed E. M. F. was raised, then this difference in the two cases continually diminished. When the impressed E. M. F. was 200 volts, then the resonant rise with iron was more than without it.

There seems to be in the iron a certain reluctance against getting into full swing when under the inductive action of a resonant current. The higher the impressed E. M. F. and the lower the frequency the easier it is to overcome this reluctance. *When the iron has once been set into full swing, then it is possible by careful manipulation to lower the impressed* E. M. F. *and increase the frequency without getting the iron out of this swing. In a closed magnetic circuit this reluctance seems to be exceedingly great.*

With a frequency of 50 periods per second the resonant rise

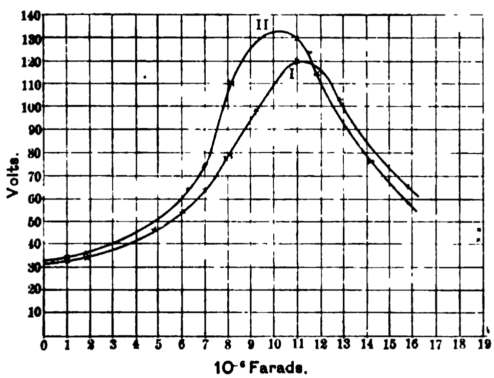

FIG. 12.

with iron was invariably more than without it. Two features in the peculiar behavior of iron were brought out by the resonance experiments with this low frequency machine. These two features, I venture to suggest, deserve more than ordinary attention.

The first peculiar feature is exhibited in the two curves Figure 12. Curve II was obtained when the whole iron wire core (about 500 wires each 40 cm. long and 1 mm. in diameter) was inserted into the inertia coil. The other curve was obtained when only one-half of the core was employed. *The frequency was maintained constant.* From the critical capacity in the two cases it is evident that there was much less difference in the coefficient of self-induction in the two cases than one might expect from the difference in the quantities of iron, that is, if we suppose that in this case also the natural period of the circuit is independent of the frictional resistances due to magnetic hysteresis. But even if we drop this hypothesis, still the small difference in the capacity in the two cases appears as puzzling as ever. We have resonance in this case also as in any other case, but it is evident that the conditions of resonance are not determined by self-induction and capacity alone. The question arises then, what does resonance mean in this case? Well, it means the largest current and the highest rise of potential, but this maximum potential and current are not necessarily accompanied by zero difference in phase nor by neutralization of self-induction with capacity.[1] The practical bearing of this result needs no further comment.[2]

There is another point to which I wish to call your attention in connection with curves I and II of Fig. 12. It is this: Although curve II was obtained with twice the quantity of iron and therefore, with at least twice the losses due to Foucault currents and hysteresis, [since the magnetizing current which circulated in the inertia coil with resonance corresponding to curve II was considerably larger than that corresponding to resonance of curve I,] yet the rise of potential in curve II was larger than that in curve I. I do not wish to take up too much of your time in showing you all of the difficulties which one encounters in endeavoring to explain these matters satisfactorily. Suffice it to point out merely that these difficulties do exist and that they deserve our most careful attention.

Before making the next step in my discourse I wish to remark that a circuit consisting of the armature of an alternator, a large *inertia coil without iron* and a condenser, all in series, *does not exhibit any of the peculiarities just mentioned, at any rate not to any considerable extent. Such a circuit can be tuned and a high resonant rise of potential can be thus produced, provided, of course, that this rise of potential is not accompanied by so heavy a current as to produce a serious armature reaction. Foucault current and hystersis losses in the armature iron cannot, of course, influence this resonance, since they are taken care of by the motor which rotates the armature.*

1. Professor Duncan informed me yesterday that he arrived at this conclusion in a somewhat different way. See paper already referred to.

2. Especially when Dr. Duncan's more definite conclusions on this point are taken into account.

The second peculiar feature in the behavior of iron when it is under the inductive action of the resonant current of an inertia coil will be exhibited to you in the following experiment: The arrangement is just the same as that given in the diagram of Figure 7, with the following exceptions: This alternating current ammeter is in the resonant circuit. The motion of its index, which I hope can be seen all over the room, will indicate to you the progress of tuning. I watch it on this side by means of a multicellular voltmeter which is denoted by F in the diagram of Figure 7. This four-pole 1 H. P. alternator which Dr. Laudy, of Columbia College, kindly lent me, takes the place of the 16-pole alternator. Inertia coil, alternator and condenser are all in series. The driving motor can easily take care of 2 H. P., so that the speed will remain constant during all variations of the load since as you will presently see the load will never be over 150 watts. The condenser has now 16 M. F. capacity plugged in. The frequency is about 50 periods per second. The impressed E. M. F. is 45 volts. To bring the circuit in resonance with the impressed E. M. F. it is necessary to insert iron wires into the inertia coil. I insert now about 400 wires. It is, as I can see from the voltmeter reading, considerably too much and, therefore, there is no resonance as you can see from the ammeter. I take the iron wire bundle and raise it slowly, and now I can feel by the pull on the bundle and also by the voltmeter reading that I have reached the point of resonance. You can see it by the ammeter deflection. The ammeter indicates 1 ampere; in reality it is about 2 amperes. [This instrument is a lecture room model and its reading must be multiplied by a large correction factor.] Now I lower slowly the iron wire bundle again; the resonance persists. I have reached now the same position from which I started and when there was hardly any resonance effect. But as you see this time the resonance effect is very strong. *But it is a resonance which resembles very much a supersaturated zinc sulphate solution. A very small crystal thrown into such a solution will cause a sudden precipitation of innumerable small crystals. So it is with this resonance. If I now bring a single iron wire near the bundle, its mere approach upsets the resonance almost completely, as you see from the sudden drop of the ammeter needle. But if I add this wire whose mere approach upsets the resonance almost completely and even five other wires to the bundle and then by gently raising it start the resonant flow I can then lower it again* *without disturbing resonance, as you see it from the deflection of the needle.*

I shall vary now the capacity by taking out carefully one plug after the other. But I must take out these plugs without causing a spark. Here is one plug out, and here another and four more. I have reduced the capacity by 1 M. F. or nearly 7 per cent. of the total capacity. There is hardly any change in the resonance effect, as you see from the ammeter needle. Now I shall carefully put back the plugs and even more than I have taken out, but carefully avoid sparking. As you see, resonance still persists without any apparent change. *Now I take out one plug and vary the capacity by only* 0.1 M. F., *but I take it out in such a way as to produce a strong spark by the removal of the plug. The experiment does not succeed. But here it succeeds at the removal of a* 0.3. M. F. *plug. The collapse of the resonance effect is as you see almost complete. Generally I succeed with even less than a* 0.1 M. F. *plug.*

The same pecularities appear also at higher frequencies, but they are not nearly as clearly defined at frequencies over 100 *as they are at those of less than* 50 *periods per second. They are certainly not due to any change in the speed of the machine, produced by the variation of the load due to the rise of the resonant current.* Nor is it probable that they are due to armature reactions. This last point, however, deserves a closer examination than I have been able to give it as yet.

One more point I feel that I ought to mention before I end my discourse. It is a point which I think may have some practical importance sooner or later. *When a large inertia coil without iron is joined in series with the armature of an alternator and also with a condenser and then the circuit is tuned, so as to analyze harmonically the shape of the impressed* E. M. F. *curve, I find in all the machines that I have tried so far that this curve is almost perfectly free from upper harmonics, even with frequencies of* 50 *periods per second. If, however, the armature is closed by the primary of a transformer then upper harmonics will appear, as for instance, in the arrangement in Figure* 7. I do not think that these upper harmonics are entirely due to the variation of the permeability of the transformer core during each complete cycle, as has been suggested by very competent authority.[1]

1. See Professor Duncan's paper already cited; also H. A. Rowland:—"Notes on the Theory of the Transformer;" Phil. Mag., July, 1892.

A different interpretation of the phenomenon is possible. My experience in this matter favors the view that the upper harmonics are due to the interference between the electromotive forces generated in the alternator and the transformer. I prefer, however, to devote more time to the examination of this exceedingly important subject before venturing to discuss the validity of this possible new interpretation.

Allow me now to sum up briefly the main points of this discourse.

The practical bearing of "Low Frequency Electrical Resonance" in so far as I have worked out the subject seems to rest on the following characteristic features:

It brings out very prominently the purely mechanical character of the phenomena of electricity, and it does that not by referring to carefully prepared delicate experiments of a skilled physicist, but by referring to phenomena which every electrical engineer can observe every day in the testing room, telegraph and telephone stations, and in central stations for power and lighting. A full appreciation of this purely mechanical character of the phenomena of electricity is of the greatest practical importance, for we must remember that the founder of this mechanical view, the immortal Maxwell, in opening this new view of electrical phenomena contributed to the advance of this science as much as, if not more than, any other investigator of this century.

It offers a new, simple, and exceedingly convenient method of transforming electrical energy from low to high potential.

It enables us to observe very accurately the behavior of dielectrics and of iron when under the inductive action of resonant currents, and thus to determine the exact limits within which condensers can be employed in the solution of electrical engineering problems.

It offers a new and exceedingly simple method (the method of harmonic analysis mentioned above) of studying the working of alternating current machinery under various conditions of load.

It points out a new and apparently very promising direction in which the difficult but exceedingly important study of the magnetic properties of iron can be pushed ahead.

In closing my remarks I wish to thank my pupil, Mr. Milton C. Canfield, for the very efficient assistance he has given me in the preparation of this lecture.

Discussion.

The President:—Gentlemen, you have heard Dr. Pupin's admirable paper. Does the Institute wish to make any remarks on it?

Mr. Tesla:—Gentlemen, I do not know whether I can contribute in any way to the clear and skillful exposition which Prof. Pupin has made of the phenomena of resonance. They have been familiar to me for a long time. In fact, two or three years back I began to work on lines in which an observation of some of the rules advanced by Dr. Pupin is an absolute necessity, and one of the reasons why I have only reluctantly consented to deliver a lecture on some high frequency phenomena, which I have shown on two or three occasions was, that I felt inadequate to the task, because in every experiment one has to depend on certain delicate balances and one cannot always feel sure of succeeding in the experiment, especially in a public lecture. Dr. Pupin has admirably succeeded in performing before an audience a number of difficult experiments. I saw this marvellous experiment with the iron core, performed by him last week, I believe, and I have been much impressed with the importance of the molecular condition of the iron. Any observation relating to the iron must be of interest, as it may lead to valuable results, and since I am called upon, I may mention a few of my own experiences, though they might not be in direct connection with the subject treated by our lecturer. About two years ago I observed a curious effect which I abstained from mentioning so far, because I could not explain it to my satisfaction. It was this:—I had an alternator of high frequency operating an induction coil. The secondary of this induction coil was connected to a Cardew voltmeter, and I had made the windings so as to bring the deflection into the best range of voltmeter. A condenser was connected to the alternator. I varied the amount of iron in the core of the coil and observed the rise in the electromotive force. As iron wires were added the electromotive force would continuously rise up to say 100 volts, Then there was a point at which when I added a few iron wires. the electromotive force fell off, about two volts. A further addition did not make any change whatever. I observed that effect repeatedly and repeatedly, but could not satisfactorily explain it. Now in the light of the experiment of Dr. Pupin and with the knowledge we presently possess, I cannot think else than that there was something of the same nature in the phenomenon. The delicate balance which, as Dr. Pupin has shown can be easily upset by the introduction of a single iron wire in the coil, reminds me of an experience with a machine which was regulated by means of an "auxiliary or third brush," a very interesting device, but owing to the extreme sensitiveness not quite commercially available. The idea is to take a dynamo, having the field magnet in series with the circuit and connect a point of the field coil or of the external circuit by means of an auxiliary brush with a point of the commutator. The sensitiveness obtainable by this means can be anything one chooses, and so great can it be made that the slightest disturbance on the machine may upset the whole circuit, and if there be arc lamps in the circuit, for instance, one may barely tap a lamp thus causing an imperceptible movement of the carbon rod and all the lamps will go out, if the auxiliary brush is in such a position and the field coil so connected that the shifting of the neutral line is an *accumulative* effect. In order to improve the action of the lamp mechanism I had the machine constructed in such a way as to give periodic impulses of a certain intensity, which produced a noticeable vibration in the field magnet, and so well did I know the machine, operating it day and night (for at that time practically all my life was passed in the laboratory), that I could tell from these vibrations, by holding an iron bar in a certain position to the field magnet whether the machine was working properly. I noted then that sometimes all of a sudden the lights would go out. After this had occurred about a dozen times, I recognized that when I came with the iron bar near to, or touched the field magnet I disturbed the field enough to displace the neutral line and the lights would go out because the machine would discharge itself.

Another observation which I may mention about the iron, came to my attention in 1886. I then made some experiments on a so-called "thermo-magnetic generator. This *nihil bonum* principle has no doubt occupied the time of many inventors. I had attacked the problem in quite a different way, sufficiently promising to justify an engineer to enter into an investigation and see what could be done. The method I followed was to enclose in a stove an iron core which closed the circuit of a magnet, this iron core consisting of a box of iron with small sheets of iron running through offering an enormous cooling surface. I used steam passed through the interstices to cool down the iron. My expectation was that the iron would just cool down a little so as to become magnetized when steam was admitted by means of a valve. The circuit of the magnet would then be closed and the valve was automatically shut. thereupon the iron was again heated and demagnetized. To a certain measure the valve worked well, but the vibration was too slow. It generally so happened that when the steam was admitted it would be decomposed and a greenish flame would shoot out of the stove. This indicated that the temperature of the iron was too high. I would bring the temperature of the iron down by admitting two or three times the steam in pressing upon the valve. Every time I did this, the flame would shoot out of the stove. Then there would be a miss for about two or three times in succession, no flame appearing, the iron being cooled sufficiently, but upon doing it the fourth or fifth time which took about a second or two, the flame would shoot out once more. I could not explain this phenomenon. A little later, I obtained the classical work of Dr. Hopkinson on the recalesence and other properties of iron at high temperatures and plainly saw that it was recalescence. The temperature must have just been varied around the critical point. Still another fact which I recollect to have observed in experiments with high frequency currents is, that an iron rod or wire, when an arc is formed through it, burns and sputters with violence, considerably more so with high than with low frequency or direct currents. It is possible that the effect is solely due to the magnetic properties of the iron, but it might be more or less produced by the increased resistance and consequently more intense heat evolution. With great magnetizing forces an iron wire inserted in a high frequency coil is instantly brought to a high temperature. This I have before pointed out, but Dr. Pupin has observed a similar thing with low frequency currents when a condenser is in circuit and resonating action takes place.

I wish to say in regard to converting to high potential, starting from a low potential dynamo, by simply combining capacity and self-induction with the circuit, that is by resonating action, I have in my paper, which has just gone to print, dwelt specifically upon converting in this manner, and in fact I am delighted to see that Dr. Pupin has taken the views he has here expressed. My usual method was to attach to the alternator a coil consisting of a great number of sections, and to associate adjustable capacity with the coil, and then to vary the number of sections and the capacity until the best resonance was obtained. This obviated the employment of a secondary coil; but I found that in adjusting, it was preferable to convert first the low tension currents to such a high potential that the necessary capacity would become extremely small, so as to be reduced merely to that of two small plates, because of the great difficulty of producing a condenser of large capacity adjustable by extremely small steps, which it is necessary to employ when the potentials are small. If we convert to a potential of say 50,000 volts then the capacity becomes very small, and we can just use a few plates immersed in oil, which permit varying the capacity by extremely small degrees, and without sudden, disturbing breaks. This is one of the reasons why I have always preferred to convert in a secondary circuit. The adjustment by varying self-induction by means of an iron core is, as Dr. Pupin's experiment now plainly shows, not always practicable. I think that the experiments we have seen here demonstrate that we are as yet far from having studied the properties of iron thoroughly, and that much remains to be done in this direction. I agree with Dr. Pupin that the condenser, which I had thought was going to be of great help in alternating work, does not really appear to have a great future in connection with apparatus with large iron cores, as motors or transformers, because of the action of the iron, and for one more reason besides, namely, that we can, especially if we have a high potential or high frequency, always wind a coil so as to take up just enough capacity to be in resonance with the circuit, in so far as the iron core will permit.

I only have to add, that I sincerely thank Dr. Pupin, (which probably is the wish of all), and that I have profited very much by his lecture, which is a clear and precise exposition of the principle, as well as a most interesting experimental demonstration of electrical resonance with a dynamo machine.

[Adjourned.]

[Communicated after Adjournment by Dr. Pupin.]

Mr. Tesla is very kind and generous with his compliments. I do not think, however, that his statement of facts is free from ambiguity. He states that the phenomena of resonance which I have discussed were familiar to him for a long time. My discussion referred to low frequency resonance, whereas Mr. Tesla has, I think, always worked with high frequency apparatus, so that his familiarity with low frequency resonance, the subject which I discuss, is probably not based upon his own experimental investigations. At the time when the Ferranti effect called forth so much valuable discussion in England, Mr. Tesla[1] contributed some remarks to this discussion, which remarks culminated in the following:—"*The writer looked for a case of resonance,*[2] *but he was unable to augment the effect by varying the capacity very carefully and gradually, or by changing the speed of the machine. A case of pure resonance he was unable to obtain. When a condenser was connected to the terminals of the machine * * * the capacity which gave the highest* E. M. F. *corresponded most nearly to that which just counteracted the self induction with the existing frequency.*"

Mr. Tesla states also in this article that the highest value of the augmentation of the potential which he was able to obtain by connecting a condenser in series to the armature of his high frequency (10^4 periods per second) machine was *only four or five times the impressed* E. M. F. What then could Mr. Tesla infer from this result in regard to resonance effects with low frequency? Certainly nothing that was encouraging.

At no other time did Mr. Tesla, so far as I am aware, approach the subject of resonance any closer than in the article just referred to.

Perhaps Mr. Tesla means that he was acquainted with the phenomena of low frequency resonance which I discussed from the investigations of other men, like Fleming, Kapp, Glazebrook, Swinburne, Lodge, Blakesley, etc.[3] I appreciate fully the value of the labors of these men, but still I fail to see a very close re-

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1. *Electrical World*, N. Y., Feb. 21st, 1891.
2. The meaning of this term Mr. Tesla did not explain.
3. See *London Electrician* for 1889, 1890 and 1891, also "Fleming's Alternate Current Transformer," vol. ii., p. 401.

lation between their work and mine, excepting in one point, and that is, that they as well as I, have shown theoretically that it is possible to produce a high rise of potential by a combination of self-induction and capacity. *I have shown it experimentally also.* But I think that I have pointed out very clearly in the course of my lecture that I did not regard the resonant rise of potential as an end but simply as a means to an end, the end being harmonic analysis of E. M. F's, behavior of iron and dielectrics under the inductive action of resonant currents of low frequency, weeding out of harmonics by means of resonance, etc. With these phenomena Mr. Tesla does not appear to be familiar, either from his own experimental investigations or from investigations of others, excepting those phenomena which Dr. Duncan had investigated some time before me and which I mentioned in the course of my lecture.

Report of the Committee on the Provisional Programme for the Congress.

(The following is the appendix referred to in the report of the Committee on the Provisional Programme for the Congress. See page pp. 14 and 26 *ante*.)

APPENDIX IV.

Electric Notation, Abbreviations and Symbols.

The following is an abstract of a paper read by Mr. E. Hospitalier at the International Congress in Frankfort, 1891, giving the system which the Committee of the American Institute of Electrical Engineers suggested using as a basis for an International system to be adopted at the next Congress.

The object of this system is to introduce a uniform international system of symbols, abbreviations, etc., for terms used in the electrical science, which shall be as universally adopted and understood as the various signs used in algebra and trigonometry, for instance. In such a system, he states, that it is indispensable to establish a clear and precise distinction between a *physical quantity*, its *magnitude* or *value*, and the *unit* for measuring such a quantity. A physical formula shows the relation between physical quantities each of which is represented by a special symbol. The magnitude or value of physical quantities are represented by the ratios of the physical quantity and one of the same nature taken as a unit; the magnitudes or values of physical quantities are therefore simply abstract numbers. A *unit* is a physical quantity of a special magnitude which serves as a measure of quantities of the same nature; these units are designated by special names, and may be represented by abbreviations for the sake of simplification. Symbols representing physical quantities enter into physical formulæ, but units never do. In the C. G. S. system, for instance, the three basic physical quantities are length, mass and time, which are represented by the respective symbols L, M and T; the corresponding units are the centimetre, the mass of one gram, and the second; their abbreviations are cm, g, and s. For derived quantities, definitions are established by equations which show the relation between the fundamental quantities. The physical quantity, *surface*, for instance would be represented by the symbol S and defined by the equation $S = L \times L = L^2$ and the unit for measuring it is the square centimetre represented by the expression cm^2; the physical quantity, *velocity*, represented by v is the quotient of the length by a time and is defined by the equation $v = L \div T = L\,T^{-1}$ and the unit is a centimetre per second. This would establish the important and essential distinction between the symbols representing physical quantities, and the units for measuring their magnitude. He suggests using italics, Greek and script letters exclusively for symbols, while for the units and their abbreviations he suggests using Roman letters.

He suggests furthermore using the same letters for physical quantities of the same nature whose dimensions are different, or for different physical quantities whose dimensions are the same; he proposes, for instance, to use the symbol W for the physical quantity "energy," be it mechanical, thermic, chemical affinity or electric; also, to represent the moment of a force or of a couple, as it is, like work, the product of a force and a length. A length and a coefficient of self-induction have the same dimensions and are to be represented by the same symbol L to which in certain cases an index may be added to avoid confusion. He furthermore suggests adopting the system already in use of representing kilometres by km, millimetres by mm, milligrams by mg, etc. Of the six different ways of representing quotients:

$$\text{centimetre per second};\ \frac{\text{centimetre}}{\text{second}};\ \frac{\text{cm}}{\text{s}};\ \text{cm : s};\ \text{cm.s}^{-1};\ \text{cm/s};$$

he recommends using cm : s as the simplest. His suggestions for a complete system of such units and quantities as are used by electricians, is given in the following table which he claims to have modified to conform with the decisions adopted by the Congress of 1891, which have been given in Appendix I. [See next page.]

In addition he also suggests the advisability of a uniform system of reference to articles in technical periodicals; this should state the volume, the page, the *date* and in every case the year. He also recommends that in adopting new names, very great care should be taken: the word "drehstrom" for instance, (which literally means a rotary current) does not correspond to any physical reality; the proper expression would be "polyphase alternating currents."

[The undersigned calls attention to the fact that the symbols do not all conform strictly to the adoptions of the Congress of 1891, which decided that Greek letters should be used only for physical constants and angles; Mr. Hospitalier furthermore uses "small caps" for the symbols representing the units, in place of large capitals, a modification which is to be commended, as it avoids confusing A (ampere) with A (acceleration). The chief objection which English speaking people will have to this system is, the changing of the classical expression for Ohm's law from $C = \frac{E}{R}$ to $I = \frac{E}{R}$, and writing $I^2 R$ for $C^2 R$. This might be overcome by writing C for current, K for capacity, and I for moment of inertia.]

CARL HERING,
Chairman.

HOSPITALIER'S PROPOSED SYSTEM OF NOTATION.

(Reprinted from "*L'Industrie Électrique*," by the American Institute of Electrical Engineers.)

TABLE.

SYMBOLS OF PHYSICAL QUANTITIES AND ABBREVIATIONS FOR UNITS.

PHYSICAL QUANTITIES	SYMBOLS	DEFINING EQUATIONS	DIMENSIONS OF THE PHYSICAL QUANTITIES	NAMES OF THE C. G. S. UNITS	ABBREVIATIONS OF THE NAMES OF THE C. G. S. UNITS	PRACTICAL UNITS	ABBREVIATIONS OF THE PRACTICAL UNITS
FUNDAMENTAL.							
Length	L, l		L	Centimetre.	cm	Metre.	m
Mass	M		M	Mass of one gramme	g	Mass of a kilogramme.	kg
Time[1]	T, t		T	Second.	s	Minute; hour.	m h
GEOMETRIC.							
Surface	S	$S=L.L$	L^2	Square centimetre.	cm²	Square-metre	m²
Volume	V	$V=L.L.L$	L^3	Cubic centimetre.	cm³	Cubic-metre	m³
Angle[2]	α	$\alpha=\frac{\text{arc}}{\text{radius}}$	A number	Radian.		Degree; minute; second, grade.	
MECHANICAL.							
Velocity	v	$v=\frac{L}{T}$	LT^{-1}	Centimetre per second.	cm · s	Metre per second.	m : s
Angular velocity	ω	$\omega=\frac{v}{L}$	T^{-1}	Radian per second.		Revolutions per minute.	t : m
Acceleration	A	$A=\frac{v}{T}$	LT^{-2}	Centimetre per second per second.	cm : s²	Metre per second per second.	m : s²
Force	F	$F=M.A$	LMT^{-2}	Dyne.	dyne	Gramme; kilogramme.	g : kg
Work	W	$W=F.L$	L^2MT^{-2}	Erg.	erg	Kilogrammetre.	kgm
Power	P	$P=\frac{W}{T}$	L^2MT^{-3}	Erg per second.	erg : s	Kilogrammetre per second.	kgm : s
Pressure	p	$p=\frac{F}{S}$	$L^{-1}MT^{-2}$	Dyne per square centimetre.	dyne · cm²	Kilogram per square centimetre.	kg : cm²
Moment of a couple	W	$F.L$	L^2MT^{-2}	Dyne-centimetre.	dyne . cm	Kilogrammetre.	kgm
Moment of inertia	K	$M.L^2$	L^2M	Gramme-mass centimetre squared.	g cm²		
MAGNETIC.							
Strength of a pole	m	$F=\frac{m^2}{L^2}$	$L^{\frac{3}{2}}M^{\frac{1}{2}}T^{-1}$	The C. G. S. magnetic and electromagnetic units have not yet received special names. They are designated by writing "C. G. S. units" after the formula.	No abbreviations exist.		
Magnetic moment	$\mathcal{M}$	$\mathcal{M}=ml$	$L^{\frac{5}{2}}M^{\frac{1}{2}}T^{-1}$				
Intensity of magnetization	$\mathcal{I}$	$\mathcal{I}=\frac{\mathcal{M}}{V}$	$L^{-\frac{1}{2}}M^{\frac{1}{2}}T^{-1}$				
Field intensity	H	$H=\frac{F}{m}$	$L^{-\frac{1}{2}}M^{\frac{1}{2}}T^{-1}$				
Flux of magnetic force	Φ	$\Phi=H.S$	$L^{\frac{3}{2}}M^{\frac{1}{2}}T^{-1}$				
Magnetic induction	$\mathcal{B}$	$\mathcal{B}=\mu\mathcal{H}$	$L^{-\frac{1}{2}}M^{\frac{1}{2}}T^{-1}$				
Magnetizing force[3]	$\mathcal{H}$	$\mathcal{H}=\frac{4\pi NI}{L}$	$L^{-\frac{1}{2}}M^{\frac{1}{2}}T^{-1}$				
Magnetomotive force	$\mathcal{F}$	$\mathcal{F}=4\pi NI$	$L^{\frac{1}{2}}M^{\frac{1}{2}}T^{-1}$			ampere-turn	A : t
Magnetic resistance (*reluctance*)	$\mathcal{R}$	$\mathcal{R}=\rho\frac{L}{S}$	L^{-1}				
Magnetic permeability (*inductivity*)	μ	$\mu=\frac{\mathcal{B}}{\mathcal{H}}$	A number				
Magnetic susceptibility	κ	$\kappa=\frac{\mathcal{I}}{\mathcal{H}}$	A number				
Specific magnetic resistance[4] (*Reluctivity*)	ρ	$\rho=\frac{1}{\mu}$	A number				
ELECTROMAGNETIC							
Resistance	R, r	$R=\frac{E}{I}$	LT^{-1}			Ohm.	o
Electromotive force	E	$E=RI$	$L^{\frac{3}{2}}M^{\frac{1}{2}}T^{-2}$			Volt.	v
Difference of potential	e	$e=RI$	$L^{\frac{3}{2}}M^{\frac{1}{2}}T^{-2}$			Volt.	v
Intensity of Current	I	$I=\frac{E}{R}$	$L^{\frac{1}{2}}M^{\frac{1}{2}}T^{-1}$			Ampere.	A
Quantity of electricity	Q	$Q=IT$	$L^{\frac{1}{2}}M^{\frac{1}{2}}$			Coulomb; ampere-hour	c, A.h
Capacity	C	$C=\frac{Q}{E}$	$L^{-1}T^2$			Farad.	F
Electric energy	W	$W=EIT$	L^2MT^{-2}			Joule; watt-hour.	J; w.h
Electric power	P	$P=EI$	L^2MT^{-3}			Watt.	w
Resistivity	a	$a=\frac{RS}{L}$	L^2T^{-1}			Ohm-centimetre.	o cm
Conductance		$\frac{1}{R}$	$L^{-1}T$			Mho.	mho
Specific conductivity[3]		$\frac{1}{a}$	$L^{-2}T$				
Coefficient of self-induction (*inductance*)	L_s	$L_s=\frac{\Phi}{I}$	L			Henry	H
Coefficient of mutual induction	L_m		L			Henry	H

1 The International Bureau of Weights and Measures has established an important distinction in the notation of "time," according as it refers to the epoch (date of time of day) or the *duration* of a phenomenon. In the former the reference letters are used as indices and in the latter they are on the same line with the numbers, for instance an experiment begun at $9^h\ 15^m\ 46^s$, lasted 9 h 15 m 46 s, and ended at $4^h\ 31^m\ 30^s$. This method is to be recommended.

2 The distinction between the different units for angles (radians, degrees, minutes, seconds, etc.) and their notation is a matter which ought to come before a Geodetic Congress. No propositions are made here.

3 N is the number of windings, L the length of the solenoid generating the magnetizing force.

4 Abbreviations to be desired.

Compilation of Discussions, Suggestions and Criticisms, Appearing in the Technical and Scientific Press upon the Report and Provisional Congress Programme of the Sub-Committee Appointed by the American Institute of Electrical Engineers.

I. The London *Electrician*, vol. xxx, No. 768, p. 377, Feb. 3rd, 1893, comments favorably upon the programme in general.

Criticises the definition offered for impressed E. M. F. on the grounds of inaccuracy.[1]

Questions the need of a unit of reluctivity.

Acquiesces in the suggestion of the term "voltage" in preference to "P. D." "tension" or "pressure," adding, however, that a single term is likely to prove inadequate.

Acquiesces in the suggestion of using the term "transformer" in place of "converter," "dynamotor" in place of "continuous current-transformer," and "continuous current" in place of "direct current."

Objects to the substitution of the "kilowatt" for the "horse-power." No reason is, however, given for the preference of the latter, but it appears to be implied that British engineers are wedded to the term "horse-power."

Approves of the "henry," "gauss" and "weber," among the suggested units.

Objects to the name for a unit of illumination being made "candle-foot" or "bougie-metre," on the grounds that such compound names imply a product of the components. Does not suggest an alternative.

Suggests that the term "installation" be discarded.

II. The editors of the monthly bulletin of the Société Internationale des Électriciens, in the February number (No. 95), in-

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1. This criticism was challenged by one of the members of the sub-committee in a letter addressed to the Editors of *The Electrician*, and published in their issue of March 17th, vol. xxx, No. 774, p. 579. The definition was then admitted to be accurate, conditional upon a proposition that has probably never been disputed.

clude the greater part of the sub-committee's report, with comments of the following purport:—

Acquiescence is given to the suggestion that the practical electrical units already adopted and confirmed by Congresses should not be altered.

In regard to the proposed magnetic units, doubt is expressed as to whether the advantage of suppressing the coefficient $1/4\pi$ would not be sufficient to make the practical magnetic unit of M. M. F. the ampere-turn simply.

Acquiescence is expressed towards the exchange of the term "henry" in place of the term "quadrant."

It is agreed that the "kilowatt-hour" and "ampere-hour" are units in themselves sufficiently brief and explicit to be retained.

The term "Board-of-Trade-Unit" is condemned as being uninternational. It is pointed out that according to the report of the British Board of Trade Committee on Electrical Standards, which report is recommended by the sub-committee of the A. I. E. E., the deposit of silver serving to determine the strength of a current is referred to a weight, whereas it should for greater accuracy of expression have been referred to a mass.[2]

The Board of Trade proposition of the standard ohm, *i. e.*, a mercury column of length 106.3 cms. and 14.4521 grammes mass at 0°C. is favored, provided that this value should be considered as definitely fixed, and not left for future Congresses to amend.

Approval is expressed of the recommendation that no universal wire-gauge should be attempted other than an international designation of wire diameters in millimetres.

In regard to the suggested international system of notation and symbols, the difficulty of determining initial letters suited to all languages is adverted to.

The suggestion is made that the definition:—"The impressed electromotive force is the ratio of the total activity in an electrically conducting circuit to its instantaneous current strength." should for greater clearness be amended to:—"The impressed electromotive force at a given moment is the ratio of the activity to the instantaneous current strength at that moment."

The suggestion is made that since the terms "inductivity" and "permeability" are set forward as synonymous, the former needs not to be introduced.[3]

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2 This criticism appears to have been withdrawn at the next meeting of the Society, *q. v.*

3. The object of the proposal to adopt the term "inductivity" was to complete the system of nomenclature introduced by Mr. Oliver Heaviside, see *The Electrician*, p. 271, Feb. 12th, 1886, where the term "inductivity" is proposed as a substitute for "permeability," also "Electrical Papers," by Oliver Heaviside, vol. ii, p. 28. Mr. Heaviside has, however, since taken the position that "inductivity" is not synonymous with "permeability" (see *The Electrician* for Mar. 24th, 1893, vol. xxx, No. 775, p. 593), so that the exclusion of the term "inductivity," in the sense suggested by the report, is perhaps advisable.

Objection is taken to the recognition by Congress of Matthiessen's standard of conductivity for copper.

It is noted that the definition of the north pole of a magnet is that which holds in France without ambiguity.

The consideration of a nomenclature for electromagnetic waves is held to be rather beyond the scope of the Congress.

Dissension is offered to the retention of the term "voltage" to the exclusion of "P. D.," "tension," "pressure," and "difference of potential," for the reason that a voltage would be an improper term accompanying an evaluation of difference of potential expressed in electrostatic units.

III. In L'Électricien of Paris for the week commencing March 18th, vol. v, No. 116, p. 190, M. Meylan publishes a somewhat condensed statement of the report, with comments of the following purport:—

The term existing in France for the quantity designated in the report by "magnetic intensity," is "magnetic induction" or "induction" simply.

It is considered advisable for the Chicago Congress to designate the proposed standard, ohm, volt and ampere, when practically defined, as the "legal ohm," "legal volt," and "legal ampere."

The advisability of sanctioning the definition by Congress of "impressed E. M. F." is questioned on the grounds of uncertainty and inutility.

For the definition of "inductivity," the following substitute is suggested:—Permeability (or inductivity) is the ratio of induction to magnetizing force.

It is suggested that since Mr. Oliver Heaviside, the author of most of the new terms in relation to magnetic nomenclature recommended in the report, has also introduced important terms into the theory of dielectrics, these also are entitled to an equal degree of consideration, but that the advantage from the use of such terms, considering the additional strain upon the memory, is questionable.

The necessity is questioned for stating in the report that the mutual inductance of two electric circuits is constant in a medium of constant inductivity, and doubt is offered as to whether it can fail to be constant, when this restriction as to the medium is removed.[4]

The definition for "reluctivity" in the report is criticised on the score of inaccuracy.[5]

The expressions recommended in the report for the designation of alternating currents are criticised on the grounds of impropriety in terms. No substitute is, however, offered.

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4. It may be pointed out that the mutual inductance of two coils situated in a medium with the magnetic nature of iron would not generally be reciprocally equal, and that practically, coils wound upon the same iron core, frequently develop this peculiarity.

5. This criticism is valid.

It is pointed out that however desirable it may be to render the metric system of weights and measures international, or to employ the term "kilowatt" in place of "horse-power," such words as "dynamotor" and "transformer" can scarcely be considered within the scope of an international congress owing to the varied requirements of different languages.

It is pointed out that the definition for "mean alternating current," adopted by the Paris Congress of 1889, as expressed by the equation,

$$I = \frac{1}{T}\int_0^T i\,dt,$$

is meaningless unless the symbol T stands for one semi-period, a condition which by an apparent oversight has been omitted.

IV. In the bulletin of the Société Internationale des Électriciens, appearing for March, 1893 (No. 96), on page 127, the minutes are recorded of a discussion opened by the President of that Society in Paris on the 1st of March. The following is an abstract of this discussion participated in by MM. A. Blondel, Guillaume, and E. Hospitalier:—

M. Blondel suggests that the term "inductivity" is unnecessary, since it is synonymous with the already well established "permeability," and that its briefer definition would be the ratio of magnetic induction to magnetizing force at the point considered.

The definition of the "impressed E. M. F." in the report is criticized as being unnecessary, and contrary to the ideas and usages of continental writers. It is pointed out that "impressed E. M. F." as defined in the report, is described by French writers as the "active difference of potential" measured at the terminals of the circuit, and that the occasion for the use of impressed E. M. F. would disappear if the term "electromotive force" were strictly reserved to differences of potential generated in some part of the circuit in virtue of a local transformation of energy, retaining for all other cases the term "difference of potential" simply.

The term "voltage" is regarded unfavorably as being too vague, and in some degree slang.[6]

The term "coefficient of self-induction" is recommended for the quantity designated by the report under the suggested term "inductance," and this latter name is relegated to what is termed the "inductive resistance" of a circuit, and represented by K in the equation,

$$I = \frac{V}{\sqrt{R^2 + K^2}}$$

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6. **This criticism is applicable only to the term "voltage" in the French language, the English word being unexceptionable in this respect.**

V being the effective difference of potential, R the ohmic resistance, and I the effective current.[7]

The definition of "inductance" proposed in the report is also criticized on the ground that while it applies accurately to a single coil of wire, it fails to hold for more complex combinations. So that if two similar separate bobbins are selected without iron cores, and first connected in series with a traversing current I, then in parallel with a current $2\ I$, each bobbin will have the current I in either case, but that according to the proposed definition, the self-induction they offer to the current in the circuit will be reduced to one half in the latter case, while it really is reduced to one quarter.[8]

The definition suggested as a substitute is:—The coefficient of self-induction is at every instant the ratio of the counter E. M. F. produced by the variation of current to the first derivative of the current in respect to time, according to the equation,

$$L = \frac{E.\ M.\ F.}{\frac{di}{dt}}$$

where i = current strength.

The definition of "reluctivity" proposed in the report is criticized as inaccurate, since not merely the volume, but the length and cross-section of the prism of medium has to be considered when its reluctance is reckoned. No substitute is proposed, but it is added that since the reluctivity in a medium is the reciprocal of its permeability, no term is needed for its expression.[9]

In regard to the proposed unit of illumination, the term "lux" is recommended at the suggested value of a bougie-decimale at one metre, for the reason that it is commercially more convenient to describe a lamp as of 100 candles than as of 5 violles.

M. Blondel then considers the concrete standard of illumination. He points out that considerable difficulty exists at the present time in the photometry of arc lamps owing to the difference in quality between their light and that given by the usual secondary illuminating standards. He therefore recommends a sub-standard of his own, consisting of an arc lamp, shaded and screened in such a way that a certain portion of the crater be-

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7. This use of the term inductance appears to be principally confined to the treatment of strictly sinusoidal currents.

8. It is considered that this is an unfair interpretation of the definition suggested by the committee, and that the definition can be applied legitimately to circuits of any degree of complexity. If, in the case of divided circuits, the flux linked with each division of the current is assumed in the definition, and not as linked with the total current, the discrepancy disappears, and the total inductance of the circuit is then determined by the rules compounding separate inductances in multiple.

9. For a similar reason it might be urged that since a name exists for the unit of resistance, no name is needed for the unit of conductivity.

comes the standard emissive surface; and the theory upon which the application rests is that the temperature of pure carbon in the craters of all arc lamps operating under normal conditions is constant, also that the emissivity of the glowing surface in such craters is independent of their molecular structure, within the limits of practical conditions. He further suggests that photometric measurements made with this sub-standard should be designated as "by standard arc."

M. Guillaume (see also *Electrical World*, April 15, 1893, p. 283), pointed out that although the sanction and legalization of units or standards do not fall within the province of a Congress, but remain for official conferences to determine, yet that ample discussion upon such matters is desirable.

Attention was then drawn to the importance of fixing upon a uniform system of terminology for physical properties. The termination-*ility*[10] should pertain to properties of matter independent of its form or volume, while the termination-*ance* might justly be appropriated by properties of matter which are functions of those variables. By this means the following table is obtained :—

Properties of matter.		*Values depending upon form.*	
Resistibility . .	(α)	Resistance .	$(R) = (\alpha\ L^{-1})$
Conductibility . .	(γ)	Conductance	$(\Gamma) = (\gamma\ L)$
Permeability . .	(μ)	Permeance .	$(Q) = (\mu\ L)$
Reluctibility . .	(ρ)	Reluctance .	$(R) = (\rho\ L^{-1})$

The terms "inductibility" and "inductance" might be added, but it would first be advantageous to define "induction," a term now employed with several different meanings. Two of these meanings might be suppressed if "total induction" were replaced by "flux of force," while "magnetic induction" could give place to "flux-density." The importance of adopting the term "conductance" is alluded to, since by its aid the following theorems follow:

The total resistance in a series circuit is equal to the sum of its separate resistances.

The total conductance of a multiple circuit is the sum of its separate parallel conductances.

The practical importance of the "mho" in networks of distributing conductors is also mentioned.

The values proposed in the report for the gauss and the weber are endorsed, but the value of the gilb is dissented from on the ground that in order to retain a coherent practical system of units the unit of M. M. F. should be the gauss cm. and not 1/10 of the C. G. S. unit suggested.[11]

10. The termination-*ivity* might perhaps be equally admissable in this sense.

11. We think that the relations between the "practical" units should call for the value of the gilb as the gauss-quadrant.

On the question of concrete standards, a misapprehension is pointed out in the criticism appearing in the Bulletin of the Society for the previous month (February), where objection was offered to the Board of Trade specification for the ampere determination. The practical method of manipulation described in that specification, if made with proper care, cancelled the discrepancy between mass and weight, which was the subject of criticism.

It is shown that England had already officially adopted the ohm of 106.3 cms., which must be a close approximation to the true value, and that Germany was about to legalize the same value. It was important that France should also adopt a similar course of action

M. Hospitalier drew attention to the words of the report in which the new units proposed are defined in relation to the absolute units. He pointed out that these latter would be more correctly described as the C. G. S. units.

It is considered that the unit of M. M. F. need not be 1/10 of the C. G. S. unit since the difficulty of introducing the constant $1/4\pi$ would offset the advantage to be derived.

The term "bougie-metre" for the unit of illumination is strongly objected to on the ground that the combination would appear to imply the product of the two components. If the term "bougie at a metre" be inadmissable, a new word such as "lux" would be preferable.

The proposal of the report to exclude motions for altering the values of existing practical electric units is commended.

The names of the units proposed, Gilbert, Weber and Gauss are endorsed, but not their values. The instance is cited of the value suggested for flux density—the gauss—at 10^8 C. G. S. lines per sq. cm. to correspond with the volt, but which valuation would render very small fractions necessary, in order to express weak fields occuring in practise.

The suggestion of the report to retain the terms "ampere-hour" and "kilowatt-hour" unchanged as also to avoid attempts at the introduction of a new international wire guage, are all approved.

Objection is taken to the definitions suggested for "impressed E. M. F." and "inductance," and it is observed that these definitions might be replaced with advantage, but substitutes are not offered.

The definition of "reluctivity" as "reluctance per unit volume" is also criticized as incorrect, equally with such a definition of velocity as length per unit time. The phrase in this case, held to be correct, being the "quotient of a length by a time." [12]

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12. The definitions suggested in the report for reluctivity might be altered to the following:

Simple definition: The numerical value of the reluctance offered by a unit cube of the medium between opposed parallel faces.

Strict definition: Reluctivity is the reciprocal of permeability.

It is considered inadvisable for Congress to sanction the term "voltage" as synonymous with "difference of potential," and while the more universal employment of terms such as "transformer" in place of "converter," or "dynamotor" instead of "continuous-current-transformer" may be in themselves desirable, they are secondary matters confined principally to the English language.

V. The discussion on this subject was resumed by the Society at its monthly meeting of the 7th of April, as reported in the Bulletin for that month, No. 97, p. 190. Observations were made by MM. P. Boucherot and A. Blondel.

M. Boucherot does not deny the failure of the definition for inductance proposed in the report when applied to a complex system of circuits, as claimed by M. Blondel at the previous meeting, but claims that the substituted definition offered by M. Blondel is open to objections, when iron is included in the medium, for which reason the definition of the report is endorsed, with the amendment that when the medium contains iron, the coefficient of self-induction so defined should be considered as the instantaneous value only.

Reference is then made to the fact that in France the term "inductance" invariably denotes the product of a coefficient of self-induction by an angular velocity $\left(L\frac{2\pi}{T}\right)$ pertaining to an alternating current circuit whose periodic time is T. The following definition is consequently offered for the term inductance:—

The inductance of an alternating current circuit, is a positive or negative value, of the nature and dimensions of resistance, whose presence gives rise to a difference of potential like a resistance but without entailing absorption of energy." A series of analogies is then presented by which this definition is claimed to simplify language and assimilate expressions.

M. Blondel continued the discussion upon the best definition for the "coefficient of self-induction," or for "inductance" as suggested in the report. Citing the paper of Dr. Sumpner, in the *Phil. Mag.* for June 1887, p. 453, upon this subject, he pointed out that there are at least three ways in which self-induction can be defined, and while the report suggested one of these, it did not suggest that one which appeared to be the most convenient, for which reason his definition was offered as a substitute at the previous meeting.

A motion was then made and carried, to formulate at the hands of a commission, the conclusions of the Society, and to transmit the same to the International Congress at Chicago, (an abstract of the report of this commission will be found below.)

VI. In an article appearing in the *Electrical World* of May, 13th, vol. xxi, No. 19, p 352, Mr. T. R. Rosebrugh assents to the values proposed in the report for the "weber" and "gauss," but points out advantages that would be derived from making the practical unit of M. M. F. the ampere-turn, and making the value of the oersted 4π times more than that the report advocates. By this means it is claimed that the objectional coefficient $1/4\pi$ would be removed from that part of the system involving ampere-turns directly, and thrown into the part relating to reluctance. It is argued that there would be less objection to the introduction of this coefficient associated with the reluctance of a magnetic circuit since in most practical computations of reluctance some coefficients must already perforce enter, especially if, as in English speaking countries, measurements are made and computed in inches.

VII. In No. 33 of L'Industrie Électrique, appearing for the 10th of May, p. 207, M. Hospitalier deals with the subject of symbols and notation for physical quantities, and includes several matters under discussion in connection with the report of the sub-committee of the A. I. E. E.

The table of terms recommended by M. Guillaume in the discussion of the Société Internationale des Électriciens is advocated, except that the term "inductivity" is rejected as superfluous, and "permeability" retained. In accordance with the same plan, the term "capacitance" is suggested as preferable to the already existing "capacity," but this is not pressed in view of the firm hold that usage has given to the latter.

The term "linear capacity" is advocated for the quotient of the capacity of a line of conductor by its length. "linear insulation" for the product of the total insulation of such a line by its length, and "linear resistance" for the quotient of its conductor resistance by its length.

The term "coefficient of self-induction" is retained for the quantity denominated in the report under the term "inductance," but with the definition suggested in the report, remarking that special terms might well be employed to designate the secondary conditions set up when the magnetic permeability of the medium is subject to variation.

The name "henry" for the unit of this quantity is endorsed.

In view of the difficulty of obtaining a satisfactory and convenient system of practical magnetic units, it is suggested that no other than the fundamental C. G. S. units be employed, in which case the names "gilbert," "weber," "oersted" and "gauss" will not be needed.[13]

In reference to alternating current circuits, it is observed that a special name is needed for the angular velocity $\frac{2\pi}{T}$, and the

13. It may be pointed out, that the names for the units are perhaps more urgently needed in practice than any particular combination of values attached to them, and that these names might still fill a great want, if attached directly to the C. G. S. units, even if the corresponding practical values should be rejected.

term "pulsation" suggested by Dr. J. A. Fleming in "The Alternate Current Transformer" is recommended.

The term "impedance" is suggested for the denominator in the expression

$$I = \frac{E}{\sqrt{R^2 + \left(\omega L - \frac{1}{\omega C}\right)^2}},$$

and for the quantity enclosed between the brackets, the term "reactance" is proposed.[14]

The term "impressed E. M. F." is considered unnecessary. The definitions proposed in the report for the designation of alternating current systems are criticised as being lacking in rigor, but no substitutes are offered.[15]

The term "polyphase" is recommended to apply not merely to systems of more than three currents, but to systems of more than a single current, thus embracing diphase and triphase systems.

VIII. At the meeting of the American Institute of Electrical Engineers, on the 17th of May; a discussion was opened upon the subject of the Congress proposals. Mr. T. D. Lockwood restated the criticisms which he had made upon the sub-committee's report in the *Electrical Engineer* for Feb. 22nd, 1893, vol. xv, No. 251, p. 196, in relation to the definition of the poles of a magnetic needle. He suggested substitution of the term "davy" for "bougie-metre" as the unit of illumination, and the use of the term "direct current" in place of the term "continuous current."

It was pointed out by one of the members of the sub-committee that Mr. Lockwood's amendment of the first mentioned definition might well be advocated, changing the definition into the following: "The north pole of a magnet to be defined as being that which seeks the terrestial north pole, and the south pole that which seeks the terrestial south pole." The remaining criticisms were, however, matters of opinion and for discussion, and in particular that it seemed inadvisable to introduce a new name for a unit whose existence was scarcely warranted as a definition. Professor Nichols pointed out that the unit of light adopted by the Congress of 1884 (the violle), can only be considered as a temporary standard until such time as light can be measured in

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14. The term impedance has already received definition from the Paris Congress of 1889, in a less restricted sense.

15. This criticism is valid. The following amendment might be offered:
"Simple" alternating current system for the usual alternating current systems employing a single periodic current; "diphase" alternating current systems for such as employ two periodic currents differing in phase by approximately a quarter period; "triphase" alternating current systems for such as employ three periodic currents differing in phase by one-third or one-sixth of a period, and "polyphase" alternating current system for such as employ more than three currents.

C. G. S. units, and that for this reason alone a special name for the unit of illumination based upon the existing standard was undesirable.

IX. In No. 32 of L'Industrie Electrique, appearing for April 25th, page 181, M. E. Hospitalier makes certain propositions in relation to the definition of physical quantities by international congresses.

M. Hospitalier proposes that every definite physical quantity should be defined if possible as follows:—

1. By a special name.
2. An international symbol.
3. An equation expressing its relation to other fundamental or secondary physical quantities.
4. A C. G. S. unit with a name and symbol to correspond, when justified by frequent employment.
5. One or more practical units, decimal multiplies or sub-multiplies of the C. G. S. unit with corresponding designations.

Also when possible:—

6. By a fundamental standard for scientific purposes.
7. A practical standard for industrial applications.

As regards names, each physical quantity should have one and only one title. Quantities depending upon length might be specified as linear, those referred to surface as superficial or sectional, and those referred to volume as cubic or voluminal. Thus the quotient of the static capacity of a submarine cable by its length would be its linear capacity measured in microfarads per kilometre, while its insulation of a line of the cable referred to the kilometre would be its linear insulation, expressed in megohm-kilometres.[16]

Such expressions as kilometric capacity, or kilometric insulation, are pointed out to be objectionable as definitions of a quantity, inasmuch as they introduce a particular unit—the kilometre—whereas only physical quantities should appear in the definition of a physical quantity.

Thus it is illogical to speak of velocity as a length per second, specific resistance as resistance per cubic cm., magnetic induction as the number of lines of force per sq. cm., frequency of an alternating current as the number of its periods per second, etc.

When energy is referred to unit volume, as for example the energy of electrification in a dielectric, and defined by the relation $\frac{W}{V}$ where W is the energy and V the volume, the expression voluminal or cubic energy would be preferable to the term

16. Although such a convention may be desirable for definiteness in terms, it seems fair to point out that the expression, "megohms per mile" in the sense of average insulation in, or to, each mile, is not incorrect, in ideas or language, so far as English is concerned. In other words, that the meaning of the preposition "per" is not at present restricted to the operation of division.

specific energy hitherto in general adoption. The rule is, however, recommended only for new definitions, and the quotient of mass by volume for example would remain density rather than become cubic or voluminal mass.

If possible similar adjectives should be applied to specify the reference of new physical quantities to mass and time, with a view to convenience and accuracy of expression.

The importance of assigning a symbol to each physical quantity is found in the facility thus offered for the interpretation of algebraic equations, and to reduce the number of symbols necessary, it is proposed that quantities having the same dimensions should be expressed by the same symbol. The advantage of an equation of definition is alluded to, since an algebraic equation often serves to briefly and simply define a quantity in terms of other quantities. The dimensions of the former are then determined by substituting for the latter their dimensional expressions.

For convenience in dealing with C. G. S. units in frequent use, the selection of names for them is advocated, as already adopted with the "dyne" and "erg." Compound units should be separated by a hyphen when the components are multiplied, and by the word *per* when divided, as ampere-hour and metre per second.[16] The abbreviation of such compound units would be indicated by Roman letters according to the recommendation of the Frankfort Congress, taking the initial, or the first few letters of the term.

In regard to concrete standards, it is recommended that the ohm and volt that may be decided upon and defined by the Chicago Congress, should be characterized for definiteness by a suffix or characteristic term such as the Chicago ohm, the ohm $_{1893}$, or the ohm $_{106.3}$, the word ohm simply being retained for the unit defined as 10^9 C. G. S.

It is considered inadvisable and incompetent for a Congress to adopt such secondary or practical concrete standards as a definite voltaic cell for E. M. F., or a definitely constructed dynamometer as a current measurer. While the practical value of such standards is not denied, it is considered that they can scarcely form a fit subject for international agreement.

X. The *Elektrotechnische Zeitschrift*, No. 17, April 28th, 1893, page 233, publishes a translation of the complete report.

In some editorial remarks it is claimed that the propositions go far beyond the province of an International Congress. An International Congress, it states, can determine the names to be used for units, but is not concerned with the magnitude of those units.

Belief is expressed that in Germany such names as "reluctance," "inductance," "illumination" and "voltage" would meet with opposition.

Some of the propositions are claimed to give the impression that the Congress was a national instead of an international one.

Objection is made to the introduction of the violle, which is not in use anywhere.

Belief is expressed that the propositions regarding the ampere and ohm will meet with universal approval.

Regarding the Matthiessen standard, objection is made to the adoption of any standard resistance of copper[17] as the specific resistance of copper can always be referred to that of mercury.

XI. The *Electrotechnische Zeitschrift*, No. 21, May 26th, 1893, page 293, reprints in full the official discussion of the sub-committee's programme, by a committee of the Electrotechnische Verein.

Objection is made to the violle and to any secondary units based on it, on the ground that sufficient researches have not yet been made with that unit to entitle it to become a practical unit. A reference is made to a new unit based on the light emitted from platinum at a temperature below its melting point. For a practical unit of light the Hefner lamp is recommended in the exact proportions given it originally by its inventor; in this form researches made with it, in the Imperial Institute, over a period of four years, have shown it to be very reliable, and to change the dimensions would involve a repetition of this long series of tests; it is furthermore claimed that this is the only lamp among all the others that have been proposed, which meets the requirements of a concrete standard; secondary units, such as that of illumination, should therefore be based on this lamp.

The definition of impressed electromotive force is not approved; in general it is claimed that heretofore Congresses have undertaken only the adoption of units, and not of definitions.[18]

Objection is raised to a number of terms which are specifically English, and with which it is claimed an International Congress is not concerned.

A recommendation is made that the Congress should not consider definitions, that therefore all the propositions regarding "inductance," "reluctance," "inductivity," "reluctivity," etc., should be omitted, and that the Congress confines itself to units alone. The only exception made is that concerning the definition of a north magnetic pole, which is approved.

Emphatic approval is expressed of the suggestion not to change the ampere and the farad as well as any other of the six fundamental quantities; approval is also expressed of the proposition not to name the kilowatt-hour and the ampere-hour.

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17. The proposition regarding the Matthiessen standard has been misunderstood; there is no intention to define a standard resistance of copper, but merely to state which one of the various values given by Matthiessen shall be considered to be the standard, when reference is made to a Matthiessen standard. As this is not a question of international interest, it may be advisable not to bring it before the Congress.

18. This remark can only be intended to apply to Congresses previous to that at Paris in 1889.

The propositions regarding the ampere, ohm, volt and watt, are approved, with a slight alteration to the effect that the suggestions regarding the volt, watt, etc., be made to read, "these units are to be based on that ampere and that ohm."

Objection is made to stating the value of a B. A. unit in terms of the ohm, as this is a mere matter of measurement, and does not concern the Congress.

Objection is taken to the adoption of the Matthiessen standard[19] on the ground that it cannot be definitely defined until the degree of softness of copper can be indicated.

Regarding the terms "true ampere" and "true ohm," etc., it is claimed that no international decision is necessary, for when these quantities are represented by letters the true values are understood, and the few persons who are concerned with the true values will express themselves so as to be understood.

Approval is expressed of the adoption of a unit of electrical conductivity, but not of the word "mho," in place of which "thomson" is recommended.

The name "henry" is approved, although it is thought that there is no real need of a new name for the quadrant. The suggestion is made that the names "Gauss" and "Siemens" should receive first consideration in naming any new units in the future.

Regarding the magnetic units, it is thought that there will be strong opposition to them among practical engineers who universally use the C. G. S. units, against which there are no objections ; it is claimed that the phrase "lines of force per square centimeter" is preferable to the word "gauss," which requires a definition before it means anything definite ; it is therefore recommended that the C. G. S. magnetic units be made the practical units.

Regarding nomenclature and symbols it is suggested that for angles the letters β and γ be used in addition to α ; that for velocities the letters u, v, w, be used ; that for the acceleration these same letters be used in preference to the letter A ; for forces the letter f be used together with F; that static moment be distinguished from work by representing the former by the letter D; in M. Hospitalier's proposition different letters are used for field intensity and magnetizing force, two quantities which however are of the same nature, for it is immaterial, in a magnetic field, whether the magnetism is produced by iron or by ampere windings; for these reasons it is recommended to use the letter $\mathfrak{H}$ for both ;[20] it is proposed to change the letter Φ into $\mathfrak{B}$ for mag-

19. This is apparently a misinterpretation of the recommendation in the report which is that the resistivity corresponding to Matthiessen's standard be defined, and not that the resistivity of soft copper be defined. Matthiessen's standard is an arbitrary one, much employed in industrial applications, and often misquoted in valuation.

20. The letter H is so universally used at present that it would be unwise, if not altogether useless, to attempt to change it, especially for a more unusual letter.

netic flux; it is recommended to permit the use of the expressions $U_2 - U_1$ and $\frac{dU}{dl}$ for representing differences of potentials or a fall of potential respectively; it is suggested to use the letter i in addition to I for representing currents; instead of representing specific electrical resistance by a and magnetic by ρ it is proposed to reverse these two letters, as ρ is already in universal use for the electrical resistance; it is suggested to use the letter λ for specific electrical conductivity, and the letter G for the conductivity, as that is about the only letter left in the alphabet which would not create confusion.

Regarding the C. G. S. units, it is suggested to leave the choice open either to adopt Hospitalier's list, or simply to write C. G. S. units.

As minute and metre are represented by the same letter m, it is suggested to abandon the use of the former, and to write sixty seconds instead; a recommendation is made to distinguish a force of one kilogram from a mass of one kilogram by adding an asterisk when the former is meant.

Approval of the universal adoption of the metric system is expressed in the most emphatic terms, as also the designation of wires in hundredths of a millimetre.

Approval is expressed of the suggestion to adopt the kilowatt as a unit of power.

In the conclusion disapproval is expressed of the introduction of new names, as in most cases the word "unit" or "C. G. S. unit" is sufficient.

XII. The *Elektrotechnische Zeitschrift*, No. 21, May 26th, 1893, page 295, contains an article by Mr. M. von Dolivo-Dobrowolsky on "The Question of the Legalization of Electrical Units of Measurement."

He discusses the proposed definition of an ampere in which it is defined in terms of the electro-chemical equivalent per second, and points out that it would be very much better, besides being much more rational, to first define the coulomb by its electro-chemical equivalent, and then to define the ampere in terms of the coulomb. He also discusses the definition of an alternating current ampere, calling attention to the fact that, as at present defined, an alternate current ampere, if redressed, would not be equal to the ampere defined by the electro-chemical equivalent; the two amperes are therefore different depending on how they are measured; in this connection he also points out that there is a third way of measuring it, namely by means of a condenser, or by the lines of force generated, in which case the measurement would give the maximum value of the alternating ampere. In order to reduce all to the same basis, he suggests having one ampere, as defined above, and to define the "mean" value of an alternating or pulsating current as the arithmetical mean of the different values,[21] the value based on the square root of the mean of the squares, being called the "effective ampere"; no confusion can then arise. The same applies equally well to volts. He also points out that it is more rational to first define the joule in terms of the coulomb, and then the watt in terms of the joule.

Regarding the proposed magnetic units, he expressed himself strongly in favor of retaining, as practical units, the present system of C. G. S. units, and ampere-turns, claiming that they are already in universal use and that there is no objection to them; he suggests that the names proposed might be given to the C. G. S. units. (A more complete abstract of his article will be found in *The Electrical World*, July 1st, 1893, page 9.)

XIII. The American electrical journals of the week ending April 29th contain a letter from a number of electric lighting companies asking that the Congress define the general commercial terms 2,000 c. p., 1,200 c. p., etc., arc lamps, in practical electrical units; they suggest the following definition: "The term 2,000 candle power arc to mean an arc produced by 10 amperes and 45 volts potential difference between the carbons, or a 450 watt arc. The candle power of arcs produced by currents of more or less amperes or more or fewer volts difference of potential, to be rated proportionally."

XIV. The Bulletin of the Société Internationale des Électriciens for May, 1893, No. 98, continues on page 266 the discussion upon the Congress proposals, by M. P. Boucherot. M. Boucherot adduces further arguments in support of his views as to the best definition for self-induction, as expressed at the previous meeting, and referred to above.

XV.—The June number of the Bulletin of the Société Internationale des Électriciens contains the report of the committee appointed by that Society to formulate a discussion on the report of the sub-committee of the American Institute of Electrical Engineers. The committee consists of twenty members of the Society, with M. L. Raymond as chairman, M. Pellat as reporter, M. E. Boucherot as secretary, and members as follows: Messrs. R. Benoit, E. Blondel, E. Bouty, A. Cornu, G. Foussereau, C. E. Guillaume, A. Hillairet, E. Hospitalier, J. Joubert, Leduc, G. Lippmann, E. Mascart, D. Monnier, G. de Nerville, H. Pellat, A. Potier, R. V. Picou, J. Violle.

The following is a translation of the report of this committee:—

1. *Ratification of units, terms, and symbols adopted by preceding Congresses.*

The committee is of the opinion that it is extremely desirable from every point of view to leave unchanged the decisions of preceding Congresses, except in cases of contradiction or error.

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21. Both these definitions have already been adopted by the Paris Congress of 1889.

Each Congress is interested in respecting the decisions of preceding Congresses in order that its own decisions may carry respect.

2. *Denomination of certain quantities.*

In the formation of new terms, the committee recommends the employment as far as possible of the termination *-ance* to designate quantities relating to a conductor, or more generally to a body taken in a definite form, (for example, the *resistance* or *conductance* of an electrical conductor), and to reserve the termination *-ity* for quantities which characterize the properties of matter from which bodies are formed. Thus *resistibility* or *conductibility* would be specific resistance or specific conductance.

The committee considers that the terms "coefficient of self-induction" and "coefficient of mutual induction" have been in use so long without ambiguity that it is undesirable to alter them.

The same view is held concerning the terms "permeability" and "susceptibility," introduced into science by Lord Kelvin, and employed by all electricians.

Conformably with the opinion of the committee of the American Institute, the committee considers that it would be desirable to give the name "reluctance" to that quantity which has hitherto been designated under the term "magnetic resistance," namely,

$$\int_o^l \frac{d\,l}{\mu\,s}$$

l representing the length of a magnetic conductor in the direction of the lines of force, s its cross section, and μ its permeability.

The committee proposes that a name should be given to the quantity whose square added to the square of the resistance in a circuit traversed by a periodic current gives the square of its apparent resistance. The name "reactance" could be applied to this quantity.

"Impressed electromotive force," according to the definition of the Committee of the American Institute, does not appear to differ from the real electromotive force residing in the circuit, at each instant.

Concerning the term "voltage," it has been introduced into industrial practice, and should be retained, but it appears unnecessary to substitute it generally for "difference of potential."

3. *Propositions in regard to the creation of new practical units, or new names for practical units.*

On the subject of the name "henry" which it is proposed to substitute for the name "quadrant," adopted by the Congress of 1889, for the practical unit of the coefficient of self or mutual induction, the Committee refers to the principle of not changing that which has been adopted by a preceding Congress.

The name "mho," proposed by Lord Kelvin, appears advantageous for designating the inverse of the ohm, in other words, the practical unit of conductance.

The selection of a practical unit of illumination, which should be produced by the bougie decimale upon a surface placed normally to the rays of light at a distance of one metre, supplies an existing want Some objections have been raised in regard to the term bougie-metre applied to this unit.

The opinion of the Committee is opposed to the creation of the practical units of magnetomotive force, magnetic field intensity, and magnetic flux proposed by the Committee of the American Institute.

The proposed practical unit of magnetomotive force, with the name "gilbert," and with the magnitude of one-tenth of the C. G. S. unit, is much too small; on the other hand the practical units of field intensity and of magnetic flux, proposed under the names of the "gauss" and the "weber," with magnitudes of 10^8 C. G. S. units, are much too large.

It is true that the use of the gilbert would present the advantage of yielding with this unit the value of the magnetomotive force directly from the formula $4 \pi n i$, when i is the strength of the current expressed in amperes.

The adoption of the gauss would also present the advantage, that expressing the intensity of a field H in terms of this unit, the induced electromotive force E developed at the extremities of a wire l cms. centimetres long, placed perpendicular to the lines of force, and cutting them normally with a velocity of v centimetres per second, would be expressed in volts by the formula $E = H l v$.

But the simultaneous employment of the gilbert and gauss might lead to error in calculations by omission of the factor 10^9, in case it should be desired to calculate an intensity of magnetic field by dividing the length of a tube of induction into the magnetomotive force considered, for if the magnetomotive force be expressed in gilberts, and the length in centimetres, the intensity of magnetic field arrived at will be in units of 10^{-1}, and not of 10^8 C. G. S.; it would therefore be necessary to divide again by 10^9 in order to obtain the field intensity expressed in gausses.

It seems, therefore, more advantageous not to introduce these units which are inconvenient in point of magnitude and dangerous in point of accuracy for calculations.

Besides, in practice, magnetomotive force is specified by the number of ampere-turns, an expression which appeals directly to the eye, and which is readily capable of conversion into C. G. S. units.

4. *Defining and adopting: 1st. means for materializing the principal practical units by standards capable of being readily reproduced; 2nd, names, for these and for the theoretical units, by means of which they can be separately distinguished.*

The modification of the legal ohm proposed by the committee of the American Institute is a diplomatic matter that cannot be settled by the Congress.

The committee, however, recognizes that in a new definition of the practical ohm, it would be desirable to replace the definition of its section, whose precise measure is unattainable, by a specification of the mass of mercury contained in the length of the ohm.

The conference of 1884 deemed it inadvisable to materialize the ampere and volt as it did the ohm; upon the suggestion of Lord Kelvin, the ampere remained defined as the one-tenth part of the c. g. s. unit of current, and the volt as that electromotive force which maintains an ampere through one ohm.

The unanimous opinion of this committee is that it is inexpedient to revoke this decision.

The definition of a standard of resistance under the term "legal ohm" was well chosen, since standards of resistance can be made which remain equal, and which can be mutually compared with great precision, while the determination of the absolute value of a resistance is a very long and delicate operation, susceptible of much less precision.

It is quite otherwise, however, in the case of currents and electromotive forces.

The direct determination of the strength of a current in absolute measure by the employment of a dynamometer balance is a rapid and precise operation, when the electrodynamometer has been prepared with the care suitable for the construction of a standard instrument, or when its constant has been determined by comparison with one of these standards. On the contrary, the determination of the strength of a current by the weight of an electrolytic deposit is a long and laborious operation, susceptible of much less precision. If a standard of current strength (standard ampere) were defined electrolytically, the strength of a current would be determined with more difficulty and with less exactitude by the standard ampere than by reference to the theoretical ampere, for which reason such a definition is unserviceable.

The Committee recognizes, however, the great importance of the recommendations of the recent report of the Board of Trade upon the means of determining with the greatest possible accuracy the strength of a current by reference to the fact that one ampere deposits in one second 0.001118 grammes of silver (to within about 1 part in 1000). This enables an electro-dynamometer balance to be dispensed with when a degree of precision greater than 1 part in 500 is not desired.

As regards the materialization of the volt, no galvanic element retains an electromotive force constant to the same degree of precision with which this electromotive force can be obtained by comparing it with the difference of potential determined from a known current transversing a known resistance, a rapid and readily executed operation. A galvanic element can therefore only serve as a temporary standard, and by taking its temperature variations into account.

The Committee does not consider it advisable to establish a bougie-decimale standard based upon the use of the amyl-acetate lamp. The study of these lamps has shown that they are not sufficiently constant, and that their light is not sufficiently white.

Like the Committee of the American Institute, the opinion of this Committee is opposed to the creation of a standard of conductivity.

It is recommended to indicate commercially the qualities of a metal in regard to its conductivity, by the resistance of a wire of this metal, having a length of 100 metres and weighing 1 kilogramme.

5. *Definitions of certain expressions.*

The Committee points out that the terms "north pole," and "south pole" for magnets have already been adopted by the Congress of 1889.

In regard to alternating currents, the expressions of "simple currents," diphase," triphase," etc., "polyphase," are in general use, and it is desirable to retain them.

6. *International system of notation and symbols.*

In consideration of the diversity of languages, the Committee considers that propositions in this regard can only be made to advantage in Congress.

XVI. Attention is also called to the following articles on the modern ideas of the theory of electrical quantities, by Mr. Ch. Ed. Guillaume, in the April number of the Bulletin of the Société Internationale des Électriciens, 1893, page 185. "Energy as a Dimensional Unit," London *Electrical Review*, April 28th, 1893, page 495. An article on the propositions for the legal determination of electrical units of measurements in the *Elektrotechnische Zeitschrift*, No. 18, May 5th, 1893, page 245. An article on "The Physikalisch-Technische Reichsanstalt in 1891-2," in the London *Electrician*, May 26th, 1893, page 107. An article "On the Proposed Practical Magnetic Units—Gilbert, Weber, Oersted and Gauss," in the *Electrical World* for May 20th, 1893, No. 20, vol. xxi, p. 371. The *Elektrotechnische Zeitschrift*, June 23rd, page 353, contains an article by Dr. R. Wachsmuth, of the Imperial Technical Institute, in the nature of a reply to the article of Mr. Dobrowolsky referred to above; the same number, page 361, contains a report of the discussion of the INSTITUTE's proposals, at a meeting of the Elektrotechnische Verein; both of these have been received too late to be included in the above resumé; neither of them contain matter of any great importance. (See also the discussions at the May meeting of this INSTITUTE, page 422, also the contributions to the INSTITUTE on this subject, pages 429 and 432 of the present number.)

Résumé of Opinions and Suggestions Expressed in Reference to the Report of the Sub-Committee on Provisional Programme, and to the Decisions of the Forthcoming Congress at Chicago.

Opinions have been unanimous in favor of leaving unchanged the values of existing practical electrical units, as already adopted by past Congresses.

It has been pointed out that the definition of the mean intensity of an alternating current, adopted by the Paris Congress of 1889, is incorrect as stated, and calls for a slight amendment by way of explanatory addition.

It is generally admitted that an international unit of illumination is in practical demand. The unit proposed in the report, namely, the illumination of a bougie-decimale at the distance of a metre, has been well received, except in Germany, where a sub-standard unit of illumination based upon the Hefner-Alteneck lamp has been favored.

The definition of the concrete standards of the practical units, as recommended in the report, and taken from the Report of the British Board of Trade Committee on Electrical Standards, has met with general satisfaction. In regard to the ohm it has been suggested that the definition shall be regarded as final from a practical point of view, and that whatever slight discrepancy may be discovered in the future between this and the best known approximation to the true ohm, shall not affect the practical concrete standard, whose formal specification might be worded and denoted accordingly.

The retention of the terms ampere-hour and kilowatt-hour has been universally favored.

The introduction of a practical unit of conductance has met with favor, and the name mho for this unit has only met with opposition in Germany on the plea of an unclassical treatment of a name held in universal respect, the name "thomson" being advocated as a substitute.

The recommendations of the report in reference to the new practical magnetic units has given rise to considerable discussion and difference of opinion. It has long been contended in English technical journals that the practical computations of electro-magnetic machines based upon C. G. S. magnetic units, introduce burdensome coefficients, such as 10^8, with inconvenient frequency, while to fill the needs of a system of nomenclature, names for magnetic units in such computations have been improvised and employed provisionally. The German proposals have, however, been to retain the C. G. S. units, and without names for them. In France the same difficulty of framing a practical system of magnetic units into a thoroughly convenient and harmonious series has elicited a dissension as to whether any or all of the units recommended in the report should be adopted. A Canadian suggestion has been made to adopt the units of flux and flux density as proposed, but to make the ampere-turn the practical unit of M. M. F. and alter the unit of reluctance in the ratio of $1/4\pi$ to correspond. Aside from the difficulty in agreeing upon the values of these magnetic units, the names recommended for them in the report have been generally well received, the term Oersted perhaps excepted.

The adoption of the term henry for the unit of self or mutual induction has met with general consent. Difference of opinion has, however, been manifested with regard to the name of the quantity whose unit it denotes, the term inductance having been employed in France to denote the product of a self induction by an angular velocity, while in England and in America writers have been divided, some employed it in this sense, and others, perhaps a majority, with the meaning given in the report, of a coefficient of self induction, or mutual induction, simply.

The suggested definition and introduction of the term inductivity has been disfavored, and the definition for "impressed E. M. F." has been generally considered unnecessary.

The definition of the value of Matthiessen's standard of conductivity has met with opposition in France and Germany as being of national rather than international importance.

The definition of the north pole of a magnet has not been opposed, but it has been pointed out that in the definition recommended in the report the word "terrestial" might replace the word "geographic," with less danger of ambiguity.

The definition of alternating current systems as simple, diphase, triphase, or polyphase, has been favorably considered in all but the wording of the proposal. It has been suggested, however, to consider any non-simple alternating system as a polyphase system.

The use of the term voltage has met with favor in England, but with disfavor in France and Germany.

The terms "transformer" and "dynamotor" in place of "convertor" and "continuous-current-transformer" have not been opposed, except as being somewhat beyond the scope of an international Congress. The more universal use of the term kilowatt in place of the term horse-power has only met with disapprobation in England.

In France and Germany objections were made to some of the terms suggested, on the ground that they were specifically English.

The more universal introduction of the metric system of weights and measures has met with general favor.

Strong recommendations have appeared in favor of an international system of symbols and notation for electric magnetic and physical quantities generally, with a corresponding revision of the existing system of electrical and physical nomenclature. The system of notation advocated by M. Hospitalier, and generally recommended in the report, has not elicited adverse criticisms, except in the case of a few of the symbols suggested.

In conclusion your Committee submits the view that considering the very debatable nature of the subjects forming its report, the variety of opinions formed in different countries and expressed under different interests, the reception of the report has been remarkably favorable, and appears to have attained the desired object, namely a concentration of general opinion toward a concensus, in aid of the forthcoming Electrical Congress.

CARL HERING, Chairman.
A. E. KENNELLY.

ON THE NOTATION PROPOSED BY M. HOSPITALIER.

BY PROFESSOR ALEXANDER MACFARLANE.

I wish to make some observations on the "Table of Symbols of Physical Quantities and of Abbreviations" proposed by M. Hospitalier at the Frankfort Congress, and recently recommended for adoption by the Sub-Committee of the INSTITUTE on Programme for the Chicago Congress. My observations have reference to some features of the plan which it seems to me ought to be discussed and perhaps amended. The account of the sysem which I have before me is that published in the London *Electrician* for 15th of January, 1892.

The analysis upon which the system is based is thus stated: "The first and most indispensable point is to establish a clear and precise distinction between a *physical quantity*, its *magnitude*, and the *unit* which serves as a common measure in a given system to all magnitudes of the same kind. A physical formula always establishes a relation between physical quantities, each physical quantity being represented by a special symbol. The magnitude of a physical quantity is represented by the ratio between a physical quantity and the physical quantity of the same kind which is taken as unity. The magnitudes of physical quantities are, therefore, essentially abstract numbers. Finally, the unit is a physical quantity of a particular size, which serves as a common measure, multiple or sub-multiple, to quantities of the same kind, which is designated by a special name, and which allows of abbreviations intended to simplify speech or writing. The symbols of physical quantities enter into physical formulæ, but units never do."

The above would be a sufficient analysis, if all physical quantities were of the non-directed or scalar kind. But it is not so; there are many electrical quantities of the vector kind, not to speak of more complex kinds. Thus in Clerk-Maxwell's "Electricity and Magnetism," vol. 1, page 10, we find, "When we wish to denote a vector quantity by a single symbol, and to call attention to the fact that it is a vector, so that we must consider its direction as well as its magnitude, we shall denote it by a German capital letter, as $\mathfrak{A}$ $\mathfrak{B}$ etc." In the writings of Fleming, Heaviside and other electricians, such vector quantities are denoted by simple black letters, such as **A B**, which are much easier to write and to read.

To avoid interference with such higher analysis or to provide for it, we require a more extended view of physical quantities. Physical quantities are either non-directed or directed. A non-directed quantity consists of magnitude only; a directed quantity consists of magnitude and axis. Magnitude is further analyzed into ratio and unit. In M. Hospitalier's scheme, symbols are provided for magnitudes only, none for physical quantities involving direction. Thus v denotes the magnitude of a velocity, without regard to direction; F the magnitude of a force; the German $\mathfrak{M}$ means the magnitude of the magnetic moment, not the magnetic moment itself; $\mathfrak{B}$ the magnitude of the magnetic induction, not the magnetic induction itself. This defect may be removed by retaining the black letters **M** and **A** to denote the magnetic moment and magnetic induction, while corresponding italic letters M and A denote the respective magnitudes.

Another feature of the plan, which appears of doubtful utility to one who has studied the higher analysis, consists in denoting by the same symbol all physical quantities which have the same dimensions. For instance, W the symbol for work is also made the symbol for moment of a force, because the dimensions of both are $L^2\,M\,T^{-2}$. But, though their dimensions are the same, these quantities are very different in nature; work is a non-directed quantity, while moment of a force is a directed quantity. In work the two lengths have a common direction, while in moment of a force they are transverse to one another. To give the same symbol to two quantities so different in their nature is not sanctioned by established notation, and it proceeds upon a principle which is novel and not in accord with the results of the higher analysis.

In the higher analysis it is important to have not only a symbol for an angle, but also a symbol for an axis. The Frankfort Congress recommended physical constants and angles to be represented by Greek letters. An axis is also best denoted by a Greek letter. Let **B** denote a directed physical quantity; then if the Italic B denotes its magnitude, and the Greek β its axis, we get a compact systematic notation which is very easily remembered.

In the plan of M. Hospitalier centimetre per second is abbreviated by cm/s, dyne per square centimetre by dyne/cm,^{2} and centimetre per second per second by cm/s.^{2} Here we have a contradiction. If dyne/cm^2 expresses dyne per square centimetre, then by the same rule cm/s^2 expresses centimetre per second squared. The unit centimetre per second per second is properly abbreviated by (cm/s) /s; the idea cannot be unambiguously expressed without the use of a bracket. For example, 980 (cm/s)/s expresses the acceleration of gravity, while 490 cm/s^2 expresses the connection between the fall and the square of the time elapsed.

The importance of the use of a bracket in expressing a derived unit in terms of the fundamental units, is well shown in the case of the unit of specific resistance. In some English works such as Ayrton's Practical Electricity, specific resistance is expressed in terms of "ohm per cubic centimetre." In a recent paper, printed in the *Electrical Engineer*, for December 28th, 1892, M. Hospitalier criticises this usage as follows: "For the same reason, only the units of the quantities which define that quantity should enter into the definition of a unit of measure for a given quantity. Thus for example, the English persist in expressing specific resistance in ohm per cm^3 on the assumption that the specific resistance of a substance is the resistence of a cube of 1 cm. cross section between opposite faces. Specific resistance cannot be measured in ohms per $cm,^3$ it is the product of a length and a resistance, and should be measured in centimetre-ohms."

I drew attention to this matter in a paper read before the American Association for the Advancement of Science at the Washington meeting in 1891. Specific resistance is not properly expressed in ohms per cm^3 because *per* denotes proportionality, and the resistance is not proportional to the volume. The true unit can be expressed with the help of a bracket, thus: ohm per (cm per cm^2), that is, the resistance is proportional to the length divided by the cross-section. This is the direct definition of the quantity, and much more logical than the definition by means of dimensions.

In M. Hospitalier's table no names are suggested for the C.G.S magnetic and electro-magnetic units. It is to be hoped that the principle of defining them by means of their dimensions will not be adopted. Given words for the C.G.S. units of intensity of pole, current, electromotive force, and resistance, then the others can be defined by compound words which express not the dimensions, but the relations of the units to one another. Let P denote the word chosen for C.G.S unit of intensity of pole; X the word for C.G.S. unit of current; Y the word for the C.G.S. unit of electromotive force; Z the word for the C.G.S. unit of resistance. Then,

the unit of magnetic moment is P—centimetre.
" " intensity of magnetization, P per cubic centimetre.
" " intensity of field, dyne per P.
" " magnetic flux, dyne per P—square centimetre.
" " quantity of electricity, X—second.
" " capacity, X—second per Y.
" " specific resistance, Z per (centimetre per square centimetre.)
" " conductivity, X per Y.
" " resistance, Y per X $(=Z.)$
" " specific conductivity, centimetre per square centimetre per Z.

Etc. Etc.

University of Texas,
Austin, Tex., Feb. 21, 1893.

COMMENTS ON THE REPORT OF THE COMMITTEE ON THE PROVISIONAL PROGRAMME FOR THE CONGRESS.

BY DR. JOHN SAHULKA, AUSTRIAN DELEGATE TO THE CONGRESS.

[TRANSLATION.]

I. NEW UNITS.

1. In the calculation of magnetic circuits the field strengths which occur in practice would have to be expressed in very small decimals, and magnetic resistances in very large numbers. In order to have convenient numbers, it would therefore be necessary to use the units micro-gauss and mega-oersted. This makes it desirable to retain the absolute C. G. S. system of units. The practical units volt, ampere and ohm, were introduced only because the absolute C. G. S. units would have given inconvenient figures for the quantities occuring in practice. In magnetic circuits no reason exists for giving up the absolute system of units.

2. The introduction of a unit (the mho) for the electrical conductivity of a circuit is not a necessity, as all calculations can be made with the units ohm, ampere and volt.

II. NAMES FOR NEW UNITS.

1. Should the absolute C. G. S. system of units for magnetic circuits be retained, which from recently expressed opinions seems probable, the introduction of new names would not be necessary.

2 The name "mho" for the unit of electrical conductivity was probably used by some one because it was introduced by Sir Wm. Thomson; should the new unit be introduced, it would be easy to find an appropriate name (thomson). In order to be consistent, one would then have to introduce a practical unit for magnetic conductivity (reluctivity[1]), and give it a name which is the reverse of the name oersted.

3. The name henry for the practical unit of self and mutual induction is preferred to the term quadrant, because induction coefficients are not lengths.

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1 Inductivity, or magnetic permeability, is doubtless what was meant. *Tr.*

III. Definitions.

1. "The impressed electromotive force is the ratio of the total activity of an electrically conducting circuit to its instantaneous current strength." Formerly the term "impressed electromotive force" was understood to mean the difference of potential measured at an alternating current apparatus, or in case the whole circuit is considered, the total electromotive force. This cannot be meant by the proposed definition as the instantaneous current strength occurs in it. But if by impressed electromotive force is meant the instantaneous value of the difference of potential at the terminals or the electromotive force, there is a contradiction, for if, for instance, a circuit traversed by an alternating current contains a self-induction, then at the instant when the instantaneous value of the electromotive force is equal to zero, neither the current nor the accumulated energy in the form of a magnetic field, are zero, and therefore the quotient of these values will likewise not be equal to zero; there is also a contradiction if a condenser is inserted in the circuit.

2. The universally used term permeability should not be replaced by the term inductivity.

3. It is very desirable that the definitions of induction resistance (inductance), and of the coefficient of self and mutual induction, be adopted in accordance with the suggestions of the Committee, taking into consideration the total field which is linked with a definite current strength, and not by taking into consideration the electromotive force induced during separate intervals of time. In the latter case the self-induction coefficient is a different one at every step of the magnetization, and is dependent on whether the magnetization increases or decreases. The change of value of the self-induction coefficient cannot be represented by a simple formula on account of the irregular form of the magnetization and hysteresis curve. Only such a definition of the self-induction coefficient is suitable in practice, according to which one can measure these values with the usual instruments and according to which one can make calculations. Therefore, of the three different ways in which the self-induction coefficient can be defined, only that one is adapted to practice which takes into consideration the total field which is generated. In order not to obtain incorrect values from the proposed definition, it should be added that the same is correct only for conductors which are not branched. Mr. Blondel has called attention to this feature. When a *periodical alternating electromotive force*, and not a constant one, is acting in a circuit, special relations arise which make it desirable to introduce also the quantities "effective ohmic resistance" (*effective resistance*), and "effective induced resistance" (*effective inductance*); only in the case of a coil without iron, which exerts no action on any other conductor, are these quantities unnecessary. If a coil without iron is traversed by an alternating current, the difference of potential which acts

can always be divided into two components, one of which overcomes the ohmic resistance, and the other the inductive resistance. The latter component precedes the former in phase by 90°; the overcoming of the inductive resistance requires the production of no energy. But if the coil contains an iron core, the energy of magnetization will have to be generated besides the heating effect of the current. In this case also there are two components of the difference of potential; the one E_1 overcomes the ohmic resistance, the other E_2 overcomes the electromotive force induced by the periodically changing field in the coil. This component E_2 *is no longer in advance of* E_1 *by a difference in phase of* 90°, *but by a smaller angle* a. The inductive resistance could be determined by dividing this second component E_2 by the current strength I; from this inductive resistance one would obtain the self-induction coefficient by dividing by $2 \pi n$. The component E_2 can also be resolved again into two components, of which the one $E_2 \cos a$ has the same phase as E_1, while the other $E_2 \sin a$ is advanced in phase by 90°. The component $E_2 \cos a$ involves the energy of the Foucault currents and hysteresis, the component $E_2 \sin a$ involves the expenditure of no energy. The coil with an iron core acts like a coil without iron which has the ohmic resistance $(E_1 + E_2 \cos a) \div I$, and the inductive resistance $(E_2 \sin a) \div I$. These values are the effective ohmic resistance (effective resistance), and the effective inductive resistance (effective inductance) of a coil with an iron core. In the same way one also obtains this effective resistance for every transformer, motor, condenser, etc. For the primary circuit of a transformer these values were calculated by Maxwell in the year 1865, and were represented in the above manner. The effective resistances of an alternating current apparatus can in every case be found when the current strength, the required difference of potential, and the energy consumed, are measured. The definitions of these values are as follows:

The effective resistance of a circuit is the ratio of the absorbed power to the mean square of the current.

The effective inductance is the square root of the difference of the squares of the impedance and the effective resistance.

The effective coefficient of self-induction is the ratio of the effective inductance to $2 \pi n$.

4. The definition of reluctivity cannot be given by saying:—It is the reluctance per unit volume, as one does not obtain the total magnetic resistance by multiplying reluctivity by the volume; it would be preferable to have it read:—Reluctivity is the magnetic resistance of a portion of the material in question, having a length of 1 cm. and a cross section of 1 cm^2.

5. A definition of Mattheissen's standard is superfluous as the Congress is not concerned with researches regarding this unit.

VIENNA, June 14th, 1893.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

New York, September 20th, 1893.

The seventy-ninth meeting of the Institute was called to order by President Houston at 8 o'clock, p. m.

The President:—It has been suggested, that, since the paper announced for the evening, by Mr. D. McFarlan Moore, from its interest may call forth prolonged discussion, that the President's inaugural address be postponed until after its reading. I will therefore call on Mr. Moore to read that paper as soon as the Secretary has made some announcements.

The Secretary:—At the Council meeting this afternoon the following associate members were elected:

Name.	Address.	Endorsers.
Albanese, G. Sacco	Inspector on Electrical Construction, Electrical Engineering Department, World's Fair, Chicago, Ill.	R. H. Pierce. L. Stieringer. F. W. Tischendoerfer.
Almon, G. H.	Supt. Construction, General Electric Co., 620 Atlantic Ave., Boston, Mass.	C. W. Pike. H. A. Foster. R. W. Pope.
Anderson, W. E.	Professor of Physics and Electrical Engineering, Virginia A and M College, Blacksburg, Va.	H. A. Foster. W. J. Hammer. R. W. Pope.
Auerbacher, Louis J.	Electrician and Head Salesman, The E. S. Greeley & Co., 5 and 7 Dey St., New York City.	R. W. Pope. W. J. Hammer. C. W. Pike.
Barnes, Edward A.	Electrical Expert, Fort Wayne Electric Co., Electricity Bldg. World's Fair, Ill.	H. A. Foster. F. S. Hunting. L. Stieringer,
Blake, Theodore W.	Electrician, National India Rubber Co., Bristol, R. I.	Hammond V. Hayes. John C. Lee. F.A. Pickernell.
Boardman, Harry B.	Wisconsin Telephone Co., 1530 Grand Avenue, Milwaukee, Wis.	D. C. Jackson. N. S. Keith. R. W. Pope.
Boggs, Lemuel Stearns,	Electrical Engineering Department, World's Fair, Chicago, Ill.	R. H. Pierce. Geo. M. Mayer. R. W. Pope.

BRAGG, CHARLES A.	Manager Phila. Agency, Westinghouse Electric & Mfg. Co., Girard Bldg., Philadelphia, Pa.	R. W. Pope. Cecil P. Poole. A. L. Johnston.
BRENNER, WILLIAM H.	Electrical Engineer, Montreal Street Railway Co., Montreal, P. Q.	J. H. Vail. Wm. J. Jenks. A.E.Winchester.
BRIXEY, W. R.	Proprietor and Manufacturer, Day's Kerite Wire and Cables, 203 Broadway, New York City	R. W. Pope. T. C. Martin. Geo. M. Phelps.
BURTON, WILLIAM C.	Edison Illuminating Co., 238 27th St., Milwaukee, Wis.	R. W. Pope. D. C. Jackson. H. A. Foster.
CETTA, WILLIAM J.	Electrician, General Electric Co., 398 Broad Street, Newark, N. J.	Wm. J. Hammer. H. A. Foster. R. W. Pope.
CREAGHEAD, THOMAS J.	President and General Manager, Creaghead Engineering Co., 296 Plum St., Cincinnati, O.	Chas. F. Scott. L. B. Stillwell. Albert Schmid.
DECKER, DELBERT H.	Solicitor of Patents, with H. C. Townsend, 5 Beekman St., New York City.	H. C. Townsend. C. O. Mailloux. Ed. L. Nichols.
DODGE, OMENZO G.	Professor of Math. U. S. Navy, Chief Inspector, Electrical Engineering Dept., World's Fair, Chicago, Ill.	R. H. Pierce. Geo. M. Mayer. H. A. Foster.
DOW, ALEXANDER	Engineer, Chicago District, Brush Electric Co., Box 433, Wheaton, Ill.	Geo. Cutter. Charles Wirt. R. W. Pope.
EICKEMEYER, RUDOLF	Pres't, Eickemeyer & Osterheld Mfg. Co., Yonkers, N. Y.	C. P. Steinmetz. R. W. Pope. H. A. Foster.
FITZMAURICE, JAMES S.	Chief Engineer, The Electric Light Branch, 300 Kent Street, Sydney, N. S. W.	Gustave Fischer. W. J. Spruson. R. W. Pope.
FLEGEL, GEO. C.	Electrical Editor, The *Railroad Telegrapher*, Argos, Ind.	R. W. Pope. H. A. Foster. M. O'Dea.
FRENCH, THOMAS, JR.	Professor of Physics, University of Cincinnati, Avondale, Cincinnati, O.	W. J. Hammer. H. A. Foster. R. W. Pope.
GREENE, S. DANA,	Assistant General Manager, General Electric Co., 44 Broad St., New York City.	W. J. Hammer. F. J. Sprague. T. C. Martin.
HAMMATT, CLARENCE S.	Supt., Jacksonville Electric Light Co., Jacksonville, Fla.	N. S. Keith, A. F. McKissick. R. W. Pope.
HASKINS, CLARK CARYL	Electric Light Inspector of Chicago, 582 West Congress St., Chicago, Ill.	R. W. Pope. G. A. Hamilton. T. C. Martin.
IRVING, FRANK K.	Consulting Electrician, Franklin Electric Co., 67 McDougal St., Brooklyn, N. Y.	E. T. Birdsall. T. Wolcott. L. Wm. Serrell.
IWADARE, KUNIHIKO	Electrician, Osaka Electric Light Co., Osaka, Japan.	A. E. Wiener. James Burke. Harry N. Marvin.

JOHNSON, J. N.	Electrician in Cable Dept., Western Union Tel. Co., 16 Broad Street, New York City.	John L. Hall. R. W. Pope. L. A. McCarthy.
KELLER, E. E.	Manager and General Supt. of Exposition Work, Westinghouse Electric and Mfg. Co., 5506 Monroe Ave., Chicago.	R. W. Pope. R. H. Pierce. R. N. Bayles.
KIRKEGAARD, J. GEORG	General Incandescent Arc Light Co., 172 President Street, Brooklyn, N. Y.	W. J. Johnston. W. D. Weaver. W. A. Rosenbaum.
MERRILL, E. A.	Electrical Engineer, Pierce & Miller Engineering Co., 42 Cortlandt St., New York City.	H. L. Lufkin. A. C. Crehore. Cary T. Hutchinson.
MOTT, S. D.	Electrical Engineer, Passaic, N. J.	Wm. J. Hammer. W. C. Lawton. W. T. M. Mottram.
NUTTING, SAMUEL E.	Electrican and Supt., Nutting Electric Mfg. Co., 241 So. Scoville Ave., Oak Park, Ill.	Wm. D. Ray. L. L. Summers. Carl K. MacFadden.
PAUL, J. S.	Post-Graduate Student in Civil and Electrical Engineering, Princeton College, Princeton, N. J.	W. J. Hammer. H. A. Foster. R. W. Pope.
PEDERSEN, FREDERICK MALLING	Assistant Electrical Engineer, Crocker-Wheeler Electric Co., 327 W. 34th St., New York City,	M. I Pupin. F. B. Crocker. Gano S. Dunn.
REBER, SAMUEL	Lieut. U. S. Army, Johns Hopkins University, Baltimore, Md.	D. C. Jackson. Harris J. Ryan. Louis Duncan.
ROSS, NORMAN N.	Electrical Engineer, Canadian General Electric Co., 350 Main St., Winnipeg, Manitoba.	W. M. Rutherford. John Langton. R. W. Pope.
SCHOEN, ALLEN MCGEE	Electrician, South Eastern Tariff Association, Atlanta, Ga.	A. F. McKissick. R. W. Pope. Thos. D. Lockwood.
SHRADER, WILLIAM	Professor of Electrical Engineering, University of Missouri, Columbia, Mo.	R. W. Pope. Harris J. Ryan. Brown Ayres.
SEITZINGER, HARRY M.	Consulting and Constructing Engineer, Wilkesbarre, Pa.	F. N. Sanborn. R. W. Pope. G. A. Hamilton.
STEVENSON, EDWARD W.	Electrician, Brush Electric Illuminating Co., 210 Elizabeth St., New York City.	T. C. Martin. Geo. M. Phelps. A. E. Kennelly.
STRATTON, MILTON G.	Assistant Electrician, Lawrence Gas Co., 256 Essex Street, Lawrence Mass.	Walter C. Fish. F. Sheble. C. J. R. Humphreys.
SVENTORZETZKY, CAPT. LOUDOMER	Military Engineering Acad: emy, St. Petersburg, Russia.	R. W. Pope. N. S. Keith. H. Cushman.
WAIT, HENRY H.	Assistant Electrical Engineer, Western Electric Co., 4919 Madison Ave., Chicago, Ill.	E. M. Barton. C. R. Cross. Thos. D. Lockwood.
WARNER, ERNEST P.	Electrical Engineer, Western Electric Co., 227 So. Clinton St., Chicago, Ill.	Thos. D. Lockwood. R. W. Pope. H. A. Foster,

WEST, JULIUS HENRIK	Electrical Engineer, 3743 Locust St., Philadelphia, Pa.	R. W. Pope. H. A. Foster. N. S. Keith.
WHITTEMORE, CHARLES F.	Secretary and General Supt. Davis Electrical Works, Chicopee Falls, Mass.	Ed. L. Nichols, H. J. Ryan. J. W. Hyde.
WORSWICK, A. E.	Electrical Engineer, Mutual Light and Power Co., Montgomery, Ala.	A. F. McKissick. R. W. Pope. N. S. Keith.
WRAY, J. GLEN	Cable Tester, Chicago Telephone Co., 104 Linn St., Janesville, Wis.	A. V. Abbott. D. C. Jackson. John E. Davies.

Total, 48.

THE PRESIDENT:—I take great pleasure in introducing to you Mr. D. McFarlan Moore, who will read a paper entitled "A New Method for the Control of Electric Energy."

A paper presented at the 79th Meeting of the American Institute of Electrical Engineers, New York, September 20, 1893, President Houston in the Chair.

A NEW METHOD FOR THE CONTROL OF ELECTRIC ENERGY.

BY D. McFARLAN MOORE.

The rapid growth of electrical science, and the almost daily unprecedented addition to it of new inventions, have caused those well versed in the present state of the art to greet new theories with a certain amount of doubt, owing to the large number of false alarms that have been sounded to tell the world that all present theories and methods will, in the future, be history; and that a new and revolutionizing principle has been discovered.

Be that as it may, there is reason to believe that this paper treats largely of an heretofore uninvestigated branch of electrical science and phenomena which may be of great value to the commercial world, and of even greater interest to the scientific world.

The key to this branch of science may be expressed in a few words, viz:—A varying-in-pressure contact in a vacuum, produced by a variable magnetic field.

The present means for the control of electric currents—ohmic resistance—is crude at its best, as in any form it is an uneconomical device. All existing forms of rheostats, regulators, resistance boxes, controllers, etc., are bulky, clumsy means for accomplishing a desired object. This is conspicuously illustrated in the case of stage regulators, which often occupy valuable floor space and consist of a tremendous weight and length of wire. That the design of apparatus for controlling current is a matter of great importance and is essential to all electrical installations, is exhibited in the fact that over half of the catalogues issued by electric manufacturing companies are given up to this class of electrical goods. Many problems in electrical engineering are considered impracticable, simply because of the weight and space required for resistance to enable the control of the current as desired.

When one takes into consideration the multitude of switches of every conceivable shape, construction and design, but all having one aim in view—which none perfectly accomplish—viz:—to break a current without deleterious sparking, it seems that, as all are designed on the same principle, a new principle should be employed which would perfectly accomplish the object desired, that is; break the current with practically no sparking. It would also be a very convenient feature if each switch would control the brilliancy of all lights on the circuit which it controls.

The regulation of intensity of the incandescence of an electric filament has been a problem, the solution of which has been sought after by electricians in general the world over, and partial success has been attained by the use of the only known means, "dead resistance;" but this method can never be commercially successful, on account of very objectionable bulk, weight and heating of the resistance.

The public in general demands the control of electric lamps and the great convenience gained thereby.

The massive regulators of dynamos, and starting boxes of motors should be supplanted by devices occupying but a few cubic inches of space. This is especially the case where the motor is not stationary, as in electric railway work.

There is room for a cheap ampere meter, and still more room for a cheap mechanism to be used as an electric gauge; that is, an instrument which can be adjusted or set so that a current that has passed through it will have any desired quantity or quality. This device could be substituted in place of fusible cutouts and the multitude of appliances of a similar nature. It would be of great value in telegraphy. The application of electricity in medicine should also be improved. Lastly, the reducing of phosphorescent lighting to a commercial basis is desirable.

It is hoped a solution of these and many other problems lies in the principle stated at the beginning of this paper, *i.e.*, a varying-in-pressure contact in a vacuum produced by a variable magnetic field, and it is doubly valuable in that it can be applied both to the alternating and the direct current. One of the simplest forms of apparatus designed for meeting these conditions is shown in Fig. 1. The varying-in-pressure contact is shown at A, caused by the variable magnetic field due to the movement of the magnet B by thumb screw C. The evacuated bulb F contains the armature D, supported by spring E, permitting it to vibrate rapidly, producing pulsations or interruptions of the current. The rate or degree of these interruptions depends on the strength of the magnetic field surrounding the armature (in this case acting through the glass), which in turn depends on the distance of the magnet from it, varying inversely as the square of the distance. Varying the magnetic field, and therefore the pressure on the contacts, changes the amperes and volts of the current passing through the device. With properly de-

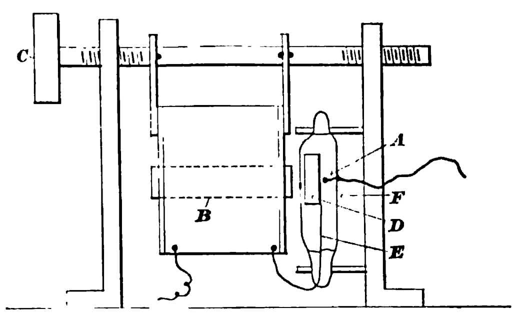


FIG. 1.

signed apparatus the movement of the armature is so slight that to all appearances one would think it at rest. In the discussion of this method of current control, let us select that form of translating device known as the incandescent electric lamp. Fig. 2 is a diagram showing the action of a vibrator in series with a lamp, without the contacts in a vacuum.

Its action is as follows:—Contact made at A; current flows through magnet and armature is attracted, breaking the circuit; magnet loses its power and spring S again closes the circuit.

Fig. 3 shows the device as applied to each individual lamp, and it will be noted that in this case the vacuum of the lamp is utilized for the vacuum of the contacts as well. The size of the armature is made as light and small as possible not only on account of the general law of all mechanical construction, viz:

make moving parts as light as is compatible with strength so that the position of the lamp will have no effect on the incandescence of the filament, but to minimize the amount of residual gas in the metal composing the armature, as any foreign gas will be detrimental to the filament.

In Fig. 4 a socket of ordinary dimensions is shown constructed on the same principle. A socket of this kind can be adjusted to regulate a lamp of almost any C. P. within limits.

In both of these instances the movement of the magnet pro-

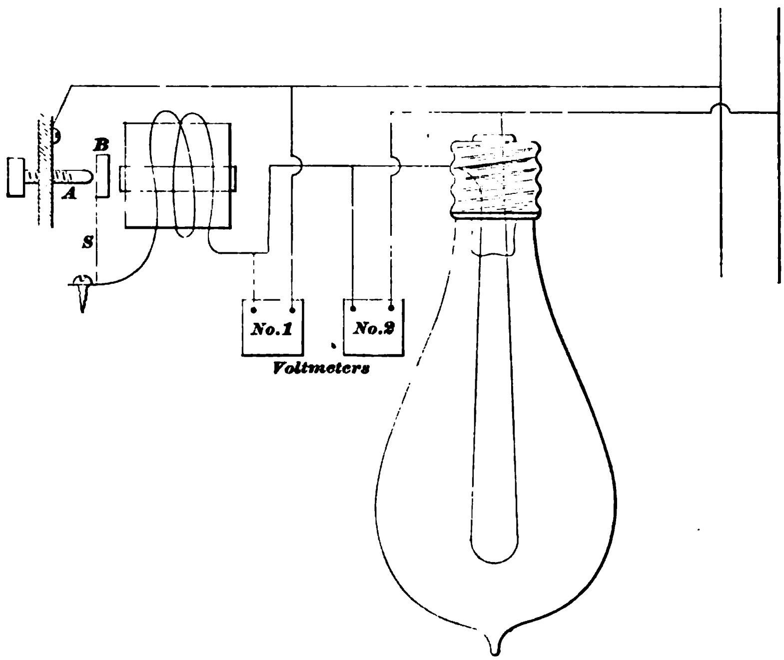


FIG. 2.

duces the regulation, but it is evident the result would be the same by either the movement of the vacuum or the core of the magnet, as shown at A, or by throwing in and out coils of the magnet. The filament emits continuous light with an illuminating power corresponding to the rate of vibration of the armature. When the armature is not within the inductive distance of the magnet, it is at rest and the filament being in circuit, the light is at its maximum. An attachment to the means for adjusting the magnet is used to break the circuit when it is desired to extinguish the light. Tests show that the volts and amperes vary directly with the light. This is apparent from the following table, a 16 C. P. lamp being used:

Amperes.	Voltmeter No. 2 (see Fig. 2).	Voltmeter No. 1.
.42 full C. P.	115	0
.30	84	31
.17	52	63
.11	35	80
.07	20	95
.04	15	100
.03	10	105
.02 just visible	5	110

Still lower readings were taken, but below .02 ampere the heating of the filament was not discernible by the eye.

As shown by the readings of voltmeter No. 1, the more rapid

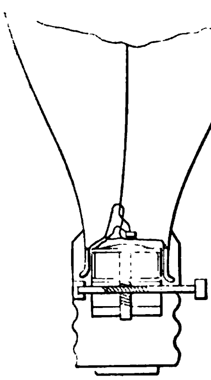


FIG. 3.

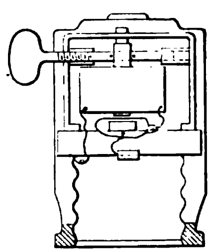
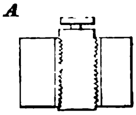


FIG. 4.



the vibrations, the more E. M. F. is required by the contact breaker. Inasmuch as the contacts are in a high vacuum they will remain bright and effective, and the light is regulated without the use of bulk resistance. Again, with this arrangement the current is not thrown on or off the filament suddenly, thus increasing its longevity owing to more gentle treatment. The slight sparking in lamps individually regulated will use up the residual oxygen in the evacuated bulb of the lamp, thus making it more perfect, and lengthening the life of the lamp.

It was curious to note that practically as well as theoretically an armature suspended, as shown in Fig. 2, is never perfectly at rest. When a photometrical test would probably decide that the lamp was burning at full C. P., and the screw A pressing the armature B as tightly as the construction of the apparatus would allow against the core of the magnet, if a sounding board is brought into requisition the vibrations of the armature are plainly discernible.

An automatic make-and-break, or varying-in-pressure contact, or what has commonly been called, and what has long been sought after, a scientifically formed "poor or imperfect contact," can be used in one sense as a transformer. It will be interesting to note the effect of a large number of lamps of this description working at one time from one dynamo, because if they should accidentally become synchronous the load on the dynamo would be very varying. But such a result is probably impracticable if not impossible.

Now let us again refer to Fig. 2 and note the changes as the lines of force acting on the armature vary.

The vibrator is in series with the lamp, because if in shunt, the action is reversed and the liability to short-circuit makes it impracticable.

(1) A and B separated, lamp out.

(2) A and B in contact, lamp full, because B is not in magnetic field of magnet.

(3) Continue to advance A, B enters field, rapid vibrations commence and lamp suddenly becomes dim.

(4) Continue to advance A, lamp gradually brightens until full C. P. is reached.

Fig. 5 shows these periods reproduced diametrically. It will be noted that there are two periods where the lamp can be regulated. Period 2—3 is of too short duration for practical application, however, and by the proper adjustment of the spring can be done away with entirely. Period 3—4 covers the greatest range in C. P. and should be used. The delicacy of the spring supporting the armature determines somewhat the position of point 3, that is the minimum in C. P. of the filament.

The voltaic arc appears between the terminals of a source of electricity. In the case of the arc lamp, it consists mainly of volatilized carbon. The electrodes are therefore consumed, first by actual combination with the oxygen of the air, second by volatilization. The ohmic resistance of the arc increases with its length and decreases with its area of cross-section. Polarization sets up C. E. M. F., and the extra current produced by self-induction retards magnetic action and causes more sparking. With metallic electrodes the color of the arc is characteristic of the metal and is of greater length for the same current than that formed between carbon electrodes. To cause electrical vibratory motion, the current must either be greatly weakened or broken. This will cause sparking which when the life of the contacts is taken into consideration (even though they be in vacuo) should be reduced as much as possible. However, further experimenting may prove the possibility of current regulation by varying the length of the arc in vacuo by means of an exterior magnetic field. Also the construction of a truly incandescent arc lamp on this principle.

Sparking can be relieved in many ways:

(1) Sub-divide the current as much as practicable.

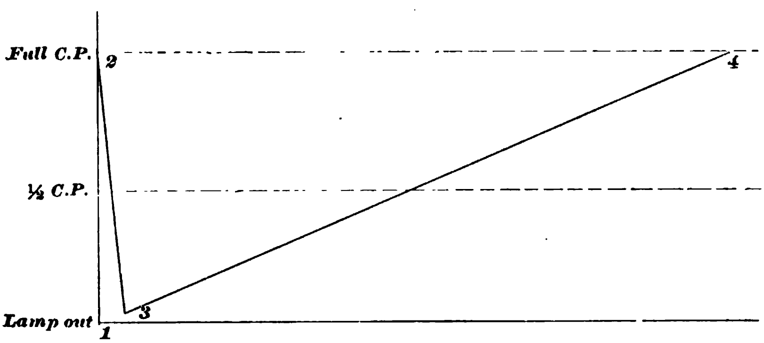


FIG. 5.

(2) The more rapid the vibrations the more rapid the make-and-break, and consequently the less the spark. To cause rapid vibrations, the point of application of power should be as close to the point of oscillation as possible, or the point of contact should be as far from the point of oscillation as possible.

Magnetic blast, condensers, non-sparking metals, unlike electrodes, high resistance, sheath of copper around bobbin of magnet, metal foil between layers of magnet, separate coils, etc., are all familiar methods.

It is the C. E. M. F., or automatic "choking effect," of the alternating current that permits of its beautiful control without loss, such as is sustained by controlling the direct current entirely with ohmic resistance.

With the method under consideration, the C. E. M. F.'s of the pulsating or intermittent direct current are taken advantage of. That is, both those in the arc and in the magnet.

Besides, when controlling current entirely with ohmic resistance (which can never be completely annihilated), it is constantly in circuit, whereas with the method proposed for lamp control, the only ohmic resistance, that of the arc (that of the magnet being insignificant), is in circuit but one-half of a unit of time, and if there be any foundation for the theory of the persistency of the vision of the human eye, then the filament of the lamp is also dissipating heat but one-half the time. Some energy, however, is expended in the continual magnetization of the armature; in fact, this is another reason why the glow of the lamp filament is diminished and why there is a C. E. M. F. in the magnet. The greater the magnet capacity, the less the self-induction and impedance.

The two greatest desiderata in connection with the advancement of electrical science, and those which, because they have not been satisfied and fulfilled, have, in a measure, for a number of years barred further progress in the most rapidly advanced industry in the history of the world; are first, the use of the efficient low voltage lamp on the efficient high voltage current, and second, the prolonging of the life of these lamps. The manufacture of high voltage incandescent lamps is a subject much agitated at present, owing to the fact that the benefit to be derived from a high voltage is almost incalculable, as the capacity of the mains now used by central stations might easily be trebled. But not only does the successful solution of this problem seem impossible in this direction, but it is now unnecessary. The greatest advantage of the alternating system over the direct current system is, that by means of converters, lamps of low resistance, and therefore high efficiency, can be used. It appears then, that the main advantage (without some disadvantages) of alternating systems can be transferred to the continuous system by using in each socket, or on every circuit, varying pressure contacts, causing an interrupted current of any desired voltage, thus increasing the possibilities of this system to an enormous extent owing to the immense saving in copper conductors.

If an appurtenance of this construction is operated by a centrifugal governor attached to a motor, the best results as regards economy and regulation can be obtained, as the voltage will then vary directly as the torque, and the electric energy required will be proportional to the power developed. Owing to the minimum size of this device it can be made an integral part of the motor. The salient disadvantages of a resistance box that can now be overcome by constructing apparatus as described, are, first, size; second, weight; third, susceptibility to changes in temperature, that is, variation in resistance; fourth, poor contact caused by the burning of clips and loosening of wires; fifth, heating and the great fire risk consequent thereto. The new

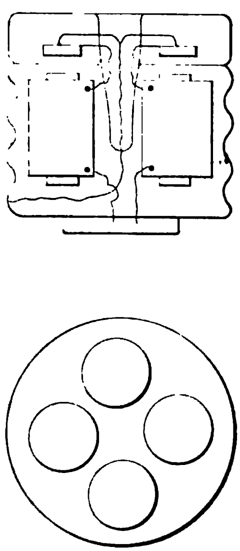


Fig. 6.

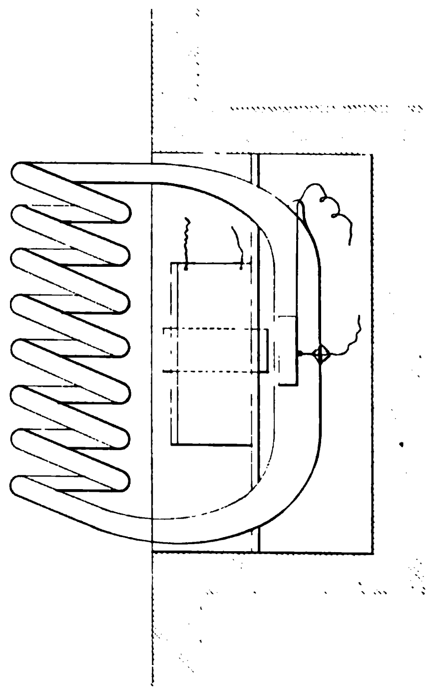


Fig. 7.

method is certainly unique, and its application will be the means of affording a great convenience under many circumstances.

Owing to its constancy, due to its non-susceptibility to temperature changes, it can be adopted, when the adjustment screw is used in connection with a graduated dial, with great facility as an electric gauge.

Experiment shows that the continuous sparking (though slight) of the contacts in a vacuum, sets in motion the ether molecules causing that beautiful soft blue phosphorescent light similar to that produced by Geissler tubes. While Maxwell was the first to note that a disruptive discharge was oscillatory and set up vibrations in the ether, Professor Henry demonstrated it experimentally. The conductive discharge which occurs when vibrating metallic electrodes are used, and therefore of small resistance, probably consists of partial discharges producing oscillations which in turn are due to the induction of the current upon itself in the magnet. But the quantity of current passing, bears a relation to the amount of disturbance and the greater the impedance the less the current; also the more rapid the vibrations or frequency of interruptions, the more prominent becomes the self-induction and the less the true ohmic resistance in the control of the current. These oscillations are increased if the arc is in a strong magnetic field, as it raises the E. M. F. but deteriorates the electrodes more rapidly. If this magnetic field be variable, the effect is magnified, and since free atoms or molecules are made to collide with one another by varying the surrounding potential, we can in a measure account for this phenomenon in that theoretically following every impact the electrostatic force falls from its maximum quantity to zero.

Now by increasing this effect, a light may be produced of greater efficiency than is now obtained by the incandescent filament which is very inefficient. This method of lighting is a radical departure from anything heretofore proposed, as it will be producing phosphorescent light due to ether oscillations without the use of an alternating current (although there is no reason for not using it) at high tension, with expensive and impracticable condensers and transformers. The value of this system will be more clearly discerned when it is remembered that the elements above mentioned as being done away with, are those which stand in the way of phosphorescent lighting becoming commercial at present.

Merely the use of this simple and inexpensive device with the direct or alternating low tension current, produces phosphorescent light. There is no objection, however, to using high voltages; the effect is simply increased, because the greater the stress between the terminals and the more intense the magnetic field, the more rapid will be the interruptions of the current. And the greater the number of impacts per unit of time, the more intense the heating effect and the more economical the light will be. The most economical potential (there must be such) for given electrodes can only be found by trial, because the greater the heat the less the permanency of the vacuum. The phosphorescence is probably in a measure at least caused by the atomic disturbance due to the actual mechanical movement of the armature in the vacuum; the impacts and the static electricity that may be caused thereby in conjunction with the rapid disruptive discharges. This disturbance or action of varying electrostatic force consists in the rapidly moving or vibrating of charged atoms or molecules coming violently in contact with one another, producing friction, heat, light. In fact, the generation of light due to friction within or without a bulb containing rarefied gas, and caused by inductive action, is a subject of much interest. Experiment seems to indicate (see plates 8 and 9, L, R, S, T, W, X) that the amount of phosphorescence depends more on the number of contacts in different parts of the vacuum per unit of time than on the influence of a surrounding magnetic field.

By reason of the enclosed space in which miniature incandescent arcs can be made to occur in rapid succession throughout its extent, the heat resulting therefrom is conserved and aids in accomplishing the result sought for, a luminosity of maximum radiation emanating from a source of considerable area.

If any inert gas, such as oxygen or nitrogen be substituted for the vacuum, the phosphorescent effect will most likely be intensified on account of more vigorous molecular action, but the life of the electrodes may be shortened. It might be advisable with metallic electrodes to use carburetted hydrogen, because when decomposed it would not affect the metal. Owing to the actual contact of the electrodes, their volatilization or disintegration due to the arc is reduced to a minimum, that is much less than is the case when high tension discharges occur. Of course, the degree of disintegration and the amount of deposit in the bulb depend largely on the nature of the electrodes, aluminium probably being the best. However, the more perfect the vacuum, the less the deoxidization and volatilization, because combustion requires oxygen.

Fig. 6 shows one form of phosphorescent lamp complete, its size a little larger than the base of an ordinary lamp, the C. P. being developed by multiple contacts.

A matter of great importance in the design of a lamp of this description is the form and nature of the evacuated space, partially determined by the melting point of the glass, also whether

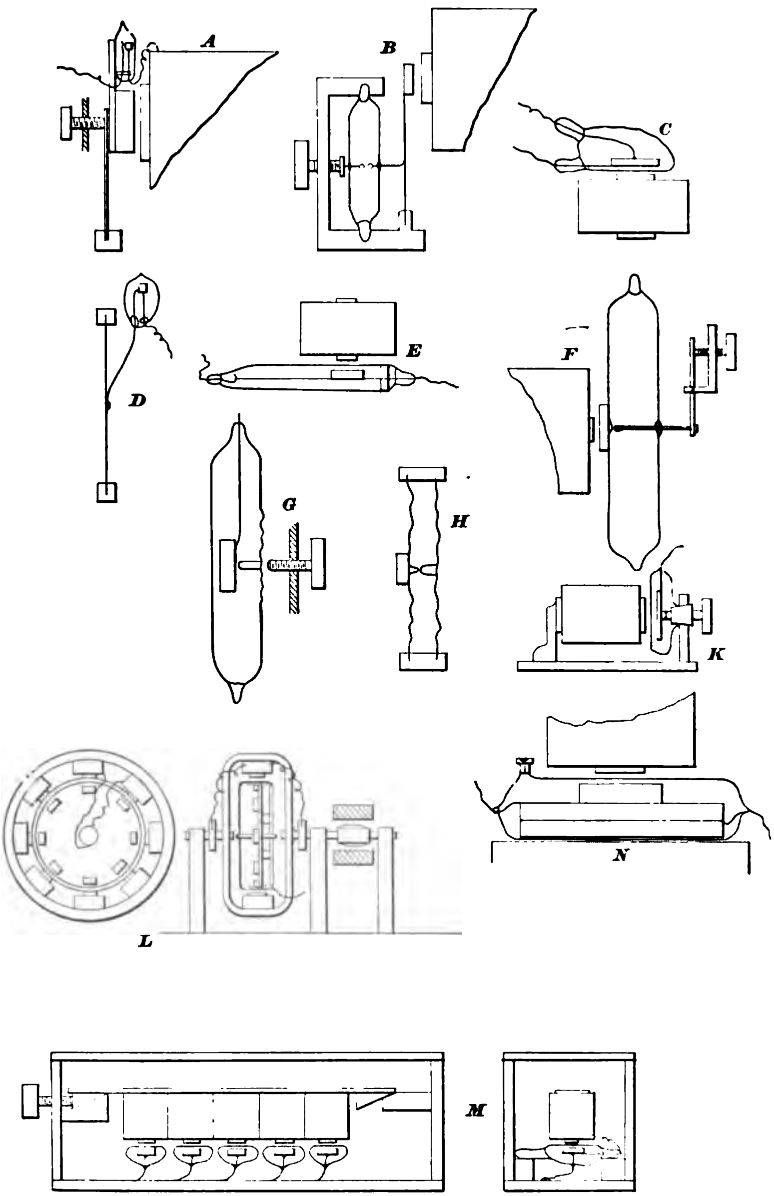


Fig. 8.

it is advisable to use double or even triple bulbs, one within the other.

Fig. 7 denotes one of the many forms of lamps that suggest themselves, but experience only can prove that which is most efficient.

The study of this branch of physics and of these highly interesting phenomena brings to our minds innumerable ideas in connection with them, which cannot be treated individually in a short paper. A few only of these ideas are illustrated in plates 8 and 9.

It will be noted that the point in Fig A is that the bulb does not contain the armature, but is mounted upon it, and therefore can be made very small. As shown, its operation is due to the principle of momentum. This is not the best form for producing phosphorescence, in that the heat in the armature caused by dynamic action is not dissipated in producing molecular disturbance in the bulb. Fig. B, elasticity of the glass bulb is suggested. Fig. E, rapid vibrations and quick break. For rapid action use short, thick magnet. Fig. L, friction within bulb and electromagnetic rotation. Figs. M and N, resistance boxes where gravity is utilized in place of springs.

In Fig. N carbon plates are used for the electrodes, and the bulb should be made of the first quality of refractory glass. On account of the expansion of the gases, formed by excessive sparking, a valve is shown to permit of their escape before a disruption of the glass could occur. This pressure, together with the absence of combustion, bears an important relation to the disintegration of the carbon.

P, is a graduated dial attachment to indicate character of current passing. U, vacuum piston instead of spring. W, movement of metal screens upon each other, pendulum action, producing maximum sparking. T, screen with revolving brushes, resulting in a brilliant phosphorescent lighting effect. V, suggests transmission of light by phosphorescent tubes generated in concealed portions O O.

Owing to the immense production of incandescent lamps, the manufacture of evacuated bulbs has been so perfected and cheapened, that general current control with this system will probably be many per cent. cheaper than any method now in use.

There is a large variety of rheostats and current controllers

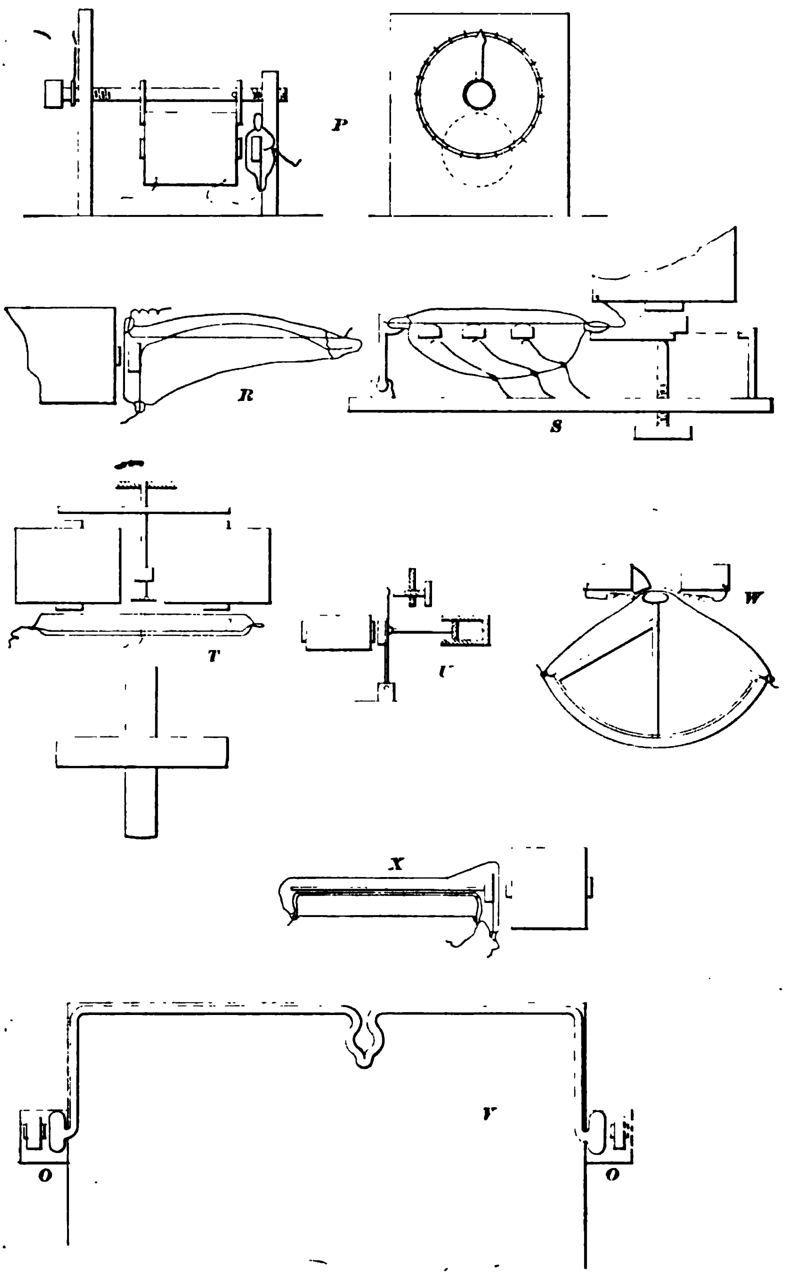


Fig. 9.

on the market designed to be used medically, and since the interrupted current, owing to its peculiar effects on the human system, is now being used in medicine, no better form of current controller could be devised than by employing the principle under consideration, as more delicate currents can be supplied than by any other method or means, owing to the steady rate of vibration caused by the non-deoxidization of the contacts in the vacuum. This is clearly proven in that the pitch of the note produced by the vibrating armature is almost constant.

A varying-in-pressure contact could be used with advantage in connection with all delicate measuring instruments. It would materially aid in making them "dead beat," as it could then be made impossible to strain them by suddenly turning on and off the current.

It is hoped that this paper has been of interest to the Institute, and that it will give new impulse to investigations leading to the discovery of a more economical method for transforming mechanical motion into light, and an ideal means for controlling electric currents.

DISCUSSION.

THE PRESIDENT:—You have heard Mr. Moore's paper. The discussion on it is now in order. You will bear in mind that it is generally understood that all papers presented to the INSTITUTE are open to free discussion.

MR. LAURENCE J. ZIMMERMAN:—Some time ago, while experimenting in another direction I obtained similar results by using incandescent lamps. But I came to a different conclusion altogether from that of Mr. Moore. He says in substance, that the light varies in brightness, owing to a more or less perfect or imperfect contact, resulting from varying the pressure at contacts. But I came to the conclusion from my experiments, that the light varies because the watts which are actually delivered to the lamp vary in proportion to the time in which the lamp is actually in circuit during a unit of time (it being apparent that the lamp is in circuit only while the vibrator spring and contact point are in contact electrically), or to put it into a mathematical expression:

$$C \times E \times t_1 = X$$

in which X is the watts delivered to lamp, when t_1 is less than a unit of time, t_1 is the time in which the lamp is in circuit during a unit of time, and $C\,E$ is the current and E. M. F. or watts consumed by lamp at full C. P.

In my experiments I did not use a vibrator such as Mr. Moore describes; I used a commutator of peculiar construction, in which the contact was as nearly perfect as a brush and commutator contact could be, but obtained the same results as Mr. Moore did with a vibrator, showing that such results could not have been caused by a varying pressure at contacts, or by poor or imperfect contacts.

MR. CHARLES CUTTRISS:—I would like to ask about a few points, because I had occasion ten years ago to use an incandescent lamp, so arranged as to produce this very effect. This was in the winter of 1883. I used it on some small eight candle power lamps I got from England, and it was practically used during the most of the winter. The difficulty I found was that it ran nicely for an hour or an hour and a half, but by-and-by the points would oxidize, or rather build up. In two or three hours, the lamp would go up to its full candle power. But in the experiments I was conducting at that time, the defect was immaterial, because I could trim the points up again and continue. This is one point,—I would like to know if Mr. Moore finds this trouble on a three or four hours run? Last year I built a device almost exactly like the one he shows in his drawing. I have a socket in my pocket that I built then, and the trouble I found with that was very much the same. It would not remain constant. The effect was very nice while it remained constant. But, in spite of all precautions, the points would build up, or would fuse. My idea was that it might be useful for a sick room. In the day time, when noise was around, you would not notice the hum of the armature, but as soon as the room get quiet you would think there was a dozen musquitoes right around your head. I could not reduce that noise. It is quite possible, if I put it in a vacuum with a vibrating armature, that the sound would not be transmitted. I merely mention this to show that the idea has been used years ago to a certain extent. It certainly is practicable. Whether it is practicable to leave it all night, or not, I do not know. I would like to know whether Mr. Moore can answer the query as to whether the points build up and alter the rate of vibration in that way, and whether he finds humming to an extent that would be annoying to a nervous person.

MR MOORE:—It makes a great difference whether the contacts are in a vacuum or not. It was to obviate that very trouble of the inconstancy of the vibration, that the idea of putting the contacts in a vacuum suggested itself to me. Without the vacuum, the system appears impracticable, as upon it depends the steady or constant rate of vibrations, that can be obtained by no other means. The noise can be avoided by being careful in the construction of the vacuum, not to place the armature too close to the glass. With the device properly designed, there should be no noise at all—either with or without a vacuum.

MR. EDWARD P. THOMPSON:—The last speaker but one, said something about the idea being old. Very often ideas are old, but they are not on record. I am able to say that the combinations set forth by Mr. Moore, are new, so far as the records are concerned of the patent offices of three or four different countries. It seems to me that such a device has certainly got to come into use, before *domestic* lighting can be pronounced successful. In business places no need for regulators exists, except for purposes of economy. Perhaps others here, as well as myself, have been asked by people, "Why is it that regulators are not put on incandescent lamps, so that we can have the same use with them as we can with gas?" Sometimes that objection has been answered by saying that the lamp can be turned off, and you do not need any match to light it again. But it is often necessary, without regard to any other consideration, to be able to turn down the light, or to modify the light, in dwelling houses. In reference to any controller outside of electrical means, I came across a certain kind of controller, too simple perhaps to be mentioned before an electrical society, but at the same time a patent was granted upon it several years ago. The claim reads about like this: "In combination with an incandescent electrical lamp, of an openable, opaque cover." It is used on ocean steamers, and on the Chicago, Milwaukee and St. Paul R. R. It is very easy to see that you will not have any electrical difficulties there. The patent was sold at a very fair price. You might say it would be anticipated by some kind of cover for a gas lamp or for an oil lamp. But in those cases you could not cover up the lamp and yet allow the supply of air for the support of combustion to come in, and the products of combustion to go out. That is about as simple a device, as I suppose you will ever get, in the way of regulating the light, where convenience and not economy is desired—simply have a cover that can be opened more or less.

MR. LOCKWOOD:—I confess that my train of thought has largely run upon the same line as that of the last speaker. Of course, it is quite impossible that a paper presenting such radical features as the paper we have listened to, should be presented without drawing forth considerable discussion, and I think it is unnecessary to say, discussion largely of a critical kind, and that certain, at least, of the views presented in discussion must necessarily be opposed to those presented in the paper. Perhaps that is due to the less amount of thought which the persons hearing the paper have been able to bestow upon the subject. But it is invariably so with great reformers—they have always met with a very considerable amount of opposition—from Luther down.

If I understand the thought of the paper, it is that the method of adjustment of regulation proposed, is that of interposing a variable contact in the circuit, preferably enclosed in a vacuum, and of substituting this for the ordinary determining resistance coils. The paper, I think, does not state in terms that the contact should necessarily be vibratory. On page 438, it is stated that, "The regulation of intensity of the incandescence of an electric filament has been a problem, the solution of which has been sought after by electricians in general the world over, and partial success has been attained by the use of the only known means, 'dead resistance'." The author has apparently forgotten the alternative of shunts. But I suppose that he will consider these also to employ waste of energy. Few of us, Mr. President, I apprehend, are unaware that a vibrating contact in a circuit raises the average resistance of such circuit, or that the variations of the range of the vibration also varies that average resistance. It may also be, that some of us have contemplated an industrial application of these facts. But I scarcely think that any of us would have thought of calling them an "uninvestigated branch of electric science." The author is undoubtedly correct in his view, that in operating such an appliance in connection with an incandescent lamp, a large portion of the result achieved is due to the counter E. M. F. of self-induction, owing to the alternate magnetization and demagnetization of the electro-magnet. If I remember rightly, Poggendorf, in 1855, as recorded in the Philosophical Magazine, made some experiments with induction coils, in which the circuit-breaking points of one coil were used to control the magnetization and demagnetization of the other, and the effects at that early period, were by him ascertained to be mainly attributable to self-induction. But I think Mr. Moore will find, if he pursues the subject, that at least some portion of it is properly attributable to the real resistance of the vibrating contact. In my special branch of electric work we find that there is very considerable margin indeed, between the lowest and the highest resistance of two contacts which are not in constant vibration, when they are set into vibration by the human voice. There are, however, Mr. President, as it seems to me, two very cogent reasons which tend to prevent an appliance from going into use. One is, that the appliance is not known,—a very good reason, I am sure you will agree with me; and the other is, that it is not wanted. I suppose that there are also two reasons which may operate to prevent a known thing from being wanted; one is, that there is no call for the function which it subserves; the other is a general amount of distrust as to the trustworthiness of its operation. It is perhaps a fact that in private life we really see more of the uses of incandescent lamps, than we do in the study of electrical applications, and I think the average user of an incandescent lamp is perfectly well content either to have it turned out, or turned on to its full strength. Of course, the theatrical representations, such as are mentioned on the first page of this paper, do require different degrees of light, and it has occurred to me that in preference to putting an electro-magnet and a vibratory contact in each lamp, or associating one with each lamp, it would be better to adopt the expedient suggested by the last speaker, and have a certain amount of opacity in some of the globes, perhaps making the globes of different colors, and that this, together with the ordinary resistance contrivances, would be more likely to give general satisfaction and be more economical in the long run, than would the expedient suggested by the paper.

Now if we come to the absolute merit of such devices as are suggested, I call upon the gentlemen present who have ever been connected with the telegraph business, to bear me witness that a man would not wish a worse thing to happen to his worst enemy than to have a loose contact in the circuit of a translating device. Any person who has had the privilege of locating a swinging break [laughter], will be able to appreciate the danger of such a perpetuated loose contact. One of the evils of such a thing is, that it is impossible to make a loose contact stable. It is a good deal like the electro-magnetic leaks of our friend Professor Silvanus P. Thompson, by which he hopes to achieve telephonic transmission through the long ocean cables. I have been acquainted with certain telegraphic and telephonic lines, in which permanent leaks have been established of given resistance and given impedance. But it was also impossible to keep them just so, and the resistance and impedance of such leaks would grow less in spite of all we could do. It is another illustration of that pet theory of Professor Oliver Wendell Holmes, or perhaps it was Edward Everett Hale—at any rate it was a Boston man—who believed in the total depravity of inanimate things. Now earth leaks, as we all know who have ever done any testing, are very difficult to obtain if we want them. If any one goes out to make a test on a line and has to hunt for a ground, it is a difficult thing to get one. On the other hand, if you do not want an earth leak, you can find them on every bush. I say this from actual practical experience that there is no going back on, right here in New York City.

There is one other thing that had almost escaped my attention. Telegraphers know how unstable is an instrument depending on a spring adjustment. There have been times in the telegraphic experience of this country when an operator has fought for circuit perhaps twenty-four hours. I know of one case where an operator fought with the clock all night for circuit. But there have been many times when there has been no clock in consideration at all, but when there have simply been weather troubles, and on account of leakages pretty well disseminated over the circuit, it was quite possible for a single circuit to have three independent messages going on, one at each end and one in the middle. That, of course, was only possible when there was a battery in the middle of the line. It was not possible to keep an adjustment five minutes in twenty-four hours. I rather think that such an adjustment is an unstable and untrustworthy thing to connect with translating devices that we have to work with day after day.

I think there is no call for incandescent lamps that will turn low. One of the greatest merits, it seems to me, of the incandescent lamp is that it is a particularly simple and uncomplicated contrivance, something that the wayfaring man, even though a butcher or restaurant keeper can unscrew if it breaks, and screw another one on without any trouble. We do not want any electrical appliance which is provocative of profanity. Some little time before we adjourned for the summer recess, one of our esteemed members, Mr. Carl Hering, read a paper which, as I remember it, indicated that lamps were kept in use too long anyhow; that it was desirable that lamps should be discarded before reaching their limit of life, and that it would be conducive to economy to discard them at some prior period, (and so far as I could see, and I had the honor of being in the chair on that occasion, where I could see the faces of all the members,) that paper met with a marked degree of approval. Now the excellence, as I have said, of that present incandescent lamp is its simplicity, and it is not at all difficult to imagine an aged and irascible man wrestling with a lamp such as depicted in the paper, but slightly out of adjustment, until the lamp reaches, what Mr. Carl Hering denominated in that paper, its "smashing point."

MR. EDWARD P. THOMPSON:—A former speaker has taken sides partly against the contents of the paper. I would like to try to counterbalance the effect by a few brief remarks. I followed the paper through, and the tone of it strikes me as experimental and theoretical, rather than relating to a perfected device. The gentleman who just spoke, has alluded to loose contacts and the undesirability of the same, but Mr. Moore proposes to overcome these difficulties, by the use of an evacuated space around the contacts. Mr. Moore is, I think, entitled to the credit of proposing and experimenting upon vibrating contacts in a vacuum, combined with means for varying the rate of vibration for purposes of regulation. As to the practicability of his devices, no one could probably foretell at this stage.

Vibrating contacts outside of an evacuated chamber, produce an unpleasant though faint buzz, which should be largely reduced in the absence of air. Wasting or burning away of the contacts should also be diminished in a vacuum. By having the contacts always in contact in a vacuum, and arranged to vary in pressure upon one another, (as for example, by the use of carbon) as intimated by the author, the sparking should be practically nothing. His proposition for thought and experiment upon making a regulator by using successive and intermittent impulses, so rapid as to be imperceptible, and means for regulating the rapidity of succession or duration of impulses, is worthy of attention and further experiment, especially if directed to the application of an exhausted bulb. The great aim should be to discover if it is possible to effect the reduction of the current to zero between the impulses.

Mr. W. J. Hammer:—I believe there is a very large field for the application of an incandescent lamp that can be turned up and down. I say this, after looking through the subject pretty thoroughly. I would like to call attention to the clause which Mr. Lockwood referred to, on page 438, in which it is said that "The regulation of intensity of incandescence of an electric filament has been a problem, the solution of which has been sought after by electricians in general the world over, and partial success has been attained by the use of the only known means 'dead resistance.'" I do not see any reference in the paper to that beautiful little device invented by Ries, and known as the Ries regulating socket, which contains a reaction coil or choking coil, or as it may be termed a transformer with one winding —the light being controlled by cutting in and out of circuit sections of the coil, and thus changing the ratio between the impressed E. M. F., and the counter E. M. F. produced by an alternating current. That is a little piece of apparatus that has proven highly efficient. It has met with extensive use. In this connection I might say that shortly after Mr. Moore devised this socket I had the pleasure of seeing and handling it. At that time it was in its simplest form, a vibrator in connection with a socket, and I tried it and it certainly worked very well. I told Mr. Moore and the gentleman associated with him that I did not believe in its practicability, for the very reason that Mr. Cuttriss mentioned, that it would oxidize and build up very shortly. But Mr. Moore spoke then of using such things as shunts and condensers, and putting it in a vacuum, and various other ways which he called attention to in this paper, and I agreed with him then, that the use of a vacuum arrangement would probably be the most efficient, if it could be done. He seems to have accomplished it. I think the paper shows a good deal of knowledge for an experimental paper, and from the little I saw of the apparatus, I think it is well worthy of the consideration of the INSTITUTE, and if he succeeded in making that work by employing the contacts in a vacuum, I should think that he had something that was more or less of a commercial article. Certainly there is a large field and a considerable demand for an incandescent lamp that will regulate. If I was standing on the platform there and reading a paper, I would be very apt to wish to turn that electric lamp up a little higher. Mr. Lockwood speaks of the "smashing point" of an incandescent lamp with such a regulating device in a humorous way. I presume, his experience with the average telephone has suggested such a contingency.

MR. P. B. DELANY:—I simply wish to state, Mr. President, that I have had some little experience with automatic vibrators in the field of telegraphy, and therefore can testify as to the stability of even open contacts, and their wonderful reliability. I was very much interested in Mr. Moore's paper to know what the effect was in putting an automatic vibrator in a vacuum. I had always conceived the idea that such a vibrator would be much more reliable and steady than when exposed to the air, not only on account of the sparking at the point of contact, but of the air displacement. Mr. Lockwood, referring to the experience of telegraphers, very properly describes the annoyance of loose contacts, but surely he knows as well as anybody that all automatic vibrators depend for uniformity of vibration upon the contact that is made between the contact finger and the vibrating reed. In regard to apparatus that may be controlled by such vibrators, it is probably well known that the foundation of the system of harmonic telegraphs, such as Dr. Gray has invented, depends upon the uniformity of pitch of different automatic vibrators, which enable him to work four, five or six circuits. My own experience covers a branch of telegraphy, wherein it was still more necessary than in the Gray system, probably, that we should have an absolutely uniform rate of vibration. In the synchronous multiplex system, automatic vibrators are used, and the contacts are exposed to the air, and all the disadvantages arising from change of temperature, and what is still more detrimental, change of humidity or moisture in the atmosphere surrounding the contact points. And still with the apparatus open to all these detrimental effects the distributer mechanism, controlled by these automatic vibrations, is rotated, with the aid of correcting impulses, without a variation of speed of a five-hundredth part of a second from morning till night. The system is in use all over England, and requires no particular attention, and yields four or six distinct circuits, according to the length of the wire. Therefore, I am very much pleased to hope that with an automatic vibrator in a vacuum a still more reliable rate of vibration may be obtained. I have always thought it might be so, and I think that perhaps aside from the electric lighting features, referred to in this very interesting paper, there may be other uses found for the utilization of this gentleman's device.

Mr. Moore:—Owing to the great length of a portion of the discussion, I shall be unable to answer it in detail, but in reply to Mr. Hammer's remarks I would say, that I too have often admired the beautiful method of controlling the alternating current that is now on the market. But the device under consideration, can be applied with equal facility both to the alternating and direct currents, and is a much simpler device.

With reference to what has been said on telegraphy, I merely offered that as a suggestion, having in view the principle of harmonic telegraphy.

Mr. Chas. P. Steinmetz:—I have listened with a great deal of interest to this ingenious paper. I must say, however, that after reading the title, "A New Method for the Control of Electric Energy" and the introduction of the paper, I was somewhat disappointed when I came to the substance of it, and found that it dealt with nothing else but a circuit-breaker acting in a vacuum. Looking more closely into it, however, there are certainly some interesting and novel features in putting the circuit-breaker (which we know from experience, does not work under ordinary conditions) in a vacuum, and so making it a reliable piece of apparatus. In this way I believe the paper offers us a new and interesting idea, but I do not think it should be called "A New Method for the Control of Electric Energy."

This interrupter will, undoubtedly, work very well in incandescent lighting, where small currents are used in non-inductive circuits, but in motor or other work where large currents and inductive circuits have to be dealt with, I do not think an interrupter can work, because it will not break the circuit, but the current will continue to flow and cross the gap as an arc. In an inductive circuit, the flow of current establishes a magnetic field, which represents a certain amount of stored energy, a few foot pounds, perhaps, or many thousands of foot pounds in large dynamo fields, and before the current can be broken, this potential energy has to be expended, and it is expended in an arc across the points of interruption, or if by main force you stop the current suddenly, then the energy is expended by striking through the insulation. You all know from experience with the Ruhmkorff coil and the quicksilver interrupter, that you can make any coil strike through the insulation by replacing the alcohol which covers the quicksilver of the interrupter with a very well insulating oil, so breaking the current quickly.

In incandescent lighting, however, the method will work very well and avoid unnecessary waste of energy, and may as a dimmer of incandescent lights find a large field of useful application.

MR. GANO S. DUNN:—One of the speakers mentioned that in regulating the incandescent lamp, he did not know how much of its diminished candle power was due to the ohmic resistance of the contact, and how much was due to the interruption of circuit, by the vibration. The value of the device depends on this point. In a lamp where the total energy is small, it would be difficult to determine; but I would like to carry the discussion into the domain of motor control, where the amount of energy is large. Taking this device as the counterpart of the choking coil, what is its efficiency as a controller? Mr. Moore mentions that controllers for motors produce heat, which is a waste of energy, and objectionable for insurance reasons. If his device is substituted for the ordinary rheostat, how much energy will be saved, or how much less heat will be produced, for the same amount of regulation? I would also ask, what is the largest amount of power he has ever controlled.

MR. MOORE:—I would state that, of course, this system has not been put into commercial operation as yet, and that the paper before the INSTITUTE is largely a series of suggestions as to the advantages that may be gained by such a system. If we consider, for example, the case of lamps individually regulated, it is certainly theoretically very efficient. The vibrating contacts are less than .01 of a sq. inch in area.

When the degree of incandescence of the filament is brought so low that it is not descernible to the eye, it indicates that there is the equivalent of 1,000 or 1,500 ohms resistance in circuit, while the measured resistance of the magnet was .4 of an ohm, so that by properly constructing a device which has only .4 of an ohm resistance in itself, you can get a resistance of 1,500 ohms.

MR. DUNN:—The point I wanted to get at was, how hot does the controller get, when it reduces the current in the lamp to say, one half?

MR. MOORE:—The device, when in operation, acquires a temperature equal to that of an ordinary incandescent lamp, and therefore the question of heating is not vital.

MR. A. E. KENNELLY:—The title of this paper is certainly wide, but the contents are narrowed down to a particular application of electric energy. It is suggested that motors can be controlled in this way. But there is no evidence before us that they can be effectively so controlled, and the difficulties in the way, as pointed out, are very great, owing to the large spark that will certainly take place, and the consequent danger of burning up the vibrator. There is, however. a large field for such a device when controlling small currents where the spark is not so dangerous, and no doubt, there is considerable practical advantage to be gained by turning a lamp low. We who use incandescent lamps much, do not notice this need, because we have accustomed ourselves to dispense with the capability, but persons who have not been so accustomed, do notice it There are two aspects under which this device may be examined. One is as an energy-saving device, and the other as a convenience-giving device. If it is intended to employ it in a lamp on a 200-volt circuit, and limit with this device the amount of current which the lamp shall receive, so that it shall give sixteen candle-power and no more, that is one way of regarding the question. Or if you take a 100 volt lamp on a 100 volt circuit and simply desire to turn that lamp low and obtain a reduced efficiency with the lamp in process, that is another way of looking at it. It is needless to say that the first method is certainly very advantageous, but very dangerous, because if the vibrator once fails, the lamp will be destroyed. The method particularly defined here is dimming a lamp. From some experiments with vibrating contacts of this description, I take it that the function of a vibrating contact of this kind is only in a minor degree in the resistance of the contact, as you would measure it when metal touches metal. Far the greater advantage lies in the time executed in free space by the spring, when out of contact with the point. It is obvious, that if you give a spring an amplitude of .1 of an inch, it cannot remain in communication with its contact point for more than a fraction of its period and only during that fraction can the voltage be maintained upon the lamp. It is for that very reason that the figures on page 441 give us no reasonable idea as to the relative amount of energy developed in the contact breaker or in the lamp. For example, take the last reading, .02 of an ampere and five volts on the voltmeter across the lamp. One might suppose from that at first sight, that the energy in the lamp was .1 of a watt and there was an energy of 105 volts, multiplied by .02 amperes or 2.1 watts in the contact breaker. That would be altogether, of course, incorrect. In the case of pulsating currents of this kind we can no longer rely on the ordinary multiplication of amperes and volts, because just at the time the current is active, the electromotive force is not active. All we can ascertain from these measurements is, that energy to the amount of the product of full supply pressure, and mean observed current is expended in the lamp and vibrator together. Now assuming that the value of this particular device is contained, for the present at least, in the dimming of the lamp, and its convenience to the public on that account, what we really want to know is, how long will it last. Can Mr. Moore tell us how long a vibrator, operated in this way, will keep at work? Will a vibrator last such a number of hours on trial as to justify the expectation, that the vibration will last the lifetime of a lamp? If there is anything in the paper that it is desirable to know it is that, and I have looked in vain for it in the paper. Probably the convenience of such an attachment would be very considerable if dimming could be effectually gained, the only disadvantage being, that if anything goes wrong with the contact, you cannot get at it. It is in the vacuum. But let us hope that Mr. Moore has some knowledge that will enable him to give us information as to how long we may expect that contact to last.

MR. MOORE:—In accordance with the experiments that I have made, although I have been severely handicapped in this direction, the contacts have not shown appreciable wear. I have not made continuous runs of 600 or 800 hours. But I have placed the controller in circuit a day at a time, and three or four days in succession, but no change of any consequence occurred. It is a matter of great interest as to what further investigations will develop. As stated, the paper was written in the hope that it will cause further investigations to be made in this line, which evidently, according to good authority, has not been investigated, and is an unexplored field of great proportions,—the exploration of which may be richly rewarded.

THE PRESIDENT:—I should think a difficulty would arise in the actual working of Fig. 3, on p. 441, where the vacuum of a lamp is utilized for the vacuum of the contacts. Here, as I understand, a contact screw passes through the chamber into the vacuum. From my experience with vacua, I should think you would find it difficult to obtain a working vacuum with anything movable entering into it. It might be done, though I question it.

MR. MOORE:—Allow me to explain, Mr. President. The armature and contact only, are in the vacuum. The illustration is rather indistinct; but by carefully examining the same it will be noticed that the magnet is not within the vacuum, and therefore the regulating screw does not enter the vacuum.

DR. WILLIAM E. GEYER:—I think the paper in using the phrase "varying-in-pressure contacts," implies that the regulation is in large part effected by a change in resistance, self-induction also coming in, and some of the gentlemen who have discussed the paper have assumed that it is largely a question of resistance. I think that is a mistake. As I understand it, this apparatus works not so much by any change in resistance or by self-induction coming in, as by the relative time that the contacts take place. Take for instance Fig. 2. The screw A is far to the left, so that the vibrating portion, B, strikes it only at the extreme end of its excursion. The contact is very short, and the current is on the lamp for just a moment. If the screw is turned further up, the current will last a longer time. The pressure will not be notably greater, and the current is on the lamp longer. When finally the screw is turned up very far, the contact will last for a considerable portion of the time of vibration, and the current is on longer still, and has a chance to heat the filament up. Of course, the time comes when the contact is continuous, and then the current has its full opportunity to heat up the filament. The rate of vibration, as I understand, would make no very particular difference, because it is only the relative time of contact which counts. If the vibrations are twice as fast, there will be twice as many contacts. The only thing counting is the relative time of contact.

THE PRESIDENT:—I see a distinguished honorary member here. I would like to ask Mr. Preece to take a seat on the platform.

Gentlemen, you see there was wisdom in my having Mr. Moore's paper come in the beginning, for I felt quite sure that it would cause considerable discussion, which it has done and I wanted to ensure full time for such discussion.

I will now give you a brief inaugural on the International Electrical Congress and World's Fair of 1893.

Inaugural address of the President at the Seventy-ninth Meeting of the American Institute of Electrical Engineers, New York, September 20, 1893.

THE INTERNATIONAL ELECTRICAL CONGRESS

—AND—

WORLD'S FAIR, OF 1893.

BY EDWIN J. HOUSTON.

GENTLEMEN:—During the past few months a mighty human stream, daily increasing in volume, has continually flowed towards a single centre of population in the North American continent. This stream, fed from all parts of the earth, is formed by the many millions who are journeying toward the World's Fair, to do honor to those four hundred years' growth of the greatest Republic the world has ever seen.

Many of us have but recently left this mighty stream. Like others we visited Chicago to become eye-witnesses of the nation's growth. But not for this alone; perhaps the principal incentive to most of us has been a desire to take part in the International Electrical Congress of 1893, not only because we are especially interested in the progress of electrical science, but also because we are members of the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS, with whom the first conception of this Electrical Congress originated.

It has occured to me that in a brief inaugural address to the INSTITUTE, I could not select a better topic than the International Electrical Congress and World's Fair of 1893. I will, therefore, give you a few thoughts on this subject.

As in our journey we crossed the mighty continent, we saw on every side evidences of its vast mineral and agricultural wealth. As we passed through its populous cities and at last reached Chicago we wandered through its streets and examined with admiration the long rows of stately buildings. In all these things we rejoiced at the growth of a nation that has achieved so much in so short a time. But was it not in that extra-mural, younger, but potentially greater Chicago, yclep'd "The Great White City," that we were most impressed? Its grandly proportioned buildings that arose as if by magic would have rendered a distant journey profitable, as a mere architectural display But when we crossed their portals and examined the rich exhibits, collected from all parts of the world, we rejoiced in these proofs both of the nation's and of the world's progress.

But the growth of the American Republic during the four hundred years that have passed since its discovery by the Genoese Navigator, marvellous as it has been, is less astonishing to us as electricians than the extraordinary development in that vigorous science, electricity, to which many of us have devoted the energies of our lives. Contrasting the World's Fair of Chicago, of 1893, which embraces exhibits in all departments of man's skill and industry, with the Philadelphia International Exhibition, of 1884, which, as you remember, was devoted wholly to electricity, I think you will agree with me that we have ample reason to rejoice at the progress made in the electrical field during the past nine years.

I have no sympathy with the unthinking critic who expresses disappointment with what he chooses to style the meagerness of the electrical display of the Chicago Exhibition. He probably estimates the value of an exhibit by the number of square feet of floor space it covers, rather than by its inherent possibilities. To my mind many exhibits whose actual demand for floor space are limited to less than one hundred square feet, would, were they estimated from the standpoint of their true value, demand more than the entire area of the exhibition grounds for their display.

Such critics fail to appreciate the fact that not in the Electricity Building alone, but throughout the entire exhibition as well, there is to be seen a grand display of the wonder working force of electricity. Almost countless arc and incandescent lights turn night into day, both in the buildings and over the extended areas outside them. Powerful search lights flash their bright beams far and wide beyond the gates of "The White City." Electric launches on the lagoons, and electric cars on the Intramural Railway show by actual practice the power electricity possesses in systems of transportation. Systems of telegraphic and telephonic communication vie with systems of time transmission, annunciator, fire, burglar and temperature alarms, both in Electricity Building and elsewhere, not only as exhibits pure and simple, but also in that shape which we, as practical men, so delight to see as representing the highest type of scientific achievement; viz., in every day practice.

I think no one will question the completeness of the electric motor exhibit. Both in Electricity Building and elsewhere, electric motors in many forms are to be seen performing work, varying in amount from that required to move the most delicate machinery, to that capable of driving a full size locomotive. Nor is the exhibit of dynamos incomplete. An excellent display is made in Electricity Building, while a fair proportion of the entire floor space of the Palace of Mechanic Arts is occupied by working dynamos, not so much as an exhibition, as a great central lighting station established for the illumination of the buildings and grounds.

Perhaps the most striking proof of the advance that has been made in electricity since the time of the International Electrical Exhibition of 1884 in Philadelphia, a period less than a single decade, is that many of the achievements of electricity have proved such boons to the world, and have so thoroughly come into every day use that they have ceased to be regarded as wonders. It therefore, no more enters into the minds of those who are operating electrical apparatus embodying such achievements, that they form proper objects for exhibition, than it would for them to exhibit any other universally good thing, such as sunshine, air or water.

Even we, who know better, are apt to follow the lead of the unreflecting and are often too ready to relegate some nine-days wonder of electricity to the domain of the common and prosaic. We speak through a conducting wire, and the world wonders that the potentiality of the intricate waves of articulate speech can be so transmitted. We examine the mechanism of the apparatus, understand its operation and promptly cease to wonder, since we see that such mechanism must be operative. We extend the distance through which we can thus carry on intelligible speech, until it reaches to over a thousand miles. Again a nine-days wonder, and the world freely gives its plaudits to the brainy men who have contributed to this success; but in a short time even this achievement is tacitly relegated to the ordinary and the common, and it therefore ceases to be regarded as a thing worthy of special exhibition.

When, through the courtesy of the American Bell Telephone Company, I was permitted, as were doubtless many of you, to carry on an extended conversation between Chicago and New York, or even between Chicago and Boston, by the long-distance telephone, the apparatus must have seemed to you, as it did to me, a great achievement, and one which particularly accentuated the wonderful recent growth of electrical science.

But these inventions do not stand alone. There are many others. Take, for example, the telautograph, of Gray; the radiophone, of Bell; the Cuttriss syphon-recorder, of the Commercial Cable Company, the practical welding process of Elihu Thomson; or consider some of the many brilliant discoveries which Tesla gave to the world but a few years ago, but which are even now almost regarded as old; or consider some of the possibilities at which the same investigator hinted in his recent lecture in Chicago on mechanical and electrical oscillators; or look at the almost innumerable improvements in the details of apparatus, or of systems of distribution, little things in themselves, but such little things as determine the difference between success and failure; or consider the thousand and one other novelties with which you are acquainted, and which doubtless called from you much admiration for the ability of their ingenious inventors.

But I will go no further in this direction. I will content myself to submit to your judgment, being sure of a favorable verdict, the correctness of the belief above expressed, that electrical science has made wonderful progress since 1884, and that such progress has not failed to receive proper and extended exhibition at the World's Fair in Chicago in 1893.

The Chicago Exhibition still exists. It may yet be visited and compelled still further to yield its intellectual benefits.

But the Chicago International Electrical Congress of 1893 has come and gone. That it has accomplished much lasting good, I think there can be no reasonable doubt. That it might have accomplished more under a more liberal leading there can, I think, be equally no doubt. But, taking all in all, I feel that the electrical fraternity throughout the world are to be sincerely congratulated on the results of its work.

I shall not speak of the excellent papers read before the different sections of the Congress. You are as well acquainted with them as I am. I may, however, state in this connection that out of some thirty of these papers no less than fifteen were by members of the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS, and that out of seventeen American papers no less than fourteen were by members of the INSTITUTE. This, I think, is a record for which I may be permitted officially to congratulate you.

The work of the Chamber of Delegates was especially of a satisfactory character. Though the good work might have been profitably extended to a number of additional topics, such, for example, as a more definite discussion of the magnetic units, and an adoption of practical units of light and of illumination, and to some definite action on a uniform system of general notation, yet I think you will agree with me that the work actually accomplished has been of a very valuable character.

Through the courtesy of Prof. Edward L. Nichols, the official secretary of the Chamber of Delegates, I am permitted to present to you the following official report of the work accomplished by the Chamber of Delegates.

REPORT OF THE CHAMBER OF DELEGATES AND CLOSING MEETING OF THE CONGRESS.

The Congress was brought to a close by a general meeting of all the sections in Columbus Hall, at the Art Institute. Dr. Elisha Gray presided, and the official delegates occupied the platform. Prof. Edward L. Nichols, Secretary of the Chamber of Delegates, then read the report of that body containing the following recommendations:

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:

As a Unit of Resistance, the International Ohm, which is based upon the ohm equal to 10^9 units of resistance of the C. G. S. system of electromagnetic units, and is represented by the resistance offered to an unvarying electric current by a column of mercury at the temperature of melting ice, 14.4521 grammes in mass, of a constant cross-sectional area and of the length of 106.3 centimetres.

As a Unit of Current, the International Ampere, which is one-tenth of the unit of current of the C. G. S. system of electromagnetic units, and which is represented sufficiently well for practical use by the unvarying current which when passed through a solution of nitrate of silver in water and in accordance with the accompanying specification deposits silver at the rate of 0.001118 grammes per second.

As a Unit of E. M. F., the International Volt, which is the E. M. F. that, steadily applied to a conductor whose resistance is one international ohm, will produce a current of one international ampere, and which is represented sufficiently well for practical use by $\frac{1000}{1434}$ of the E. M. F. between the poles or electrodes of the voltaic cell known as Clark's cell, at a temperature of 15° C., and prepared in the manner described in the accompanying specification.

As the Unit of Quantity, the International Coulomb, which is the quantity of electricity transferred by a current of one international ampere in one second.

As the Unit of Capacity, the International Farad, which is the capacity of a conductor charged to a potential of one international volt by one international coulomb of electricity.

As the Unit of Work, the Joule, which is 10^7 units of work in the C. G. S. system and which is represented sufficiently well for practical use by the energy expended in one second by an international ampere in an international ohm.

As the Unit of Power, the International Watt, which is equal to 10^7 units of power in the C. G. S system, and which is represented sufficiently well for practical use by the work done at the rate of one Joule per second.

As the Unit of Induction, the Henry, which is the induction in the circuit when the E. M. F. induced in this circuit is one international volt, while the inducing current varies at the rate of one international ampere per second.

The Secretary also read the following report of the committee appointed to consider the standard of light:

They have had much discussion upon the various forms suggested for practical standards, and in particular upon the two special forms of lamps known as the amyl-acetate lamp of von Hefner-Alteneck and the pentane lamp of Vernon Harcourt. The only practical lamp actually presented to the committee is the new von Hefner lamp which, although it has been laboriously tested, at the Reichsanstalt and reported accurate to within two per cent., has not received any extended trial in other lands. On the other hand it was reported that the pentane lamp in its recent improved form was preferred in England for the photometry of gas lights. There is the objection to the pentane lamp, that the composition of the commercial pentane is not sufficiently well defined; and to the amyl-acetate lamp that its color is too red in hue; finally objection to all open flame lamps is that they are too liable to be influenced by the changes in the pressure, temperature and moisture of the air. It is admitted on the other hand that no electric lamp suitable for use as a convenient practical standard has yet been realized. Under these circumstances there was a sharp division in the committee between those who advocated the von Hefner lamp as an independent standard, and those who desired to maintain a *statu quo* until further researches should have been made in various countries.

It was proposed by Drs. Budde and Lummer that the Hefner-Alteneck lamp, constructed exactly according to the specifications of Mr. von Hefner-Alteneck, be introduced as a provisional, practical standard of light, and that the problem of determining its value in terms of an absolute unit be left to subsequent investigation. On vote this was lost by two votes for, and four votes against. The following motion proposed by Messrs. Palaz and Thompson, and amended by Drs. Budde and Lummer was then carried unanimously:

Resolved, That this committee, while recognizing the great progress realized in the standard lamp of von Hefner-Alteneck and the important researches made at the Reichsanstalt, also recognizes that other standards have been proposed, and are now being tried, and that there are serious objections to every kind of standard in which an open flame is employed; it is therefore unable to recommend the adoption at the present time of either the Von Hefner lamp or the pentane lamp, but recommends that all nations be invited to make researches in common, on well-defined practical standards, and on a convenient realization of an absolute unit.

(Signed,)

J. VIOLLE. E. BUDDE. EDW. L. NICHOLS. A. PALAZ.
SILVANUS P. THOMPSON. O. LUMMER.

The electricians of America ought especially to feel satisfied with that part of the work of the Chamber of Delegates which resulted in the recommendation of the adoption of the name Henry for the practical unit of induction.

The early work of Prof. Joseph Henry in the domain of self and mutual induction is too well known in this assembly to need comment. The principle of naming the practical electical units after distinguished electricians who have passed from their labors, having been established, it appeared eminently proper to confer this honor on that illustrious electrician, whom we are proud to claim as an American. Its adoption is especially gratifying to the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS, who first proposed the name. There was too a peculiar fitness in thus bestowing this honor on America at a time, when the whole world has united in sending its choicest products to our great exhibition to honor the four hundredth year of the discovery of America.

Knowing, as we do, the desire of France to retain the name quadrant for the practical unit of induction, it was especially gratifying to us to learn that the resolution to name this unit the Henry, after the distinguished American electrician, was introduced into the Chamber of Delegates by Mascart, of France, and seconded by Ayrton, of England.

I know how much easier it is to find fault with an enterprise and to show how it should have been conducted, than it would be to carry it on successfully oneself. I have, therefore, no little hesitancy in criticising any part of the work of the late Electrical Congress; but, looking at the recent Electrical Congress as I experienced it, I trust I may be pardoned if I point out some respects in which I believe it could have been improved. I do this, not for the purpose of finding fault with the conduct of the Congress, nor of criticising any of its officials; for, on the whole, the results achieved are excellent. I criticise it only with the hope of preventing similar errors in the future, and to correct impressions that have prevailed, especially among our foreign friends, that all the arrangements for the Electrical Congress were in the hands of the INSTITUTE. The only work with which our INSTITUTE was officially connected, was that considered by the Chamber of Delegates. As this was prepared by the Sub-Committee on Provisional Programme, and printed in the TRANSACTIONS, beginning with the issue for January, 1893, you are of course familiar with it. Hospitalier's system of notation was also translated and reprinted in the TRANSACTIONS, and copies furnished to the Chamber of Delegates.

I think all who attend any of the many congresses held under the suspices of the Electrical Congress Committee of the World's Fair Auxiliary will agree with me that wretched judgment was displayed in holding the sessions of the congresses in the Art Institute. This building, as we know from bitter experience, was entirely unsuited for the purposes of a learned assembly, being situated in a noisy city, alongside a line of railroad that was carrying an enormous traffic, and directly opposite a great railroad yard, where the constant shifting of cars, the puffing of locomotives, the blowing of whistles and the ringing of bells rendered very much of what was said at the Congress inaudible.

I believe I but voice the general sentiment when I object to the distance the place selected for holding the meetings of the Congress, was from the Fair grounds. The selection of this site necessitated a considerable loss of precious time in going between the exhibition and the Congress; time whose value can be estimated only by the loss of the rare opportunities for culture afforded by the rich exhibits within the grounds.

The division of the Congress into different sections; to wit, Section A, Pure Theory; Section B, Theory and Practice; Section C, Pure Practice, was also in my judgment exceedingly unfortunate.

There may be such a thing as pure theory apart from practice, though I am disposed to doubt it; for, theory is, or at least should be, based on facts, *i. e.*, on practice, and cannot be properly dissociated from it. But, however this may be, I feel sure there can be no such thing as pure practice apart from theory; to my mind, the two necessarily go hand in hand and cannot, therefore, be separated.

But apart from this, the division appears to me to be unfortunate as well as arbitrary; for there can be no doubt that the greater part of the advantage derived from congresses or other assemblies of intellectual men comes from the contact of mind with mind; not from the contact of similar, but of dissimilar minds. Any attempt, therefore, arbitrarily to divide a large cultured assembly into classes, must, I think, result disastrously.

This division of the Congress into sections was also unfortunate, inasmuch as it prevented the members of the Congress from hearing many papers or discussions on subjects in which they were interested, from the impossibility of simultaneously attending the several sections. This difficulty might have been lessened, had the programme been arranged so as to give the titles of papers, and the order in which they would be presented in each section, thus affording an approximate idea of the time. This practice is well understood and followed in meetings of most technical societies.

I think it was a grave mistake to have made the sittings of the Chamber of Delegates private. No harm could possibly have resulted from permitting the general Congress to listen to the debates. On the contrary, I believe much good must have so resulted. Indeed, I see no reason why the members of the general Congress should not have been permitted to take part in the discussion, though not permitted to vote.

But, to my mind, the most unfortunate feature was the preliminary work of the Electrical Congress of 1893, as carried on by the Committee of the World's Congress Auxiliary. Notwithstanding the fact that the determination to hold an International Electrical Congress, originated with the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS, and notwithstanding the excellent prior work of the INSTITUTE in arranging preliminary details, the World's Congress Auxiliary, an organization created for the purpose of conducting the many different congresses that were to assemble in Chicago during the World's Fair, made it a cardinal principle of procedure, that no society should be permitted to take charge of the work of a congress.

When the varied character of the numerous learned congresses that were expected to meet in Chicago during 1893 is taken into consideration, we may be able to form some idea of the herculean labor the World's Congress Auxiliary thus assumed. We are, therefore, less surprised at their failure in many cases to invite the most prominent men to represent the particular subjects involved.

Where the interests in any line of intellectual work were represented by different societies of fairly equal importance, we are willing to admit the advisability of preventing the influence of a single society dominating that particular congress, though even this case affords no reason why such congress should not be conducted by a joint committee fairly representing the different societies. But the INSTITUTE OF ELECTRICAL ENGINEERS is confessedly the representative electrical society of the United States. I submit, therefore, that the policy of excluding the INSTITUTE from the conduct of the Electrical Congress, and of persistently denying it official recognition, was both unfortunate and indefensible.

For my part, I am unable to see how any small body of men, such, for example, as those forming the World's Congress Auxiliary, no matter how profound or extensive their erudition, could hope to be able to properly and efficiently arrange the meetings of so many different learned congresses so as to ensure the presence of the men most fitted to undertake the different characters of work.

I, therefore, feel that the refusal of the World's Congress Auxiliary to permit the work of each particular congress to be arranged and carried on by the representative society or societies in the branches to which such c ngress related, was a radical error which must in most cases have seriously interfered with its usefulness, and which should be especially avoided in the future.

It has occured to me, that it might, perhaps, be advisable to place on record a brief history of the part taken by the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS in the World's Electrical Congress of 1893.

I am indebted to our secretary, Mr. R. W. Pope, and to the official records of the INSTITUTE, for the facts.

The AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS, being generally recognized as the national electrical organization of America, deemed it advisable, as early as 1889, to take steps towards identifying itself with international work, thereby securing proper recognition for the important electrical researches of Americans which had hitherto been ignored, not by reason of their lack of importance, but because no organized body representing America had ever participated in the International Congresses of Europe. Five delegates, representing the INSTITUTE, were accordingly appointed to attend the International Congress held in Paris in 1889. As it was believed that a World's Fair would be held in America in 1892, to commemorate the discovery of the continent, the INSTITUTE determined that an International Electrical Congress should be convened at that time in whatever city the Fair might be held.

In August, 1889, a committee was appointed by President Elihu Thomson, to make preparations for such proposed International Electrical Congress of America.

The postponement of the date of the World's Fair to 1893, and the holding of an International Electrical Congress at Frankfort in 1891, rendered it unnecessary for this committee to do anything more than to take advantage of the coming Frankfort Congress again to notify the world of their intention of holding an International Congress in America during 1893.

On September 16th, 1890, the following resolutions were adopted by the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS:

Whereas, It has been the custom in the nomenclature of electrical units to perpetuate the names of men who have contributed most to electrical science; and,

Whereas, In the names thus far adopted, the eminent services of Americans have not been recognized; therefore,

Resolved, That in the opinion of the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS a just distribution of the honors thus bestowed necessitates a recognition of one or both of America's great electricians—Benjamin Franklin and Joseph Henry;

Resolved, That the name of Henry should be given to the practical unit of self-induction, since he was the discoverer and greatest investigator of this phenomenon, and because this unit at present is called a quadrant, which is merely a numerical value and not a suitable name.

Resolved, That this INSTITUTE recommends to electrical societies and electrical engineers the general use of the name Henry for the unit of induction, as being the quickest and surest way to secure its final adoption.

Upon the receipt of an invitation extended by the management of the Frankfort International Congress of 1891 to the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS to take part in its deliberations, five delegates were appointed by the Chair to attend the Frankfort Congress, at the general meeting of the INSTITUTE, on May 21st.

The adjournment of the Frankfort Congress, to meet in Chicago in 1893, was considered as settling the fact that an International Electrical Congress was to be held at that time, and the INSTITUTE, therefore continued its preparatory work, and on October 27th, 1891, its committee on the International Congress of 1893 reorganized as follows, viz.:

T. COMMERFORD MARTIN, Chairman.
RALPH W. POPE, Secretary.

PROF. W. A. ANTHONY,	T. D. LOCKWOOD,
PROF. ALEX. GRAHAM BELL,	C. O. MAILLOUX,
PROF. FRANCIS B. CROCKER,	PROF. HENRY MORTON,
PROF. CHARLES R. CROSS,	DR. EDWARD L. NICHOLS,
DR. WILLIAM E. GEYER,	GEORGE M. PHELPS,
LUDWIG GUTMANN,	FRANKLIN L. POPE,
GEORGE A. HAMILTON,	NIKOLA TESLA,
COL. CHARLES H. HASKINS,	PROF. ELIHU THOMSON,
CARL HERING,	EDWARD WESTON,
PROF. EDWIN J. HOUSTON,	DR. SCHUYLER S. WHEELER,

A. E. KENNELLY.

Sub-Committee on Provisional Programme,

CARL HERING,	PROF. W. A. ANTHONY,

A. E. KENNELLY.

But these preparations for an International Electrical Congress were rendered futile by the decision of the officials of the World's Fair, that the organizations of all the world's congresses that were to be held in connection with the World's Columbian Exposition should be placed in the hands of the World's Congress Auxiliary, and that all details connected with the proposed

Electrical Congress should be placed in the hands of the World's Congress Auxiliary Electrical Committee, of which Dr. Elisha Gray was chairman.

At a meeting of the INSTITUTE'S Congress Committee, held in New York in December, 1891, Dr. Gray emphatically stated that no society would be permitted to take charge of the work of a congress, the World's Congress Auxiliary Electrical Committee being created for that express purpose. The INSTITUTE unavailingly contended that it should at least receive official recognition in the conduct of the Congress. Having solely in view the advance of electrical science, in the interests of harmony it reluctantly passed the following resolution, December 23, 1891:

Resolved, That the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS, having already taken action during the past three years, by correspondence and otherwise, towards the holding of an International Electrical Congress in connection with the Columbian World's Fair, hereby expresses its desire and intention to co-operate, by all means in its power, with the World's Congress Auxiliary of the World's Columbian Exposition, through its Electrical Congress Committee, in furthering the gathering of such a congress at Chicago in 1893, and in making it a successful and worthy representation of the best electrical science and practice in all parts of the world.

With this resolution the INSTITUTE relinquished all responsibility for the management of the Congress, but continued the work of preparation by the appointment of a sub-committee on a provisional programme.

There are several important topics that failed to receive final or sufficient treatment by the Chamber of Delegates. Prominent among these are the practical units of light and of illumination. It has been proposed that the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS take up and complete this unfinished work.

I deem the proposition extremely valuable, and would suggest that the INSTITUTE take up the work after substantially the following plan; viz., to increase the regular committee of the INSTITUTE on Units and Standards, and appoint a number of subcommittees to whom shall be entrusted the carrying on the necessary experimental work on which to base practical standards of light and of illumination.

I would suggest that the sub-committees be constituted as follows: That a chairman be named in as many of the colleges, universities, or other learned institutions as possess the necessary physical or electrical laboratories as the INSTITUTE may select, and authorize each chairman to name additional members, subject to confirmation by the INSTITUTE, from their colleagues in their respective faculties, or from proper parties residing in the vicinity, to carry on, without expense to the INSTITUTE, the necessary investigations.

This work should be distinctively INSTITUTE work. Therefore, no one should permitted to serve on such committees, or to take part in such work, who is not a member of the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

Our distinguished Honorary Member, Mr. Preece, of England, has kindly offered to undertake such work in England, and to associate with himself and such other parties, as we may determine. I would earnestly suggest that the INSTITUTE authorize me to appoint the necessary sub-committees, so that they may proceed at once to the work suggested.

In conclusion permit me to say that I esteem it a very high honor to have been elected by your votes to the presidency of the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS. Even to belong to this INSTITUTE is an honor that is not conferred inadvisably; therefore, to be called to represent so distinguished a body of men is something for which I may, perhaps, be pardoned if I feel proud.

During the pleasant week of the International Electrical Congress of Chicago, I have, as far as possible, embraced the opportunity of entering into more intimate relations with many of its members, and have thus acquired a better acquaintance with its personelle. From what I know of the early history of the Electrical Congress, from what I saw of its actual work, and from the knowledge I have gained of those of its very many members who were also members of the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS, I feel that our INSTITUTE has not failed in contributing its full share towards whatever success the Congress has achieved.

MR. LOCKWOOD:—I sincerely trust that I shall not even for a single moment be thought guilty of disrespect to the Chair, though I deliberately transgress the accepted rules of order, and turn my back wholly on Cushing's Manual (not being accustomed to manual labor), by addressing my remarks to the gentleman sitting at your left, the Secretary of this INSTITUTE. Mr. Secretary and gentlemen of the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS, you have with myself heard, with feelings of pleasure and admiration, the inaugural address of the President of this INSTITUTE. We should, I am sure, consider ourselves fortunate, and unite in our felicitations that we have as our presiding officer for this year a gentleman who has so thoroughly proved his powers of observation; who has combined with those powers of observation the faculty of so happily putting them into exceptionally graphic language, and who possesses the literary faculty of being capable to develop in so short a time, not only a thoroughly readable, but a thoroughly eloquent and therefore listenable a production.

It appears to me that his comments upon the necessarily co-operative and co-ordinate relations of true theory and true practice, and his very decidedly expressed opinion that what God has joined together, no man or committee should put asunder, especially are of such a character as to commend themselves to every one.

And I have very great pleasure, Mr. Secretary, in moving a vote of thanks to the President for his able and instructive inaugural address, with the request that he permit the same to be published in the proceedings of the INSTITUTE.

[The motion was seconded by Mr. Phelps, and carried unanimously after being put by the Secretary.]

THE PRESIDENT:—Gentlemen, I thank you.

I know that I voice the sentiments of the INSTITUTE when I take the liberty of calling on our distinguished honorary member to give you in a brief address, some of his impressions of the Congress, or anything else that he may feel like talking to us about. [Applause.]

MR. PREECE:—Mr. President and Gentlemen, if it had not been for the alacrity of Mr. Lockwood, I think that I should have claimed the indulgence of this meeting to have broken your rules, and asked you to allow me, in our usual English fashion, to propose a vote of thanks to your President for the clear, able and extremely interesting address that he has given you. But my wish has been forestalled, and I am asked to give you my thoughts, or an address on anything. Of course, I cannot go beyond Chicago, and I cannot forget the Congress. At the same time I did feel a certain sensation of satisfaction and pride when, in the earlier part of his address, your President referred to the four hundred years' history of this great Republic. When he made that allusion, I could not forget the fact that for about three hundred years this so-called republic was acting under the guidance of the Old Country at home. [Applause.] So that I feel that of whatever progress you have made here, we can at any rate claim some little share. Action, sir, and re-action, we have all been told, and, I have no doubt, you have often taught, are exactly similar, and, while in the past, a hundred years ago or more, you taught us a very severe lesson by foolishly chucking some tea into the sea, we on the other hand have learned a much greater lesson, and ever since then have allowed all our children to do just whatever they like, and they always do it. [Applause.] I cannot also forget that your President referred to the history of electrical enterprise in this country during the past nine years, from 1884. It happens that that year was exactly coincident with my last visit to this great country. I was present at the exhibition in Philadelphia, in 1884, and when I look back to the history of the past nine years, I cannot shut my eyes to the fact that every single advance of any consequence or any value has taken place in this country. You alluded especially to the introduction of motors. There is nothing that strikes us, who come over from the other side of the Atlantic, so much as the marvelous, the wonderful advance that has been made in the extension of electric railways. I have come over here not alone to attend the Congress, not alone to see the World's Fair, but to see, to the utmost of my ability, during the time allowed me, all progress made in all branches of electrical industry, and while the advances in telephony, and the advances in electric lighting and other applications of electricity are great and striking, there is nothing so wonderful and so astounding as the development of electric railways, and particularly of electric railways in Boston. You made some allusion, too, to the wonders of science, and that we, who come day by day, in the performance of our allotted tasks, to the application of science to the wants and purposes of mankind, are apt to get rather careless and indifferent to the wonders of science that are displayed about us. So far as I am personally concerned, I do not agree with you in that respect. There is not a single day of my life that I do not feel the utmost wonderment at the advances of science, and especially of electricity, and although nothing is more startling and wonderful than the reproduction of the human voice, and the production of the electric light, still all these fade when you come to think of the marvelous influence of light in producing pictures. There is something in the interaction of those undulatory movements that constitute light and produce pictures, and those undulatory movements that you allude to as being in the domain which Tesla is now exploring; there is something in the connection between light and sound, and electricity and motion generally, that indicates a reason why the conveners of the Congress at Chicago should have divided the subject into pure theory and pure practice. Theory covers a field. There is a field that is apart from practice. There is a field in which the philosopher wanders and gives play to his imagination, and I cannot help thinking that we are now, thanks to the practice of the past, gradually approaching a shore on which we can pick up pebbles that will sooner or later give us a clue and an indication of the actual existence of the marvelous power or force that sometimes we call electricity, sometimes energy, sometimes light, but which is really the evidence of some wonderful agency which exists in nature, of which at the present moment we are absolutely ignorant. And therefore it is, that electricity shadows before us something that enables the old gentlemen,—you and I, sir,—to wander at will in fields that will warrant future congresses in justifying the division of the Congress in Chicago into two classes at any rate, practice and theory.

Now you must not forget this,—that one part of the Congress at Chicago was governmental and official. It was called together by the government of the United States; it was responded to by every government in Europe, and we met together there. I am delighted to hear you grumble and growl. It is one of the privileges of the Anglo-Saxon race to be able to growl to their heart's content. By your grumbling and your growling you save the necessity for anybody else doing it. It would be extremely ungracious, it would be inhospitable, it would be outrageous on the part of a guest like myself to grumble and growl. At the same time I am glad to have it done. [Applause.]

Now I interrupted you, sir, when you suggested that the henry was proposed by Mascart and seconded by myself. I should have been delighted to second it. Nothing on earth that I know of would have given me greater pleasure than to have seconded that proposal, and I was prepared to do it. But we thought there was an additional grace in its being seconded by Professor Ayrton, for Professor Ayrton had himself proposed a name for this unit—a secohm—and he in conjunction with Professor Perry had invented an instrument to measure the unit, called a secohmmeter, and there was something particularly pretty in the notion that the henry should be proposed by such a Frenchman as Mascart, and seconded by such an Englishman as Ayrton. [Loud applause.]

You also implied that the work of the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS did not receive its full meed of praise. I do not care two pence about the World's Fair, or the World's Congress Auxiliary, or what they may have done, but I know this, that I am expressing the sentiments of every single foreigner—I do not call myself a foreigner, as you know—but I express the feeling of everybody there, that there was one haven of rest where we could all go to receive a little comfort, and that was the office of the INSTITUTE OF ELECTRICAL ENGINEERS of America, and there was one cheery face that always welcomed us there, was glad to see us and who did all he could to make our visit to Chicago as pleasant as it could be made. And it was pleasant. I do not think that I can refer to any visit that I have ever made anywhere, that I shall recall with greater pleasure than my visit to Chicago, and especially it will be associated with the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS; not because they did as much as they could do, but because of all the titles and honors that I hold, there is not one which I feel a greater pride in holding than that of being an honorary member of the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

Sir, it was my intention to propose a vote of thanks to you. I do not propose that vote of thanks, because it has been done. But I desire to express to you my great obligations for giving me such a capital resumè of what you saw at Chicago and what I saw. I do not mind telling you, gentlemen, that I came over to this country to steal all I could, and I intend to go back and make use of all I have learned, and one thing I certainly shall do, I shall make free use of your ideas, sir, and in the report I make to the Institution of Electrical Engineers, and in the reports that I make to her Majesty's government of what I have done and seen here, I shall certainly refer to the address delivered to this INSTITUTE, and also to the good work that has been done by this INSTITUTE, membership in which I feel such a pride of holding. [Applause.]

There is just one point that I wish to refer to.

Your President alluded to the fact of the work left undone. He said that I was prepared to carry on that work in England, as your honorary member, and I shall do so, not alone because I am an honorary member of this INSTITUTE, but because I am deeply interested in the adoption by the electrical fraternity of a unit, not of light, but of illumination. I want to know what is the light on that paper. I do not care a button where it comes from. It may be gas, or a candle, or the sun, or the moon. We want to be able to express in something that we understand as clearly as we understand the ohm, the ampere or the henry; we want to express exactly what is the illumination on that paper; and therefore it is that I support for that purpose with all my power the proposal of your President, for the establishment of a committee to deal with this matter. We will work at it very hard in England and I hope that we shall be able to do something to enable you to come to a decision.

THE PRESIDENT:—Gentlemen, the hour is running on. Will you take any action in this matter? There is really no motion before the INSTITUTE. I think the INSTITUTE can do very excellent work in this connection.

MR. F. W. JONES:—I move Mr. President, that the Chair be authorized to appoint a committee to carry out the suggestions made in the Inaugural Address of the President.

[The motion was carried, and the Chair subsequently appointed Mr. A. E. Kennelly, Dr. Wm. E. Geyer and Mr. Carl Hering.]

MR. STEINMETZ:—I would like to say a few words on a matter that has been considered several times here, viz.: the establishment of local branches or chapters of the INSTITUTE. I wish to sound the sentiment of the INSTITUTE about the establishment of a chapter in Lynn.

As you know, there is an electrical organization there, having something like 70 or 90 members, and about the same tendency as our INSTITUTE here. At the last meeting of this organization—the Thomson Scientific Club—the question was brought up whether it would be advisable to join hands with the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS. No definite action was taken, but I was instructed to ascertain the state of feeling here in New York. The main reason was and is, undoubtedly, the rapidly increasing influence and importance of this INSTITUTE as displayed during the time of the Electrical Congress in Chicago, and as expressed in the scientific papers published in the TRANSACTIONS of the INSTITUTE. We knew that this question had been discussed here by the INSTITUTE frequently, and that, as I believe, the general sentiment was towards the establishment of such chapters.

Now I should like to hear what the members think about this question, and how they would stand if the proposition to unite with the INSTITUTE were brought up by the Thomson Scientific Club of Lynn. Our idea was, that some kind of an arrangement could be made so that election to membership would take place in the same manner as before, by the Council of the INSTITUTE; that the local chapter would have the right to elect local members, which would have full vote on all local matters, but have novote on general mattters, and that at the time of consolidation, the members of the local organization who are not members of the INSTITUTE, become local members.

In Lynn out of 70 or 80 members of the local organization there are about ten or fifteen members of the INSTITUTE, and the cause for this small percentage is that the tendency of the local organization is about the same on a smaller scale as that of the INSTITUTE. Most of the electrical engineers there think it their duty to build up the local organization, and they do not care to be members of the two organizations, and therefore do not become members of the INSTITUTE, but now that it has reached such importance, they would like to be members of it without giving up their local organization.

Furthermore I think there would be no difficulty in having papers read before the local organization printed in the TRANSACTIONS under the same conditions as they are now printed, *i. e.* that the author of a paper sends it in for acceptance a sufficient time before the meeting to have the advance copies printed, or in other words that members of the INSTITUTE have the right to read their paper before the local branch, instead of before the general organization; or where it is desirable, to read the paper personally at the one, and by proxy at the other meeting. I would like to hear from the members what their opinion would be upon such a proposition.

MR. HAMMER:—I would suggest that, as this same question has recently come up in connection with the city of Cincinnati, the city of Philadelphia, and the city of Chicago, the Thomson Scientific Club be requested to send a report, embodying their views, to the Council, and have it taken up in connection with these other applications, and then have the Council submit the matter for the approval of the INSTITUTE.

THE PRESIDENT:—I think that is a very sensible suggestion.

THE SECRETARY:—As I told the gentlemen at Chicago, we could not force them to hold meetings, and we could not prevent their holding meetings if they wished to. We have the opinions of single individuals as to what they thought they could do. But we have not as yet had any united request from any number of members that would signify that they wished to hold meetings. In Lynn we have about twenty members. The proper procedure would be for them to draw up a proposition, signed by enough names to give it weight and then send it to the Council, when it can be taken up and considered.

MR. STEINMETZ:—I would not recommend the INSTITUTE to take any action at present. My object was first to present the matter and see how its members would regard such a proposition.

[Adjourned.]

EMERY ON COST OF STEAM AND WATER POWER.

[See page 119, *ante.*]

[COMMUNICATED DISCUSSION BY L. B. STILLWELL.]

I have read Dr. Emery's paper on the cost of steam power, with supplement relating to water power, with great interest. The figures which he presents—coming from a man of such undoubted ability and wide experience—are of the utmost value to all who are interested in the cost of power, and to none are they of greater interest than to those who at the present time are called upon to advise with reference to the installation of plants for the utilization of water power by electric transmission.

The tables which Dr. Emery has prepared, representing results obtainable in approved steam engineering practice, supply the engineer who is designing an hydraulic and electric transmission plant with data concerning the cost of operation of a possible competing steam plant, more complete and more carefully analyzed, perhaps, than any that had been published up to the time when Dr. Emery's paper was read. It goes without saying that no transmission plant should be installed without a careful comparison of the annual charges against such a plant with the annual charges against a steam plant located where the motors are to be located, and doing the same work; but reliable information regarding the proper annual charges against such a steam plant have been very difficult to procure. I am sure that Dr. Emery's paper will be frequently consulted by those who may have occasion to consider proposed installations for the transmission of power.

In presenting a paper so comprehensive in scope, and dealing with a subject so complex, Dr. Emery has made no attempt to be perfectly general, and has confined himself within the limits of average conditions as regards cost of fuel, wages, interest upon investment, etc. For example, he makes no calculations based upon the use of coal costing more than $5 per ton, nor less than $1 per ton. There are, of course, many places—especially in the west—where good coal cannot be purchased for $5 per ton; and, on the other hand, there are perhaps, other localities where a fair quality can be procured for less than $1 per ton. The limits which he has assumed, however, are fairly stated, and the method is so clearly indicated that corrections for peculiar local conditions can be readily made.

As a starting point in the preparation of his paper Dr. Emery has assumed a unit of 500 H. P. delivered at a speed of 250 to 350 R. P. M. "corresponding to the jack-shaft speed of slow engines. "and the actual speed of high speed engines," and in his discussion of the subject of water power he still keeps the jack-shaft in view. Of course a comparison such as this, based upon a special kind of service, and limited to large units, altogether leaves out of consideration some of the most marked advantages of electric transmission. It does not apply in the least to those cases in which transmission is combined with distribution of power; that is to say, where the energy is transmitted from water power across country to a secondary station, conveniently located as regards the market, and thence distributed to motors, large and small, supplying power for the manifold work of a city or town. It applies only to those cases where we have transmission "in block," and even in these, the conditions assumed are such as impose serious disadvantage upon the plant utilizing water power by means of electricity.

It is, I think, pertinent to the discussion of Dr. Emery's paper to point out briefly wherein the transmission plant will in general compare much more favorably with the steam plant than would be inferred from a casual reading of the supplement wherein he touches upon the subject.

Dr. Emery finds that at Lawrence the general hydraulic plant, exclusive of turbines and hydraulic connections from the high canals to the lower levels, has cost about $77 per horse power, and that the expense at Lowell has been even greater. He states that in addition to this, the mill owners have expended for turbines and connections an average of $65 per horse power for the average power utilized. This makes the total cost of developing water power on the Merrimac about $142 per horse power, which Dr. Emery says "is about the limit of cost at which water power "can be developed in competition with steam." He estimates the annual charges upon such an hydraulic plant as follows:

Depreciation	2½ %
Repairs	1½ "
Taxes	1½ "
Operating expenses	2 "
Interest and dividends	10 "
Total	17 %

Seventeen per cent. upon an investment of $140 represents $23.80 per horse power, or "about the same as shown in the tables "with economical engines and coal between $2 and $3 per ton." In this way he arrives at the conclusion that "the highest allow-"able cost for the complete development of water power from "the dam to the jack-shaft appears to be about $140 per horse "power, utilized on a ten-hour basis," and a little farther on he says:

"When the power of a waterfall is to be delivered at a "distance, the allowable cost of actually developing the power "must be decreased by that necessary to transmit the power and "actually deliver it to a jack-shaft at a given distance. An "electric transmission is undoubtedly the most economical for "such a purpose. If we add to the cost of the electric dynamos "that of the buildings, of the hydraulic connections to the canals, "of the turbines, of the line, and of the installation, and finally "add the cost of the motors, so that the power is, according to "the assumption, delivered to a jack-shaft, the total cost of what "may be called the 'electrical transmission' cannot, probably, at "present prices, be put in for $140 for each net horse power "delivered; so on a ten-hour basis no expenditure could be "allowed for the general development of the water power, but "only for the simplest hydraulic connections to existing canals, "etc."

Installations for transmission of power by electricity may, for convenience of analysis, be divided into two classes:

First:—Installations for transmission in block, in which the energy is delivered to a single point, and there utilized.

Second:—Installations for transmission and distribution, in which the energy is transmitted to a secondary station conveniently located with reference to the market for power, and thence distributed to motors of various capacities located at many different points. In both classes it is usually convenient and profitable to combine with the power service, a certain amount of lighting service.

Dr. Emery's comparison is made with a case falling within the limits of the first class, as above defined, and his conclusion amounts to this, namely: "*Where 500 horse power is to be delivered to a jack-shaft for ten hours per day, and where this power cannot be utilized during the remaining fourteen hours out of twenty-four, an investment in hydraulic and electric plant not exceeding $140 for each net horse power is allowable.*

In view of the conditions assumed in the comparison this is, I think, most encouraging for the future of electric transmission. In very many places where water power is available—if not on the Merrimac—an actual investment of $65 per horse power will easily cover the cost of the hydraulic installation, and, indeed, throughout many parts of the west, a transmission installation, starting with an investment of $65 per horse power in hydraulic plant alone, would be thought to carry a not inconsiderable handicap. When compelled to compete with engines using about 20 lbs. of feed water per indicated horse power per hour, and with boilers that evaporate 8.5 lbs. of water per pound of coal, when using coal worth about $2 per long ton, such a plant meets steam, if not at its best, at least entrenched behind very favorable conditions. When, in addition to all this, the service is limited to the operation of a line shaft during but ten hours out of the twenty-four, steam should be able to make a good showing, if ever. It is, therefore, I think, most encouraging to those interested in the possibilities of electric transmission from water power, that Dr. Emery finds that an hydraulic and electric plant complete, even under the conditions assumed, can cost as much as $140 per horse power delivered, without making the annual cost of such power exceed the cost of steam power. Dr. Emery remarks that if power can be sold throughout the whole twenty-four hours, the investment in the transmission plant may be doubled, and upon this point it should be noted that the chances of selling power during at least a part of the fourteen hours per day during which the line shaft is assumed to be idle are usually far greater in the case of the transmission plant than in the case of the steam plant. It is the peculiar advantage of electric transmission that the power is delivered at the end of the transmission circuit in form available for the most convenient and efficient distribution for both lighting and power purposes. After the line shaft is shut down it should often, if not always, be possible to utilize the electric energy which has been used to drive the motors by employing it to supply lighting circuits.

And now let us see how Dr. Emery's deductions must be modified, if we are considering the second of the above-defined classes for electric transmission.

Of the $140 constituting the limit of investment in hydraulic and electric machinery for ten-hour service, Dr. Emery charges against the hydraulic plant $65, and against the electric plant $75. We may, with sufficient approximation to accuracy, further divide this as follows:

Investment in hydraulic machinery............	$65	per H. P.
ELECTRIC PLANT.		
Generators..................................	25	" "
Transmission circuits, including step-up and step-down transformers, if any............	25	" "
Motors.......................................	25	" "

It is obvious in the case of the transmission plant we shall not have to purchase motors, for use in the secondary or sub-station, as the energy delivered from the transmission circuit, or from the secondary terminals of step-down transformers, if these are used, is ready for distribution, and we are not called upon to operate a line shaft at the sub-station from which the local circuits radiate. On the other hand, a competing installation, using steam as the source of power must purchase electric generators to put the energy which it develops into form suitable for distribution. As compared with transmission in block, therefore, the second and far more general class of transmission plants saves $25 per horse power, because motors at the sub-station are not necessary, while the steam plant requires an additional investment of $25 per horse power, for the reason that, in order to compete, it must add electric generators to its steam equipment. The net advantage in favor of the transmission plant is, therefore, roughly, $50 per horse power. Following out the line of Dr. Emery's reasoning we may say, then, that in a case in which power is to be distributed in comparatively small units to a number of points it will be good economy to invest $65 + $50

= $115 per horse power in hydraulic apparatus, and in the development of the water power, even in those cases in which the service is limited to ten hours per day.

When the power, or a considerable part of it, can be utilized during twenty-four hours per day, instead of ten, the allowable investment in the hydraulic and electric machinery required by a transmission plant, as calculated from Dr. Emery's premises, is, as he points out, very largely increased, and it would not be difficult to find very many instances in which the figures which he assumes for operating expenses, interest, dividends, etc., would point to the conclusion that it would be good economy to invest several hundreds of dollars per horse power in hydraulic apparatus alone.

Parkhurst on Galvanometers.

[See page 270, *ante*.]

REPLY TO DISCUSSION, COMMUNICATED BY THE AUTHOR AFTER ADJOURNMENT.

Though hardly necessary perhaps, I still desire to add a few words to the discussion upon my paper upon "A Modified D'Arsonval Galvanometer," as I would probably have had that privilege had I have been present at the meeting.

Referring to the remarks of Mr. Willyoung, I would say that I *have* balanced resistances as stated by me on page 279, and that there was no mistake about it. I have done the same thing in constructing resistance coils, using one of the largest and best forms of Edelman reflecting galvanometer as the indicator, and have balanced down to absolutely no reflection. This may be extreme accuracy and not ordinarily required; but *I wanted to see if it could be done. and did it*, though great care as to temperature, etc., had to be taken. I have frequently seen the case where *just a change of tension* in the wire being measured would throw out the balance that had been previously obtained, showing one of the many factors that must be considered in balancing resistance for extreme accuracy.

I intended to insinuate nothing; but I now do say that, judging from their products, many instrument makers do *not* understand what *good* instruments are, that is, good in *all the mechanical detail*, for I have seen many that no one could repair if out of order; they had to go back to the maker for some simple thing, and it looked as though the maker had purposely made the instrument in that manner so as to control the repairs. I do not mean to say that anyone and everyone should try to repair every instrument, but when one has an instrument with a fibre suspension, that suspension is liable to be broken, and one would like to be able to put in a fibre with the least trouble in the way of dismounting, etc., and in the simplest manner. Yet I have seen instruments of high first cost in which no one but an expert could replace a broken suspension, and even the expert needed two pairs of hands and the patience of Job to succeed.

The particular form of D'Arsonval instrument, shown by Mr. Willyoung, is new to me in some of its details, and I am obliged to him for its introduction.

The copper coil frame was used by myself and a friend of mine in some instruments that we made in 1886, but neither of us thought it worth while to try to secure patents.

The first D'Arsonval instrument I made was a copy of the Carpentier instrument referred to, and was made in 1886-7. It was not entirely satisfactory as first made, and improvements were made from time to time, finally resulting in the instrument described, as I could find no one that made and sold the instrument I wanted. I did not, and do not, intend to claim any originality for the principle of the instrument described, for I well knew that it had been invented and made use of years before the date of my article.

As to the data on instruments, I may have been fortunate in the foreign instruments I have seen, and unfortunate in those of American make, for I merely recorded the results of my observation, and have no reason as yet from observation to change my opinion. Some of the German instruments are so made that one can see for himself the number of turns of wire and count them, and the radius is given or can be measured; on others the complete data was given. But nevertheless I am glad to know that there are American-made instruments that do contain all the data necessary, carefully measured and recorded. As I referred to tangent galvanometers alone, the fact (which I already knew) that resistance boxes are carefully and properly made and certified to has no bearing on the case.

As to the Weston instrument, no one admires it or appreciates its full merits more than I do. At the time I made my first instrument with fibre suspension, I tried to make the same form of pivot support with spiral springs to control the coil and allow the entrance and exit of the current, but had to give it up for want of proper springs, as steel hair springs would not answer my purpose, and I could then get no other.

There must be some mistake as to scale of the illustration, for the model I have constructed, made from the original drawing, has a bore for the polar space exactly 1½-inch diameter, and that is not "very nearly two inches," as I make it out. "If at first we don't succeed, then try, try again," is my motto in such a case, and if I find the coil too heavy then I certainly will throw it away and "start all over again," as I have done many times before, and if I can find any better scheme for supporting the mirror (I already have one in mind and yet have not seen the instrument shown), I certainly will try it.

I have to thank the gentlemen for their remarks. It is only by discussion that we learn other's ideas, and I seem to have succeeded in the accomplishment of one object, *i.e.*, causing some attention and discussion upon some points that I thought of interest and that would be elucidated thereby.

Associate members elected at Council Meeting, June 6th, 1893.

Name.	Address.	Endorsed by
BARTLETT, EDWARD E.	Member of Firm of Bartlett & Co., 23 Rose Street, New York City.	W. J. Hammer. T. C. Martin. Nikola Tesla.
BREITHAUPT, E. CARL	Electrical Engineer, Berlin, Ontario, Canada.	Louis Duncan. G. A. Liebig, Jr. H. S. Hering.
CRAIGIN, HENRY A.	Engineer, Westinghouse Electric and Mfg. Co., 15 Charles St., Boston, Mass.	Chas. R. Cross. Giles Taintor. Harold Binney.
d'ROMTRA, ARTHUR	Electrical Engineer, H. Ward Leonard & Co., 216 West 44th St., New York City.	W. J. Hammer. Jos. Wetzler. Ralph W. Pope.
EMMET, W. L. R.	Electrical Engineer, General Electric Co., 44 Broad St., New York City.	D. C. Jackson. H. F. Parshall. Gilbert Wilkes.
FREY, CHARLES P.	Electrician, The E. S. Greeley & Co. Laboratory, Greenbush, N. Y.	T. J. Smith. G. A. Hamilton. Ralph W. Pope.
KEITH, NATHANIEL S.	Electrical Engineer, Electrical Engineering Co., 711 Jones St., San Francisco, Cal.	Ralph W. Pope. W.F C. Hasson. Geo. P. Low.
LAWTON, W. C.	Electrician in Motor Department, Edison Electric Illuminating Co., 808 Greene Ave., Brooklyn, N. Y.	W. J. Hammer, T. C. Martin. Ralph W. Pope.
LANMAN, WILLIAM H.	General Electric Co., 44 Broad Street, New York City.	W. J. Jenks. W. J. Hammer. R. T. Lozier.
MANN, FRANCIS PETTIT	Electrician, Westinghouse Electric & Mfg. Co., Pittsburgh, Pa.	W. D. Weaver. C. O. Mailloux. W. J. Hammer.
ROPER, DENNEY W.	Student, Cornell University, Ithaca, N. Y.; Residence, 414 Langdon St., Alton, Ill.	H. J. Ryan. E. L. Nichols. Ernest Merritt.
WAYLAND-SMITH, F.	Resident Agent, Syracuse Steel Foundry Co., 26 Cortlandt St., New York City.	Gano S. Dunn. C. O. Mailloux. Ralph W. Pope.

Total 12.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

NEW YORK, October 18th, 1893.

The 80th 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:—I have the following announcements to make. At the meeting of Council this afternoon the following associate members were elected:

Name.	Address.	Endorsed by
BENTLEY, MERTON H.	Chicago Telephone Co., 221 Scoville Ave., Oak Park, Ill.	A. V. Abbott. A. S. Hibbard. C. K. MacFadden.
FLEMING, RICHARD	Supt. and Electrician, Rockford Electric Mfg. Co., Rockford, Ill.	W. A. Kreidler. F. B. Badt. A. E. Braddell.
McCROSSAN, J. A.	Manager and Electrician, Citizens' Telephone and Electric Co., Rat Portage, Ontario.	Norman N. Ross. Ralph W. Pope. G. A. Hamilton.
OSBORNE, LOYALL ALLEN	Engineer and Electrician, Westinghouse Electric and Mfg. Co., 29 Plane St., Newark, N. J.	Philip A. Lange. F. N. Waterman. Charles A. Terry.
ROBB, RUSSELL	Electrical Engineer, with Stone & Webster, 4 Post Office Sq., Boston, Mass.	C. A. Stone. E. S. Webster. Theo. Spencer.
SYKES, HENRY H.	Assistant Electrician, American Telephone and Telegraph Co., 75 Hicks St., Brooklyn, N. Y.	F. A. Pickernell. L. Stieringer. Thos. D. Lockwood.

Total 6.

The Council also, this afternoon, authorized the following change in the plan of announcing the election of associate members. That is, the names which are handed in to the Secretary will be printed in the TRANSACTIONS one month in advance of action by Council, so that if any objection be raised to proposed candidates, members will have a month to file that objection with the Secretary. I have to announce, in accordance with this

action, the following names which will be printed in the next issue of the TRANSACTIONS, together with such names as may be handed in hereafter:

Name.	Address.	Endorsed by.
ETHERIDGE, E. L.	Inspector, Electrical Engineering Dept., World's Columbian Exposition, 66 No. Oxford Street, Brooklyn, N. Y.	L. S. Boggs. O. G. Dodge. Wm. H. Cothren.
LINDNER, CHAS. T.	Inspector, Electrical Engineering Dept., World's Columbian Exposition, Tacoma, Washington.	L. S. Boggs. O. G. Dodge. W. H. Cothren.
WILLIAMS, FRANK A.	Safety Insulated Wire and Cable Co., 25 Washington Avenue, Newark, N. J.	W. J. Hammer. F. R. Upton. W. T. M. Mottram.
MOORE, D. MCFARLAN,	Electrical Engineer, General Electric Co., 44 Broad Street, New York City.	T. C. Martin. Jos. Wetzler. Edwin J. Houston.
MANSFIELD, ARTHUR NEWHALL	Assistant Electrician, American Telephone and Teleg. Co., 153 Cedar St., New York City.	F. A. Pickernell. Chas. R. Cross. G. A. Hamilton.
STURTEVANT, CHARLES L.	Patent Attorney, Atlantic Building, Washington, D. C.	W. A. Rosenbaum. Townsend Wolcott. F. L. Freeman.
PUFFER, WM. L.	Assistant Professor of Electrical Engineering, Mass. Institute of Technology, Boston, Mass.	Chas. R. Cross. H. V. Hayes. Geo. W. Blodgett.
COMSTOCK, LOUIS K.	Contracting and Consulting Engineer, Monadnock Building, Chicago, Ill.	Frank J. Sprague. C. T. Hutchinson. Geo. P. Low.
MUSTIN, HERBERT S.	Assistant Electrician, City of Hoboken, Police Headquarters, Hoboken, N. J.	Edw. Durant. James Hamblet. J. P. Wintringham.
BROWN, HAROLD P.	Consulting Electrical Engineer, General Electric Co., 44 Broad St., New York City.	W. J. Hammer. W. J. Jenks. A. E. Kennelly.
WARNER, CHAS. H.	Consulting Electrical Engineer, 50 Broadway, New York City.	W. J. Hammer. H. A. Foster. Edwin J. Houston.
MCCLURG, W. A.	Manager, Electrical Dept., Plainfield Gas and Electric Light Co., 25 Madison Ave, Plainfield, N.J.	R. W. Pope. E. A. Merrill. W. M. Miner.
ROBERSON, OLIVER R.	Electrician, Western Uuion Telegraph Co., 195 Broadway, P. O. Box 3393, New York City.	James Hamblet. G. W. Gardanier. Alfred S. Brown.
WASON, LEONARD C.	Head Draughtsman with F. S. Pearson, 199 Harvard Street, Brookline, Mass.	J. P. B. Fiske. Chas. R. Cross. F. S. Pearson.
SAHULKA, DR. JOHANN	Docent of Electrotechnics, Technische Hochschule, Vienna, Austria.	Ralph W. Pope. N. S. Keith. Townsend Wolcott.
STEVENS, W. LE CONTE,	Professor of Physics, Rensselaer Polytechnic Institute, Troy, N. Y.	Samuel Sheldon. Edw. L. Nichols. James Hamblet.
SHEA, DANIEL W.	Assistant Professor of Electrical Engineering and Physics, University of Ill., Champaign, Ill.	Samuel Sheldon. H. V. Hayes. Dugald C. Jackson.

McKAY, C. R.	Consulting Engineer, 140 South Main Street, Salt Lake City, Utah.	Louis Duncan. Samuel Reber. H. S. Hering.
CAPUCCIO, MARIO	Electrical Engineer, Piazza Statuto 15, Corino, Italy.	T. C. Martin. Ralph W. Pope. G. S. Albanese.
HUDSON, JOHN E.	President, The American Bell Telephone Co., 125 Milk Street, Boston, Mass.	H. V. Hayes. Francis Blake. I. H. Farnham.
SAGE, HENRY JUDSON	Electrical Engineer, Telephone Dept., Western Electric Co., 227 S. Clinton St., Chicago, Ill.	Chas. R. Cross. E. M. Barton. P. H. Alexander.
MARTIN, WILLIAM B.	Supt. of Construction, Western Electric Co., 393 Pleasant Ave., New York City.	J. Stanford Brown.
FROST, FRANCIS R.	Assistant in Electrical Testing, Bureau of Awards, World's Fair, Ithaca, N. Y.	R. B. Owens. B. F. Thomas. Chas. E. Emery.
SERVA, A. A.	Assistant, Bureau of Awards, World's Fair, North Industry, Ohio.	R. B. Owens. B. F. Thomas. Chas. E. Emery.
REQUIER, A. MARCEL	Electrical Engineer, Westinghouse Electric and Manufacturing Co. Pittsburg, Pa.	Chas. F. Scott. F. Stuart Smith.
JAEGER, CHARLES L.	Inventor, Maywood, N. J.	J. N. Johnson. Ralph W. Pope. Chas. E. Dressler.
REDFIELD, GEORGE W.	Student in Electrical Science, Galesburg, Ill.	R. W. Pope. Fred. DeLand. Wm. D. Ray.
McCROSKY, JAMES W.	Graduate Student, Johns Hopkins University, 1104 McCulloh St., Baltimore, Md.	R. B. Owens. W. E. Shepard. H. A. Foster.
COREY, FRED. B.	Electrical Engineer, A. B. See Manufacturing Co. 442 Henry St., Brooklyn, N. Y.	H. H. Eustis. Geo. D. Shepardson. Alonzo B. See.
NORTON, ELBERT F.,	Inspector, City Electrical Inspection, 15 City Hall, Chicago, Ill.	C. C. Haskins, C. G. Armstrong. Alex. Dow.

Total 30.

Members are requested to scrutinize this list carefully, and promptly notify the Secretary if there be any objection to their election.

Report of Meeting of Board of Examiners, October 3d, 1893.

Present—Messrs. W. B. Vansize, *Chairman*; E. T. Birdsall. G. A Hamilton and E. P. Thompson; R. W. Pope, Secretary, present *ex officio*.

	Approved.	Disapproved.	Laid over for further consideration.	Total Co·.sidered.
Applications for Transfer Reconsidered.	3	2	—	5
New Applications for Transfer Considered.	10	3	13	26
Totals.	13	5	13	31

THE PRESIDENT:—If the INSTITUTE will pardon me, I would like to bring up, somewhat out of place, an important matter which will take but a moment. At the last meeting I made certain recommendations in my inaugural address concerning work which it was proposed that the INSTITUTE should undertake, some few matters which were left incompleted by the late Electrical Congress. The committee appointed for this purpose, now offers the following provisional report. I have been asked to read it, as chairman of the committee.

"*Report of the Committee appointed by the* INSTITUTE *to consider certain matters of general importance in electrical engineering that have incidentally been left undetermined by the recent Electrical Congress at Chicago.*"

"Your committee considers that there are four subjects of sufficient importance to call for the consideration of the INSTITUTE."

"1. The search for an accurately reliable concrete standard of light."

"2. The establishment of a unit and standard of illumination."

"3. The recommendation of certain practical magnetic units."

"4. The establishment of certain precedents in regard to nomenclature."

"Your committee recommends that an appeal be made to the leading universities and colleges of the country, possessing electrical laboratories, for aid in dealing with the first two considerations regarding illumination."

"The last two considerations are recommended for relegation to the regular committee of the INSTITUTE on units and standards."

EDWIN J. HOUSTON, Chairman.
WILLIAM E. GEYER,
A. E. KENNELLY,
CARL HERING.

DR. EMERY:—I move that the report be accepted and that the action recommended be taken.

[The motion was carried.]

THE PRESIDENT:—Gentlemen, I would like to say a few words. I trust the INSTITUTE will do all that it can to further work of this character. I feel that in undertaking it the INSTITUTE is entering upon an era of usefulness greater even than the marked usefulness it has shown in the past. We certainly have in the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS an exceedingly able body of men. We are competent to take up questions of this character and thoroughly deal with them. The Council urges on each and all of you the importance of this work. As to whether or not it will be successful depends of course entirely on the amount of intelligent effort which is put into it. If we all pull together and work in this matter we can do a great deal of good to that science of which we are so fond. It has been determined to make this distinctively INSTITUTE work, so that nobody will be asked to take part in the work who is not a member of the INSTITUTE.

I would like to say on behalf of the committee that I represent, that it will be pleased to receive suggestions from any members of the INSTITUTE, either as regards the method of carrying on this work, or the names of the parties to whom it would be proper to relegate it. The work will be apportioned somewhat in this way: it is our idea to invite co-operation of every institute of learning, such as a college or university, or any institution that has a good working physical laboratory. Local sub-committees will be appointed to undertake experimental work to determine these quantities.

I take great pleasure in introducing to you Dr. Frederick Bedell, who will read a paper on "Hedgehog Transformer and Condensers."

A paper presented at the Eightieth Meeting of the American Institute of Electrical Engineers, New York, October 18th, 1893. President Houston in the chair.

HEDGEHOG TRANSFORMER AND CONDENSERS.

BY FREDERICK BEDELL, K. B. MILLER AND G. F. WAGNER.

The transformer is an instrument which still continues to attract the attention of the scientist and of the engineer, and no method for its investigation and study is more interesting than the experimental method whereby the instantaneous changes in the periodically varying quantities are made known. We refer to the method of instantaneous contact. The transformer with an open magnetic circuit has been the subject of much controversy, and it is upon such a transformer that the following experiments were made. We have no intention, however, of reviving the question of "open *versus* closed magnetic circuit transformers," which received so much attention a few years ago.

In this investigation a modification of the method of instantaneous contact was applied to the study of an open-magnetic-circuit transformer known as the "Hedgehog," experiments being made, first with the transformer under usual conditions, and then with condensers connected in parallel with the primary.

A large part of the credit for this work should be given to Prof. Ryan, who was interested in it from the start, and to whom we are indebted for valuable assistance throughout the investigation.

For convenience, we will divide the paper into three parts, describing: first, the apparatus and methods of measurement; second, the experiments with the transformer alone under the ordinary conditions of working on a 1,000-volt circuit; and third, the experiment in which condensers were shunted around the primary of the transformer.

PART I.

APPARATUS AND METHOD EMPLOYED.

For the complete analysis of alternating current phenomena, we should know not only the value of each changing quantity at every part of its period, but we should know the phase relations between the several varying quantities; that is, the relations between their respective zero and maximum values. To enable us to do this, the method of instantaneous contact has come into use, in which a revolving contact is made at a particular part of the period in such a way that we may ascertain the value at that particular instant of any of the varying quantities measured. This method is of historical as well as scientific interest, inasmuch as it was, originally devised simultaneously on each side of the Atlantic, and has since been modified and developed by many investigators.

An interesting account of the development of the method of instantaneous contact is given by Dr. Nichols in his address, as Vice-President, before the physical section of the American Association for the Adancement of Science, upon "The Phenomena of the Time-infinitesimal,"[1] and a brief review may now be in place, previous to the description of the present modification.

In the year 1880, Joubert[2] made use of the device in his study of the changes in potential of an alternating current dynamo and between the terminals of the Jablochkoff candle, and pointed out the use of the quadrant electrometer in alternating current measurement. In the same year B. F. Thomas, in this country, devised the method independently, and made use of a condenser and ballistic galvanometer. His paper before the American Association for the Advancement of Science[3] that year was published by title only, and his experiments were unpublished until presented, by request, at a meeting of this INSTITUTE[4] last year.

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1. E. L. Nichols: *Proceedings Am. Assoc. for the Adv. of Sc.*, Madison Meeting, vol. xlii., 1893.

2 Joubert: "Sur les Courants alternatifs et la force electromotive de l'arc electrique." *Comptes Rendus*, 91, p. 161, July 19, 1880.

3. Henry Morton and B. F. Thomas: "Observations on the Electromotive Forces of the Brush Dynamo-electric Machine." *Proceedings A. A. A. S.*, vol. xxix., p. 277, 1880.

4. B. F. Thomas: "Notes on Wiping Contact Methods for Current and Potential Measurement." TRANSACTIONS, A. I. E. E., vol. ix., p. 263, 1892.

In 1888, the method was used by Duncan, Hutchinson and Wilkes,[1] who applied it to the study of induction coils and transformers, and obtained the first complete set of curves for this class of alternating current apparatus. In the same year it was used in France by Meylan,[2] in a study of the vibratory call-bell of Abdank, and at Stevens Institute, in an investigation of the Westinghouse Alternator by Searing and Hoffman.[3]

Then followed its use by various investigators, Ryan and Merritt,[4] Humphrey and Powell,[5] Tobey and Walbridge,[6] Marks,[7] Herschel,[8] Fortenbaugh and Sawyer,[9] all of whom used it in the study of alternating current phenomena and have communicated their results before this INSTITUTE. Subsequently the method has been employed for different lines of investigation by Archibald and Teeple,[10] Thompson,[11] Ryan,[12] Hopkinson,[13] and a modification has been used by Duncan[14] in which simultaneous curves are rapidly obtained by the use of several dynamometers.

This is the history of the method from its first use to the writing of this paper.

The features introduced in the method as employed in the present investigation are two; first, the use of a revolving contact-maker, in which the contact is made by a needle passing

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1. Duncan, Hutchinson and Wilkes: "Experiments on Induction Coils." *Electrical World*, vol. xi., p. 160, 1888.

2. Meylan: "Sur les Apples Magnetiques." *La Lumière Électrique*, vol. xxvii., p. 220, 1888.

3. Searing and Hoffman: "Variation of the Electromotive Force in the Armature of a Westinghouse Dynamo." *Journal of the Franklin Institute*, vol. 123, p. 93.

4. Ryan; "Transformers." TRANSACTIONS, vol. vii, p. 1. 1889.

5. Humphrey and Powell; "Efficiency of Transformers." TRANSACTIONS, vol. vii, p. 311.

6. Tobey and Walbridge; "Investigation of the Stanley Alternate-current Arc Dynamo." TRANSACTIONS, vol. vii, p. 367.

7. Marks; TRANSACTIONS, vol. vii, p. 324.

8. Herschel; TRANSACTIONS, vol. vii, p. 328.

9. Fortenbaugh and Sawyer; TRANSACTIONS, vol. vii, p. 334.

10. Nichols; "On Alternating Electric Arc between a Ball and a Point." *American Journal of Science*, vol. xli, p. 1.

11. M. E. Thompson; "Study of an Open-coil Arc Dynamo." TRANSACTIONS, vol. viii, p. 375.

12. Ryan; "Relation of the Air Gap and the Shape of the Poles to the Performance of Dynamo-electric Machines." TRANSACTIONS, vol. viii, p. 451.

13. Hopkinson; "Dynamo Machinery and Allied Subjects," p. 187.

14. Duncan; "Note of some Experiments with Alternating Currents." TRANSACTIONS, vol. ix, p. 179.

through a water-jet; and, second, the use of a condenser to displace the zero of an electrostatic potential instrument, so that readings are taken at the best portion of the scale.

A general view of the revolving contact-maker is given in Fig. 1, and a detailed view in Fig. 2. The whole instrument is supported by a stationary frame F. The shaft S is connected to the

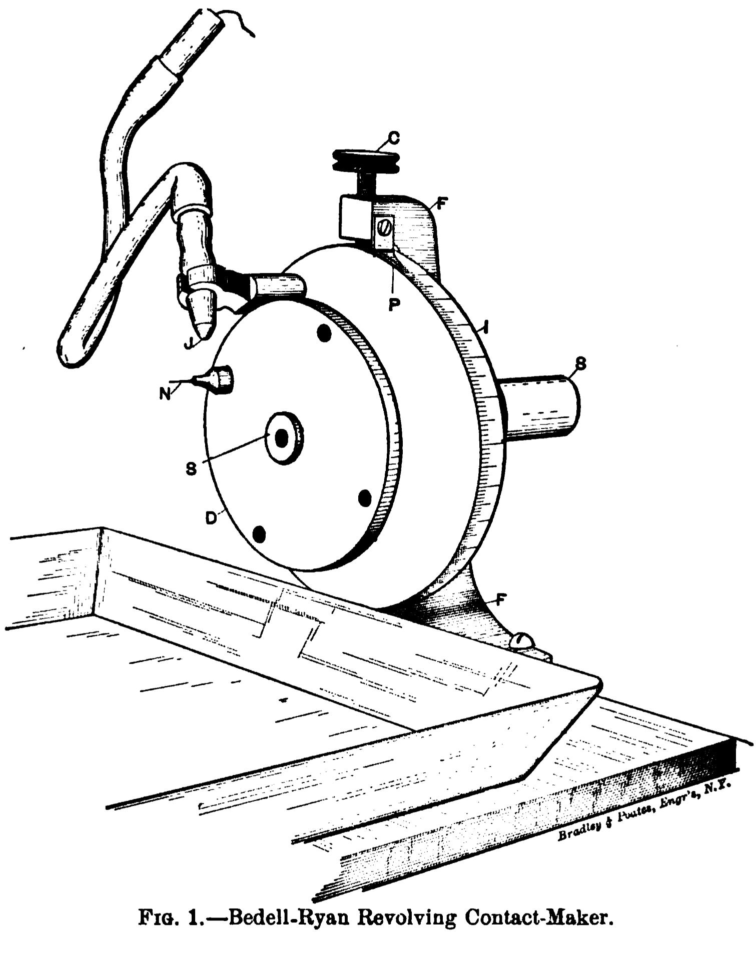


FIG. 1.—Bedell-Ryan Revolving Contact-Maker.

armature shaft of the dynamo by a coupling (not shown) on the end of the rod R, and carries the disk D, which revolves with it. The needle N projects from this disk and forms one of the electrodes of the contact. The other electrode is a fine water-jet (not shown) issuing from the nozzle J, well insulated by hard rubber from the rest of the instrument. This fine jet is maintained from a jar of water, several feet above, to which it is connected by a flexible rubber tube. Electrical connection is maintained with the water-jet by a wire w, which passes through this tube and is

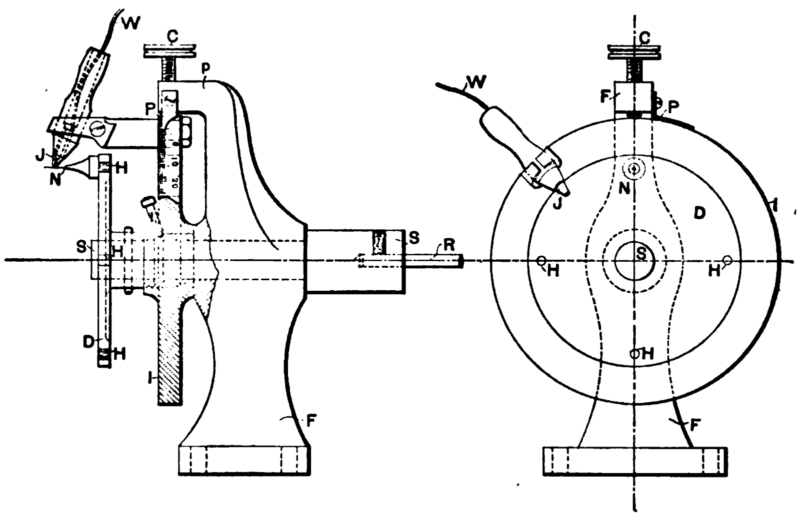


FIG. 2.

soldered to the nozzle J. Electrical connection with the needle-point electrode is obtained through the shaft and frame of the instrument. The needle cuts the water-jet once in every revolution of the armature of the dynamo and makes a contact which is well defined and unvarying. The nozzle of the water-jet is carried

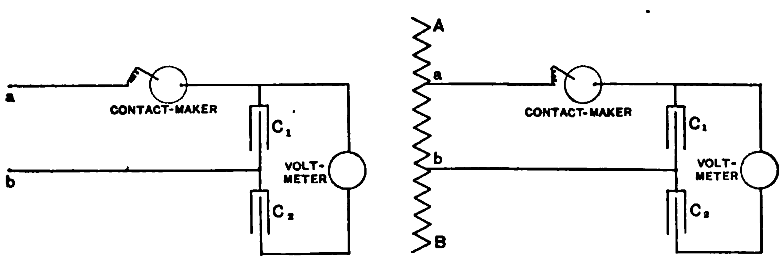


FIG. 3. FIG. 4.

upon an index-disk I, which can be turned into any position by revolving it upon a projecting collar of the frame, which forms its bearing. The water-jet is held in any position by securing the index-disk by means of the set-screw C in the top of the frame.

Its position is indicated by the pointer P, upon the scale on the circumference of the index-disk, which is graduated in degrees.

The needle cuts the water-jet very near the nozzle, at a point where the jet is quite stiff, due to the head of water. The nozzle is radial, and the jet keeps the direction of the nozzle for some little distance before being materially deviated by gravity. A little salt was put in the water to increase the conductivity of the jet. Pure water would not work; acidulated water corroded the nozzle, thus changing the jet.

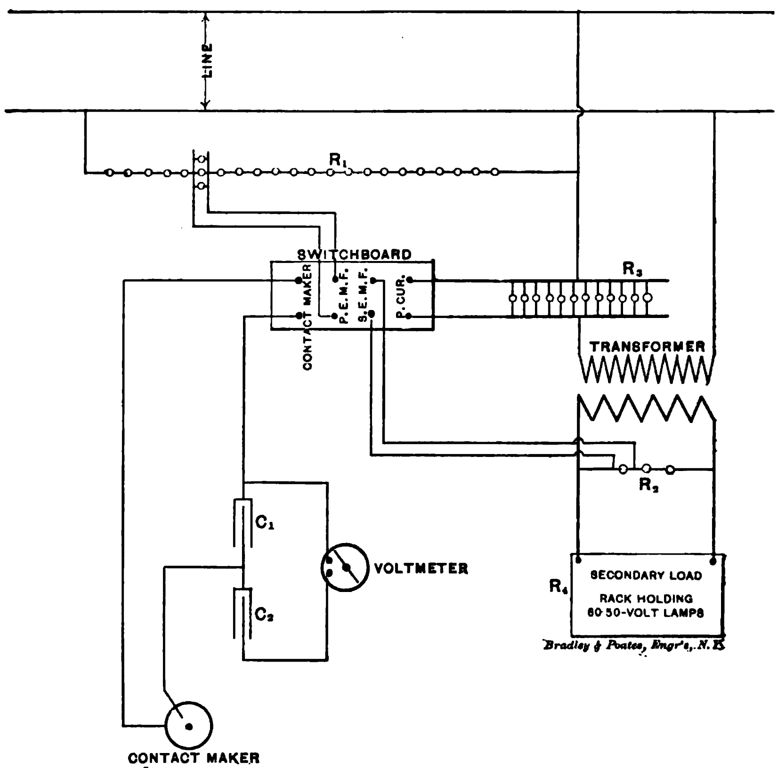


FIG. 5.—Diagram of Connections, Transformer Test.

It was after working for some time with various mechanical contact-makers, that this water-jet form was devised. It came up to our expectations in every respect, the contact being perfectly constant and reliable, and free from the changes always found in a mechanical device, due to the wearing away of the contacts. This constancy is particularly necessary in an instrument which is to be used in an extended investigation, during which any change would be fatal. By maintaining a fine, strong jet, for which a head of five or six feet is ample, and using a fine needle close to the nozzle, the instrument may be used with great precision and needs but little attention, running every night for weeks with scarcely any interruption. Of course, the accuracy of the instrument is increased as the diameters of the disks are made greater.

For use in another investigation, in which it was desired to obtain measurements for several consecutive cycles, without

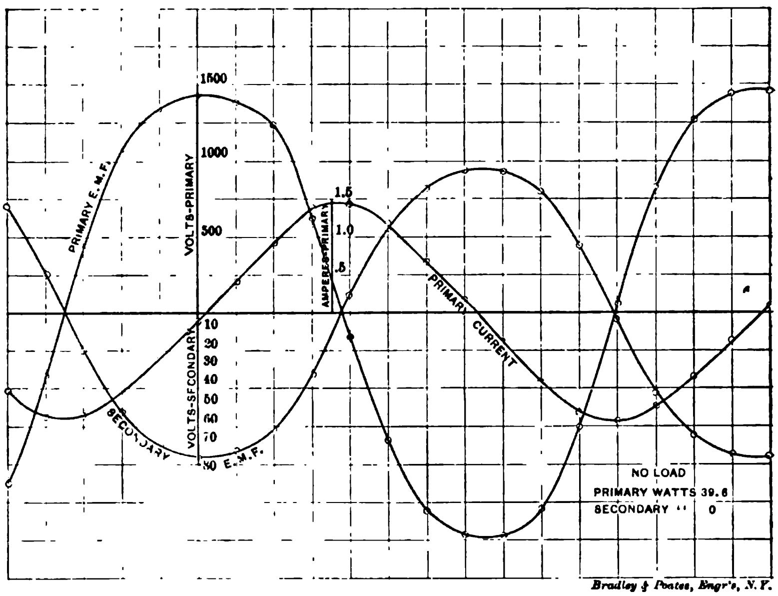


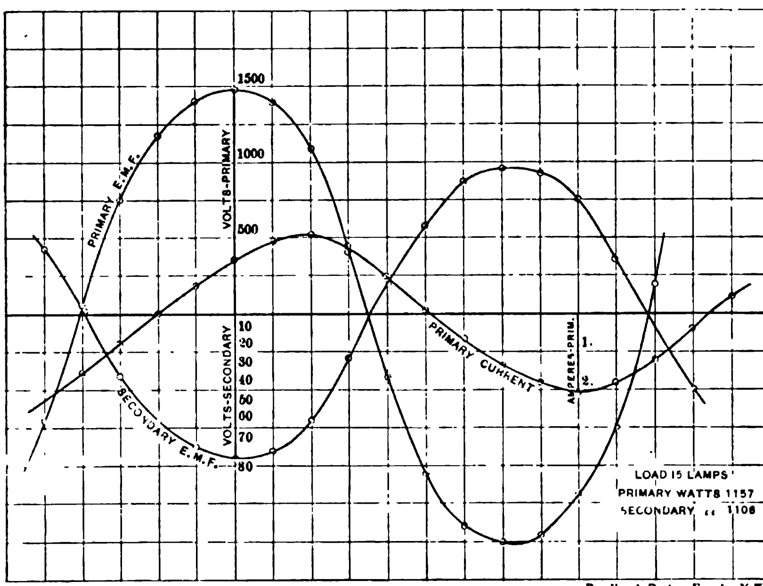
FIG. 6.

interruption, the instrument was made so that the needle could be secured in the disk D, by screwing it into any of the four holes H, thus making it possible to have the contact made with the armature in any desired position, without moving the nozzle more than forty-five degrees from the vertical.

This contact-maker was used with a Thomson Multicellular voltmeter, as shown in Fig. 3. The difference of potential between *a* and *b* is to be measured. The condenser C_1, is kept charged to this potential to be measured, being connected through the contact-maker. The voltmeter used reads between 40 and

120 volts. Its zero was displaced by the condenser c_2, in series with it, which was kept charged to about 80 volts; that is, the voltmeter reading indicated the sum of the potentials of the two condensers c and c_2. so that a reading of 80 volts indicated that there was no difference of potential between the terminals of condenser c_1, or between *a* and *b*; a reading of 85 volts indicated 5 volts difference of potential and so on.

When a difference of potential beyond the range of the instrument was to be read, the apparatus was arranged as in Fig. 4; for instance, suppose it was desired to obtain the difference of po-



Bradley & Poates, Engr's, N.Y.

FIG. 7.

tential between A and B. These points were connected by a non-inductive resistance, and the difference of potential was measured around a known portion, *a*, *b*, of this resistance, and the whole difference of potential between A and B calculated.

Measurement of current was obtained by the potential instrument in a similar way. A non-inductive resistance was inserted in the circuit, in which the current was to be measured, as in Fig. 4. From the curve representing the instantaneous values of E. M. F. around these lamps, the square root of the mean square value of the E. M. F. was determined. The lamps were previously calibrated by a continuous current and their resistance determined for any value of E. M. F. at their terminals. For an alternating current their resistance was ascertained from this calibration for the square root of the mean square value of the E. M. F. at their terminals and this value of their resistance was used to obtain the instantaneous values of the current from the instantaneous values of E. M. F. in accordance with Ohm's law.

The multicellular voltmeter is ordinarily a slow instrument to read. Readings were quickened by a pneumatic damping arrangement, devised by Professor Ryan, consisting of two rubber tubes, each connected at one end to the same rubber bulb, their other ends terminating in fine glass tubes leading down through the glass cover of the voltmeter. By pumping air from the bulb through one or the other of these tubes, a draught could be produced against the needle, so as to oppose its motion and bring it quickly to rest.

Part II.

Test of Transformer Alone.

The transformer upon which these experiments were made, is a 60-light, Hedgehog transformer, built on the general lines of an old-fashioned induction coil. A thin casting of gun-metal, cross-shaped in section, runs through its centre and supports the flanges of the spool. The four angles of this casting are filled with soft iron wires, running length-wise, thus forming a cylindrical core. These wires are considerably longer than the castings and are spread out at their free ends, thus making a partial return for the magnetic lines. Half the secondary turns are first wound around this core, then all the primary, and finally the other half of the secondary. The primary consists of 1426 turns of wire 0.072″ in diameter, arranged in twelve layers. It has a resistance of 2.748 ohms and weighs twenty-nine pounds. The secondary consists of seventy-three turns of 19/0.058 cable, in two layers. Its resistance is .0149 ohms, and weight 12.5 lbs. The transformer is designed for a primary potential of 1,000 volts, with a frequency of 130.

This transformer was supplied with current from a Westinghouse alternator with a frequency of 133. The potential was kept constant at 1,154 volts by varying the exciting current. The connections were as shown in Fig. 5. The primary E. M. F. was measured by measuring the fall of potential around a portion of the non-inductive resistance R_1, which consisted of a series of 50-volt incandescent lamps arranged between the mains, as shown. The secondary E. M. F. was similarly found, by measuring the fall of potential around one of the three lamps, R_2, placed in series across the secondary terminals. The primary current was found from the fall of potential around the incandescent lamps, R_3, in the primary circuit. The number of these lamps was so adjusted that the fall in potential would be in the range of the voltmeter. The lamps were carefully calibrated for current at different pressures by means of a direct current, and curves

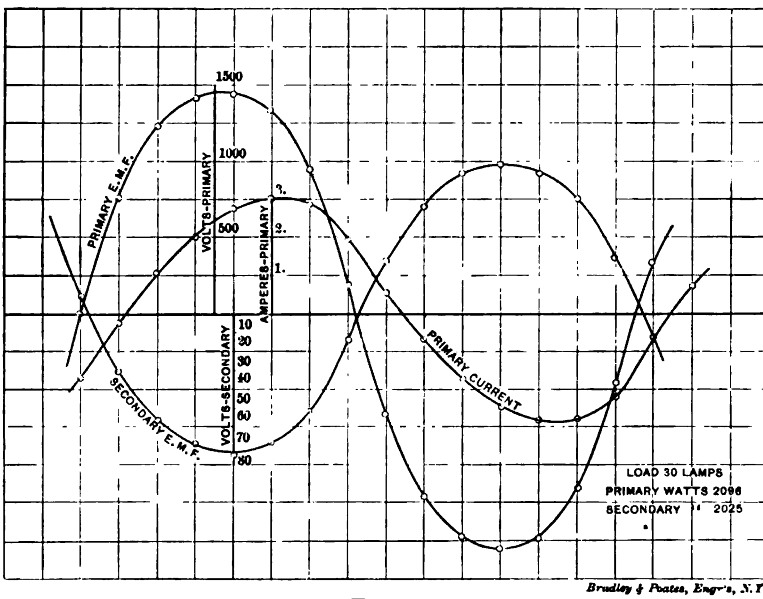


FIG. 8.

drawn, from which the current could be determined for any pressure.

It is evident that the drop through these lamps would diminish the total impressed E. M. F. of the primary, and a correction was accordingly made for this, by deducting from the instantaneous values of the total line E. M. F., the instantaneous values of the fall in potential around these lamps R_3 to obtain the actual impressed E. M. F. of the primary.

Wires were led from the terminals of the various resistances, around which measurements were taken, to a small hard-rubber switch-board above the voltmeter, by means of which connections were readily made for measuring the several E. M. F.'s. Instead of obtaining each complete curve independently, a reading corresponding to a certain position of the armature, was taken for each curve, without changing the position of the contact-maker. The water-jet was then moved to a new position, and readings again taken, one for each curve. This method prevents the relative displacement of the different curves.

The secondary load consisted of a non-inductive resistance, R, composed of sixty 50-volt incandescent lamps, mounted upon a

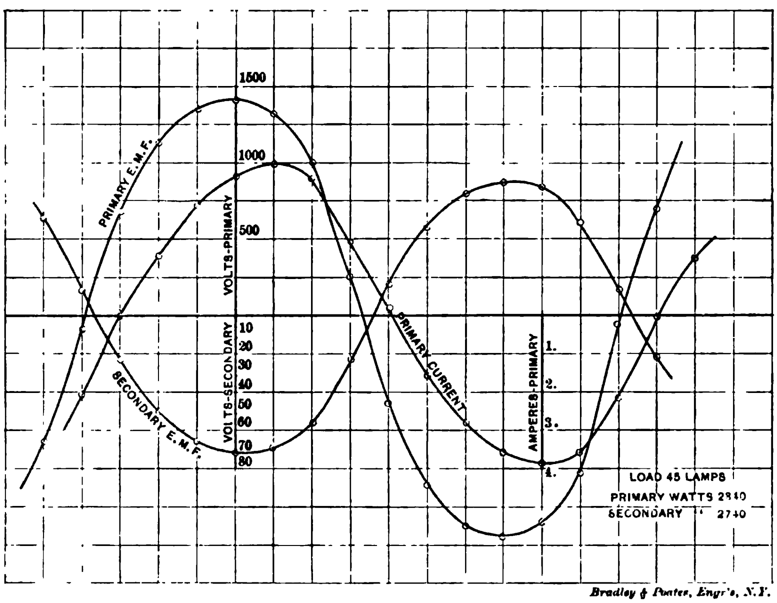


FIG. 9,

frame in such a manner that any number of lamps could be turned on or off. It was planned to measure the secondary current by means of a calibration of these lamps, but it was found on recalibrating them, at the end of the work, that their resistance had increased considerably, so this method had to be abandoned. The secondary current was finally determined by constructing a right triangle, the hypothenuse of which was equal to the primary current at the given load, and one side of which was equal to the primary current at no load. The third side, multiplied by the ratio of transformation, gave the secondary current.

To find the square root of the mean square value of the primary current, a curve was drawn, using the instantaneous values of the E. M. F. around the primary current lamps, R_3, as ordinates, and a second curve drawn, the ordinates of which were equal to the square of the corresponding ordinates of the first curve. The mean ordinate of this second curve was found by a planimeter, and its square root found. This represented the square root of the mean square of the difference of potential around the lamps. By reference to the calibration curve, the current could readily be determined. Although somewhat laborious, the method proved to be extremely accurate.

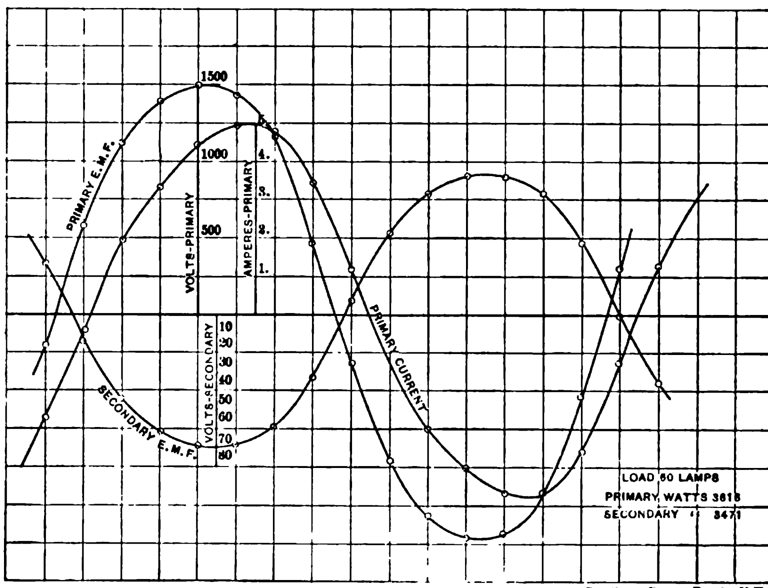


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FIG. 10.

The square root of the mean square value of the secondary E. M. F. was measured directly by the multicellular voltmeter.

Five separate runs were made, first for no load, with the secondary on open circuit, and then with loads of 15, 30, 45 and 60 fifty-volt incandescent lamps. With lamps of this resistance, the last run was quite beyond full load, inasmuch as the primary E. M. F. was 15 per cent. above that for which the transformer was intended. The results of these five runs are shown in Figs. 6 to 10.

At no load, the lag of the primary current, behind the primary E. M. F., is almost 90 degrees, which is greatly in excess of the lag shown by similar experiments upon a transformer with a closed magnetic circuit. This lag diminishes rapidly as the load increases, as seen in the successive curves, but it is always greater than for the corresponding load of a closed magnetic circuit transformer. This is on account of the large magnetizing current in the open-magnetic-circuit transformer, the effect of this component of the total primary current being quite marked, even at full load.

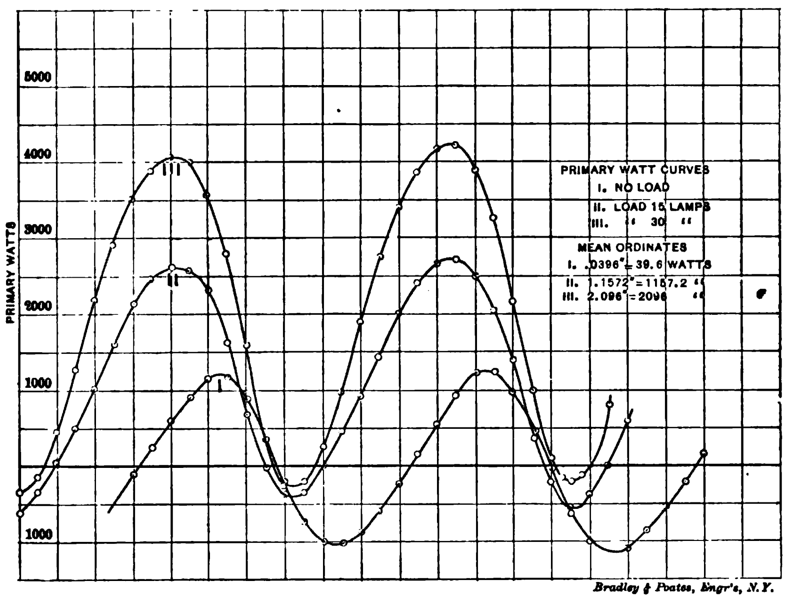

FIG. 11.

The secondary E. M. F. at no load, is almost opposite in phase to the primary E. M. F.; that is, it is almost 180 degrees behind it. As the load increases, a small but distinct increase in this lag is noticed, due largely to increased leakage.

The power supplied to the transformer was found by finding the products of corresponding instantaneous values of the primary E. M. F. and current. These products were positive or negative, according to whether the two factors were of the same or different sign. The products thus found were plotted as curves, showing the primary power, Figs. 11 and 12, positive values being plotted above the axis, and negative below. These areas were found by a planimeter, their algebraic sum taken, and the mean ordinate thus determined.

Although at no load the primary current is large, the fact that it lags so nearly 90 degrees behind the primary E. M. F. causes the energy expended to be small. This was found to be a total of 39.6 watts. Of this the primary copper losses, $R_1 I_1^2$, amounted to 2.58 watts, leaving 37.02 for the core losses,—hysteresis and eddy currents. If we assume the loss in the core to vary as the

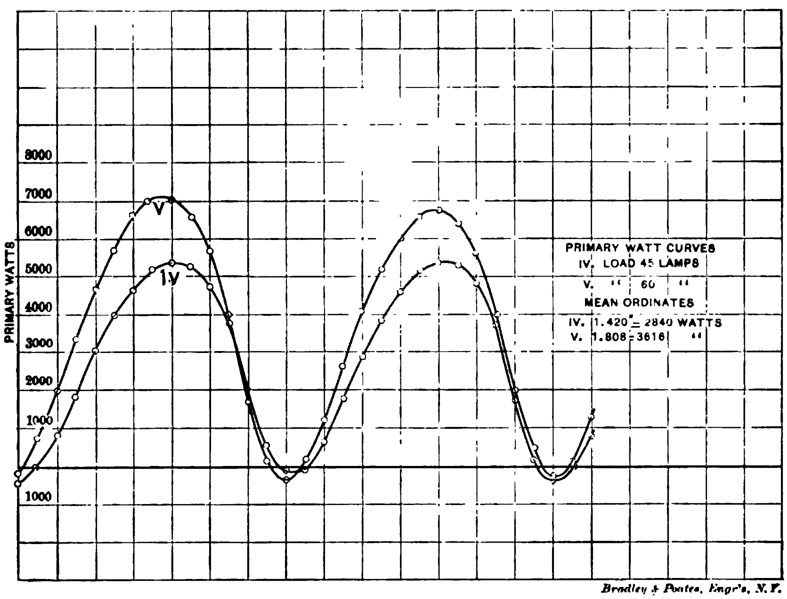

FIG. 12.

1.6 power of the E. M. F., this loss for the designed primary E. M. F. of 1,000 volts would be

$$\frac{1{,}000^{1.6}}{1{,}154^{1.6}} \times 37.02 = 29.4 \text{ watts.}$$

In any transformer, if the primary E. M. F. is a sine-curve, the current curve would also be one, in case there were no hysteresis or eddy currents. In the no-load diagram of this transformer, the primary current curve is not materially different from a sine-curve 90 degrees behind the E. M. F.; while in the no-load diagrams of closed magnetic circuit transformers supplied from this same alternator, the curves for primary current depart widely from sine-curves, and are much nearer the primary E. M. F. in phase. Thus the form and position of the current curve in the open magnetic circuit transformer show the hysteresis and eddy currents to be small, as compared with the transformer with closed magnetic circuit.

The efficiency of the transformer was found by taking the ratio of the secondary to primary power. The primary power was found from the watt curves in Figs. 11 and 12. The secondary power was found by deducting the primary and secondary

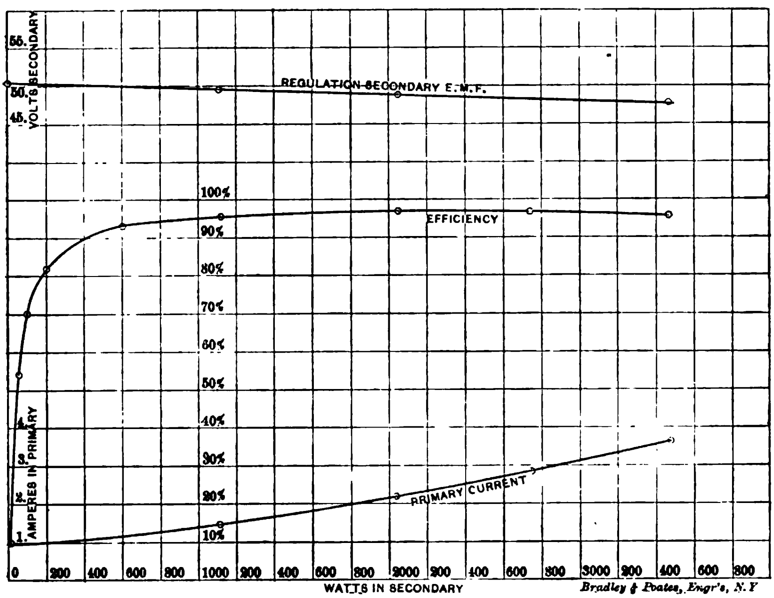

FIG. 13.

copper losses, $R_1 I_1^2$ and $R_2 I_2^2$, and the core losses from the primary power thus found.

The curve for efficiency is given in Fig. 13, and is seen to rise very rapidly as the load increases, reaching 90 per cent. at about one-eighth of full load. From quarter load it is nearly constant, rising to 96.6 per cent. at two-thirds load. It remains practically constant until full load is reached, and then falls off slightly on over load. The all-day efficiency, calculated on a basis of five hours at full load out of twenty-four, is 91.8 per cent.

The regulation is seen by the curve in Fig. 13, showing the secondary E. M. F. for different loads. There is a fall of about 2.5 volts in the secondary, between no load and full load. This curve is drawn for a primary E. M. F. of 1000 volts.

The primary current is shown by a curve in the same figure. Even at no load it is quite large, although representing very little

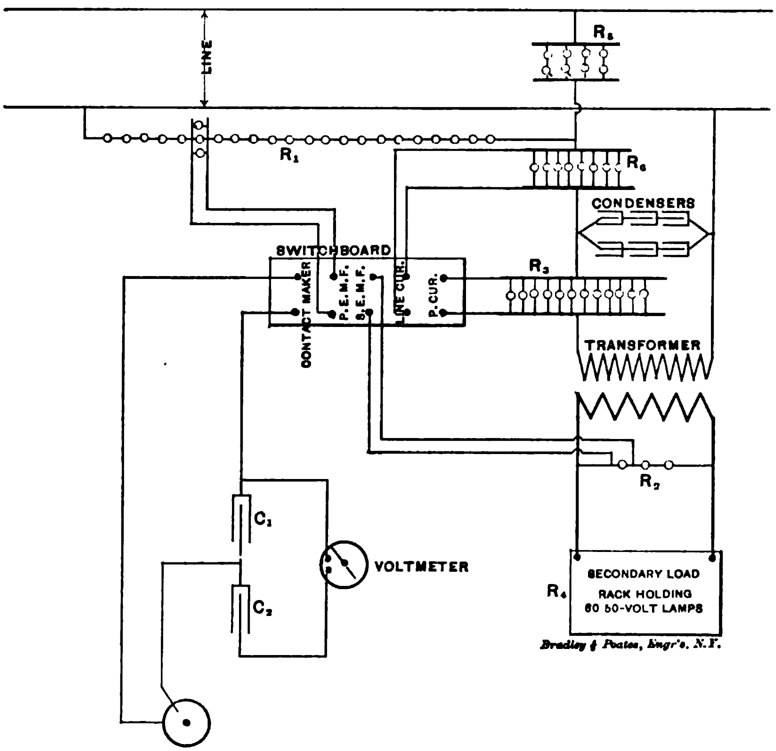

FIG. 14.—Diagram of Connections, Transformer Test with Condenser.

energy expended. Clearly the disadvantage of such a current is that it increases the losses in the line and in the primary conductor. If this line current can be successfully reduced, one of the chief objections to the open-magnetic-circuit transformer will be removed. This can be done by the use of condensers.

PART III.

TEST OF TRANSFORMERS WITH CONDENSERS.

This series of experiments was substantially the same as those just described in Part II., with the addition of a set of condensers of proper capacity placed between the terminals of the primary,

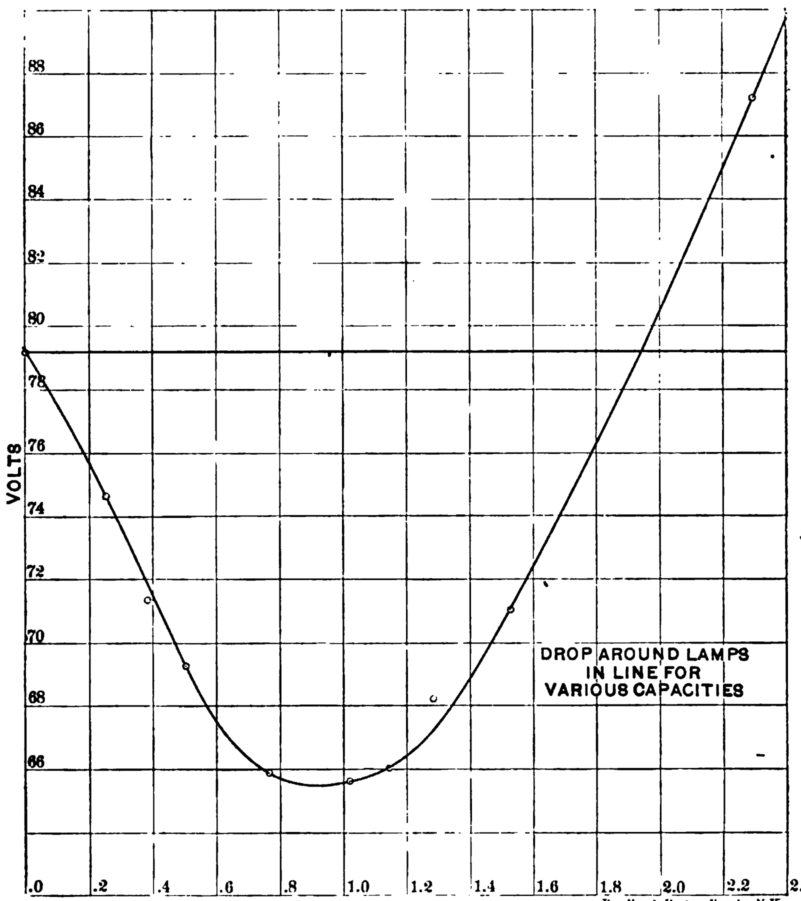

FIG. 15.

in order to reduce the line current. A diagram of connections is shown in Fig. 14, in which R, R_2 and R_3 are, as before, non-inductive resistances for the measurement of primary and secondary E. M. F.'s and the primary current, respectively. The resistance R_5 was introduced to lower the potential supplied the transformer to prevent breakage in the secondary lamps, which were found in the previous run, to be running at too high pressure. The E. M. F. of the supply was thus reduced from 1154 to 1089 volts. The resistance R_6, was used to measure the line current, that is, the current supplied to the transformer and the condensers.

The condensers used were six in number from the Stanley Laboratory, and were intended to be used commercially on a 500-volt circuit. The plates are of tinfoil, the useful part of which

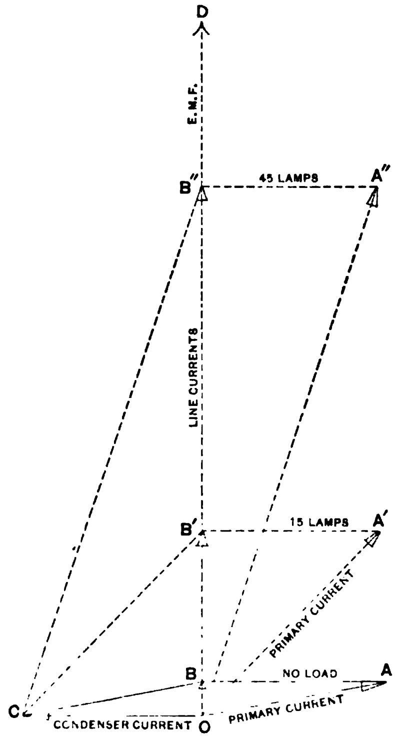

Fig. 16.—Addition of Condenser and Line Currents.

is of the following dimensions: length, 10½ inches; width, 8 inches; thickness, 0.0007 inch. The dielectric is of waxed paper, 0.0043 inch thick. There are sixty-five sheets of tinfoil (total) in each slab, and two of these slabs are placed together in one tin case. The capacity of each condenser was about 1.5 microfarads. When one condenser was supplied with an E. M. F. of 574 volts, the loss was found to be 4.4 watts, representing an efficiency of 96.9 per cent. This data is taken from a paper[1] presented at the

1. Bedell, Ballantyne and Williamson; "Alternate-current Condensers and Dielectric Hysteresis." *Physical Review*, vol. i, No. 2, p. 81, Sept.-Oct., 1893.

Madison meeting of the American Association for the Advancement of Science upon some experiments performed by Messrs. Ballantyne and Williamson and one of the authors.

By arranging the condensers in various combinations, it was possible to obtain a number of different capacities, ranging from 0.25 to 2.25 microfarads, without subjecting any one condenser to more than 500 volts difference of potential.

The proper capacity to be used in parallel with the primary is that which will bring the line current in phase with the E. M. F. When this result is brought about, the line current is a minimum.

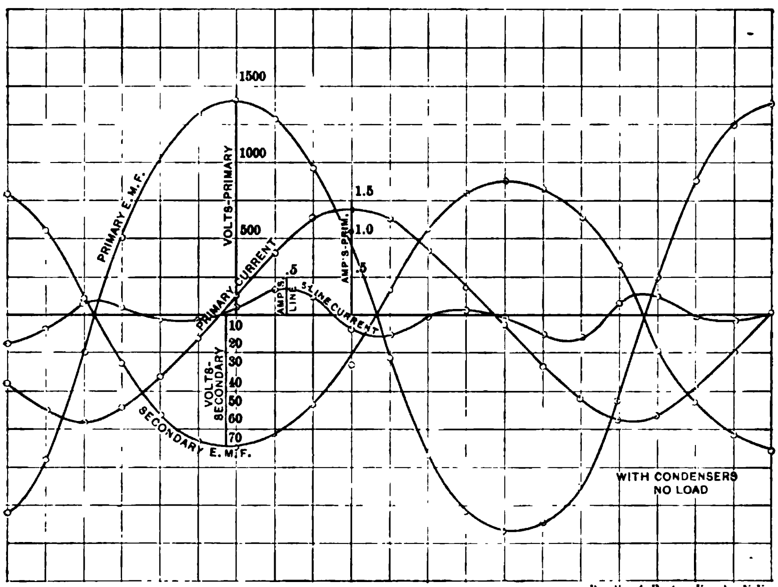

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FIG. 17.

The amount of capacity to be used may be predetermined theoretically, or ascertained experimentally. Predetermined by an analytical and graphical method, of Dr. Crehore and one of us, the proper capacity was found to be very close to one microfarad. Fortunately, the capacity required is almost constant for all loads of the transformer. The proper capacity for the condensers was ascertained by trial as follows: the transformer was run on open circuit with the condensers arranged in various combinations, so as to obtain different capacities, and the fall of potential around the incandescent lamps, R_5, in the line, was measured by the multicellular voltmeter. Square root of mean square values were obtained by direct reading, no instantaneous values being taken. This fall of potential was ascertained for different capacities, and the results obtained are shown in Fig. 15. The minimum drop occurs when the current is a minimum and in phase with the E. M. F. This is seen to be obtained by a capacity of almost one microfarad, the same capacity as calculated. The six condensers were therefore used, three in series and two in parallel, giving a capacity of 1.02 microfarads.

Three runs were now made, one with the secondary circuit open, and the other two with loads of 15 and 45 lamps each. Readings were taken for determining the primary E. M. F., secon-

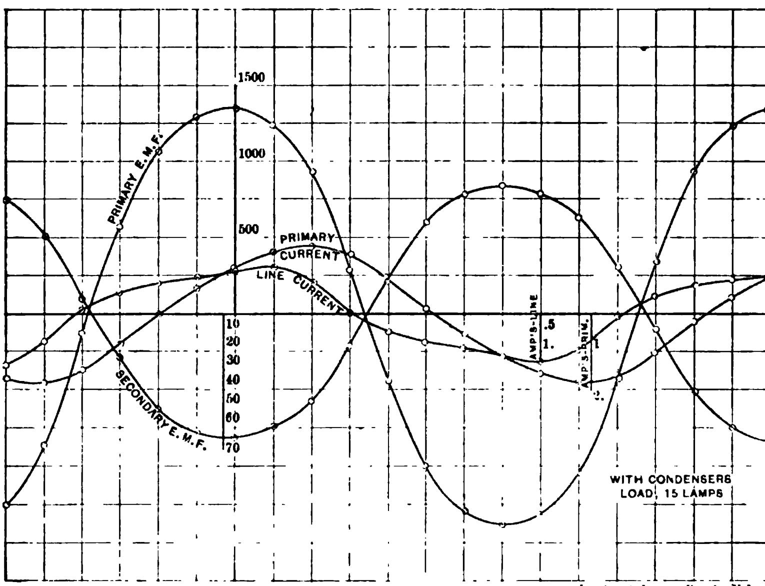


FIG. 18.

dary E. M. F., primary current and line current. By primary current we mean the current in the primary of the transformer, and by line current, the current supplied to the condensers and transformer together, as measured by the fall of potential around the incandescent lamps R_6.

The results of these three runs are given in the following table, showing clearly the effect of the condenser in diminishing the line current, especially at no load.

Lamps in Secondary.	Primary Current.	Line Current.
0.	.95	.187
15.	1.24	.867
45.	3.07	2.78

At no load, the line current is seen to be less than one-fifth of the primary current, or the value it would have if the condensers were absent; even under load the reduction is considerable. These results are better shown by the polar diagram in Fig. 16, drawn on the supposition that the currents are harmonic. On no load, O C is the condenser, current, O A the primary current, and O B the resultant line current, much smaller than either component. For 15 lamps, the condenser current is, as before, O C, the primary current is O A′ and the resultant current O B′. Similarly for 45 lamps, the resultant current is O B″. In all cases

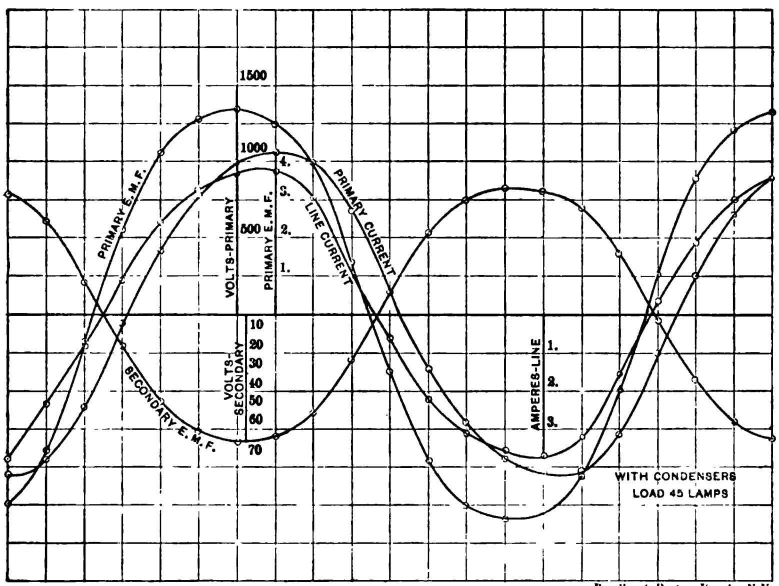


FIG. 19.

the condenser current is ninety degrees behind the impressed E. M. F., O D, and has a value of about .93 amperes. The fact that the currents are not strictly sinusoidal, causes the representation to be an approximation, which although not exact, is however, useful for practical calculations and for illustration.

The results of the three runs are shown by the curves in Figs. 17, 18 and 19. The line current is the algebraic sum of the primary and condenser currents, as just explained; and, if these currents were harmonic, it would be in phase with the E. M. F., if the condenser were properly proportioned. In Figs. 18 and

19 this is seen to be practically the case. The irregular character of the line-current curve in Fig. 17 is explained thus. It is the algebraic sum, (the arithmetical difference), of two almost equal and opposite currents, and is very small compared with either. If these two currents were exactly sinusoidal, the resultant curve would be sinusoidal. As a matter of fact, although very nearly sinusoidal, they are not exactly so, and these variations have a marked effect upon their resultant, which is their arithmetical difference. The line-current curve has a marked depression, which brings the portion, which would naturally be the crest, over the axis; that is, for a brief interval in each half period it has the opposite sign, and the current flows in a direction opposite to its flow during the other part of the same half-period. With a load of 15 lamps this depression has a slight effect, but is scarcely noticeable for a load of 45 lamps, the line-current curve in Fig. 19 differing but little from the primary current curve. It is ahead in phase, and with harmonic currents and the exact capacity required, it would be in phase with the E. M. F. as shown in Fig. 16.

CONCLUSION.

In regard to the apparatus and the method of measurement, as described in Part I., we would conclude that the method of instantaneous contact is a valuable one for the investigation of alternating current phenomena, and may be made accurate and precise for refined laboratory research, while at the same time it is capable of meeting the requirements of rougher practical work, where convenience and durability of apparatus is paramount to extreme precision.

The results given in Part II., show the transformer upon which the investigation was made, to possess two valuable features, high efficiency and good regulation.

The experiments with the condensers, Part III., demonstrate the practicability of their use to diminish the line-current in transformer circuits, and points to their more extended use, as their manufacture is perfected and cheapened, not only in this, but also in other systems of alternating current distribution.

Cornell University, June, 1893.

Discussion.

The President:—You have heard Dr. Bedell's paper. Discussion of the paper is now in order.

Mr. A. E. Kennelly:—I think we are much indebted to Dr. Bedell for giving us a paper which contains so many points of interest. In the first place, the revolving contact maker is, I think, very ingenious, for if there is an objectionable feature about revolving contacts it is the mechanical trouble experienced in keeping the contact certain, non-vibrating, and definite in position. The only hope that has hitherto generally been offered to obtain success with a contact of that kind is to make the circle of revolution so large that even with a large contact area, the period of contact shall not be unduly prolonged. This method seems very promising and ought to satisfy a well defined want. The paper also shows, incidentally, that a single instrument, namely, an electrostatic voltmeter can be forced into making all the measurements required for the determination of the currents, as well as their phase relations, and electromotive forces in a transformer. The diagram does not show just how the 80 volts extra was maintained, and I would like to ask Dr. Bedell how this was accomplished, because that is rather a weak point, of course, about a measurement of this kind. It introduces an error, owing to the question of constancy in the additional electromotive force. It might be preferable, possibly, to have an electrostatic voltmeter which would span the full pressure, and of course such instruments can be made. They were described at the Chicago Congress, for example. The paper also points out that it is possible by the aid of hysteresis to roughly double the frequency of an alternating current, although not perhaps to produce very symmetrical waves in the doubled frequency. It is also noticeable that this particular model of transformer very nearly approaches to that ideal instrument which has been termed the "phantom" transformer, inasmuch as the hysteresis is so far reduced and the distortion of the primary current is so far reduced by the large air-gap in the magnetic circuit that the transformer may be said to have nearly constant primary, secondary, and mutual inductances; and it becomes a simple matter in a transformer of this kind to predetermine the current and voltages that it can sustain. I have, however, to differ from Dr. Bedell as to the method of predetermining simply the capacity in a primary circuit which shall neutralize most completely the primary current. The method of Dr. Bedell is no doubt the accurate and fundamental one. But I think that a simpler, a more practical method, can be obtained by treating the condenser as a resistance, and the primary circuit on the transformer as a resistance, and then combining those two by the rules of impedance. I think this plan is simpler and more readily grasped.

Dr. M. I. Pupin:—I have only two remarks to make. I do not agree with Dr. Bedell nor with Mr. Kennelly that the question of minimizing the line current by means of condensers can be treated mathematically in the case which we are discussing. Theoretically that condenser capacity would give the minimum line current which would be in resonance with the self-induction of the primary of the transformer. The difference in phase would then be zero. That is all very nice when there is no iron in the circuit. But as soon as iron is brought into the circuit then the conditions change completely; that is to say that capacity which will give the minimum current will not reduce the phase to zero. This observation was made some time ago and communicated to this INSTITUTE by Dr. Duncan. It was made somewhat later by myself. There seems to be no theoretical rule which would enable one to calculate beforehand the proper capacity, and the best method to follow is simply to try experimentally. But do not expect that you will have the minimum difference in phase between the line current and the impressed electromotive force when the line current is minimum.

One point of special interest which has been brought out by Dr. Bedell in this very valuable paper is that with no load and the condensers in parallel with the primary, the line current follows the first upper harmonic of the primary electromotive force. I observed myself that if there are any upper harmonics at all in the current, that the first upper harmonic is especially strong, the second upper harmonic, which has five times the frequency of the fundamental, is very weak, and that the third, which has seven times the frequency, is vanishingly small. I have observed that this upper harmonic will always appear, if you have a condenser in series with the primary of the transformer, or in multiple, provided that the secondary is open. But as soon as you close the secondary, then the upper harmonic seems to get weaker, and as the load increases, the fundamental becomes stronger and stronger until at full load the primary current seems to be very little different from a pure sine curve. It must be observed, however, that the secondary load was feeding a bank of incandescent lamps or an electrolyte resistance. I do not think that we are in a position yet, to explain that phenomenon by assuming that the primary current is distorted by the action of hysteresis. I am perfectly aware of the fact that Dr. Fleming and Professor Ayrton and our own most distinguished scientist, Prof. Rowland, have investigated this point and that they have come to the conclusion that the distortion of the primary current is due either to the variation of the permeability of the iron with the induction, or to the action of hysteresis. But in a paper read before this INSTITUTE I ventured to suggest that a third explanation is possible. The suggestion was this: When the secondary of a transformer is open and there is in the primary, either in series or in parallel with it, a condenser, that then the two electromotive forces generated in the circuit, one by the generator and the other by the primary in the transformer, bear a peculiar relation to each other, owing to the peculiar behavior of the back E. M. F. generated in the primary of the transformer, which behavior seems to be due to the following process of magnetization of the transformer core:—As the primary current goes up the induction goes up, until it has reached a maximum As the current decreases, the induction does not decrease with it in step; it lingers some time in the vicinity of the maximum point until the current in the opposite direction is strong enough to drag the induction down as it were with a jerk, and then the induction will go to the other side with a rush. The E. M. F. of self-induction generated by this uneven process of magnetization is far from a simple harmonic. The interference with the electromotive force generated in the primary of the transformer and the electromotive force generated in the alternator, will give a resulting electromotive force which will be a complex harmonic, even if the impressed E. M. F. is a simple harmonic. A condenser placed in parallel with the primary seems to strengthen the complex harmonic E. M. F. of self-induction. If, however, we close the secondary of the transformer then, according to my experience, the secondary current acts somewhat like a buffer. We have two magnetizing forces in the transformer then, so that at no moment can the induction go back with a rush. The secondary current acts like a regulator and makes the induction increase and decrease gradually and in the simplest possible way, and the simplest possible way is the simple harmonic way. My attention was first drawn to this peculiar behavior of iron by the following experiment:—If the primary of a transformer is placed in series with the condenser and the circuit tuned, that is brought in resonance with the impressed electromotive force, then the resonance effect is practically nothing, as long as the magnetic circuit is closed,and even with an open magnetic circuit it is a very difficult thing to obtain strong resonance effects. But as soon as the secondary circuit is closed, then the secondary current seems to add magnetic elasticity to the iron. The magnetization then gradually gets into swing, and resonance effects can then be easily produced even with a closed magnetic circuit, they are, however, always very much smaller than with open magnetic circuits.

There is another remark that I think ought to be made in this discussion, and that is that although the paper of Dr. Bedell shows that in open magnetic circuits we can diminish to a very great extent the line current, I do not think that this investigation shows that we can do the same thing in a closed magnetic circuit, and therefore I think that Dr. Bedell's final remark—that this paper points to the more extended use of condensers in diminishing line currents—needs a modificarion, because, where the condensers were intended to be used to a large extent, was in connection with closed magnetic circuit transformers and in motor work, and in motor work we deal with more or less closed magnetic circuits.

DR. EMERY :—Although it is late, I think the gentlemen present will be happy to hear a word in regard to the experiments with the 10,000-light alternators at the Exposition, and such a statement is applicable in this connection since, curious as it may appear, the current during low loads, which has been discussed here, came very near defeating the experiments. The special engines provided to operate the large alternators were designed for 150 pounds steam pressure and it was expected that this pressure would be furnished. There were, however, only two sets of boilers in the long row at the Exposition which would carry this pressure and, as all were necessarily connected together ordinarily, the pressure did not exceed 125 pounds, so that such of the alternators as were operated by the special engines could not be run with full load. It was expected that arrangements could be made whereby one of these engine dynamos could be connected independently to boilers which would carry the proper pressure, in which case the judges could have made the tests in the daytime or when convenient. After long waiting, with encouragement that proper appropriations would be made, it was decided that there was no money available for the purpose and an attempt was made to do the work at night. By carefully estimating the loads after 11 P. M., it was considered that the high pressure boilers would operate enough of the special engines to carry the whole load on the alternating system, with the connections of the other boilers and engines shut off the special pipes and drums through which the high pressure was being conveyed. In preliminarily canvassing the matter it was not suggested that the load at that time was largely made up of watch lights in the buildings, on circuits which generally carried many lights. Of course such circuits were not of the kind that the manufacturers wished to operate in an efficiency test, as they required plenty of current, but would record comparatively few watts. The consequence was that it became necessary to run as many engines as on other nights, to take care of the watch light circuits and in addition to run the engine to be tested with fully loaded circuits, such for instance as those supplying the dome of the Administration Building, the outside lights on Agricultural Hall, etc. The power from the high pressure boilers was therefore insufficient and it seemed for a time that we should not be able to make the full load test of the engine dynamo. Everybody was interested, however. The assistant operating mechanical engineer was on hand, and by putting in more boilers and keeping the pressure close up to the blowing off point—the engine representative at the same time slightly adjusting the cut-off on his engine so as to maintain full speed, I am gratified to say that we obtained 1200 indicated horse-power with a 1000 H. P. engine and were able to carry a satisfactory load on the dynamo. To accomplish this all had to remain on duty until daylight the next morning and this sort of work had to be repeated twice in making other tests, simply from lack of means. It was pleasant to all however, to succeed, and in due time the results will be announced.

Dr. Bedell:—The discussion by Mr. Kennelly and Dr. Pupin, as well as some questions which have been asked informally by various members, lead me to make some remarks by way of reply and addenda. The iron in this particular transformer was doubtless particularly good, and the iron losses therefore smaller than found by Dr. Fleming in the paper referred to. The paper to which I referred was read before the Institution of Electrical Engineers on November 24, 1892. Dr. Fleming gives the results of extended investigations upon the various prominent types of transformers—the Thomson-Houston, Westinghouse, Kapp, Ferranti, Swinburne's "Hedgehog," and other transformers, the investigation being originally undertaken with the purpose of comparing the performance of the "Hedgehog" with that of some closed-magnetic-circuit transformers of the same size. The work was most painstaking and the paper most elaborate and carefully prepared. The methods used were: (I.) the three-voltmeter method; (II.) the three ammeter method; (III.) dynamometer-wattmeter (Siemen's) method, which eventually proved the most satisfactory; and (IV.) Swinburne's wattmeter. The 'Hedgehog" transformer upon which the experiments were made had an output of 3,000 watts, and was designed for a primary potential of 2,400 volts with a frequency of 83 alternations per second and was so operated.

By the three voltmeter method, Dr. Fleming found that from no-load to full-load the primary current rose from 0.76 to 1.566 amperes, the secondary current from 0 to 29.93 amperes, and the secondary E. M. F. fell from 102.1 to 98.8. At no-load the power factor was 0.063. The power taken up in the transformer at no-load was 112 watts and 237 at full-load. He however concludes: "The power-factor of the "Hedgehog," even at full-load, is still small and we were not satisfied with the general nature of the results given at all loads by the three-voltmeter method for this transformer."

The three-ammeter method was open to objection in the same way as the three-voltmeter, and Dr. Fleming proceeded to the dynamometer methods. With the dynamometer-wattmeter, the efficiencies obtained for 0.1, 0.2, 0.3, etc., of full-load up to full-load were 71.7, 82.9, 87.6, 89.8, 01.2, 92.2, 92.8, 93.2, and 93.5 for full-load.

The no-load losses by methods I., II, and III., were about 112 watts, or 37 per cent. of the output of the transformer. With Swinburne's wattmeter the no-load loss found was 74 watts. Dr. Fleming attributes the discrepancy to a probable electrostatic capacity in the wattmeter which would produce an error in the results similar to those produced by self-induction.

Dr. Fleming attributes a considerable portion of the transformer losses to eddy-currents in the secondary copper and considers the function of the iron core of a transformer "to be the preservation of the lines of induction in the proper place, and making them thread through the aperture of the secondary circuit without intersecting through the mass of copper in that circuit." Mr. Swinburne considers that most of the loss is in the iron core. In a letter I have just received from him, he says that the iron in the specimen furnished Dr. Fleming was of a poor quality—this occasionally occurring on account of the frequent annealing undergone by the wire of which the core is made in the course of manufacture. The reasonably small losses obtained in the present investigation would seem to sustain Mr. Swinburne in this statement. I see no reason to doubt Dr. Fleming's results.

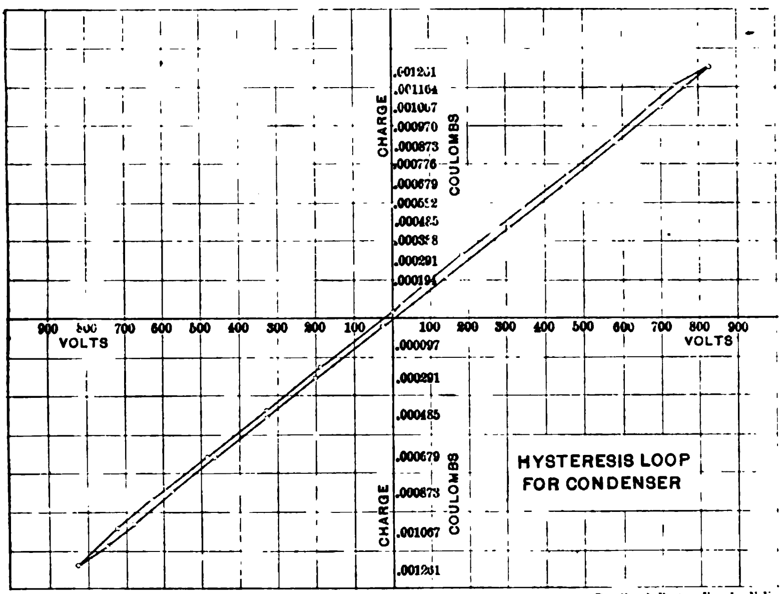
Mr. Swinburne sent a condenser to Dr. Fleming to use with his transformer. The condenser's capacity (about $\frac{1}{8}$ microfarad) was not the proper value to bring the line current into phase with the E. M. F.—the condenser current being 0.458 ampere and the no-load transformer current 0.76 ampere.

The figures claimed by Mr. Swinburne when he sent his transformer to this country agree substantially with those given in the present paper and are as follows. Full-load losses: primary by working current, 27.4 watts; primary by idle current, 4 watts; iron, 22 watts; secondary, 30.5 watts. Full-load efficiency, 96.46 per cent. No-load waste of power, 0.87 per cent. of the full load. Secondary pressure: no-load, 50.5; full-load, 48.7 volts. Exciting current, 0.95 ampere. Frequency, 130. Primary E. M. F., 1,000 volts.

Referring to the question asked by Mr. Kennelly in regard to the maintenance of the difference of potential around the condenser by which the zero of the voltmeter was displaced, I would say that a difficulty does arise as he states. The condenser was charged at convenient times and a correction made for leakage afterwards. The leakage was ascertained by the rate of the loss of potential. This was found to be about one volt each hour and a correction was made accordingly.

Some further remarks in regard to the losses in the condensers in parallel with the primary may prove of interest. Electrostatic phenomena in connection with alternating currents have not as yet been the subject of such careful investigation as the electromagnetic; and, whereas the experiments of Ewing, Steinmetz and others have cleared our ideas in regard to the losses caused by magnetic hysteresis or that due to the reversal of the electromagnetic field, the phenomena of dielectric hysteresis which is produced by changes in the electrostatic field have been little more than hinted at, much less made the subject of careful study. This was the state of affairs when the investigation already referred to, of the dielectric phenomena, was undertaken. Various methods were tried and more suggested for the study of this electrostatic hysteresis, and the method finally adopted as giving the most satisfactory solution of the problem may well be referred to here as being closely allied to the subject in hand.

After endeavoring to obtain an hysteresis loop for a condenser by means of ascendiug and descending curves of charge and discharge obtained by means of a ballistic galvanometer, with but partial success due to the effect of the time element, resource was had to the method of instantaneous contact just described. From the product of the instantaneous values of current and electromotive force a watt curve is readily constructed, in which the positive areas indicate the power given to the condenser and



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the negative areas, the power returned to the circuit from the condenser. The difference in these positive and negative areas indicates the loss in the condenser.

By obtaining the area of successive portions of the current curve, the quantity of charge in the condenser at any time may be determined, since $q = \int i \, d \, t$. This was done by a planimeter.

The data from which to plot an hysteresis loop was thus obtained. Such a loop, shown in the figure, is obtained by plotting as abscissæ the instantaneous values of E. M. F. and as ordinates the instantaneous values of charge found from the integration of the current curve as just described. The area of this loop is $\int v \, d \, q$ and is a measure of the energy dissipated in hysteresis per cycle.

The existence of such a loop for dielectric hysteresis has for a long time been suspected and it is interesting to see the exact form which it takes, and to note its similarity to the corresponding curve of magnetic hysteresis in iron.

Of course the investigation of the subject is not complete until the electrostatic hysteresis is determined for various dielectrics and for different potentials and frequencies.

The losses in these condensers were so small that it can not be said that dielectric hysteresis prohibits the use of condensers in practical alternating current work.

[*Communicated after adjournment by* MR. CHARLES P. STEINMETZ:]

Being unable to be present at the reading of the paper, I forward a few remarks to the matter under discussion. This paper of Messrs. Bedell, Miller and Wagner is of high interest from many points of view. First, in so far as it adds something to the elucidation of a controversy which in alternating current engineering assumed an aspect similar to the universal wire gauge of which our Secretary told us to-day—I mean the question of open circuit *vs.* closed circuit transformers.

This transformer of Mr. Bedell is a 60-light, or 3,000-watt transformer. It is of interest, therefore, to compare its behavior with the 3,000 watt hedgehog transformer tested by J. A. Fleming a year ago (Institution of Electrical Engineers, Nov. 24, 1892).

	Fleming found:	Bedell finds:
Efficiency at full load............	93.5 %	96 %
" " 1-10 "	71.7 %	87 %
Current at no load, in per cent. of full load current..............	48 %	30 %
Core loss........................	114	29.4 watts.

Especially noteworthy is the reduction in the core loss and the consequent increase in the efficiency in the present transformer.

It is to be remembered, however, that the efficiencies given by Mr. Bedell are not observed, but calculated from copper and iron losses.

Very interesting is Fig. 17 which shows the line current to be a wave of higher frequency, so that here experimentally the primary current has been dissolved into the sine wave of magnetizing current, as supplied by the condenser, and the higher harmonic wave of energy current which, however, consists of two components again, an energy component of normal frequency, and the wattless complex higher harmonic mainly consisting of a term of triple frequency as shown by Fig. 17.

Very curious is the behavior of the primary current, which is just opposite to what is known of the closed circuit transformer. In closed circuit transformers the wave of primary current is greatly distorted at no load, and becomes more and more sine shaped with increasing load.

In this open circuit transformer we find the wave of primary current at no-load practically symmetrical (Fig. 6); with increasing load the primary current becomes distorted while the E. M. F.'s retain their sine shape, and the primary current, as shown in Fig. 8 and 9, bulges out at the rising, and bulges in at the decreasing side.

Now this phenomena is very curious and fully deserves further investigation, since it makes the splendid efficiency curve of Fig. 13 doubtful.

Anybody who has investigated the various distortions of alternate waves will in this particular distortion suspect the effect of a hysteretic loss not existing at no load.

Due to the open magnetic circuit considerable distortion of the magnetic field may be caused by the counter-magnetomotive force of the secondary circuit. The existence of this distortion of magnetism is shown by the gradual displacements of E. M. F.'s with increasing load. An effect due to the same cause we probably see in the distortion of the current wave. How far this effect implies an additional energy loss remains to be seen by further investigation.

Probably this question may be decided by taking instantaneous values of secondary current also, which would enable us to trace the origin of this remarkable distortion.

With regard to the way of determining the effective value by calculating the squares of instantaneous values, plotting them in a curve and measuring its area, this must be very tedious. Three years ago I pointed out a much simpler method in *Elektrotechnische Zeitschrift*, June 20, 1890: Plotting the instantaneous values of the alternate wave in polar co-ordinates, i. e., as radii corresponding to the time as angle, the area of the curve represents the sum of squares of the values, i. e., the effective value, so that *the effective value of an alternate wave can be derived directly by measuring the area of the curve of instantaneous values in polar co-ordinates.*

This is another feature which makes the use of polar co-ordinates preferable in alternating current work.

October 19, 1893—Lynn, Mass.

THE PRESIDENT:—Is there any further discussion on this paper? If not, I will call for the paper by the Secretary, on "Monthly Meetings of the Institute; Their Origin and Proposed Development."

A paper presented at the Eightieth Meeting of the American Institute of Electrical Engineers, New York, October 18th, 1893, President Houston in the Chair.

MONTHLY MEETINGS OF THE INSTITUTE.

Their Origin and Proposed Development.

BY RALPH W. POPE.

Introduction.

Now that the membership of the Institute has become so widespread, accompanied by the natural tendency to increase most rapidly in cities, the question of holding meetings at different points in the country is assuming marked importance. The recent revival of this subject is largely due to a discussion[1] which took place at the annual meeting, May 16th, 1893. Attention was called at that time to the fact, that Council had already considered the advisability of the holding of meetings in other cities than New York, but that nothing had been accomplished, due largely to the failure on the part of any number of members to express a desire that such meetings be held. In order that the details of this movement may be properly understood, as well as to correct erroneous impressions that have arisen, owing to lack of information upon the subject, the writer has undertaken to prepare this paper. His familiarity with the history of the Institute, as well as the practice of similar national organizations, must be his excuse for undertaking the work.

Rules Regarding Meetings.

In the organization of the Institute, the rules of the American Institute of Mining Engineers were very closely followed, and provision was made for one annual and two regular meetings. The Rules also provide for the holding of special meetings, as follows:

1. Transactions, vol. x, p. 305.

"Special meetings may be called whenever the Council sees "fit; and the Secretary shall call a special meeting on a requi-"sition signed by fifteen or more members." [See Rule 6.]

EARLY SUGGESTIONS.

The immediate predecessor of the writer in the office of Secretary, Mr. T. C. Martin, in his remarks at the annual meeting, May 19th, 1885, when the INSTITUTE was but twelve months old, called attention to this rule:

"Several of the members in New York," he said, "have writ-"ten to me and have objected to the infrequency of the meetings "held during the year. They say that the members ought to be "brought together in New York oftener, that papers might be "presented and read, and that the New York members might "form a branch or sub-section of the society, and they think that "the society would be greatly strengthened by some such step "as that, but it will be seen that Rule 6 makes no provision at all "for any meetings of that nature, except that they shall be called "whenever the Council sees fit, and that the Secretary shall call "such a meeting on a requisition signed by fifteen or more mem-"bers. It seems to me that the society would derive great bene-"fit from occasional meetings of the New York members."

No action was taken upon this suggestion, although at the same meeting, a committee for the revision of the Rules was appointed, which reported at a special meeting called for the purpose, December 8th, 1885. At this meeting the following six members were present: the late Major Michaelis, the late Sidney F. Shelbourne, John Bogart, Henry B. Slater, Joseph Wetzler and the writer. The outlook for meetings of any kind at that time was decidedly gloomy.

THE MOVEMENT CHECKED, BUT NOT ABANDONED.

Matters moved along in practically the same groove until the next annual meeting, held May 18th, 1886. The following is a quotation from the report of your present Secretary, who had then held office for one year:

"The importance of the various electrical problems which are "continually being brought to public notice, points to the advisa-"bility of holding monthly meetings, for the discussion of current "topics. There are in New York and vicinity a sufficient num-"ber of members to make such meetings successful, especially if "those who are capable, and have had ample electrical experience "will perform their duty in the preparation of suitable papers "for presentation on such occasions. I can see no reason why "such a gathering might not be held monthly, in conjunction "with the regular meetings of Council, under existing arrange- "ments. A suggestion of this kind was made by my immediate "predecessor, but not having been acted upon, I feel it my duty "to state that I consider something of this kind absolutely es- "sential to the future prosperity of the INSTITUTE, and earnestly "recommend that the matter be taken up at this meeting. If "possible, I should be glad to see at least six members or associ- "ates now appointed, who should in turn present suitable papers "for discussion on such occasions. At least one meeting of this "character might be held early in June, and upon its success "would depend the arrangements for future gatherings. I have "already been promised a paper for this purpose, and it is very "probable that gentlemen who could not comply with our request "to submit papers at the present time, will be glad to do so at a "more convenient season. It also appears probable that if suit- "able arrangements were made for reporting the proceedings of "these meetings, many important electrical papers would be "brought before the INSTITUTE, which are presented to other "societies, although perhaps entirely foreign to their special "field. This is largely the result of a popular interest in electri- "cal researches now existing, which we should avail ourselves of "to stimulate the growth of the INSTITUTE.

"With the inauguration of such a policy as I have outlined, "your Secretary will feel far better satisfied with his work, "and the existence of the INSTITUTE will gradually become more "generally known, and properly appreciated by those for whose "benefit it was founded."

Notwithstanding this suggestion, there appears to have been a disposition to take up the consideration of that weighty and inexhaustive topic, which has haunted technical societies for many years, viz:—"A Universal Wire-Gauge." After a discussion which left the wire-gauge question exactly where it stands to-day, excepting that three or four new gauges have since been added, Mr. Mailloux availed himself of a lapse in the proceedings to call attention to the Secretary's recommendation in the following words:

"Has anybody taken up that suggestion about the monthly "meetings? It strikes me we ought not to let an important "matter like that pass by. There is quite a sentiment in favor "of that project in the body of the institution, and I must say, "for my part, that I favor it greatly. I believe in spreading the "light all we can and keeping the INSTITUTE awake, and I do not "think there is anything that could conduce more than monthly "meetings to awakening a general interest in electrical matters, "not only among the members, but with the general electrical "public. I know that abroad it is customary for societies to "meet oftener than three or four times a year. You almost for-

"get that you have been there after three months and forget "acquaintances that you make. Then again the frequency "of meeting should be made to excite interest in certain matters "and perhaps to replenish the literature of electricity to a certain "extent People do not have time to write and elaborate their "ideas and let others have the benefit of them, but if they have "a chance to come together once a month, or once in two months "and exchange ideas, I have no doubt that friction would have "a tendency to benefit us all and keep us bright. In that way "we would provoke discussion, which is healthy for us all, and "at the same time we are producing material that is subsequently "published and is going to be beneficial. It will help to supply "a long felt want in literature."

I do not know that Mr. Mailloux has ever made any pretensions at being a prophet, but he certainly gave promise of being a statesman. The matter was laid aside however, and the irrepressible wire-gauge topic was again recognized. Then the question of the title of Past-President was discussed, and time was apparently slipping away, judging from the tone of the following remarks which bear evidence of impatience on the part of the Secretary.

"I know it is getting late and perhaps some of the members "are tired, but I do not want to see the meeting slip away with-"out taking some action on the monthly meeting business. I have "no desire to see action taken on it merely as a matter of form. "There are monthly meetings and monthly meetings. The New "York Electrical Society has monthly meetings, and with a larger "membership than ours, the meetings are not as well attended as "they should be, and it is very often the case that meetings of "this kind lack interest, and for that lack of interest members do "not come. If they came, there would be interest, and if there "was interest they would come. This a question that has been "discussed I presume in every society that has been organized "in New York City. But there is one thing about New York "City that appears to operate against the success of evening "meetings, and that is this gap which intervenes between office "hours and eight o'clock. It is a good while to eat dinner in, "and it is hardly sufficient time to go home and return in; I "have talked to different members of the INSTITUTE about a plan "by which we could meet at some down-town or up town hotel, "making arrangements for a monthly supper. If we stay we "have to buy our supper anyhow. We could get the hotel to "furnish a room for discussion. We could meet there at six "o'clock and take supper or dinner and the meeting could con-"vene at half past seven. We can all be there and get through "the work in decent season. That would also give an oppor-"tunity for those who wish to dine at home in preference to "remaining in the city, to go home and get back with the as-"surance that there will be a quorum when they return. I be-"lieve that by this means and by the appointment of one of our "members to present a paper for discussion—not necessarily a "very elaborate paper, but simply sufficient to bring up points "for discussion, that we may have very interesting meetings. "We stand very differently from some other societies. For in-"stance, there is the Telephone Exchange, and the National Elec-"tric Light Association—societies which are devoted to special "branches of electrical work. They supply topics enough to "bring half a-dozen papers before a meeting, on their practical "branch; while we cover the whole field and consequently we "have a vast range of subjects to choose from, and most of our "members are competent to discuss many features of all of them. "I merely present this outline as a matter for your consideration, "for I believe it is essential to the welfare of the INSTITUTE "that we should get together oftener than once a year, and be-"come acquainted with each other and make our existence "manifest."

The matter terminated at this meeting by the appointment of a committee of three on motion of Mr. Mailloux, to consider the subject and report on the following day. This committee was comprised of Messrs. C. O. Mailloux, R. W. Pope and Geo. M. Phelps, Jr.

A STARTING POINT.

The report of this committee accepted on the following day, May 19th, 1886, was as follows:

"The committee on monthly meetings respectfully report that "they consider such an arrangement desirable, and recommend "that they be authorized to formulate a plan for such report, "subject to the approval of the Council. In the judgment of "the committee it would seem advisable that these monthly "meetings be held at the same time and place as the Council "meeting, and they would recommend that the next Council "meeting, on the first Tuesday in June, be set as the date of this "first meeting, at which the committee can then report on a more "detailed plan."

"*Resolved*:—That the committee on monthly meetings be and "is hereby authorized to make arrangements for such gatherings "as may be considered for the best interests of the INSTITUTE, and "their action be reported to the Council at its next meeting for "approval."

The late Mr. Shelbourne feared that as summer was approaching the plan might fail. In the discussion he said:

"Of course, I believe in these monthly meetings, and I believe "in organizing them and carrying out that plan as soon as prac- "ticable. But we are just on the edge of summer, when every- "body goes out of town, and people prefer to loll on verandas "with palm-leaf fans in their hands to getting together in meet- "ings. I think that if we are going to have undue haste in this "matter, perhaps we will defeat the very object we are trying to "promote.

The Secretary however feared to let go his hold, and again came to the support of a measure which seemed continually eluding his grasp, hence his reply:

"Mr. Shelbourne has brought up the very point in regard to it "that I mentioned in my report, that is, that unless we have a "meeting the first of June, it would be well to lay it over until "autumn, and it was for that very purpose it was brought up, "that we might have simply one, in order to start it before sum- "mer, for fear that so long a time would intervene before the "next one, that instead of undue haste we would have undue "negligence, perhaps."

THE FIRST SPECIAL "MONTHLY" MEETING.

Following the plan thus outlined, the first special meeting was held on June 8th, 1886, at which about 30 members were present. In calling the meeting to order, in the restaurant of the Mills Building, 15 Broad St., President Franklin L. Pope spoke as follows:

"It gives me very much pleasure to see so large an attendance "on this occasion, the first of our monthly meetings, and it affords "us excellent encouragement as to what we may expect in the "future. It is an old maxim that we should always attend to "business before pleasure. I think we have put in an hour of "good solid business, and I had hoped that we should next have "the pleasure of listening to a paper on the subject of Incandes- "cent Lighting from Central Stations, by Mr. H. M. Byllesby of "the Westinghouse Electric Company; but Mr. Byllesby has un- "fortunately become entangled in a complication of business that "has come up during the past few days, which has occupied his "time so fully that he is unable to be present, and in order, "therefore, that the meeting may not be disappointed, we have "asked Dr. Otto A. Moses, to favor us with his presence this "evening, and to make a few remarks on the same subject, with "which he is as you all know, very familiar. Dr. Moses has "very kindly responded to our request at considerable incon- "venience to himself, and will now open the discussion."

AUTHORITY GRANTED BY COUNCIL.

This meeting was followed by others held on December 7, 1886, Feb. 6, and March 15, 1887.

According to the minutes of Council these meetings were authorized as follows:

"November 16, 1886.

The Secretary, from the Committee on Monthly Meetings reported that Mr. Martin had offered to prepare a paper upon the application of electricity to street railways and that it was desirable that Council authorize the holding of a special meeting in accordance with the by-laws.

On motion of Mr. Shelbourne it was voted that the Committee on Monthly Meetings be authorized to act at its discretion in the calling of such a meeting with full power as to place and circumstances."

"January 11, 1887.

The Committee on Monthly Meetings was authorized to call another special meeting in February to be accompanied by a dinner and the Secretary was authorized to provide for its being stenographically reported."

"March 8th, 1887.

The Committee on Monthly Meetings reported that they had had in view an excursion to Plainfield in lieu of the March meeting, but that it was decided that it was not expedient at present, and it was agreed that a meeting and dinner should be held at Cable's restaurant on March 15th."

This was the last meeting of that season.

On October 2nd, the Council appointed a Committee on Papers and Meetings, since which date arrangements for all meetings have been made by the Committee in co-operation with the Secretary.

The following is an extract from the Introduction to the fourth volume of the TRANSACTIONS:

"The rapidly extending field of electrical industry has developed a corresponding growth of scientific electrical research, and acting upon the supposition that beneficial results would follow a straightforward attempt to encourage the interchange of ideas upon the electrical questions of the hour, the Council has, during the past year, appointed more frequent meetings, the records of which are now published in full. The good effects of this policy have been manifest, not only in the steady numerical growth of membership, but in the widespread recognition of the scientific standing of the INSTITUTE as a body. Some of the most distinguished electrical engineers of the world have appeared before it, and the thoughts of its members have been stimulated to renewed activity by the information elicited during the various discussions which have taken place."

THE MEETINGS CONTINUED REGULARLY.

The season 1887-88 marked the regular appointment of meetings, which were held monthly from September 20th, 1887 to

April 10th, 1888 with the single exception of the night of the great blizzard, March 12th, 1888, when stenographer Ryan waited in vain for even a glimpse of the Secretary, who had spent the day watching a new window thermometer at his home in the snowbound city of Elizabeth, N. J. The paper scheduled for that evening did not materialize until February 12th, 1889.

During the past five years, meetings have been held practically every month during each season. A gradual change was made however in the practice of combining a meeting with a dinner. It was found that there were a few who did not approve of the plan, while others thought that a dry paper and a dry dinner were not conducive to the longevity either of the members or the INSTITUTE. In addition to his responsibility for the interest of the meetings, the Secretary was expected to vouch for the competence of the caterer, and the skill of the chef, while certain famishing guests would implore him to hurry up that dinner, regardless of its condition. The object sought, had however been attained. A fair audience was always in attendance, and the TRANSACTIONS will bear witness to the fact that the discussions have been well sustained. The plan of meeting over a dinner table was abandoned after less than a year's experience—not because it was a failure—but for the reason that it had proved successful in providing that momentum which has since been maintained. Acquaintances had been formed, and that *esprit du corps* so essential to an organization of this character had gradually been established.

GOOD EFFECTS OF THE PRACTICE.

The object of the writer in directing your attention to this sketch of the experience of those who undertook the task of organizing meetings in New York, is to show that the present standing of the INSTITUTE is largely due to the persistence of the local members in forcing the issue. The necessity of this action was not generally appreciated at that time. As will be seen, the movement was in imminent danger of being side-tracked to make way for the all conquering wire-gauge question, which like a weed driven from a well cultivated garden, may find refuge upon the premises of the next door neighbor. After the American Society of Mechanical Engineers, upon whose domain it has recently encroached, has passed through experience similar to that of the electrical organizations, the wire-gauge family will perhaps be large enough to organize a society by itself, when the writer trusts that the various members of that prolific race, may follow the historic example of the Kilkenny cats.

At the expiration of the fiscal year May 17th, 1887, the total paying membership of the INSTITUTE was 98, classified as follows:

New York and vicinity	55
Other places	43
	98

One year later the membership was 322, classified as follows:

New York and vicinity	152
Other places	170
	322

The present membership is as follows:

New York and vicinity	248
Other places	494
	742

This increase was largely due to the holding of more frequent meetings, by which officers and members were stimulated to greater exertions, while the electrical public generally, were taught to appreciate the value of such an organization. It is scarcely necessary to state that in 1886 there was no call for meetings to be held elsewhere. It is, however, proper to add that the necessity of considering the question has been more or less prominent since the year 1888. It will, perhaps, be remembered that at about this time there was a widespread movement for the establishment of electric clubs. It had no doubt been assumed, that the failure to establish permanent local electrical societies was due to the fact that they lacked the incorporation of certain social features, and were therefore avoided by an element which would otherwise contribute to their success. Attempts were made to combine the scientific and practical, with the social features, and the plan had the appearance of being a good one. It was sufficiently attractive to divert the support of electrical men to such local organizations, and the policy of the INSTITUTE has been to carry on its own work without the slightest attempt to intrude upon the field already occupied by local associations. Any movement in this direction, would no doubt, have led to jealousy and antagonism, productive of more harm than good It is doubtless true that the organizers of local associations, have seen no way by which they could accomplish their object, excepting upon a strictly independent basis; certainly the INSTITUTE has never offered a plan by which they could assemble under its auspices.

AUTHORITY FOR HOLDING LOCAL MEETINGS.

Instead of the Rules of the INSTITUTE *not* providing for local meetings, it appears to the writer that the opposite is the case. The rule governing meetings has already been quoted. It was supposed in 1886 that some amendment to the Rules was necessary to meet the proposed conditions, but at the annual meeting, May 18th, 1887, it was officially announced from the Chair that no further legislation was necessary. At that meeting the following episode occurred:

MR. MAILLOUX:—I would like to call attention to the fact, that no action has been taken yet to perpetuate the arrangement in vogue last year, for monthly meetings.

THE PRESIDENT:—[Mr. T. C. Martin.] It is within the discretion of the Council.

MR. MAILLOUX:—It was intended to have some amendment to the Constitution at this meeting, was it not?

THE PRESIDENT:—It was found that no action was necessary.

[The general meeting then adjourned.]

It would therefore appear that under Rule 6, meetings may be called at the discretion of Council, or by the Secretary upon the petition of fifteen members. The assumption is, that in the opinion of those who suggested this rule, the grouping of fifteen members was of sufficient importance to warrant the holding of a meeting. No restriction exists as to the place of meeting, due, no doubt, to the well known practice of national societies of holding their meetings in various parts of the country.

The history of the movement for holding monthly meetings in New York City, has been given you for the purpose of showing that they were authorized *only upon the manifestation* of an earnest desire on the part of a sufficient number to hold such meetings. It is evident, however, that unless the privileges conferred by the Rules are pointed out, or until some person evinces sufficient interest to inquire what steps are necessary to secure such privileges, the Council is not in a position to act.

This question has already been brought up by the inquiry of Dr. Perrine, at the annual meeting, May 16th, 1893, already referred to, and by Mr. Steinmetz, at the monthly meeting, September 20th, 1893. At an informal meeting of the Chicago members, at the World's Fair quarters of the INSTITUTE, September 2d, 1893, the occasion was availed of to point out what proceedings were necessary, on the part of members, to bring about meetings of the INSTITUTE in Chicago and vicinity. A committee was appointed to canvass the membership and advise the Council, or petition the Secretary, as might seem most expedient as provided by Rule 6.

There has been a disposition to characterize this movement as contemplating the organization of "branches" or "chapters." The reason for this may have been that no other plan seemed feasible, and even this, as may readily be seen, would require a revision of the Rules. In considering this question, the Council has been confronted by various obstacles.

If at the proposed meetings, papers were read and discussed in addition to the ordinary schedule of the year, the publication of this additional matter might incur an expense beyond the present income of the INSTITUTE. Such a plan might result in a sufficient growth of membership to warrant assuming the risk, but this could be demonstrated only by experience. There has not at any time been a sufficient indication of a strong desire to hold meetings in other cities, consequently the Council has not seen any necessity of arranging for their appointment.

PROPOSED PLAN OF ACTION.

Having called attention to the points requiring consideration, the writer will direct your attention to a plan originally suggested by Mr. Martin, Chairman of the Committee on Papers and Meetings, but which has been tentatively discussed, and elaborated, and finally molded into what appears to be a practical form.

The basic principle is the division of the monthly meetings into sections—not because of the discussion of different subjects—but for the purpose of overcoming the geographical difficulty which confronts us. There might be for example, in the case of the 81st meeting, a New York section, a Chicago section, and a Lynn section, which would be called together upon the same date. The paper announced for the meeting is printed and distributed in advance to the members. At those points where the various sections are to meet, and the author is not present, the member selected to present the paper, should be provided with an early proof so that he may be prepared to reply to any questions that may arise. These sectional meetings might be presided over by regularly elected members of the Council, or a temporary chairman; either course being available. The details of arrangement for such meetings would necessarily be in the hands of a local committee or secretary. Notes of the discussion could be taken by the secretary, and afterwards written out in full by the speakers, or a verbatim report could be made if the necessary expense was provided for. In any case the report of the discussion might be sent to the secretary, and revised by the Editing Committee. In case of duplication of ideas, the points raised could be made more emphatic by inserting in brackets, or as a footnote, words to this effect: "in the discussion of this paper by the Chicago section, Mr. A. raised a similar point."

It appears reasonable to suppose that the division of a meeting into sections at a distance from each other, would awaken renewed interest in the proceedings, due to the thoroughness with which the papers would be discussed, as well as the novelty of the plan. It is also evident that with the aid of the long-distance telephone, each section might be placed in communication with that at which the author was present, and thus secure satisfactory replies to any question that might arise. This is a simple suggestion in the way of meeting objections which have been raised to the absence of the author from meetings where a paper is read by proxy. As a matter of fact, however, the reading of papers by proxy is quite a common practice in all societies, although the presence of the author is desirable for many reasons.

It is well to understand that the popular idea of a successful meeting, is one where an audience is present sufficiently large to fill a room, although the actual proceedings may be participated in by a very small number. The normal audience of INSTITUTE members might be enlarged by the establishment of an informal list of "provisional members" or "auditors," who would be simply invited guests, and who might be permitted to take part in the discussion under certain restrictions. The names of these provisional members would not be placed on the INSTITUTE list, and they would not be entitled to the printed TRANSACTIONS, until they were regularly proposed for membership, and elected by the Council in the usual manner.

In cases where a local organization already exists, its membership might be considered as "provisional members" of the INSTITUTE or "auditors" as already suggested. The latter title or an equivalent would be preferable as not implying membership of any grade. Notices could be mailed to these either by the Secretary of the INSTITUTE, or the local secretary as might be most convenient.

QUESTIONS OF FINANCE AND POLICY.

There remains at least one question to be solved in connection with this plan. No provision has been made for the additional expense arising from the establishment of sectional meetings. It would appear proper that a small fee be required from provisional members, which would have a good effect in assuring them that they were properly entitled to attend the meetings. The amount of this fee would depend upon the local expense incurred. The question of meeting these expenses would necessarily be first in order, and a provisional plan should be devised at the outset. In the case of Chicago, the Armour Institute has offered to the INSTITUTE very desirable quarters for local meetings free of charge. No doubt similar arrangements could be made with other colleges, where advanced students could be placed on the unofficial list and given the benefit of experience in meetings of this character. The faculty of the Massachusetts Institute of Technology has recognized the necessity of such practice, and provides for the holding of meetings where papers are read and discussed in accordance with the practice of technical societies. There would be a decided advantage in giving these students' meetings a more practical character, and by possibly raising the standard of the meetings by actual participation in the proceedings of an established national society.

In the case of papers originating with members within the jurisdiction of any section, the regular procedure would be for papers to be submitted to the Committee, or the Secretary as is now the case, and if accepted, to be printed and circulated in the ordinary manner. The author could then be given the option of reading it at whichever section might best suit his convenience.

The standard of papers read would doubtless be fully equal and perhaps even higher than that already attained. This is a question however, that would be settled more satisfactorily by experience.

There is little doubt that the introduction of the practice of monthly meetings, has been of great benefit to the INSTITUTE, not only in the immediate vicinity of headquarters, but throughout the country. It has also been of great advantage to electrical practice generally. By increasing the importance of these meetings, in the bringing together of our members in various parts of the country, the INSTITUTE would attain still higher standing and the value of membership appreciated accordingly. The possibility of sectional jealousy will be reduced to the minimum, and any possible supremacy of one section over another will be due simply to the ability and energy of its members, rather than to their mere number.

THE PRACTICE OF OTHER ENGINEERING SOCIETIES.

The questions discussed in this paper have been brought forward in various forms in every national society ever organized in this country. They have been met in an imperfect manner by many of them. The American Society of Civil Engineers holds meetings twice each month in New York City, appointed by the Board of Direction. It also holds an annual convention at any point in the country which may be selected by vote. The American Society of Mechanical Engineers holds its autumn meeting in New York City, and its spring meeting elsewhere. Last year it took place in San Francisco. The American Society of Mining Engineers holds its meetings at various points in the country, three times each year. The American Institute of Architects is organized on the chapter system, holding local meetings and an annual convention of delegates. The writer has examined this system with considerable interest, but does not consider it adapted to the present needs of electrical engineers.

As was pointed out in the discussion of this question at the annual meeting, it had been ignored by the American Society of Civil Engineers until the local societies had in many cases gained such strength that they declined to be amalgamated with the national body.

As one instance of the importance of such societies your attention is directed to the Engineers' Society of Western Pennsylvania, which was incorporated in 1880. Its charter permits the admission of civil, mechanical and mining engineers, geologists, architects, managers and superintendents of railroads, mills and manufactories and other persons engaged in scientific and mechanical pursuits pertaining to engineering, either as active or honorary members. It holds monthly meetings and prints its transactions upon the same plan as the INSTITUTE, issuing temporary monthly parts and a volume made up from them at the end of the year.

Engineering Societies of America,

[Compiled from a Table prepared by Prof. J. B. Johnson of St. Louis, Dec. 21, 1892.]

Name of Society.	Date of Organization.	Total Membership all Grades.	Entrance Fee.	Annual Dues.	Number of Meetings Yearly.
National.					
Am. Soc. of Civil Engineers........	1852	1543	$30 00	$25 00 / 15 00	20
Am. Inst. of Mining Engineers ...	1871	2398	10 00	10 00	3
Am. Soc. of Mechanical Engineers .	1881	1552	25 00	15 00	2
Am. Inst. of Electrical Engineers...	1884	643	5 00	10 00	10
Total,		6136			
Local or Sectional.					
Boston Soc. of Civil Engineers	1848	302	10 00	6 00 / 4 00	11
Engineers' Club of St. Louis.......	1869	180	10 00	10 00 / 5 00	18
Western Soc. of Engineers.........	1869	428	5 00	10 00 / 7 50	12
Engineers' Club of Phila..........	1877	458	5 00	10 00 / 5 00	..
Civil Engineers' Club of Cleveland..	1880	155	5 00	8 00	12
Engineers' Soc. of W. Penna	1880	420	5 00	7 00 / 5 00	20
Denver Soc. of Civil Engineers.....	1882	70	10 00	12 00 / 8 00	25
Engineers' Soc. of St. Paul.......	1883	45	5 00	4 00	8
Engineers' Club of Minneapolis.....	1884	30	5 00	5 00	10
Technical Society of Pacific Coast ..	1884	250	5 00	12 00 / 6 00	12
Canadian Soc. of Civil Engineers...	1887	683	10 00	8 00 / 6 00	16
Montana Soc. of Civil Engineers ...	1887	54	5 00	10 00	12
Engineers' Club of Kansas City....	....	49	5 00	8 00	9
Engineers' Club of Cincinnati......	1888	128	1 00	5 00 / 3 00	12
Engineering Association of the South.	1889	122	5 00	8 00 / 5 00	9
Wisconsin Polytechnic Society......	1890	43	5 00	10 00	10
Engineers and Architects' Club, Louisville, Ky..	1891	74	10 00	18 00	12
Total,		3491			

Its membership is not geographically restricted as its title implies, but members are accepted from all parts of the world and number 540, a large proportion of whom are probably not members of any other society. This society has also permitted the establishment of what is known as "the chemical section" within its own ranks, and jurisdiction, being practically an independent organization, meeting on different nights and having a distinct set of officers and rules.

A table is given herewith showing the relative numerical strength of these various societies. It will be seen that all of the minor societies hold more frequent meetings than two of the largest national societies. This table is incomplete, especially as regards local electrical societies and clubs, and the membership statistics have not been brought down to this date. It is given as an instructive example of the tendency of the times.

There can be no doubt in the mind of any reasonable man that some plan should be devised by which this demand for local meetings can be provided for. How to attain this desired end; how to reach and interest distant members; how to make each member feel that he has value received in return for the money invested; how to make the INSTITUTE the leading technical society in the country; these are a few of the problems that your Secretary invites you to consider and assist him in solving.

MR. R. W. POPE:—As this paper has been presented in advance, and is now in your hands, instead of reading it in full I propose to utilize my time by reading a portion of the communications I have received, after which I would be pleased to have Dr. Emery open the discussion. I did not learn until yesterday that the American Chemical Society, which is not included in the printed list, had been reorganized on a basis of local sections, something as is suggested in the paper. Dr. C. A. Doremus, one of our own members, is on their committee on membership, and I sent over to him for a copy of the constitution and by-laws of the American Chemical Society. His letter in reply to mine is of interest.

Correspondence.

[FROM DR. C. A. DOREMUS.]

About two years ago, to meet the demands of chemists throughout the country, the American Chemical Society was re-organized on a basis very nearly like that of the British Society of Chemical Industry. As the society is now organized, there is a general council and officers elected each year at the general meeting, which, owing to the fact that the society is incorporated under the laws of the State of New York, is of necessity held in this city. There is also a board of directors. The voting is done by ballot and in part by proxy, but the chief point is that all sections of the country are represented in the general management. The society has now several local sections, the New York, Rhode Island, Washington and Cincinnati, and I think Brooklyn also. The meetings of these sections are, of course, held at the respective points, but the papers read are published in the journal of the society, of which Prof. Edward Hart, of Easton, Pa., is the editor. Of course, by this method it is impossible to have discussions of the papers presented, but after all, this does not seem to be so absolutely necessary. Since adopting this form of organization the society's influence and membership have greatly increased. As Chairman of the Committee on Admissions I signed to-day 33 applications for membership from chemists in every part of the country.

For the past year or so we have held one general meeting outside of New York. The Congress at Chicago was very successful. I regret very much that a previous engagement which I could not break prevents my being present this evening at the meeting of the INSTITUTE; but I am quite certain that any step taken in the direction of that which you propose will be followed by a vastly increased influence of this body, both in the science and practice of electrical engineering.

New York City, October 18, 1893.

[FROM A. J. BLOOR, SECRETARY NEW YORK CHAPTER, AMERICAN INSTITUTE OF ARCHITECTS.]

Yours of yesterday is before me. Having been active in the formation of the Chapter system, and in the correspondence leading to it, I can tell you, in general terms, that it was adopted after much discussion, as the best means of utilizing in the interest of the architectural profession and in the most effective way, the centralizing and separative—the individualistic and federalistic—in short the centripetal and centrifugal forces and tendencies which underlie, in various degrees of distribution, all human effort, in common seemingly with every department of mental and mechanical dynamics in the universe. Our chapter system is analagous to the political system prevailing between our Federal Government and the various states, and it is necessarily adopted in this country for all important fraternities, secular or religious, and for all large schemes of commerce and finance. I may add that our initiative has served, as an example, for several of the architectural brotherhoods of Europe; and that when several years ago, the Western Association of Architects was consolidated with our Institute, the Chapter system was perpetuated under the vote of a very large majority, notwithstanding the efforts of a faction more intent on personal aggrandizement than on altruistic practices.

New York City, September 26th, 1893.

[FROM MR. GEORGE HILL.]

The problem that is correctly stated, usually contains in that statement an indication of its correct solution. In the paper by Mr. Pope, we have a statement of a problem that has not only been affecting the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS, but every other national technical body, practically since their organization.

It is eminently right and proper that the headquarters of a national institution should be in some one of the great centres of population. At the same time it is a necessity that there should be in other of the great centres, and many of the lesser centres of population and especially in those smaller manufacturing towns where the industries are largely concentrated, many men of the same profession who are anxious to meet their fellow workers for the interchange of ideas, and the advantages which flow therefrom.

The history of the American Society of Civil Engineers, and the growth of associations of similar interest in different parts of the country not associated with it, indicate an unfortunate line of development, which may serve as a warning for the INSTITUTE. On the other hand, the number of papers read at the Civil Engineers and the discussions evolved thereby, show that the profession in New York is fully alive to the advantages flowing therefrom, and as a consequence, not monthly meetings, but semimonthly meetings were held and are well attended. The civil engineers, however, have but one meeting place, and the need for technical societies elsewhere has developed other local societies whose membership is 57 per cent. of that of the national societies. This may be taken to represent the alternative which will follow from a refusal of the INSTITUTE to sanction meetings in places other than New York. On the other hand we have the unsatisfactory nature of the Engineering Congress at Chicago, as indicating in a measure what might happen were the INSTITUTE to authorize meetings in different places for the discussion of papers simultaneously discussed in New York. That is to say the volume of discussion would be so great that it would be exceedingly difficult to avoid burdensome repetition in the discussion, and serious offense if each man did not have his discussion printed in full in the TRANSACTIONS, losing him as a possible discussor of other papers. A further disadvantage would be in the extremely limited number of papers which would be read during the year. In electricity, the field is so extremely wide, the various matters requiring attention so numerous, that there is practically no limit to the number of subjects available for papers that could be discussed with advantage, while the acquisition of data is so great, that in many cases yearly publication of papers on the same subject would be extremely interesting.

Should we then attempt to discuss at any one meeting all of the papers submitted by an active earnest membership, with a dozen different branches scattered through the country, we should have either a mass of discussion which would not be advantageous by reason of its repetition, or a single discussion of each paper.

While the plan adopted at the World's Congress was defective by reason of the fact that discussions in the various sections were going on simultaneously, it seems to me that it contains the germ of the solution of the problem, which would follow in the general lines laid down by Mr. Martin, and to which Mr. Pope calls attention on page 507. That is to say, the INSTITUTE would have a number of sections, but departing from the suggestion in other particulars.

The plan that I would advise would be :—

First. The establishment of a series of sections wherever a number of members of the INSTITUTE, say a dozen, were willing to meet regularly.

Second. The discussion by them at any one meeting of some one of the papers submitted for reading at this meeting as may be determined upon by a vote of the membership of the section.

Third. Should any member of a section desire to discuss a paper, which his section does not care to discuss, he can present his discussion to the New York section, where it would be embodied in the discussion printed in the TRANSACTIONS. The question of a meeting room would be simply solved, since in every town sufficiently large to have one dozen or more members of the INSTITUTE there would be some place for the advancement of science, which could be availed of at a nominal expense. For reporting the proceedings the members attending the section could no doubt arrange with a stenographer for what would be a nominal sum for each one of them, but a considerable sum to the stenographer to be present once a month for the purpose of a report.

The operation then would be, that papers would be submitted to the Council as at present, but it would be necessary to have the Committee on Papers divided into sections so that the papers submitted could be promptly passed upon. They would be sent out for publication two months before being read instead of one month as at present. This would give sufficient time for the preparation of a careful discussion where desired. It would also enable the membership to determine each month the paper that they desired to discuss next month, and then secure the concentration of any one section on some particular paper.

Concerning the subject of papers, it seems to me as though the sub-division adopted of pure theory, theory and practice, and pure practice, is in its fundamental principles good. Reviewing briefly the papers that have been presented during the past year, there are only two or three that have been of any practical use to me because my work is entirely with buildings, and my interest in electricity is that of using the most convenient of all possible forms for the distribution of energy. I cannot try experiments; I can only attempt to give my client those things that are sanctioned by the best of present practice. Therefore, the papers that interest me are those connected with the generation of current for lighting, heating, elevating, ventilating, etc. Methods, therefore of generation and distribution, possess a vital interest, and papers like Mr. Hering's, Mr. Marks', Mr. Moore's and others of a similar nature are of great use to me when brought to a practical issue. Papers however, on Hedgehog transformers and condensers, while of ultimate value as bringing into use methods which may prove economical in the future, are not of present and immediate value. Still, as I am only one of nearly 800, I cannot complain, because the interests of others must be considered. If however, we could have a dozen papers each month, there would no doubt be one every month that would be of value to me.

Another point that would be served by a subdivision would be an economizing of time. Such papers as those of Mr. Moore and Mr. L. B. Marks, are of very great value beyond a doubt. At the same time not having reached the commercial basis, they, I think, properly belong to the realm of theory, and were they classified as such, those circumstanced similarly to myself, would be saved the time necessary to read them, because the amount of reading that we must necessarily do in order to keep posted with those things that are of immediate value is so great as to be burdensome, and it is therefore very desirable to avoid adding to it by going through papers which, although intensely interesting, are not necessary for the successful prosecution of our work.

As an illustration of the desirability of allowing more time to elapse between publications of the papers and the meeting, I may state that through no fault of the committee I feel very sure, I only obtained the TRANSACTIONS on the morning of the 18th and by sending specially for it, a copy of Mr. Pope's paper on the 17th. This gave an insufficient amount of time to prepare a discussion thereof, which should be in all of its branches well considered, and this must be my excuse for not presenting my views in better shape.

In conclusion, while I do not entirely agree with any of the suggestions made, I am so convinced of the importance of carrying out some one of them, that no matter what can be agreed upon by members of the INSTITUTE, it may be assured of my hearty support. No plan that may be adopted after such discussion as has been heard can fail to be valuable, and it is only a question of degree that is involved.

New York City, Oct. 18th, 1893.

[FROM MR. CHARLES P. STEINMETZ.]

After returning from New York last month I reported to the next meeting of the Thomson Scientific Club that the sentiment of the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS was, in general, favorable to the establishment of local meetings in Lynn. In consequence thereof a Committee was appointed by the Thomson Scientific Club, consisting of the following members of the INSTITUTE: Mr. Chas. P. Steinmetz, Chairman; Dr. Louis Bell, Mr. H. F. Parshall, Mr. J. P. B. Fiske and Mr. Franklin Sheble, to investigate the feasibility of organizing a local branch of the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS in connection with the Thomson Scientific Club, also to draw up necessary papers for presentation to the INSTITUTE as an expression of opinion of the Thomson Scientific Club with regard to consolidation, and to lay the report before the Club before submitting it to the INSTITUTE. The Committee has not reported yet and so we cannot bring this matter before this meeting of the INSTITUTE.

I will say, however, that the sentiment of the Club is in general agreement with the views expressed in your paper.

If years ago the INSTITUTE had offered any inducement for local meetings probably most of the local organizations would not have come into existence. Now, however, where there are local organizations I think the INSTITUTE should recognize the existence of these organizations and not start meetings independently, but in connection with, or in consolidation with, such organizations—as here in Lynn with the Thomson Scientific Club.

The risk that the number of papers printed in the TRANSACTIONS would increase does not exist at all since anybody who wants to communicate a paper to the INSTITUTE does this anyway whether he is able to go to the meeting or not, and no paper read before the local meeting would be printed in the TRANSACTIONS which has not passed the Editing Committee of the INSTITUTE in the regular way. Naturally many papers read before the local branches will not be communicated for printing in the TRANSACTIONS. At least in local organizations like that in Lynn which does a considerable work of an educational nature, many papers are read which are valuable and of interest to the hearers, but are not intended by the author for publication in the TRANSACTIONS. These papers would neither be sent in to the Editing Committee nor printed in the TRANSACTIONS; and papers worth printing in the TRANSACTIONS written by the members of the INSTITUTE are I hope at least, even now not withheld from the INSTITUTE by the absence of the author, so that I cannot see how the size of the TRANSACTIONS could possibly be increased by having the local meetings.

It will hardly be necessary (in many cases not even practicable) to have the local meetings simultaneous with the New York meeting. A communication between simultaneous meetings will be generally excluded anyway by the time difference. For instance when the Chicago meeting starts a discussion the New York meeting is probably ready to adjourn.

Besides, local organizations having social and educational tendencies will frequently have more meetings,—the Lynn organization, for instance, has during the season two meetings per month. These meetings may continue in the same number as the local meetings.

Lynn, Mass., Oct. 17th, 1893.

[FROM MR. F. A. BOWMAN.]

I have read with great interest your paper on "Monthly Meetings" and entirely agree with you as to the importance of bringing the work of the INSTITUTE more forcibly to the notice of the members distant from New York. With the membership covering the area it now does, a comparatively small number can take personal part in the discussions and a great number are simply subscribers to a technical periodical. A reference to the by-laws of the Canadian Society of Civil Engineers will show a complete scheme for the formation of branches and the discussion of papers at different centres. One branch at least has been formed under these regulations. If some scheme of this kind for "sections" or "branches" were granted I think it would materially add to the number and variety of the papers offered. Members would take a more personal interest in the INSTITUTE, and would write papers on the various problems they would have to meet and overcome in their work. The number of theoretical papers would not be less and the number of practical ones considerably increased. As all papers would have to pass the Council before being read, only those of real value would come before the INSTITUTE. If I understand the objects of the society aright there is room in the TRANSACTIONS not only for the splendid theoretical papers which do appear there, but also for papers on the practical questions encountered by electrical engineers every day. Why should we not have a paper from Mr. Blank on the plant he installed in such a mine, the difficulties he met with and the means he used to overcome them, with a few figures of costs. Or another member has erected plants all over the country, could he not give us a short paper on "poles," what woods he found most suitable in different localities, their relative life, etc. A glance at a years transactions of the Institution of Civil Engineers shows that they contain not only large and important papers by leading engineers, but also many smaller ones describing engineering works or giving a few notes on some material used. It seems to me if the INSTITUTE is to take and hold the foremost position in the electrical world that it is entitled to, that the scope of its papers must be widened, and that one of the best means to this end is to increase the personal interest of the members by bringing the papers and discussions nearer home to them in some such way as has been suggested.

New Glasgow, N. S., October 24, 1893.

[FROM MR. A. E. KENNELLY.]

Many thanks for your letter of yesterday and for the paper accompanying it, which I have read with interest. Here is a scheme that appears feasible to me, and I wonder how it will appear to you:

When, say more than 15 members are resident in any city, an appointment on application can be made of one of them as Local Honorary Secretary. To these local honorary secretaries the INSTITUTE can provide advance sheets of the papers, to the desired number, this closing the attitude of the INSTITUTE to those cities. If the members in Cincinnati say, receiving these advance sheets, like to meet together and discuss the papers, why so much the better; but their elections to the chair, their actions and resolutions will be entirely their own and in no way officially connected with the INSTITUTE. Written discussions upon the papers forwarded to the Secretary of the INSTITUTE, either directly or through the local honorary secretaries, would receive the attention of the Editing Committee, but they would be received as emanations from the members and not from the local union association, gathering or chapter which they might form.

This plan would enable distant members to participate in the benefits of the papers and meetings of the INSTITUTE, but would prohibit the possibility of any official action in any chapter detrimental to the interests of the main body or headquarters.

Orange, N. J., October 17, 1893.

Discussion.

The President:—We would be pleased to have Dr. Emery open the discussion.

Dr. Emery:—Mr. Chairman, in a previous discussion of this subject various difficulties were pointed out, one of the most important of which was that referred to by Prof. Houston, our present President, as to the qualifications of members of the local societies who would naturally wish in case of consolidation to be called members of the Institute. Some of the difficulties which have been referred to will be overcome by the suggestion in the paper that the out-of-town meetings will simply be sections of the meetings here and discuss the same papers. There are, however, other suggestions in the paper which I wish to criticise for reasons to be stated, and finally offer a substitute for consideration The paper speaks of having the local members who were not members of the Institute known simply as "auditors." It is believed that after a little consideration it will be seen that the local societies will wish some recognition, at least among themselves, of an entirely different character. There must be positions for the leaders of the local ruling classes rewarded by the customary evidences of local respect which keeps all organizasions alive. To accomplish this without infringing on the rights of our own members, it is in my opinion necessary to encourage rather than discourage the formation of local societies, which may have to a certain extent their own aims and purposes, social and professional, under the control of their own officers and yet regularly hold meetings as chapters of the Institute under such rules as the Institute may prescribe. Without taking the time at this late hour to fully state all the considerations, I proceed at once to read a draft of some resolutions I wish to submit for reference in the proper direction, and which I trust will aid in directing the discussion to the details of a desirable plan instead of to the more general considerations of advisability on one hand and the difficulties to be encountered on the other My suggestions are as follows:

Skeleton of Resolutions Designed to Authorize the Formation of Local Chapters of the American Institute of Electrical Engineers.

1. The Council may in accordance with the several terms of these resolutions, upon proper application, grant privileges to form local chapters of the Institute to hold meetings as sections of those of the Institute, and to read and discuss papers presented at the regular meetings of the Institute held in the city of New York.

2. A local society is to be formed preliminarily with at least 25 members with a name distinctively different from that of the Institute, preferably that of the——Electrical Society. Such local society shall have as members at least 10 charter

members of the INSTITUTE, and such society shall through a committee formed of a majority of charter members of the INSTITUTE make application to the Council of the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS, for proper authority to form a chapter under the conditions of these resolutions. Societies already formed with a name and membership in accordance with the above requirements may also make application the same as if formed for the purpose.

3. Before making the application the local society is to organize under its local name and pass a constitution and by-laws providing for the conditions herein required and which are in other respects to be satisfactory to the Council of the INSTITUTE.

4. The fees prescribed for such organization to be separated into two classes, (*a*) a specified amount for the support of any local work of said organization, which is to be prosecuted independently of the work of the INSTITUTE; (*b*) a specified amount (but little less than that required for INSTITUTE membership), which is to be transferred after the chapter is formed to the Treasury of the INSTITUTE. A part of the latter to be applied to the publications of the TRANSACTIONS of the INSTITUTE and the remainder returned to the credit of the local treasurer to be used under the direction of the local board to pay the expenses of the chapter so far as the same are chargeable on INSTITUTE account. The local fund to be drawn upon for all matters not strictly relating to the work of the INSTITUTE.

5. The membership of the local body shall consist of three classes. (1) Local charter members of the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS who shall also be entered as members of the local society and pay class (a) dues. (2) Associated members of the local society or those who pay a sufficient sum into the local fund to entitle them to the local privileges as well as to the TRANSACTIONS of the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS and the right to participate in the discussions. (3) Members of the local society who shall have the privilege of attending its meetings, but shall have no right to participate in the published discussions of the INSTITUTE papers or to copies of such papers.

6. The Council of the INSTITUTE after inspection of the petition, the constitution and by-laws, the list of members and the representations of the committee, may in their discretion issue authoriiy to such committee to form a chapter in accordance with the terms of these resolutions, to be known as the ——— Chapter of AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS. The chapter to be organized with a local president, vice-president and secretary, all of which shall be charter members of the INSTITUTE. After the first year a vice-president of the society shall be elected as the local president.

7. The local officers of the INSTITUTE thus appointed shall report their appointment to the Council and thereafter meetings

of the chapter may be held locally under the direction of charter members of the INSTITUTE subject to the direction of the Council of the INSTITUTE; the papers read at the meeting of the INSTITUTE may be read by proxy in the local chapter and discussed. and a report of the discussions sent to the Secretary to be embodied with that of the INSTITUTE meetings and of other chapters. When the associated members of the local society take part in the discussions they shall be distinguished in the printed discussion as members of the local society.

8. The Council to have power to make preliminary rules and regulations necessary to carry the spirit of these resolutions into effect and to submit from time to time for the consideration of the INSTITUTE such further resolutions as in their opinion seem necessary.

Gentlemen, there are a great many reasons for the various provisions I have made which I cannot state at so late an hour. I will only call attention to the fact that the chapters must be self-sustaining and cannot be a burden on the membership at large. I therefore provide that the local dues which entitle certain of the local members to copies of the proceedings of the INSTITUTE and the right to join in the discussions must be nearly as large as those paid by INSTITUTE members. This should have the effect to bring the treasury balance on the right side. It is believed also that it will have the further effect of increasing the regular membership of the INSTITUTE, for such of the members of the local societies as are qualified to become members of the INSTITUTE will naturally prefer to pay a little higher dues than to be simply "associated members" of the *local* society and so distinguished in the reports of the discussions. If those who have taken the pains to examine the various objections that have been made will also examine carefully the resolutions I now propose, I think much will be found which will prove of value in arriving at a solution of the problems, and without further remarks I hand my suggestions to the Secretary.

MR. WEBB—I should like to make two or three remarks before we close and to introduce a resolution. This question appears to be approaching the condition of what we might call the "silver bill" of the INSTITUTE. It has been discussed already many times in Council meetings, and members of the Council individually have thought over it and considered it. It has been brought up at the general meetings and also at a special meeting in Chicago, and on that occasion I dare say you remember that Mr. Preece, one of our honorary members, spoke on the subject, and the gist of his remarks was an argument for centralization. We are rather apt to look abroad for precedents in settling difficult points in the conduct of our scientific societies, because the societies over there are much older than most of ours. But the conditions in this country are entirely different, and in a great many cases we cannot be guided by foreign practice; this is a case where we have got to meet the conditions that confront us, without any regard to precedents. I think it is quite natural that where there are a number of members of the INSTITUTE in a city which is so far away from the headquarters that they cannot attend the meetings, that they should desire to hold meetings themselves, and that as they are members of the INSTITUTE they should want to hold those meetings in the name of the INSTITUTE. It seems to me a very natural desire, and at the same time it is equally natural that here, in the calm climate of headquarters, we should see a great many dangers and difficulties in the way of those meetings being carried on. There are a great many practical difficulties that occur in the handling of such meetings. Dr. Emery's very carefully thought out scheme providing for chapters, points out one of the dangers right away—that we may have so many chapters that they will burst our binding. We may have so many chapters that in a very short time they will break up the INSTITUTE, and that is just the risk that we want to avoid most scrupulously. I think this subject is one that requires very thorough consideration and very deliberate action on the part of the INSTITUTE, and I will just read this resolution that a few of us who take great interest in the matter have drafted, to see if it meets the approval of the members present:

Whereas, Requests and suggestions have been received from members in various parts of the country looking to the holding of local meetings for the reading and discussion of papers; and

Whereas, It is the sense of this meeting that any measures proposed with a view of increasing the usefulness of the INSTITUTE to its distant members should have earnest consideration and careful action; it is hereby

Resolved, That the President be requested to appoint from the Council a committee to consider the subject of local meetings, such committee to be supplied with copies of all correspondence and papers on the subject in the files of the Secretary, and that the Council report as early as possible to a monthly meeting of the INSTITUTE: first, upon the expediency of local meetings: and second, if they deem such meetings expedient, to recommend a suitable plan for the organization of local meetings and for their proper relation to the general body.

MR. PHELPS:—I move the adoption of that resolution. And if I may be permitted at the same time, move that the very suggestive paper of Dr. Emery be referred to that committee for consideration.

[The motion was carried.]

THE PRESIDENT:—Of how many shall it consist?

MR. WEBB—That is left to the discretion of the chair.

MR. PHELPS:—I move that it be made five.

[The motion was carried.]

THE PRESIDENT:—The Chair appoints Mr. Webb, Mr. Phelps, Mr. Hammer, Mr. Kennelly and Dr. Pupin.

[Adjourned.]

A Lecture given at Columbia College, before the New York Electrical Society and the American Institute of Electrical Engineers, May 17th, 1893; read by title before the Institute, September 20th, 1893.

A BRIEF GLANCE AT ELECTRICITY IN MEDICINE.[1]

BY DR. W. J. MORTON.

It is, I confess, with considerable trepidation that I, a physician, touch upon electricity before an audience whose life work this subject is; but if there be one reason more than another why I esteem it a high honor and a privilege to address the members of this society upon the subject assigned to me, that of electricity in medicine, it is that I look upon the occasion as one which may serve to interest electrical engineers—experts in electricity—in the efforts which medical men are making, in these modern days of electrical science, to ascertain the possibilities of electricity to cure disease. Our certainties we ever have with us; they are a legacy from the fathers of science; it is the limitations that can be set to our knowledge that we would ascertain. And if by chance these limitations, like the horizon of an otherwise boundless plain, ever keep in advance of our pursuit, we have, nevertheless, left behind us an abiding knowledge of the new ground we have traversed.

In one sense a similar problem continually confronts the members of your own profession; you have your grand dynamos and the laws of economical distribution of power and light; you have your telegraph system, already pretty well worked out to its limits; you have your subtle telephone and telautograph, but you have also a thousand and one problems, whose limitations have yet to be determined. You have yet to run a train one hundred miles an hour; yet to distribute power and light on a Teslaic system without conductors; yet to make a practical telephone talk out loud; yet

1. By the action of Council in ordering the publication of this paper, the original Editing Committee, George A. Hamilton, *Chairman*, Franklin L. Pope and Francis B. Crocker, is relieved of all responsibility in the matter.

to communicate thought by electro-magnetic waves alone, and yet to tell us what electricity is. You have yet to ascertain what you can do by electricity and what you can not do.

But, your pursuit of truth, evasive as this ever is to all, is upon far easier lines than is ours as physicians, and therefore we ask your forbearance and your aid.

You deal with science—with electricity—applied to inanimate things. While the general laws of this science are open to all of us equally, you apply your laws to masses of metal, to wires, to carbon and to inorganic chemicals; you express your applications or your theories in unerring mathematical formulæ or in graphic representation; the work you claim to have accomplished may be verified by your peers, by aid of cunningly devised instruments of precision.

Not such is the position of the physician. While if highly gifted and accomplished, he may possess your knowledge of the physics of electricity, in his applications of his knowledge he is confronted with a problem the most perplexing that has ever been presented to the attention of mankind, and that is the problem of the laws which govern life in animate objects—the problem of biology. Perversion of these laws constitutes disease, and the physicians query is, can electricity avert and correct this perversion?

Electro-physics is indispensable to electro-therapeutics. The representative of the former is the electrical engineer, as is the physician the representative of the latter. What greater glory to the engineer than to turn at times from commercial and industrial labors, and direct the light of his knowledge in upon the highways and the byways where dwell the sufferings and sorrows of humanity.

Section I.

Electricity presents itself to physicians for medical use in three conventional modalities, termed respectively: galvanism, faradism and franklinism. The electrical engineer knows no such terms; to him electricity differs mainly in volts and amperes. And, practically, we also work upon the same lines. But time-honored uses and convenience justify us, I think, in retaining for the present our crude classification, since our apparatus consists almost entirely of voltaic cells, volta induction coils and electrostatic machines.

From the galvanic battery we obtain a high current strength and low voltage; the current is continuous or constant, though often used also as an interrupted current. Its effects upon the human tissue are mainly electrolytic and cataphoric—the tissue is affected chemically and physically, just as inert matter would be, and, in addition, is affected physiologically; the thousandth part of an ampere, viz., a milliampere, is the practical unit, and therapeutic dosage is expressed in terms of n milliamperes, passing for n^1 minutes. Current strength is measured by a milliampere meter. Administrations are percutaneous, and through the mucus membrane, or by needle puncture. In the former case, the resistance offered by the skin is high—varying from one thousand to forty thousand ohms—and we require from forty to sixty high voltage cells to overcome it. Two important features come into play, one current density, which is a question of the size of the electrode, and the other the practical fact that the degree of pain excited in the nerves of the skin with accompanying injury to the skin erects the real barrier to increasing the current strength. Ten milliamperes to a square inch of skin is about the limit of endurance, and from ten to twenty to two square inches an average percutaneous current. Current density is as essential an element of expressing the dosage as is the reading of the needle, and the superficial area of the electrodes should always be given.

From a faradic apparatus or induction coil, we obtain a current of much greater electromotive force, but of decreased current strength. The salient feature of this current is that it is an interrupted current; from the primary winding of the coil is obtained a pulsating current—its flow is always in one direction, thus resembling an interrupted galvanic current, plus the current of its self-induction, and that of the magnetic induction of the iron core. From the secondary winding is obtained an alternating current, whose electromotive force may be very high. An instrument to measure this current is greatly to be desired.

According to Harries and Lawrence an ordinary medical induction coil with seven volts E. M. F. supplied to its primary and a current strength of 500 M. A., gives in its secondary a voltage of about 216 and only 12 M. A.

From a franklinic apparatus or electrostatic machine, we obtain a current of enormous voltage and very low current strength. The physical and physiological effects are largely due to electromotive force and instantaneousness of current flow rather than to current strength.

There is still another form of electric manifestation of energy, which is a newcomer to electro-therapeutics, in fact, in its present guise a newcomer to electrical science—I refer to high frequency, high potential currents.

They fall out of line of our medical classification, for they are neither faradic nor franklinic alone—their production requires Ruhmkorff coils or alternating current dynamos, condensers and transformers; they far transcend our volta induction coil products in frequency of interruption and electromotive force, and our franklinic machines in current strength.

The fame of the high frequency, high potential current is already invading the quiet domain of electro-therapeutics, and it is to that branch of our subject that I shall ask some attention to-night.

From the great initial difference in voltage, amperage, and absence or presence of interruptions of the three forms of current in medical use arises a corresponding difference in their physical and physiological effects upon the human organism, and from a study of these differences the physician must decide upon which form of current to use in given cases.

To galvanism and faradism we will give but prefatory attention; but to franklinism fuller consideration, since its study is inextricably entwined with high frequency, high potential currents—in short, it is itself of this nature, and as such invites a vivid interest, for we have as yet no other apparatus for obtaining these currents in medicine.

Section II.

In confronting our subject of electricity in medicine, it is necessary to establish some data, and this we may do most briefly. We have to consider:

(*a*) The nature of the agency, electricity, we wish to apply.

(*b*) The nature of the gross substance, the human body, to which we wish to apply electricity, and the reactions of electricity upon it.

We are then in a position to examine the principles, the instruments and the methods of application, viz., the science and the art of electro-therapeutics, or, at least, that small part of it which may be examined in the brief time before us.

Electricity.—Electricity to-day is believed to be a vibratory or wave motion of the ether, that continuous, imponderable, and incompressible fluid that fills all space. By a state of motion either in it or of it, the ether conveys light and manifests the phenomena termed electric and magnetic. In this sense, that they are motions of the ether, light and electricity are identical —both are ethereal vibrations—both travel at the same rate of speed, and both have measurable wave lengths—both may be refracted and reflected by impact upon prisms and mirrors, and both are subject to wave interference. In submitting the human body to electricity, therefore, we are submitting it to a physical force, to a rate of motion of the ether—not to an entity or individual chemical element, like iron, arsenic, quinine or other medicine. We treat disease by ether vibrations. There is but one organ of the body capable of responding specially to ether vibrations, and this is the eye, just as the ear responds to air vibrations. The rest of the human body is affected by the electric ether vibrations just as is other gross matter of the same sort. That it is a form of energy that we apply to human beings may be illustrated by a crude parallel. Supposing one has given a quick snap by the hand to a rope attached to the ceiling of a room and imparted to the rope a wave motion easily visible; we have the exciting power, the hand, the electromotive force; we have the wave propagation, the rope, the current; and we have the energy of the imparted motion now exerted upon the ceiling, or, if attached to proper mechanism, capable of doing mechanical, chemical or other work. This is the sort of work that is done in the human body.

The importance of taking this fundamental view of the effect caused by ether vibrations is obvious, especially in connection with high potential, high frequency currents; but, to more im mediately bring the general subject down to a working basis, we may start from a different standpoint, that of:

The Properties of Currents.—In using electricity to cure disease, the physician may be said to resemble a huntsman, who has a certain number of arrows in his quiver, each adapted to different purposes. He selects an arrow—a property of the current—some special thing it will do, and projects it against a disease or morbid condition. At least, this is what he should do, but it is unfortunately a fact that physicians are wofully ignorant about what electricity will do upon inanimate matter, and more so upon what it will do upon animate. The general impression seems to be that electricity is to be injected into a human being by a small sponge electrode, much as one would inject a medicated fluid in through the skin by a hypodermic needle, and that the electricity was then to run about the system, hunt up the diseased spot, make a diagnosis, and chase out the disease, much as a ferret might run a rat out of a hole. I am not sure that this is not the idea of the electrial engineer.

The properties of the current useful in diseases are:

A. Physical.
1. Heat.
2. Light.
3. Magnetism.
4. Induction (electro-magnetic).
5. Chemical, or electrolytic.
6. Cataphoric.

B. Physiological.
1. Contraction of protoplasm.
2. Excitation of nerve and muscle.
3. Electrotonus.

1. Heat may be employed indirectly to heat small metallic loops, with which surgical operations may be performed. This is known as galvano-cautery.

The actual heat produced in the body by its resistance to currents of the current strength commonly in use is inappreciable.

The franklinic current modifies bodily temperature, according to my experiments, in a marked degree, probably by its action upon thermic nerve centres.

High frequency, high potential currents raise the temperature of the body.

2. Light may also be used indirectly in incandescent lamps to illuminate the cavities of the body—to transillumine the tissues, and thus detect foreign bodies, or dead tissue; it has also been successfully used to cure neuralgia. The intense light of electric welding processes produces effects upon the skin which have been compared to those which are induced under the scorching of a torrid sun. The effect is, however, more far reaching than this explanation admits of, for those subjected to this intense light suffer from discharges from the nose and eyes, and from a dry cough, as well as from skin symptoms.

3. The magnetic property of currents is also indirectly made use of to actuate mechanism, like surgical drills and saws, and to cause mechanical vibration, transferred to tissue.

Magnetism, itself, whether from permanent magnets, the most powerful of electro-magnets, or from solenoids, seems to have little or no important effect upon human tissue. The wonder is that it has ever been expected to have such a great effect, since no one would select the ingredients of a human body as suitable ones to collect the electro-magnetic lines of force. I do not, however, accept as final, the experiments of Kennelly and Peterson, for the reason that closed conductive circuits were not provided for in their experiments, and that alterations in the excretion of carbonic acid and urea were not looked for.

4. Induction effects, due to the passage of currents through the body have received little attention. It must be supposed that there are a very great number of closed inductive circuits.

5. Chemical or electrolytic. This is one of the most important properties of the current, and in connection with it, a passing glance must be given to the nature of the electrolyte. The human body, for practical purposes of the administration of electricity, may be considered as a semi-fluid mass, whose conducting power is due to its salts, held in solution. As a conductor of electricity, it is, practically, about a two per cent. solution of common salt. It is at once evident how important a part electrolysis, or chemical decomposition, and cataphoresis, or electric transfer of fluids must play in this human electrolyte.

Representing by 1 the conductivity of muscle, it is ordinarily stated that the resistance to the current is as follows:

Muscle	1.
Nerve	2.5
Cartilage	2.5
Bone	6.
Skin deprived of epidermis	100. to 500.

Starting upon the fact that tissues behave as conductors like a solution of the salts they contain, I should prefer to arrange a table of the important tissues, as follows:

Blood	1.7 %.
Muscle	1.5 "
Brain and nerve tissue	1. "
Cerebro-spinal fluid	1. "

the resistance increasing as we read downward.

Obviously, by the law of derived currents, each component of the mass will receive its ratio of current, some more, some less, according to their conducting power, but none escape, and there is therefore, by the way, no evidence as has been recently claimed by a French observer, that electricity cannot reach the brain or spinal cord, because its flow would be completely diverted by the blood and the muscles. No one tissue of the composite mass will take all the current.

The simplicity of my comparison of the conducting capacity of the tissue to a two per cent. solution of the chloride of sodium requires some modification. Our electrolyte is not homogeneous like such a solution, but it is composed of millions of cells with membranous enveloping material, and of varying physical composition. We must, therefore, take into account the distribution of ions to secondary electrodes within the mass, constituting internal polarization and active chemical decomposition within the mass itself, as well as at the two principal electrodes. The salient and interesting feature I wish to bring out is, that the action upon tissue is electrolytic, that this electrolysis takes place in the intrapolar region as well as at the ministering poles, and that the electrolysis is at the expense of the inorganic salts of the tissues. Now these salts constitute but from one to two per cent. of every tissue, and yet they are absolutely essential to the health and the life of the tissue. And we may, hence, see how it is that a comparatively small current strength, even of a few milliamperes, conducted electrolytically by the solutions of inorganic salts, decomposing and diminishing them in amount, produces the profound effects seen in electro-therapeutics, for the function of the tissues depends upon its physical integrity, and it is its physical integrity which suffers by the passage of the current. In further proof of the reality of electrolysis, human tissue may be charged like an accumulator or secondary battery. Larat has demonstrated that when a charging current of about 25 milliamperes is applied by two electrodes to the arm, for instance, for some minutes, the charging battery may be removed, and a current in the opposite direction be detected of from ½ to 1 volt.

Experiments upon animals demonstrate the profound structural changes set up in tissue by the passage of the current.

G. Weiss, taking a healthy frog, submitted one leg to a few milliamperes of current for several minutes, and placed the frog back in the aquarium. At the end of a week the frog's legs were tested to determine their muscular excitability. The leg not submitted to the current gave contractions from ten to twenty times greater than the other. Again, having submitted the legs of a living frog to electrolysis, he killed the animal a month later, and discovered under the microscope that the muscles were completely altered. He says that recent experiments have shown him that a single application for five minutes of a current of two milliamperes suffices in the case of frogs to produce alterations visible by the microscope several days afterwards.

We may make a distinction between intrapolar electrolysis, as thus far described, and polar electrolysis. The former is medical, the latter is surgical. The former does not disrupt and destroy tissue, the latter tears tissue asunder and destroys it with acids and alkalies; it is an action directly at the electrode, which is commonly a metallic needle or needles, and its purpose is to destroy tumors and other diseased growths. In this surgical destructive procedure, termed often "electrolysis," or galvano-caustics, each pole forms a little chemical laboratory by itself, where the human tissue itself provides the chemicals for its own destruction. At the positive pole collect oxygen, and chlorine; at the negative, sodium, and potassium hydrate and hydrogen. Nascent oxygen directly oxidizes tissue, chlorine attacks the H_2O of tissue, takes the H_2 and leaves again nascent oxygen to oxidize them. Muscle tissue electrolysed gives sulphuric, hydrochloric, nitric and phosphoric acids at the positive pole, and sodium, potassium and ammonium at the negative pole.

Metallic electrolysis. Some recent advances in the application of electrolysis to medical practice are of great value and deserve to be better known. One of these I now have in mind may be termed metallic electrolysis of soluble metals. Long familiar as a feature to be avoided, or mentioned by authors in a desultory way, Gautier, of Paris, has seized upon this idea to erect upon it most successfully a method, the rationale of which is quickly appreciated.

An iron needle connected to the positive pole, plunged into human tissue, is quickly attacked by the oxygen and chlorine set free at this pole—is converted into oxychloride of iron, a double salt, and is soon completely destroyed. The same is true of any metal attacked by oxygen or chlorine. A copper needle or bulb or other conveniently-shaped electrode penetrating tissue or laid against mucus membrane, especially within the cavities of the body, is converted into oxychloride of copper, zinc into oxychloride of zinc, and so on. Gautier terms these electrodes "soluble," in contradistinction to electrodes of gold and platinum, frequently employed to avoid these very effects. The benefits of this method promise to be far-reaching when fully introduced into practice. In gynæcological work the positive copper sound introduced within the uterine canal affords not alone the intrinsic advantage of the current, but also the further benefit due to an antiseptic salt whose permeation *into* the tissue sets up most active alternations in the nutrition of the mass. Usually, a solution of such metallic salt is painted or sprayed upon the mucus membrane, but by Gautier's method it is first topically applied, and is then driven inwards by the principle of cataphoresis—for, by happy chance, the decomposition of the metal and its propulsion inward are vested in the same pole, the positive. The same reasoning holds good in nasal catarrh or rhinitis atrophica and hypertrophica and ozoena. A copper bulb swept over the nasal, throat and mucus membranes deposits and drives in the copper salts. The results of this treatment, in my hands, at least, far surpass the most sanguine claim of any other treatment. By this means, also, gonorrhœa may be promptly cured, unhealthy ulcerations and discharging cavities may be antisepticized and healed, tumors be dispersed or caused to disappear, and the granular eyelids of conjunctivitis to assume a healthy state. The method has before it a brilliant future.

And, finally, we cannot close our leaflet upon electrolysis without a word in regard to one of the greatest extensions of electricity into the domain of medicine, that of the treatment of diseases peculiar to women, by the methods devised and advocated by Apostoli, of Paris. The labors of Apostoli have worked a revolution in gynæcological practice. By means of a dispersing pad electrode, whose current density is large, applied externally on the skin, and a small metallic electrode, whose current density is small, applied internally, Apostoli has been able:

(*a*) To apply currents of great current strength internally, viz., 50 to 500 milliamperes.

(*b*) To obtain internally the special action of either pole, the positive to control hemorrhage and to dry tissue; the negative to liquefy, soften and increase blood in tissue.

(*c*) To obtain a marked intrapolar effect (electrolytic, cataphoric and vascular) upon fibroid tumors, exudations and morbid growths.

These and other effects indissolubly associated with Apostoli's name have done more than any other modern work to place electro-therapeutics upon a sound basis.

I feel that I have scarcely touched upon the role of electrolysis in medicine; its proper elucidation would require a volume.

Cataphoresis.—Next to electrolysis, cataphoresis is undoubtedly the most important property of the galvanic or continuous current. By cataphoresis is meant the flow of a liquid with the current from the positive toward the negative pole. Physically, cataphoresis is demonstrable by many simple experiments. If a fluid is put in a U-shaped tube and electrodes enter the fluid in each arm, the fluid rises on the negative side. If a porous diaphragm is inserted in the tube, the fluid which is driven to the negative end cannot again descend by gravity, and soon gathers in large quantity in the negative compartment. If the fluids on either side of the diaphragm differ in saline density, then by the laws of chemical osmosis there is a flow from the less dense to the more dense compartment. In this instance, if a current is in the same direction as the osmotic flow, the latter will be greatly accelerated; if in the reverse direction, it will be retarded. Cataphoresis does not readily take place if the resistance of the liquid is too small or too great. Human tissue is well adapted to cataphoric action. Fluids flow to the negative pole and remain there, owing to the membranous character of the meshes of tissues which enclose them, and which resemble in this respect the porous diaphragm. Like electrolysis, cataphoresis is inseparable from every application of the continuous current. In inflammatory exudations of all kinds, in rheumatic thickenings of tissue, in tumors like fibroids of the uterus and others, the fluid in the mass is increased by the negative pole, thus creating a physical change, while by electrolytic decomposition the same diseased tissue is impoverished in the salts so essential to its vitality.

Cataphoric Medication.—The cataphoric property of the current renders available, also, a very interesting practice of introducing medicines into the human body and blood stream through the skin or mucus membrane. The process is termed cataphoric medication. In this manner iron, mercury, quinine, morphia, cocaine and a further great variety of medicines may be caused to affect the patient without entering the stomach. The practice is of great value to produce local anæsthesia by cocaine, and also in many cases, as in skin diseases, where a potent local effect of a medicine is desired. There is no good reason why the "medicated bath" should be an adjunct of quackery. If the medicated water of the bath is made the positive electrode, a patient in this bath, with hands and arms out of it, and in connection with the negative electrode, will receive, by cataphoresis through his skin, a large amount of almost any desired medicine, and this, too, in purest and most active state of the medicine.

The reserve statement is interesting, and that is, that by cataphoric demedication, medicines like, for instance, arsenic and mercury, may be removed from the patient, and be visibly deposited upon the negative electrode, according to the usual rules of electroplating. In passing, we may for a moment indulge our imagination in a future practice in cataphoric medication or conveyance of, say, metallic substances into the body, which might constitute a new method of preservation of the bodies of the dead. Electrically permeated throughout its mass by an unoxidizable metal or metallic salt, the body would remain most lifelike, as well as practically imperishable. We may thus electrically, in truth, transform Crœsus into his own gold—a sort of electrical petrification.

Electrolysis and cataphoresis are therefore prime physical factors in electro-therapeutics.

It is evident that we cannot alter the physical substratum of a living organism without in equal ratio allowing its function. Modern physiology, or the laws of vital function, concedes that vital phenomena are chemical changes subject to the conditions of similar changes in the chemist's laboratory.

To pursue this part of our subject further would be to enter upon the vast domain of electro-physiology, and this time forbids. I can only point out the general pathway of the

PHYSIOLOGICAL PROPERTIES OF THE CURRENT.

Muscular Contraction.—The gross physiological phenomena with which we are familiar is that muscles are caused to contract. If the electrical impulse is instantaneous, the contraction is instantaneous, and immediately subsides. Up to about 20 impulses per second this rule holds good, but beyond that the muscle has not time, between the impulses, to subside from its state of contraction, and remains in permanent contraction so long as the varying current continues to flow. The excitation of the muscle is primarily effected by means of the nerves which supply it, but it may also be caused to contract independently of its nerves. Using a galvanic current, muscles contract more or less quickly to single impulses, according to which pole is used, and according to whether the circuit is made or broken. Employing the symbol C for cathode, c for closure, c for contraction, O for opening, and A for anode, the relative order of contraction, as established by Pflüger is:

$$C c c > A c c > A\,O c > C\,O c, \text{ viz.:}$$

cathodic closure contraction is greater than anodic closure contraction, which in turn is greater than anodic opening contraction, which likewise in turn is greater than cathodic opening contraction. This amounts to saying that neuro-muscular excitability is most marked at the negative pole. But that the law is not comprehensive, is shown by the fact that currents several times reversed excite muscular contractions far more actively than a negative pole does.

The muscles thus far referred to are made up of striped muscular fibre; they constitute the bulk of the human body. And while they contract to a varying current as outlined, they do not contract to a continuous or unvarying current, except it be very powerful.

Opposed to the striped muscular system, there is another composed of smooth, muscular fibres, which makes up the heart, the uterus and other important organs, and exists in the intestinal walls. This system contracts powerfully to a continuous current, and not so readily to a varied current.

And this distinction leads to a very important differentiation in electro-therapeutics, and one which is scarcely yet recognized in practice. Intestinal movements, for instance, in dangerous or other cases of occlusion and paralysis of the bowels, will be best set up by a continuous current, which would not cause a contraction of the biceps or other muscle composed of striped muscular fibre. The reduction of this physiological fact to a practice we owe to Boudet de Pâris.

Nerve Excitation.—The nerves of the body are mainly of two kinds; the motor, which convey outward from the brain and other centres those neural impulses which lead to muscular movement; and the sensory, which carry inward to the brain and other centres the sensations of pain, touch, temperature, etc., from the external world.

Electricity affects both classes of fibres, as well also as the special cellular nervous substance in which they start, or in which they terminate. The positive pole diminishes the excitability and the conductivity of a nerve, while the negative pole heightens them. The former is therefore sedative, as, for instance, in neuralgia, while the latter is stimulating, as in paralysis. This fact of the diametrically opposed physiological properties of a current at the points of its polar application also admits of a physical explanation. As Baron von Humboldt first demonstrated, acids which accumulate at the positive pole depress the excitability of nerve tissue, while alkalies, which accumulate at the negative pole, heighten the excitability.

The above facts, relating to nerves, are known as electrotonus.

Again, living muscular tissue becomes acid by fatigue, and alkaline in repose or rest. It is evident that a positive pole will increase fatigue by increasing acidity, while the negative pole will overcome fatigue by neutralizing the acid.

Electro-Diagnosis.—To every muscle of our body a nerve goes to set the muscle in motion and to maintain its nutrition. If this nerve is cut, bruised or diseased, so that the conducting pathway from the nerve centre in the spinal cord or brain to the muscle is abolished, or if the centre itself is destroyed by disease or otherwise, then the muscle suffers and actually degenerates. There are many of the most serious diseases of the nervous system in which the muscle thus suffers. Electricity enables the physician to decide if the muscle and its nerve are impaired, and adds greatly to the certainty of a diagnosis. The test is simple. An electrode is applied at a point on the skin, previously determined and mapped out on charts, by aid of dissections upon the dead subject, just over where the nerve plunges into the bulk of the muscle. If the faradic or induced current is turned on, it will be found that the muscle, if degenerated, has lost in a greater or lesser degree its power of contracting, simply because in its changed condition no single electric impulse of this current lasts long enough to affect it.

If the galvanic or continuous current is turned on, the muscle contracts to single impulses, because they last longer—but this singular fact now appears—viz: that, whereas in the healthy muscle the negative pole causes the greatest contraction, in the diseased muscle the positive pole causes the greatest contraction.

This law of excitability to rapidly varying currents (over 20 and under 5,000 per second), and reversal of the normal order of degree contraction to a single impulse of the galvanic current is termed the reaction of degeneration of erb. It is the physician's great reliance for prognosis or diagnosis in many most serious diseases of the nervous and muscular systems.

Nutrition.—No general fact associated with the administration of electricity is more familiar than that the general health —the nutritional processes—are promptly and greatly improved. The exchanges between the blood and the tissues are augmented— the patient absorbs more oxygen and excretes more carbonic acid and urea, and he gains in weight and comfort. But as these tissue exchanges are set up more especially by general administrations, like the franklinic, and by the sinusoidal current with large electrodes of D'Arsonval, we will defer their further consideration until later on.

In general, electricity has been found useful in diseases of the brain, spinal cord, and of the nerves, like paralysis, atrophy, spasm, contractions, anæsthesia, neurasthenia or nerve exhaustion, hysteria, migraine, melancholia, hebephrenia, epilepsy, and general paresis—in anæmia, rheumatism, gout, in many diseases of women, and in numerous surgical affections.

In all of these diseases, the pathology once established, either in fact or in hypothesis, a given property or properties of some given modality of the current is applied to effect a cure.

We have already said enough to point out the enormous power possessed by electricity over some of the functions of living tissue. There is no recess of our bodies so deep but what nerve fibres penetrate to it, or muscular fibre helps to make up its mass. No part of our material mass can escape its influence. And when we speak of high frequency, high potential currents, we shall have occasion to refer more specifically to special and important modifications of function.

The trouble is not that electricity does so little in our human bodies, but that it does so much—we cannot classify, systematize, and properly direct our power. We pause, confused, in the multiplicity and in the interwoven character of tissues and their functions—become bewildered in the problems of biology, the process of vitality—the process of disease. If we falter, let us not blame our noble agency, which is more than ready to do all that we require of it; but blame rather the shortcomings of our own knowledge.

Section III.

High Frequency, High Potential Currents.

In view of the prodigious progress of electrical science and practice in recent years, it is evident that medicine, conservative as it may be, cannot long remain without the pale of the new advances. No one science can stride forward without dragging the others with it. There is a correlation of sciences, just as there is of energies. To-day, medicine is occupied—is preoccupied, with bacteriology. And no less fascinating is this study, than are substantial the benefits which it has already conferred upon humanity. Electricity in medicine has had one "innings," and that an unsatisfactory one. Its advent was premature, the nature of the agency itself and its phenomena were uncomprehended, and electro-therapeutics became a synonym for a "vasty deep" of speculative deductions, ignorant methods, and, more unfortunately still, of crafty practices of charlatanism and quackery. The old electro-therapeutics still labors under antiquated physics, confused electro-physiology, and observations colored by the fancies of the observer; the new is being built upon experimentation in the physiologist's laboratory, and in the electrician's. The old relied upon *local* applications at the tip of small sponges—by local treatment it sought locally to cure disease; the new, while not neglecting the localizing method, goes further, and by a general application, affects the entire organism, the pulse, the temperature, the lung and the other excretions; in short, all that constitutes the nutrition of the individual. It seeks by exciting the highest degree of the health of the individual to make human tissue an unfit habitation for disease and morbid conditions.

Our antitheses may possibly make the line of distinction too strong, but they will answer our purpose as they serve to emphasize the fact that *general* administration of electricity as a curative agent is quite as, if not more, important than the exclusive local administrations so long in vogue.

This transition to obtaining general effects from electricity in medical use involves a corresponding transition in the nature of the apparatus employed, and in the method.

The new era of the introduction of general effects was initiated by the revival of the use of statical or franklinic electricity in 1879 by Charcot of Paris. The electrostatic bath and spark was a general treatment, and one, as we are only just now learning with good evidence, of great power, if applied from large machines. But we must here, also give credit to Beard and Rockwell of our own country, who as early as 1868 recognized the nutritional value of general electric treatments and formulated their ideas in a valuable technique comprised under the titles of "central galvanization," and "general faradization."

Franklinism must furthermore be classified under high potential, high frequency currents, and it will be here considered from that point of view. This classification is based upon the self-evident potential and upon the oscillatory nature of spark discharges. What the physical effects in a circuit influenced by electrostatic discharges, particularly in a circuit acted upon inductively, may be, we have only learned in recent years from Mr. Tesla. His genius has flooded the scientific world with a new illumination of ideas and facts, and this illumination has spread backward and invested our older observations with a new and vital interest.

The writer, after a visit to Paris in 1880, and a study of Professor Charcot's work with statical electricity, introduced the subject—general electrostatic administration—to the profession in this country in a formal paper, read before the New York Academy of Medicine, March 3, 1881, bringing home machines and electrodes which served for models to American manufacturers of these instruments.

In this same publication, he first described a method of obtaining high potential, high frequency currents from the influence machine, and pointed out its use and value in therapeutic work, under the title of "A new induction current in medical electricity." Such a current had been hitherto unknown. Its physiological effects as then described, are the Tesla effec s, as more recently produced, while its physical effects in the light of Tesla's subsequent investigation, are now seen to be similar. The current was named the "static induced current." But the writer's publication attracted but little attention, since it was confined to medical circles. and the time was not yet ripe for a comprehension of the peculiar effects and the action of currents of an induction circuit, produced by greatly increasing the frequency of the interruption in a primary circuit by aid of a small spark and condensers.

The mechanical means to secure great rapidity in the oscillations or alternations was identical with that pursued ten years later by Mr. Tesla, and later on by D'Arsonval, Thomson, Hertz, and Lodge, viz:—the use of condensers and a small spark gap, across which a spark was continuously discharging.

Tesla's first publication appeared in February, 1891.

In March 1891, Elihu Thomson made the observation that currents whose current strength was fatal or dangerous to human beings at a slow rate of interruption, could be taken through the body with less danger when the rate was largely increased, and Dr. Edward Tatum, of Yonkers, N. Y., carried out physiological experiments and demonstrated this.

To M. D'Arsonval of Paris, we must accord the development of the physiological effects of high potential, high frequency current. To him we owe a systematization of the facts and a vast amount of original research, while to Messieurs Gautier and Larat, besides their own original labors, we are indebted for the promulgation of the sinusoidal current in its medical applications.

The peculiarity of the physiological effects of high frequency, high potential currents, is that:

a. Currents whose current strength is fatal or dangerous to human beings at a slow rate of interruption, may be taken through the body without danger when the rate is largely increased. One may easily verify this statement with an ordinary medical induction coil, whose primary is supplied from a small alternator. A slow rate gives painful shocks, and as the rate is increased, the current becomes more and more bearable up to a point where almost no effect is appreciated. At the high rates:

b. The sensory nerves are not affected—*i.e.*, little or no pain is felt. (Morton, Tesla, Thomson, D'Arsonval.)

c. The motor nerves are not affected—*i.e.*, little or no movement takes place. (Morton, Tesla, Thomson, D'Arsonval.)

d. Increased tissue changes are manifested by a freer absorption of oxygen, and increased elimination of carbonic acid. (D'Arsonval.)

e. No rise in the temperature of the more central parts of the body. (D'Arsonval.)

f. Dilatation of blood vessels, resulting in lower blood pressure. (D'Arsonval.)

g. Lamps become incandescent when placed in circuit with persons who experience no sensations. (Tesla, D'Arsonval.)

The writer's arrangement of 1881, for the condenser currents from influence machines, may be calculated to afford 1,500,000 oscillations or alternations per second. Mr. Tesla employed 20,000 per second without Leyden jars, and the frequency with Leyden jars was calculated to equal from 1,000,000 to 1,500,000 per second.

Professor Thomson's alternating current was derived from an alternating dynamo machine, whose highest number of alternations was about 8,000 per second.

M. D'Arsonval's arrangement appears to admit of absolute accuracy in the rate per second up to 10,000 per second, since he employs a specially constructed alternating current dynamo, whose product is a sinusoidal or even wave current. He states, that by the aid of powerful Ruhmkorff coils, condensers, and induction coils, he carries the frequency of interruption up to 1,000,000 per second, while the current strength is five amperes. When we compare these figures with the 200 per second rate of the ordinary medical induction coil, it is plain that we are treading on different ground, and there is no doubt that in high frequency, high potential currents, that is to say in currents which are of high electromotive force, and which alternate or flow in opposite directions a great number of times per second, we possess in medicine an ally as valuable in the cure of disease as any other type or modality of current like the galvanic and the faradic or ordinary frequency of interruption.

To-day, then, we may obtain the currents under discussion:

1. From Morton's apparatus, viz., from the influence machine by condenser currents; (1881.)

2. From Tesla's apparatus; (February 21st, 1891.)

3. From Thomson's apparatus; (March, 1891.)

4. From D'Arsonval's high frequency sinusoidal apparatus; (February 24, 1891.)

But, since it is the Tesla experiments, performed with apparatus capable of affording an alternating electrostatic field of great energy, which have in reality brought into prominence not only the electrostatic phenomena of influence machines, but also all electrostatic phenomena of currents, I think it best to abandon the historical order in favor of one which has served, and continues to serve to demonstrate the subject most fully, and by effects utterly impossible to obtain in degree from any influence machine.

FRANKLINISM OR ELECTROSTATICS IN MEDICINE.
(*Statical Electricity.*)

Before touching more fully upon any one of these three specialized forms of high potential, high frequency currents, we must study briefly that progenitor of all high potential, high frequency currents, the electrostatic or friction or influence machine, its product, its uses, and place in medicine.

Referring in a recent interview to the possibility, already proved by experiment, of the transmission of energy through the air. Mr. Tesla says: "The plan I have suggested is to disturb by powerful machinery the electricity of the earth, thus setting it in vibration. Proper appliances will be constructed to take up the energy transmitted by these vibrations, transforming them into a suitable form of power, to be made available for the practical wants of life. *Primarily, the agent that I propose to use consists of an old fashioned electrical spark* such as they derive from a Leyden jar. This is rapidly discharged back and forth a great number of times. Each time that the spark seems to pass, it is in reality passing hundreds of thousands of times, and each time it passes it sets up these waves in the ether, which extend out into space. In other words, the result of the experiment is to disturb the equilibrium of the ether."

Primarily also we may say, in a minor degree, in medical administrations of statical electricity, the agent which the physician uses is the spark; the electric ether vibrations are similarily set up, and the appliance attuned to take up the energy transmitted by these vibrations is the human body, with all its complexity of delicate physiological mechanisms, of delicate cell and fibre, themselves already vibrating with the function of life.

The spark then demands our first attention. [Assistant here turned the crank of a large Holtz machine.]

A few brief revolutions of the wheels of this great Holtz machine, and you see a torrent of sparks passing between the discharging rods. One of our number[1] kindly allows himself to be mounted upon this insulated platform connected by a brass rod to the machine, and attest for us all the peculiar sensations of the electric charge, while again when I draw sparks from various parts of his body, you yourselves can see the results of the muscles thrown into activity. A spark to the biceps raises a dumb-bell

1. Mr. W. J. Jenks, Member of the INSTITUTE.

grasped in his hand; another to the triceps throws the arms violently backward; his fingers extend and flex, as the corresponding muscles are touched; a spark to nerve trunks produces the familiar hand postures, just as when the ulnar or median nerves are excited in galvanism or faradism. Before your eyes are taking place profound excitations of nerve and muscle, visible, objective evidence of the power of this current which has been said by some doctors to be superfical, and not to penetrate beneath the skin. Were our patient's temperature below the normal, as is more commonly the case than is suspected in many chronic diseases, you would find that it rose to normal. In fact, had we time to examine our patient more critically, we could determine with positiveness alterations in his pulse and temperature, and the amount of urea, and phosphates excreted. We could, in fact, establish great alterations for good in the chemical organic exchanges or oxygen combustions which are the essence of health, and equally the essence of cure Evidently we have before us a medical instrumentality of great power, even though it be the ancient Holtz machine whose phenomena have been too long neglected, both by the electrician and the physician.

Striking as are the differences between the electric product of the influence machine and our small galvanic and faradic exciters of electric energy, in reality the differences are more apparent than real, and the kinship of the different modalities of electricity, however excited are most easily established.

With the electric product of this machine we may repeat in lesser degree any experiment which may be accomplished by the galvanic or faradic currents.

That form of electricity, known as statical, has an enormous E. M. F. and low amperage; that known as galvanic, an enormous amperage and low E. M. F.; but neither is without one element or the other; it is merely a question of degree or ratio.

The so-called dynamic or current electricity of the physician is also static, while the static is also dynamic or current electricity. While a Holtz machine exhibits a voltage of 100,000 to a spark one inch in length, or say, easily a total voltage of 1,000,000, represented by a spark ten inches in length, a voltaic cell exhibits a total voltage of only 1½. But the cell represents many amperes, while the machine represents merely a very small fraction of an ampere.

In medical work no deductions based upon these facts can be justly cited for the purpose of making invidious distinctions as to their respective curative value. Preponderating C. S. does its special work, and preponderating E. M. F. does *its* special work. It is merely a question of the work to be done. Knowing these differerences, the physician selects one or the other, together with some one or more of its special properties, and pits a given producible effect against a known or presumed condition of disease; this is a scientific method in contradistinction to simple empiricism.

Historical.—The spark and frictional electricity have long held sway in medicine, beginning with the time in 1730, when Stephen Gray discovered conduction and insulation, and drew sparks from a boy suspended by strands of horse-hair. It was the Abbé Nollét, however, who shortly after drew the first spark with the idea of curing disease; that spark marked the dawn of electro-therapeutics. Then followed the brilliant discoveries of the older philosophers and of the doctors—both of science and of medicine—who floundered about in their attempts to establish the identity of electricity with nerve force, with vital force, with life itself, and thus hoped to find the elixir of life, and probe that deepest mystery. The miasm of their speculations yet clings to the purlieus of medicine. And yet to the mind unaccustomed to scientific thought, the modern view that electricity and light are identical, seems almost as marvellous.

In connection with early electro-therapeutics, it is interesting to us Americans to recall that Benjamin Franklin in 1752 in Philadelphia was treating paralytics by shocks from his Leyden jars. One patient, a young lady, who had for ten years been tortured with convulsions, thus quaintly describes her experience:

"At length my spirits were quite broke and subdued with so "many year's affliction, and indeed I was almost grown desperate, "being left without hope of relief. About this time there was "great talk of the wonderful power of electricity; and as a "person reduc'd to the last axtremety is glad to catch at any "thing, I happened to think it might be useful to me. Altho' I "could have no encouragement from any experiment in the like "case, I resolv'd to try, let the e'ent be what it might; for death "was more desirable than life, on the terms I enjoy'd it. Accord-"ingly I went to Philadelphia, the beginning of September, 1752, "and apply'd to B. Franklin, who I thought understood it best "of any person here. I receiv'd four shocks morning and eve-"ning; they were what they call 200 strokes of the wheel, which "fills an eight gallon bottle, and indeed they were very severe.

"On receiving the first shock, I felt the fit very strong, but the "second effectually carry'd it off; and thus it was every time I "went through the operation; yet the symptoms gradually de-"creased, 'till at length they entirely left me. I staid in town "but two weeks, and when I went home, B. Franklin was so "good as to supply me with a globe and bottle, to electrify my-"self every day for three months. The fits were soon carried off, "but the cramp continued somewhat longer, tho' it was scarcely "troublesome, and very seldom return'd. I now enjoy such a "state of health, as I wou'd have given all the world for, this

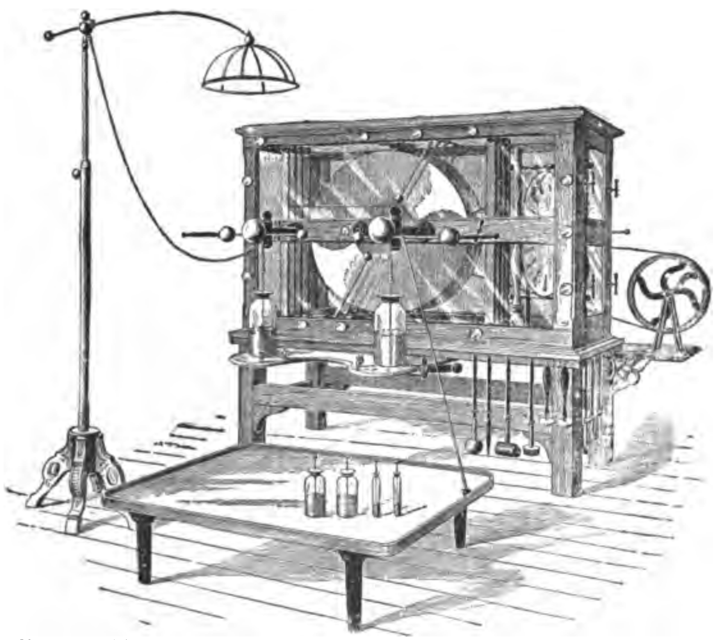

FIG. 1. Morton-Wimshurst Influence Machine for Therapeutical use.

"time two years, if it had been in my power, and I have great "reason to hope it will continue."

I will merely remark further in passing, that the discovery of the Leyden jar in 1746, the discoveries of Galvani and Volta about 1800, and of induction electricity, completed by 1840, each formed historical epochs which gave to medicine respectively, franklinism, faradism and galvanism.

THE INFLUENCE MACHINE AND ITS ADAPTATION TO MEDICAL USE.

It is here enough to say of an influence machine, that it, like any other device for exciting electricity, presents two polar terminals;

these are termed prime conductors. In the Holtz machine one vertical half of the revolving plates is always of one polarity, the other of the opposite, and the prime conductors in connection with each half are similarly of opposite polarity.

The machine I use I have had constructed by the Galvano-Faradic Co., of New York. It is in reality a Wimshurst-Holtz machine. It has eight 30-inch in diameter revolving disks, and six rectangular dividing plates. It easily gives a 12-inch spark.

Within the glass case is a small Wimshurst exciter to charge the large plates in humid weather. In addition to the usual

FIG. 2. Electrode for Bipolar treatment with induced currents from static machines.

FIG. 3. Vaginal Electrode for applying static spark to the uterus or in other cavities of the body.

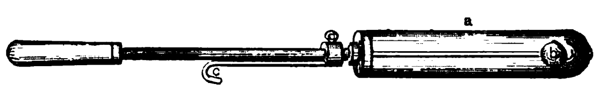

FIG. 4. Usual form of Bipolar Electrode especially insulated, is used with the Static Induced Current.

arrangements for spark and spray administrations, this machine has mechanical devices for the production of the Morton current from condensers, and furthermore a transformer for still further modifying this current. The machine is believed to embody all the modern improvements for medical and laboratory use.

Electrodes.—The electrodes most used are a brass ball and a point, one for sparks, the other for spray. The best working diameter for the ball is 1⅝ inches. To apply sparks to a precise spot or within the cavities of the body, I have devised two electrodes, here illustrated. The brass ball is surrounded by a glass tube like a test tube, and the rounded end is perforated with a small hole for the passage of the spark.

The usual form of bipolar electrode especially insulated is used with the static induced current.

Methods of Administration and Connections.

The patient is:

a. Placed upon an insulated platform by the older and classical method.

b. Uninsulated by my method of employing condenser currents.

Insulated.—The platform is connected to whichever prime conductor is desired, commonly the positive. He is now simply an extension of the conductor, and enlarges its static capacity.

In this position, the static charge escapes from the charged patient:

a. By leakage.

b. By a brush discharge, established by a pointed electrode.

c. By disruptive discharge or spark, established by a ball electrode.

The electrodes are provided with insulated handles, and their conducting portion is attached to a chain which is in turn:

a. Attached to the other prime conductor—direct method;

b. Led to the ground—indirect method.

The direct method is commonly employed, but gives a painful, irritating spark, and should never be used. The indirect method, by grounding, gives a long, clear spark, and is by far less painful.

In working with electricity of this high E. M. F., the mere act of grounding seems to me to be superior, because it affords a large capacity surface in surrounding objects, rather than because it supplies an earth circuit in the ordinary acceptation of the term.

Many physicians have objected to this treatment by saying—"Yes, true, you communicate an electric charge to the patient's surface, and you draw it off, but no effect takes place within the patient and beneath the surface." A practical answer to the objector is to administer to him a spark, when his deepest groups of muscles will contract and convince him that the effect is not superficial. A physical answer is, that the patient is actually a charged insulated conductor, and since he constitutes a conductor, by a well known law of electricity, his potential at every point, within and without, is the same. But each spark drops his potential to zero, therefore, his potential within as well as without is equally dropped to zero and we must admit that instead of being superficial, no treatment could be more complete in its effects upon every molecule of the organism submitted to it. This is undoubtedly the reason why in electrostatical treatments the entire nutritional processes of the body are affected, and corresponding cures of disease produced.

Ordinarily, administrations are either by the spray, brush discharge, or by electric bath, as it is sometimes called, and by the spark. The former is used about fifteen minutes, the latter, from five to ten. The spark should be long, clean, and percussive when deep effects are desired; and frictional, caused by rubbing the metallic ball electrode over the body, when counter-irritant, revulsive and reflex effects are to be obtained. It is a great convenience of this method of treatment that it is not necessary to remove any part of the clothing.

When the patient is treated:

Uninsulated.—By the static induced current, the output of the machine at its terminals is simply a current and no longer a spark. The patient is not submitted to a "charge," but is treated as if by a galvanic or faradic apparatus. The current itself will be referred to separately.

PHYSICS AND PHYSIOLOGICAL ACTION.

Any extended allusion to the physics of electrostatics would be entirely out of place. I shall merely ricochet, so to speak, over this territory, just touching ground at intervals where physics and medicine meet in some emphatic relation.

The experimental facts of the class-room are familiar to all. But from nature's laboratory we may derive a passing thought. We may call upon the lightning for an analogy to our spark, administered to our patient, and upon electrical conditions of the atmosphere for an analogy to his state of electric charge while sitting upon the insulated platform. In every thunderstorm we have a laboratory experiment; every day we bathe in a certain electric potential corresponding to the electrostatic bath.

What points then should be noticed? What deductions may we draw?

The lightning is a great spark; its effects are mechanical, physical and physiological. Mechanically, it often moves great masses, displaces and transports them; it performs, in short, on a large scale a cataphoresis, just as on small scale, we transport medicines through the skin, or move about fluids in the tissues. The spark of the influence machine produces a molecular perturbation at every point where it strikes, a sort of molecular gymnastics, so to speak, or displacement of tissue, accompanied by corresponding changes in nutrition—new blood flows to the part, the lymphatics increase in activity, and carry off effete material; there is a local revivification.

Again, the great spark kills people, and upon their bodies are found marks indicating mechanical rupture of their tissues. If they recover, there frequently remains extensive paralysis of parts of the nervous system; if they die, no evidence of the cause of death is ascertainable beyond the organic injury mentioned. What has caused their death? The same question has been asked, and not yet answered, in regard to the death of criminals "electrocuted," and in regard to "linemen" who fall from a pole, shocked by the electric current. Picture to yourselves the amoeba, the little cellular organism, and recall the experiments which demonstrated that with successive severe shocks of a varying current, the amoeba first assumes a spherical form, and then perhaps regains it, but finally remains, as the shock is increased, without power of motion. Live protoplasm has in short become dead protoplasm; the living animal has become a dead animal; the effect of an electric shock has been to deprive the protoplasm of the power of contractility. No microscopical changes have been found in human beings who have been "electrocuted." It is perfectly justifiable to conclude that the shock has deprived organs essential to life of their power of action. Such, it has seemed to me, on a small scale, and in a lesser degree, always consonant with the vitality of a part, may be, and undoubtedly is, the effect of a spark, an effect which, although always short of molecular death, is accompanied by profound changes in its nutritional activity.

Recently, D'Arsonval has thrown an element of terrible doubt into the conviction that most of us hold, that electricity kills the criminal at our State "electrocutions." The criminal is not killed, he says, but his physiological functions are inhibited; that is to say, brought to a standstill by the action of the electricity upon the nerves. The criminal is not therefore dead, but is, as it were, in a trance from which he may be resuscitated by proper measures, and it is the surgeon's knife at the autopsy table which in reality kills him.

He claims that many supposed to be dead from what appeared to be fatal shock have been revived by proper measures. There is but one reply to D'Arsonval's statement. It is to postpone the autopsy until it is positive from the evidence of decomposition that the body is actually dead.

And in this connection, should not the proper measures of resuscitation be studied in our emergency hospitals, and be applied to linemen and others who seem to have received fatal shocks? How many valuable lives have been saved of those who appeared to be dead from drowning? This phase of D'Arsonval's statement seems to me to be most worthy of attention on the part of this society.

The lightning has again a chemical effect, an effect of synthesis; it forces a combination of O_2, the common oxygen of the air, into O_3, or ozone, the odor of which is familiar to all during the course of thunder storms, and while our influence machine is working.

Again, the lightning has a directly physical effect in the fusion of metals, and the bearing of this fact is mainly interesting to us in establishing the fact that static electricity is kinetic, when it discharges, and obeys Ohm's law, viz.:

$$C. \text{ (current strength)} = \frac{E}{R}.$$

The fact illustrates to us the *amperes* of statical electrization, while the mechanical effects, just alluded to, illustrate the *volts*.

As regards atmospheric electricity more than one fact is of interest to us. Under a clear sky the air is always positive, and increases in the degree of its positivity as we ascend, as has been demonstrated in balloons, while the earth is always negative. Under a cloudy sky, the conditions vary; both the name of the electricity and the potential vary. Thus, we are continuously under the influence of unseen electric baths, either positive or negative. On a clear day, we are always in a negative bath; on a cloudy day, we may be in a negative or in a positive bath. The analogy to the state of the patient on an insulated stool is obvious. *Query*: Have these electrical conditions of the atmosphere anything to do with the variations in our health? It is well known that those who suffer from neuralgia, gout and rheumatism, and spinal cord diseases, and neurasthenia, and nervous people in general, feel worse on a cloudy day, or better on a clear one.

And since they are in their best health on fair days, in other words, when negatively electrified, may this not furnish some clue in general to the utility of the electric bath by insulation, and particularly, to a discrimination as to whether the positive or the negative bath should be used? It would seem that a negative insulation would be indicated in the diseases named. I will say that I have found those who are susceptible to these atmospheric changes are equally susceptible to the electric bath by insulation, and that many neurasthenics are rendered excitable and sleepless by the positive insulation, but are on the other hand soothed by the negative insulation. This in general coincides with the impressions of the older writers, who are in habit of alluding to the negative insulation as "depressing." These observations may teach us at least to be wary of attaching our patients indiscriminately to one or the other pole, as is too often done. Custom and the construction of their machines have led many continental observers to use the negative insulation, while the positive is the more frequently used in our own country. These differences in attachment may have led to the different reports as to the effect of the bath itself. While upon this subject, I should say that there is objective evidence based upon the experiments in my clinic at the Post-graduate Medical School and Hospital that the bath alone will produce an increase in the pulse, a change in the temperature, and a modification of the quantity of the urea and phosphates excreted in the twenty-four hours.

If we choose to attach importance to the electrical conditions of the atmosphere, it is interesting to note our position as human beings, situated in a strained dielectric—the air—and near a conductor, the earth. We may compare ourselves to a small spot in the glass of a Leyden jar, near the tin-foil coating. It cannot be denied that we are subject to alterations due to the differences of strain in the dielectric; it is quite another question whether this position has any important effect upon our health, and it is more than probable that like many others of the conditions under which we live, we have adapted ourselves by evolution to the relation which is said to be at the basis of life, that is to say, the adjustment of our internal organization to our external surroundings.

Can the human body itself exhibit statical polarity due to any changes or properties within it? If the skin is dry, statical electrification may be produced upon the human body, just as it is upon that of a cat, by friction, but there is no evidence that the organism itself can produce statical electricity.

And again, we may call up points in physics which invest the powerful spark of our influence machine with a pregnant meaning in relation to our human organism. To Galvani is attributed the observation that paved the way to the magnificent discovery of the development of electricity by chemical action, but it is also related that the very first phenomenon which attracted his attention was that the frog's legs, skinned and prepared for supper, hanging not far from where the spark of a frictional machine was passing, sprung into contraction at each spark. Mrs. Galvani, it is said, by the way, was the first to notice this. Each spark, in the rushing downfall of potential which made it possible, let drop at the same instant the accumulated potential of surrounding objects, among them the frog's leg and its support, and living tissue exhibited its function, that of contraction. Are we, as similar living tissue, not affected every time a spark passes in our neighborhood? Like the frog's legs, our organisms constitute an apparatus capable of responding to the ether vibrations of each spark; how susceptible surrounding space is to the ether waves set up by the spark, is little realized. Stretched wires in the same room throb with each pulsation and spark, in unison at each little break in their continuity. Nodal points, as demonstrated by Hertz, may be found on these wires, and the wave lengths measured off in yards, or the waves may be focussed, reflected, or refracted. Joseph Henry observing that a simple spark, one inch in length thrown on to a circuit of wire, produced an induction sufficiently powerful to magnetize needles in a parallel circuit thirty feet distant, remarks, that "a single spark is sufficient to disturb perceptibly the electricity of space throughout at least a cube of 400,000 feet of capacity."

With this machine I have made a simple experiment, showing the far-reaching disturbance in the ether caused by the passing spark. Glow lamps, pierced by a single conductor may be laid about the room, upon the floor, upon tables and suspended, and as the spark jumps across a small air-gap, they light up with a purple glow. Edison 16-candle power lamps will answer the purpose admirably. A small strip of tin-foil is fastened around the outside of the bulb by a rubber band, while a few yards of insulated wire is similarly attached to the brass of the lamp. A condenser circuit is thus established. And had I here an influence machine large enough, we could, of course, light up this room with vacuum tubes suspended about it, not by single long sparks, but with the spark continuously crossing a small air-gap, as I used it in demonstrating my condenser currents in 1880. These are the effects that Tesla has demonstrated upon a magnificent scale. But they may be repeated on a lesser scale with the electrostatic machine, particularly the brush discharge and the vacuum glows. They are cited here merely to show us that the electrostatic machine is, as I have mentioned, in reality a high frequency, high potential current. It is the spark that is the key-note to these effects; we do not even need an artificial condenser like the Leyden jar; the objects about, furnish the electric capacity, and the impulsive rush of their discharge cause the phenomena in the space about.

It is evident that the principal effect of the spray and the spark is exerted upon the nervous system. Motor nerves stimulated by a spark, cause muscular contraction, and thus the entire muscular mass of the body is set into movement, causing quickened flow in the blood vessels and increased health of the muscle, and that while shunning the electrolytic or chemically decomposing effects of galvanic currents.

The sensory nerves, particularly those of the skin, are stimulated, and exert upon the nerve centres a remarkable influence by throwing in upon them their impulses, due to their peripheral excitation. The circulation is also quickened by direct action upon the nerves which supply the blood vessels; there is dilatation of the cutaneous vessels, so that the patient experiences a glow of warmth, a moderate and often an extreme perspiration, and on the whole a pleasant reaction similar to that following the bath, or moderate exercise in the open air. The normal pulse is usually raised from five to fifteen beats, but the pulse of disease is raised if slow, and slowed if fast. As a result of extended observations upon the temperature, I have deduced this law, which, I think, will stand the test of further observations for the spray, viz.: that a normal temperature is slightly raised; that a temperature below normal is raised to normal; that a temperature above normal is lowered. These changes in the temperature average from one to two degrees Fahrenheit. My researches, not yet published in detail, show that the spray produces, or tends to produce a normal rate in both pulse and temperature.

Alternating Currents of High Potential and High Frequency, viz.: Tesla's Apparatus and Currents. "Rapidly Alternating Electrostatic Stresses."

It is characteristic of alternating currents, that their energy is manifested, not so much *in* the wire, as in the space surrounding it, and the greater the frequency and the potential, the more extensive do some of the phenomena outside of the wire become. In this space, energized by the alternating current traversing the wire, may be exhibited by the aid of proper devices arranged to capture and transform the electric energy, light, heat, mechanical motion and chemical action; lamps glow, metals fuse, motors are caused to spin, and chemicals to combine. And this space around the current conveying wire is not narrowed to minute fractions of an inch, but may be measured in yards. Here is a field of force within whose influence a patient may be readily brought. Such a field would be termed an alternating electrostatic field; it is most marked around the terminals of a secondary coil whose primary is conveying an alternating current. It is the same field as that associated with the spark discharges of electrostatic machines, and even in a lesser degree with our best constructed medical induction coils, as I have shown by glow lamp tests applied to them.

The electrostatic field was indeed familiar to all; but in a feeble and minor degree. In medicine it has been a prominent feature of administrations from the influence machine in both simple insulation of the patient, in the spark, and more particularly in my own method of the static induced or interrupted current, and has been in medical use in connection with that machine. But so far as relates to induction coils, little or no account has been taken of the electrostatic properties of the current. Our attention has been wholly occupied, as has been that of the electrical engineers, with the current in the wire, and not with properties of this current—the electrostatic field outside of it.

But in induction coils affording high frequency, high potential currents, for instance, from hand to hand, it must now be noted that we have to deal with two effects—1st. The effect of the direct passage of the current through the body, and: 2nd, the lateral, surging, wave impulses spreading out in every direction from the passing current. The former of these effects alone has hitherto claimed attention. That the latter effects now claim attention alike in the industries and in medicine is due to the wonderful experiments of Nikola Tesla. We have, as he modestly says, the same apparatus, the induction coil, and the same phenomena, only the apparatus is operated differently, and the phenomena are presented in a different aspect. So different indeed is the aspect of the phenomena that the entire scientific world hails their presentation as a revelation.

Mr. Tesla's first publication was in the *Electrical World*, of Feb. 21, 1891, under the title of "Phenomena of Alternating Currents of very High Frequency." Referring to effects upon the human body he writes, "the discharge of even a very large coil cannot produce seriously injurious effects, whereas, if the same coil were operated with a current of lower frequency, though the E. M. F. would be much smaller, the discharge would be most certainly injurious." The writer's experiences tend to show that the higher the frequency the greater the amount of electrical energy which may be passed through the body without serious discomfort."

The Tesla Apparatus.—Mr. Tesla obtains the high frequency mechanically, and by the aid of a well known natural phenomenon—mechanically by a specially constructed dynamo machine giving a frequency of about 10,000 per second, and naturally, by aid of the intensely rapid alternating or oscillating discharge of Leyden jars or condensers. The latter carry the frequency up to anywhere from 500,000 to 1,500,000 alternations per second. Low frequency may, however, be employed when the condensers are in the secondary circuit.

How important an element the Leyden jar may be in increasing frequency both in the electrostatic phenomena of the influence machine already in medical use, and in the Tesla apparatus, may be judged of from a calculation of Dr. Oliver J. Lodge that a common pint Leyden jar discharged through an ordinary discharging rod will possess a rate of oscillation equal to about ten million per second.

Mr. Tesla's high potential is due to the frequency and to "step up" transformation by coils. It consists practically of a high frequency dynamo whose current enters a primary wire, a secondary wire within whose circuit is included a condenser and discharging rods, and again a coil to which a part of the secondary wire acts as a primary.

The Tesla effects, as they are now frequently termed, are obtained:

a. By direct connection with both terminals.

b. In the space intervening between the terminals, or in the space about either one singly.

Using the apparatus above described with alternating current dynamos of low or high frequency, Tesla obtains remarkable effects. Among them may be noted:

a. The brush discharge.

With two cotton covered wires arranged parallel and near to one another, the wires are strongly illuminated by streams of flame, spreading from one to the other. The brush led from a circle of wire, 30 centimetres in diameter, to a small brass sphere, is formed into a beautiful cone of light, or again, concentrated upon small, thin wires, greatly intensifies the light.

With two circles of wire, respectively 30 and 80 centimetres in diameter, a continuous circular luminous sheet is produced.

An increase of the molecular or atomic vibration changes the color of the discharges from purplish to white.

In these experiments, the molecules of air about the terminals are intensely agitated, and give rise to the light.

b. Rotation of a motor connected to one terminal wire only of the coil.

c. Illumination of exhausted bulbs or lamps containing a refractory substance sealed within them, and when attached only to one terminal of the coil, the molecular bombardment within the bulb incandescenses the refractory substance, and emits light. Such substances are fused, disintegrated and dissipated by the intense electric bombardment.

Buttons of carbon, diamond, pumice-stone and carborundum were used. To protect the leading-in wire from the effects of the bombardment, a screen aluminium tube may be used, which acts electrostatically by reason of its conductivity, and also mechanically; the screen, becoming charged, economizes the energy supplied to the bulb by not taking it up after it is once charged. Again, the same exhausted bulbs are illuminated without the aid of a leading-in wire, the energy required being transmitted through the glass.

d. To excite vacuum tubes throughout the whole extent of a room, lighting up the tube wherever it is held in space, and at a distance from the conductor, the intense electrostatic field is set up by converting the oscillatory current of a condenser to a higher potential. Such a field Mr. Tesla establishes between two sheets of metal several yards in area and several yards apart. A vacuum tube held within the space between the metal sheets, glows intensely.

It becomes an interesting question to determine to what extent high potential, high frequency currents may be of value in medicine. The shock from the secondary coils above described are very light and can be taken without inconvenience. This to many has seemed remarkable on account of the very high voltage. Much speculation has been indulged in as to why these currents do not produce great muscular contraction, and great pain, and dangerous results. It has been suggested that the rapidity of the alternations is too great to excite the nerves, or again, that the current thus generated does not penetrate the conductor, but traverses only its surface. An explanation would seem to me to be that the current strength of what seems to be a killing current is comparatively small, since it is reduced enormously by each "step up" transformation. But other explanations are advanced.

In the immunity from pain and muscular contraction observed by D'Arsonval with a low frequency and a current strength of 5 amperes, another explanation must be sought for, and this is the comparatively gradual rise and fall of potential characteristic of the sinusoidal current, and physiologically vastly different in its effects from the impulsive rush of the condenser currents of Tesla and myself. D'Arsonval's high rate of frequency is obtained from an alternator and from induction coils, and condensers.

Whatever effects are to be obtained from Tesla currents must be obtained:

a. By the direct passage of the current through the person.

b. By this means, and by taking into account the lateral electrostatic surgings outward from this direct current.

c. By interposing the patient within the rapidly alternating electrostatic stresses constituting the field between the terminals, and yet not in contact with them.

Electro-dynamic induction effects need not probably be taken into account.

The extended physiological effects of the Tesla currents have yet to be investigated, since no apparatus has been available to physicians or physiologists, but judging from the powerful effects upon human beings of the currents of electrostatic machines whose output is more feeble in total energy, even if equal in potential, these currents seem to me to promise most interesting and most fruitful results.

We can in our imagination picture a future group of patients pursuing any agreeable occupation, reading, conversing, playing games, and so on, in a room whose two opposite walls are of metal connected to the two terminals of a Tesla apparatus, and thus submitted to a furious molecular bombardment in the intervening space. We can imagine the surprise of the new comers, as they see the vacuum tubes carried in their hands burst into a glow of light when they are brought within the influence of the magical and invisible stream. We know that their temperatures would be raised by the bombardment, and we cannot help believing, though awaiting the proof, that profound alterations in their nutritional processes would be produced, whose influence would be far reaching in the cure of disease, particularly of the functional type.

ELIHU THOMSON'S HIGH FREQUENCY, HIGH POTENTIAL CURRENTS.

THOMSON APPARATUS.

Professor Thomson's apparatus consisted of an alternating machine having 100 armature coils and capable of producing 8,000 alternations per second. This machine was placed at the disposal of Dr. Edw. Tatum, of Yonkers, N. Y., and he conducted extensive physiological experiments with it, bearing upon the action of high tension currents upon animal life.

Dr. Tatum's work upon the physiological effects of high frequency currents embodied in a letter to Professor Thomson, December 29, 1890, and published May, 1891, deserves much higher recognition than it has received. He established:

1. That the fatal effects of alternating currents upon animals was in inverse ratio to the frequency.

2. That the cause of pain, which limits voluntary toleration, lies chiefly in the muscular contraction produced.

3. That the cutaneous nerves were distinctly less painfully affected at the high rate.

4. That the vital mechanism was not excited at the high rate even at a pressure of fifteen volts, so as to cause a sensation of flames of light; although two or three volts similarly applied at 120 alternations, caused such flashes very energetically.

5. That the sensation of vertigo, produced by the low rate, was not produced by the high.

6. That sensations of taste were absent to the high rates.

7. That exposed motor nerves of frogs were less excitable to the high than to the low rate.

Sinusoidal Currents.

D'Arsonval's High Potential, High Frequency Currents.

Starting from this fundamental fact, that when a current passes through the human organism the excitations are due to variations in its strength, D'Arsonval first studied the effects of a single wave of known form, and then the effects of periodic waves. For careful physiological investigation his first form of apparatus was an alternating dynamo without iron, capable of furnishing 10,000 alternations per second. The current thus produced was passed through the primary of an induction coil, having no iron core. With low frequencies neither pain nor muscular contraction was produced, but the current thus generated increased the tissue absorption of oxygen and the excretion of carbonic acid. It also acted as a sedative to pain.

He next adopted the method involving the continuous discharge of sparks between the discharging rods of a powerful Ruhmkorff coil connecting two Leyden jars to each terminal by their interior tin-foil coatings and connecting their outer coatings by a thick copper wire solenoid of from fifteen to twenty turns. A wire led off from each end of the solenoid furnished the currents for his experiments in extreme frequency.

When the Ruhmkorff coil was replaced by an alternating dynamo, D'Arsonval sent through his body a current of more than three amperes without inconvenience, except for some feeling of heat in the hands.

The phenomena observed were:

1. No effect upon the organs of feeling.
2. No muscular contraction.
3. Diminution of the sense of pain.
4. Dilatation of the blood vessels.
5. Increased perspiration.
6. Increased tissue change, manifested by increased absorption of oxygen and increased elimination of carbonic acid.

7. No increase of bodily temperature.

The fact that when the number of oscilliations is extremely high, there is no excitation of the nerves and muscles, D'Arsonval communicated to the Biological Society, February 24 and April, 1891.

D'Arsonval's view is, that the nerves and their centres, if not all of the tissues, respond only to excitations of a certain frequency, and that, therefore, if these currents fail to excite pain or contraction, it is because their periods do not coincide with the periods of the nerves, producing pain and contraction.

The changes in the human body above described, are of great value in electro-therapeutics. Their practical application demands an increase in the size of the ordinary electrodes, best met, probably, by using water as in a bath.

High Potential, High Frequency Currents From an Influence Machine and Condensers, viz., Morton Currents.

In view of the interest which is now being attached to currents of a high rate of frequency and the methods of their production, the writer asks your forbearance in reading a brief quotation upon this subject, from an article by himself, "On Statical Electro-therapeutics or Treatment of Disease by Franklinism," read before the New York Academy of Medicine, March 3, 1881, and printed in the *N. Y. Medical Record*, April 12, 1881.

"A NEW INDUCTION CURRENT IN MEDICAL ELECTRICITY.

"Thus far, in describing the methods of administering static "electricity for medical purposes from the induction electrical "machine, I have confined myself to what has been previously "known on the subject. The three main methods of administra-"tion up to the present time have been by insulation, by sparks, "and by shocks."

"I now venture to add a fourth method, that of the *induced* "*current* produced by static electricity, and capable, like the "currents induced by magnets and the voltaic circuit, of causing "physiological tetanus. In other words, by a simple arrange-"ment, the frictional machine may be converted into a machine "which will do all the work of the best faradic machine."

"We thus have at command in a frictional machine all of "faradism, in addition to the static electricity; for working pur-"poses we have all the advantages of both systems."

"Taking the Holtz machine as it stands, the change may be "quickly effected. We remove the connecting-bar between the "two *outer* coatings of our Leyden jars, connect ordinary con- "ducting wires and wet-sponge electrodes to each outer coating "respectively, and, finally, connect the two *inner* coatings by the "discharging rod. The patient, of course, need not now be in- "sulated. As soon as the machine is set in motion and the con- "densers are filled, the discharging rod may be drawn out a very "small fraction of an inch, and at once a current is felt between "the two sponge electrodes, which in its general characteristics "cannot be distinguished from the ordinary faradic current. "Owing to its very high tension, however, it is necessary to have "the handles of the electrodes well insulated and free from metal "points, in order to avoid the fine prickling sparks which pass "into the hands of the operator. It is soft and agreeable, and "accompanied by no shock. This current is not to be con- "founded with the series of discharges taking place between the "*inner* coatings of the jars. This latter, in silent current forms, "produces no muscular contractions or sensations of any kind. "In slight repeated discharges it is too painful to be borne. A "superficial trial shows one difference in favor of the static- "induced, as compared with the galvano or magneto-induced "current. The static-induced both produces more efficient con- "tractions and gives less pain to the patient, where pain would "be produced by any of the three. With it the whole motor "apparatus of the body may be called in action at its several "points, nerves stimulated and other effects produced, just as "with faradism."

"The current may be regulated to a nicety by means of the "discharging-rod, ranging from an almost imperceptible tingle "up to extreme and rigid flexion of the arms, should, for in- "stance, the electrodes be held in the hands."

"This, then, is an entirely new current in medicine, and it is "not a little curious that with all the experience with frictional "machines, it should have remained undiscovered up to the "present day."

And again:

"This current is more agreeable in its administration than "ordinary induction currents;" "it has a record yet to make."

The writer continued his experiments with currents from influence machines with no further publication in the 80's, than a few lines by Dr. Ambrose L. Ranney, in his "Static Electricity in Medicine," published in 1887, in which he writes: "Dr. "Morton has also been experimenting of late upon the effects of "deriving currents from a helix of insulated wire wound upon "each of the Leyden jars of a Holtz induction machine."

But on December 2, 1890, he read an extended paper upon this high frequency, high potential current, before the New York Neurological Society (published in the *New York Medical Record* of January 24, 1891), giving a second description of the apparatus and an account of the current's physical and physiological properties and its medical applications.

Of the apparatus it was said, "That the circuit-breaker is a "pair of adjustable metallic ball electrodes, introduced at any "point of the circuit, having a narrow air space between the balls; "the circuit 'makes' when a small spark overcomes the resistance "of the intervening air, and 'breaks' when it fails to do so, and "the current is due to rapidly successive equalizations of the "differences of potential of oppositely charged condensers."

"The circuit-breaker serves:

"1. To afford time to the condensers to charge."

"2 To regulate the frequency of the discharge."

"3. To determine the strength of the current. This latter may "be varied at will and with the utmost nicety, from a just per-"ceptible to a most powerful effect."

"The spark circuit-breaker now practically represents the "vibrator in the primary of an induction coil; the specific induc-"tive capacity of the air replacing the spring and its magnetic "attractability, and I use it as in that example and in the same "relations, *i.e.*, to interrupt the primary current and by reason of "an interrupted primary to obtain a secondary current. Each "impulse in itself consists of a vast number and range of oscilla-"tions or alternations (of one hundred millions, for instance, per "second) and, we may doubtless willingly concede that a current, "whose physical propertiesso positively differ from other cur-"rents, must possess equally positive and differing physiolgical "properties."

The following diagram, among many others, was used to illustrate the condenser current, called at that time by the writer the "static induced current." [Fig. 5.]

And of the current it was said, "In the franklinic interrupted "or static induced current, each impulse consists of a series of "alternating opposite currents of almost inconceivable rapidity. "Joseph Henry discovered that each discharge of a Leyden jar "was oscillatory in character, not a single transfer of electricity "from one side of a jar to another. In making his original state-"ment, he wrote, there is 'a principal discharge in one direction, "and then several reflex actions backward and forward, each "more feeble than the preceding, until the equilibrium is ob-"tained.' His discovery was corroborated, and the theory of it "worked out by Sir William Thomson, Fedderson, Helmholtz, "Schiller and others. As now stated by eminent physicists, the "number of alternations to each spark is from one hundred thou-

"sand to one hundred million per second, according to the "capacity and inertia of the circuit. Induction coils have been "constructed, giving eight thousand vibrations per second; two "hundred per second is probably an average of the medical far- "radic coil. We cannot, therefore, compare the franklinic inter- "rupted current to such coils."

"For computing the spark interruption to be at least two "hundred per second, and the oscillations of each spark to be "one hundred millions per second, we have a current giving "twenty billions alternations per second. Vast as such a number "may seem to our minds, familiar with two hundred vibrations "per second, it pales before the desideratum expressed by Prof- "essor Elihu Thomson, who said in a recent lecture, 'What is "needed is a machine having an alternating current making five

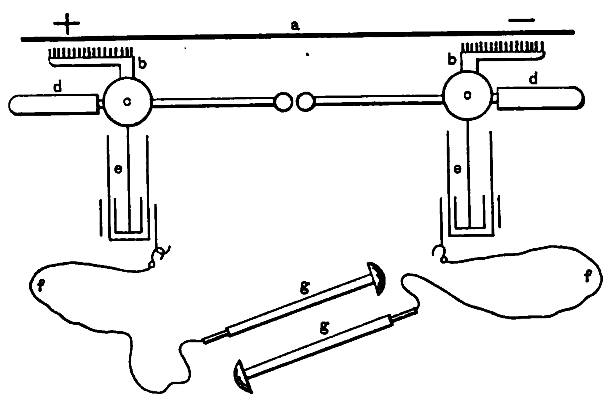

FIG. 5. Static Induced Current. Circuit of a Holtz Machine with or without Condensers.

a, Rotating plate; *b*, *b*, collecting combs; *c*, *c*, prime conductors; *d*, *d*, discharge rods; *e*, *e*, Leyden jars; *f*, *f*, conducting cords; *g*, *g*, sponge or other electrodes. Person, condenser, and circuit-breaker, in same circuit, connecting-rod between condensers removed, and discharging-rods of machine serving as circuit-breaker; but the circuit-breaker is in the primary circuit, and the person in the secondary. The make and break in the primary is accompanied with a current in the secondary.

"hundred trillions of vibrations per second, which would produce "many wonderful results.' If, then, I were to be asked how the "franklinic current differs from the ordinary faradic coil, I "should reply, that this one difference of rate of alternations "alone placed the two far apart. But, it may be urged, the cur- "rent of the induction coil is far greater than that of the influ- "ence machine in quantity. This is, of course, true. But at this "point in favor of the franklinic interrupted comes in the element "of time. 'The motor nerve,' says DuBois Reymond, 'is not "stimulated by the absolute density of the current density at any "given moment, but by variations from one instant to the other, "and the effect produced by these rapid changes, increases with "their rapidity and their greatness in a given time.' Thus, in "the great rapidity of the alternating (oscillating) currents of the "spark discharge, particularly in rapid series as in the franklinic "interrupted, we find the reason, despite smallness in quantity, "for accomplishing work in producing nerve and muscle stimu- "lation, equal at least to the comparatively slow discharge of the "interrupted galvanic, or the slowly oscillating induction coil"

"I have at least made it clear, I trust, that the new current is "strongly differentiated from the galvanic or faradic currents, "and taking into account its enormous voltage or pressure of "53,000 per linear centimetre, viz., its power of overcoming re- "sistance in the human body conductor, and its other character- "istics as above pointed out, we have a right to expect in its "medical applications physiological effects differing from those "of the other electric currents thus far brought into medical use."

"Among other purely physical experiments I have made, I "will mention that it produces a loud and clear sound in a tele- "phone resembling the sounds made by an induction coil."

"*Clinical Properties and Medical Uses of the Franklinic* "*Interrupted Current.*—Since we are dealing with a current and "not a spark, and with the familiar sponge or metallic electrodes, "we may invade the entire field traversed by galvanism and "faradism, and ascertain for ourselves such differences and simi- "larities as may exist."

"Applied to a motor point, the franklinic interrupted current "produces most vivid and persistent muscular contraction *with a* "*minimum of pain;* applied farther back on the trunk of a "motor nerve it throws large groups of muscles into contraction. "The contraction *is peculiarly painless as compared with that* "*of faradic coils, and the influence is remarkably diffusive.* "Accompanying a contraction of a large group of muscles is a "peculiar sensory sensation of lightness and buoyancy of the "member. *The painlessness, diffusiveness and buoyancy* may "all be experienced by holding the two electrodes in the hands, "and taking a current as strong as possible. Most people will "readily submit to flexions successively at the wrists, elbows, "and even to the shoulders, before insisting upon taking no "more. The arms during the passage of the current feel as if "made of cork, and this feeling of lightness persists for some "time. The quality of the current is such, that while energeti- "cally exciting the motor function of the nerve-filaments, it fails "to excite or may even annul, to an extent, the sensation of "muscular pain. Its penetrating, diffusive, painless effect, with "strong muscular contractions, adapt it admirably to general "application over the entire body as an electric in place of an "ordinary massage."

"It is, of course, applicable to every form of muscular para- "lysis, for there is no practical stimulus to nerve and muscle ex- "cept the electric, and none more energetic than this form "of it."

"Its effects upon the Hallerian irritability of the muscular "tissue necessarily includes an effect upon the local circulation "of a part and upon the lymphatics, and to this may doubtless "be referred many clinical results of relief, as in lumbago and "all forms of muscular rheumatism, subacute and chronic rheu-"matic affections of the joints, ovarian or pelvic pain, sciatica or "other neuralgias."

"The second prominent characteristic of this current is its "power of relieving pain. Leaving out of sight the part, be it "more or less, played by circulatory changes referred to, in this "respect there seems to exist a specific analgesic quality in the "current. The cotton feeling in the hands, and subjective sense "of buoyancy in the arms, is in itself an evidence of this. But "the effect upon pelvic pain, upon ovaritis, upon neuralgias, "pleuritic 'stitches,' tonsilitis, and many other pain affections is "still better evidence. In sciatica, for instance, the sensation of "pain is frequently quickly relieved and a cure obtained, though "I think in this case the cause is twofold, that is to say due to "both the circulatory and the analgesic effect. The same I "believe to be true in the pelvic and ovarian pains."

"The results in such cases, in my opinion, are far superior to "anything attainable by a faradic or a galvanic application. As "no observations on the purely analgesic effects of this current "have hitherto been made, I must leave others to test the ques-"tion for themselves. And as the paper is not clinical, but "simply to outline the subject in a general way, I cannot burden "it with cases."

These views, as to the peculiar properties and physiological effects of high frequency, high potential currents, were read December 2, 1890, and published January 24, 1891, while the electrical mechanism for raising and varying the potential and securing a high rate of oscillation by means of the spark gap and condensers was described and published in 1881, both publications being therefore prior to all others.

"The motor effect, including the circulatory, cannot be denied; "the pain-annulling effect, though clinically demonstrable, is "difficult to explain. Perhaps the extraordinary frequency of "the alternations of the current per second may explain it. "These alternations, it will be recollected, I computed might "easily amount to twenty thousand million per second. My own "view is that the great frequency, the fineness, so to speak, of "the electric vibrations, which we know as a matter of fact are "set up in the nerve-filaments, interferes with and annuls the "pain impulse. The carbon filament of a glowing incandescent "electric lamp, situated ten feet away from an influence machine "discharging sparks, has been seen to break. It had evidently "been thrown into violent vibration. If it had had a vibration "of its own before being subject to the electric vibration, it "would have lost it and fallen under the influence of the stronger "vibratory influence. Again a vibrating tuning-fork may lose "its motions by reason of the interfering vibrations of another "more powerful one, and finally vibrate in unison with it. Or "electrically a stretched platinum wire may be thrown into "visible vibration by the alternating current which at the same "time heats it to redness. Medically, I venture the suggestion "that the nerve-filaments are set into similar vibrations, and that "these overcome the pain by simple agitation of the mass of the "constituent elements of the nerve-fibre, and thus an annulment "of its capacity to conduct pain impulses, just as concussion or "anæsthesia of brain-tissue may be said to annul its capacity to "respond to sensory impressions. The experiments of Mortimer "Granville with his percuteur taught us the benumbing influence

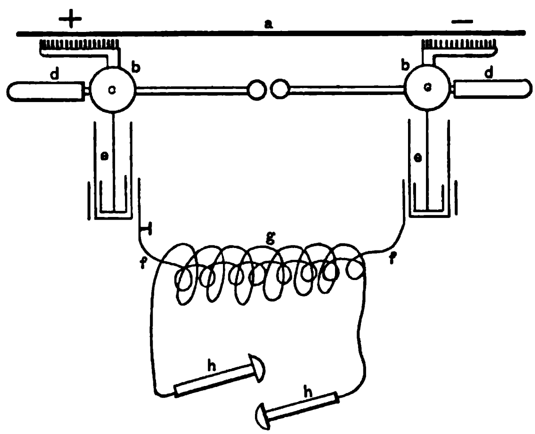

FIG. 6. Holtz Machine with Condensers.

a, Rotating plate; *b*, *b*, collecting combs; *c*, *c*, prime conductors; *d*, *d*, discharge rods; *e*, *e*, Leyden jars; *f*, *f*, primary wire of induction coil; *h*, *h*, electrodes.

"upon painful nerves of even coarse vibrations. With alterna- "ting electric impulses of twenty thousand millions per second, "more or less, we may find explanation of the analgesic effect of "the franklinic interrupted current. Furthermore, its static "quality would enable it to set up an influence in anatomical "parts regardless of the intervening medium, as witness the ac- "tion of the Phelps-Edison induction telegraph, where an induc- "tive circuit sufficient to work a telephone may be set up across "an air-space of even forty feet. Surely the analogies of electro- "physics are safe guides, and the only safe standpoint from which "to study the action of electricity upon the human body."

Fig. 6 represents a transformer for my condensed currents. Upon actuating the influence machine and approximating the discharging rods to a point where a continuous stream of sparks crosses the air-gap, the current from the two outer coatings of the condensers is caused to pass through a primary, and is taken off from a secondary. One of my coils, now in use for several years, and made for me by the Waite and Bartlett Manufacturing Company, has a *primary* of No. 32 wire of 600 ohms resistance, and a secondary of coarse wire 2 ohms resistance. Such coils worked only with a very small spark, owing to the difficulties of insulation, until the advent of oil insulation, when their efficiency became greatly increased.

Exhausted bulbs provided with a single conducting wire within, and a condenser plate without, glow brightly when placed in the

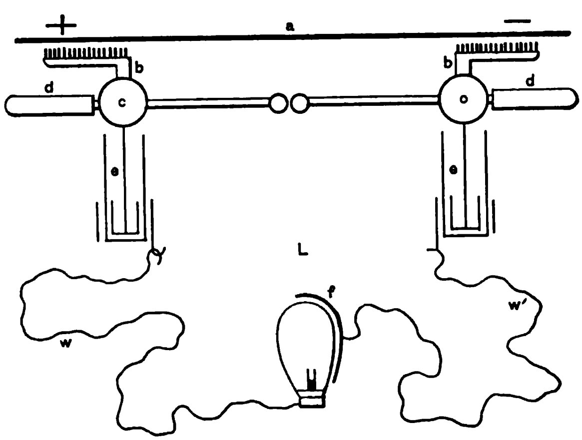

FIG. 7. Parts of Influence Machine and Condensers as before described.

L, an Edison 16-candle power lamp with broken filament; *f*, a piece of tinfoil laid against a portion of the bulb; *w*, wire leading to filament; *w′*, leading to tin foil.

secondary circuit. The physiological effects are most peculiar, and I must leave their description to another occasion. In some of my transformers I substitute flat spirals of wire for the solenoid form.

To illustrate the beautiful illuminating effect that may be produced with the condenser current, I will make but one simple experiment, first calling your attention to a diagram showing the connections.

Fig. 7, parts of influence machine and condensers, as before described, L; an Edison 16-candle power lamp with broken filament, *f*; a piece of tin-foil laid against a portion of the bulb, w w[1]; w, wire leading to filament, w leading to tin-foil.

Now that the lights in the hall are turned down, you see that the glow is most brilliant for a glow lamp effect. As we increase the size of the Leyden jars and cut down the length of the spark crossing the air-gap, the purple color changes to whitish and the effect is intensified.

When the static induced current is transformed, as in Fig. 6, the incandescent lamps are attached in the usual manner.

The physiological properties of this current are:

a. Great diffusiveness of the action whereby large groups of muscles are simultaneously caused to contract with a minimum of pain;

b. An anæsthetic or analgesic effect whereby large areas feel buoyant, and a degree of numbness;

c. A vaso-motor effect whereby perspiration is caused, and dilatation of superficial blood vessels;

d. An elevation of bodily temperature.

Any of the ordinary forms of electrodes may be attached to the terminals, and the current may thus be used externally and internally as in ordinary galvanic and faradic administration. Owing to the relatively minor degree of pain it causes, while still producing strong muscular contractions, and the cubic area through which it acts, it is peculiarly useful in reaching into the deeper parts of the body, as, for instance, when it is desired to reach the brain, spinal cord and internal organs. I have found this current of great value in diseases of the brain, spinal cord and nerves, in rheumatism, gout, goitre, anæmia, etc., etc.

In addressing such a representative body of electricians, I have perhaps been tempted too much towards the physical aspects of electricity. And if I have dwelt at some length upon my own small contributions to high frequency, high potential currents, it is that having been looked upon as riding the hobby of statical electricity a little too hard by some of my best medical friends, I now find myself—a mere fleck in the great stream—borne along by my prior publications, of 1881 and 1890, into the tide-way of one of the most fascinating of the multiform directions of advance in electrical science. For, in 1880, as has been said, I had already published the method of producing a secondary current of high frequency and high potential, by means of a primary circuit which included discharging rods (with air-gap), and condensers in this circuit, and had pointed out peculiar physiological effects due to increasing the frequency of the current by this means.

CONCLUSION.

We are now entering upon the new era of the non-medicinal treatment of disease, especially chronic diseases. The leaven that has worked toward this end was implanted in this country away back in 1860 by one of the ablest men of his day, Dr. Jacob Bigelow, of Boston, in a treatise upon "Nature in Disease." It was a protest against expecting that every disease had its medicinal antidote. The days of a general and comprehensive antidotal treatment are numbered. The first dawns of light were the recognition that many diseases were self-limited, and could be left largely to Nature's processes. Then came the recommendations for fresh air, for exercise out-of-doors, for congenial surroundings, and appropriate food. Next the recognition of the importance of massage, of Swedish movement, of hydrotherapy and of electricity, viz.: of treatments which essentially produce an increased demand for the oxygen of the air, for an increased capacity on the part of the blood to convey it to tissue, and a correspondingly increased oxidation of the tissue and assimilation of food. The patient's "respiratory circulation" is improved—consequently his nutrition improves, and, finally, his so-called disease disappears. This remark applies most strictly to that great class of diseases which oppress humanity, viz.: functional diseases, so called. Electricity leads the way in the advance; its dosage is measurable with precision; its effects are equally precise; they constitute the objective evidence of the treatment, viz.: acceleration of the heart action, of the blood circulation, regulation of the temperature, and increased absorption of oxygen, with a corresponding increase of waste products, like urea, creatinin, carbonic acid and water—the ashes of the human system—while at the same time the poisonous by-products, like uric acid, etc., viz.: materials due to incomplete oxidation, are diminished.

A human body is an oxydizing furnace, and the draught is increased electricity. If the draught is good, the fuel is completely burned, the apparatus does not choke up, the ashes are at a maximum and the highest degree of energy is derived from the mechanism—in our case, a living, human, fuel-consuming and oxydizing organization.

In short, to put the matter tersely, if crudely, we shall live longer and be healthier, if we burn out rather than rust out.

The law of life is motion—light, heat, chemical affinity, all are motion; possibly material matter itself is ether in motion—the processes of life are chemical processes—the resultant energy developed, is the mechanical force displayed by our muscles—the neural impulses conducted by our nerves, and the action of what we call our mind. Muscles, nerves and brain are but organic foci for the development of these energies by physical processes.

Electricity, judiciously employed, promotes the activity of the physical processes underlying life and health. It may even constitute the motive power which drives the machinery of life. Be that as it may, we know that we can alter the running of the vital machine for weal or for woe by intelligently turning an electric current in upon it.

We physicians, when we use electricity, are also electrical engineers; but our workshop holds its problems of health and of life and death. Do you, the members of this society, ever pause to realize that you are relieved from this responsibility? Do you wonder that those who would grasp this problem are ever striving for aid—and that you, with your trained minds, and special researches, may be of vast aid?

Are not our pathways parallel and often harmonious? That they may be is my devout wish.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

New York, November 15th, 1893.

The eighty-first meeting of the Institute was held at 12 West 31st Street this date, and was called to order by President Houston at 8 p. m.

The President:—The Secretary will please read the minutes of the last meeting.

[The Secretary proceeded with the reading of the minutes.]

Dr. M. I. Pupin:—I move that the reading of the minutes be dispensed with.

[The motion was carried.]

The President:—Unless the Institute desires to take up other work we will proceed with the regular business of the meeting.

The Secretary:—At the meeting of Council held this afternoon the following associate members were transferred to full membership:

[Approved by Board of Examiners, October 3, 1893.]

Arnold, Bion J.	Consulting Engineer, General Electric Co., 4128 Prairie Ave., Chicago, Ill.
Parkhurst, Chas. D.	Lieut. 4th Artillery U. S. A., Fort McHenry, Md.
Dion, Alfred A.	Supt. and Electrician, Chaudiere Electric Light and Power Co., Ltd., Ottawa, Ont.
Uebelacker, Chas. F.	Firm of E. P. Roberts & Co., Mechanical and Electrical Engineers, Cleveland, O.
Lloyd, Robert McA.	Electrician, 2 West 36th St., New York.

Total 5.

The President:—We will now proceed to the regular order. The first thing will be the discussion of the report of the special committee of Council appointed at the last meeting on "Local Meetings of the Institute." Mr. Herbert Laws Webb is chairman of that committee.

[Mr. Webb read the report, which is printed, as amended by the meeting, at the end of the discussion, see page 623.]

The President:—Gentlemen, you have heard this report. What action will you take on it?

Mr. Townsend Wolcott:—I move the report be accepted.

[The motion was carried.]

THE PRESIDENT:—Will you now consider the report as a whole or seriatim?

A MEMBER:—I move it be considered seriatim.

THE PRESIDENT:—If there is no objection, it is so ordered.

"The first recommendation is that "it is the opinion of the com- "mittee that provision should be made for local meetings of the "members at points inconveniently distant from New York for "the reading and discussion of papers accepted by the INSTITUTE."

"The committee is of opinion that Section 6 of the Rules of "the INSTITUTE, authorizing the secretary 'to call a special meet- "ing on a requisition signed by fifteen or more members' was "not intended to and does not cover local meetings of the char- "acter contemplated in this report."

Rule VI refers to meetings. The part under consideration reads: "Special meetings may be called whenever the Council sees fit; "and the Secretary shall call a special meeting on a requisition "signed by fifteen or more members."

MR. JAMES HAMBLET:—I move the adoption of that as read.

THE PRESIDENT:—It is moved that Sections 1 and 2 of this report be adopted.

THE SECRETARY:—Do I understand that this now becomes the official interpretation of Rule VI.?

THE PRESIDENT:—That particular part of Rule VI., certainly; so far as the calling of special meetings of this character is concerned.

THE SECRETARY:—I would like to ask, simply for my own information, if the committee, or others, in considering this rule, have taken it as based on its reading, or on what was intended by the authors of the rule that it should mean; and whether, when this meeting passes upon it, that it becomes the official interpretation of the rule. There have been prior decisions in regard to this rule—decisions by presidents and others. It is immaterial to me; but it would seem that the rule should be interpreted strictly as it is read.

MR. WEBB:—I do not see how there can be any difficulty of that kind, because we simply say that the rule does not apply to meetings of the character intended.

MR. WOLCOTT:—As I understand, the Secretary says there have been previous decisions on this same point; and if there is no objection I would like him to read the previous decisions, if they are available.

THE SECRETARY:—The decision is not in regard to meetings held anywhere in particular, because when the INSTITUTE was organized there was no really official place of meeting. The Rules of the INSTITUTE, as I stated in my paper, were based on those of the American Institute of Mining Engineers, which does not even hold meetings in New York City regularly—although its headquarters are here. Its meetings are held in various parts of the country. At the meeting of the INSTITUTE held May 18th, 1886, when these monthly meetings of the INSTITUTE were first under consideration, the President said in regard to the discussion of holding meetings:

"The discussion is an informal one. There is no motion before "the meeting. As far as this matter of dining together and "arranging about a room is concerned, that is a matter that the "individual members can do as they please about. They have "the privilege of meeting here if they want to, and if they prefer "to meet somewhere else, they can make their own arrangements. "All we have to consider, so far as I can see is, what authoriza-"tion from the main body of the INSTITUTE is necessary so as to "give the proceedings an official character."

Later on, after a committee on monthly meetings had reported, Captain Michaelis, since deceased, but who was present when the rules were amended, made the following remarks in regard to the holding of meetings:

"I happen to have an intimate knowledge of certain questions "of the constitution of the INSTITUTE, for the reason that I was "present at the meeting when they were amended. I think it "very well to have monthly meetings, but you can only reach "them in one or two ways, without an amendment to the consti-"tution. They can only be held as special meetings, unless they "be social gatherings and do not partake of the formal character "of authorized meetings where business can be done. The con-"stitution provides that meetings may be called in two ways: by "the Secretary on the written request of fifteen members; or the "Council may call a special meeting. This committee can only "make recommendations, and then either have a call signed by "fifteen members of the INSTITUTE and presented to the Secre-"tary, who would then call a special meeting, or they might "recommend to the Council to call a special meeting at such a "time as they saw fit, but that is the only way. This meeting "cannot provide for the monthly meetings. In fact there can-"not be monthly meetings until the constitution is amended. "There can only be special meetings called in the manner indi-"cated. I do not desire to make any point about it at all, but "only to state what the constitutional provisions are."

MR. GEORGE M. PHELPS:—I take it that the development of our work during this last nine years has not been founded on any very precise organic or constitutional provisions, and whatever may be the fact in regard to the growth of our monthly meetings which we have held in New York, whether they were well or ill founded upon any rules at that time, they have become by prescription and use a recognized feature of the work of the INSTITUTE, and I can see no harm in determining one way or the other, yes or no, in respect to the interpretation put upon that clause of Rule VI referred to in the report of the Committee. It seems to me that whatever the relations of these New York meetings that have been held may be to the rule, it would be very undesirable indeed to let the interpretation go forth of that portion of Rule VI referred to, which would permit fifteen members anywhere to hold a meeting that should have an official character as a meeting of the INSTITUTE. And it was with that in view that the committee thought it was in their province, since that point had been brought up by the Secretary and others—they consider it within their province to suggest an authoritative interpretation, or a limitation rather of the scope of that clause in Rule VI. It seems to me eminently desirable to have a decision on that point.

THE PRESIDENT:—Gentlemen, if you read Rule VI. carefully, I think you will find no difficulty whatever. I do not see how it is possible to put any such construction on the rule as appears to have been put on it by some. This is a special meeting of the INSTITUTE at which the regular officers of the INSTITUTE are to preside that is referred to in Rule VI. Rule VI. refers to other meetings of the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS held under its regularly elected and constituted officers, that shall be held in addition to the annual meeting. It is customary in all by-laws of societies to provide means whereby other meetings than the regularly appointed meetings can be held on a proper call of the members. But to consider this rule as applying to other meetings held elsewhere, at which the regularly appointed officers of the INSTITUTE do not preside, would be folly, to my mind. According to that, it would be possible to divide the INSTITUTE into as many separate meetings, held in as many separate places, as you could divide your membership by fifteen or more. Clearly that meaning was never intended to be put on the rule. This would be a fair case: If in San Francisco or in Lynn or in any other city, fifteen or more members of the INSTITUTE wanted to hold a meeting at which the regular officers of the INSTITUTE could preside, it might be possible to hold it there, but I do not think it would be possible under the by-laws. If you ask for my decision that would be my interpretation of the by-law.

MR. WOLCOTT:—These meetings have no legislative power. I do not see how they could divide the INSTITUTE in any sense. They are not allowed to do anything except what is done at the other meetings—simply read papers and discuss them.

THE PRESIDENT:—I beg pardon, but what are you discussing now, Rule VI.?

MR. WOLCOTT:—This kind of meeting—I said that in connection with your remark.

THE PRESIDENT:—My remark applies to Rule VI. and not to these particular meetings. I was asked for a decision, and my decision was that the special meetings referred to under Article VI. were not the same as the local meetings.

MR. PHELPS:—If you will pardon me one moment, I believe the thing immediately before us is the adoption of clauses 1 and

2 of the report—the first committing the INSTITUTE to the declaration that it is desirable to provide for local meetings, and the second clause interpreting the clause in Rule VI. as not applying to such meetings. Am I right?

THE PRESIDENT:—Yes; and I understand the gentleman wishes to speak to that motion. Am I right, sir?

MR. WOLCOTT:—Yes, sir.

THE PRESIDENT:—You have the floor, sir.

MR WOLCOTT:—If it is in order in that connection, I will ask who is supposed to interpret the Rules officially.

THE PRESIDENT:—I imagine, sir, that you elect your President for that purpose when he presides. In the absence of the President, I imagine that you elect the Council for that purpose.

MR. WOLCOTT:—I wanted to raise the question whether the Council is the official interpreter of the Rules.

THE PRESIDENT:—Perhaps you would be willing to express to the INSTITUTE what you think Council is elected for if it is not to interpret the Rules of your body.

MR. WOLCOTT:—It seems to have plenty of other duties.

THE PRESIDENT:—If there is no other discussion, Mr. Hamblet's motion is now before the INSTITUTE. Mr. Hamblet moved, that Sections 1 and 2 be approved. That question is now before the INSTITUTE. Are you ready for the question?

[The motion was carried.]

THE PRESIDENT:—I will now read Section 3. This consists of recommendations A, B, C, and D. I will read A:

"Third. The Committee presents the following plan for the "holding of local meetings of members:

"A. When not less than twenty members in any stated local-"ity shall in writing notify the Secretary of the INSTITUTE of "their desire to hold local meetings, such request shall be pre-"sented to the Council at its first meeting thereafter. The "Council shall then, upon the recommendation and nomination "of the signers of the request for local meetings, appoint a local "honorary secretary who shall be a member or associate member "of the INSTITUTE residing in the specified locality."

Section A is now before you.

THE SECRETARY:—In order to remove all question of doubt as to the interpretation of the rule, I would move that the number of members as fixed by the committee, be changed from twenty to fifteen; not because I am fully satisfied that a local membership of fifteen would be sufficient to lead to a healthy series of meetings, but because, in my opinion, which is respectfully submitted, there might be a question raised as to the interpretation of that rule in regard to the number, and I would be glad to hear from the committee as to any reasons they may have for changing the number.

MR. PHELPS:—It has just been interpreted by the meeting that that rule does not apply to this case.

THE SECRETARY:—Mr. President, I am fully aware of that. But we have with us thirty members, perhaps forty, and we have outside probably seven hundred members, and with all due respect to the gentlemen here, I presume that they are capable of reading the English language, and I simply offer that as a suggestion to remove any possible doubt as to the propriety of the report—not that the Rules be altered and made twenty to conform with the committee's report, but that the committee's report be amended to conform to the Rules.

MR. WEBB:—I should like to say that fixing the number at twenty was precisely with the object of cutting off all connection with that rule.

THE PRESIDENT:—A very sensible reason.

MR. CUTTRISS:—I cannot see that our worthy secretary's argument has very much bearing on this question—that there are only forty people present out of our membership in New York. It is the fault of the other people not being here to discuss it.

THE SECRETARY:—I am perfectly satisfied with that. It is a good and cogent reason. But at the same time it appears to me that this is a mere question of detail, and I do not wish you to think that I am insisting upon it because it was my interpretation of the Rule. I do not care to make any attempt that that idea should prevail. It is simply a question of removing this doubt, and it is a question between fifteen members and twenty members. It may be, as the chairman of the committee said, that that number was recommended to remove all relationship to the rule. So far as that is concerned, I would be just as willing to have it twenty-five as twenty or fifteen I would not for a moment be inclined to listen to ten. I think it better to go up rather than down. It is simply in the interest of having it jibe, so to speak.

MR. MARTIN:—I think there is a great deal of wisdom and good sense in what Mr. Pope has advanced, if we remember the fact that when we started our New York monthly meetings we considered that the limit in the rules for New York was good enough if it was fifteen. Whether we consider the old rule or not, let us at least be generous enough to look at our fellows elsewhere and give them a chance to start their meetings, immediately on the same basis as we started our own. I think the committee is not particularly anxious about twenty. I do not know that it will make any particular difference as to the number of monthly meetings we could start, whether it be fifteen or twenty. Mr. Pope is willing to have the limit fifteen. If that will give him ten monthly meetings to look after instead of five, be it on his head.

MR. W. J. HAMMER:—When this matter came before the committee the original limitation was fifteen, and as Mr. Webb says, when the matter was thoroughly discussed it was decided to make it twenty in order that this might not in any way clash with the words in Rule VI. There are two cities, Chicago and Boston, including the Lynn delegation, which could raise twenty members or more. Philadelphia has nearly that number, and probably in a short time they could get sufficient new members to get it up to twenty. I think the INSTITUTE should go a little slowly in this matter, whatever action is taken, and it seems to me twenty is low enough. I think it is just as well to steer clear of that point of fifteen, so that hereafter there will not be any discussion as to the interpretation.

THE PRESIDENT:—If there is any lingering doubt in the minds of the forty here, as there appears to remain in the Secretary's mind, as to Rule VI having any connection whatsoever with the calling of local meetings, I would urge on the INSTITUTE to make the number something else than fifteen, or we will find that will be urged as a reason—as an interpretation of the rule. Whereas clearly I think there can be no doubt that Rule VI has nothing to do with this case. It is a different kind of meeting entirely. Rule VI is intended to cover a special meeting of the INSTITUTE held wherever it may be held, under the direction of the regularly appointed officers of the meeting. No other than that can be a meeting of the INSTITUTE. To suppose for a moment that we could have ten or fifteen meetings of one and the same INSTITUTE is, to my mind, very illogical. If therefore there is going to be any question in the hereafter as to whether or not Rule VI applies to local meetings, I hope, if the INSTITUTE agrees with me in this very simple English, that they will carry out the recommendation of the committee in making it some other number than the number referred to in Rule VI.

MR. PHELPS:—I understand the Secretary to move, in order that we may have something before us to discuss—to move as an amendment that the number be changed from twenty to fifteen. Is that so?

THE SECRETARY:—That was my intention, yes.

THE PRESIDENT:—Is that motion seconded?

MR. WOLCOTT:—I second it.

THE PRESIDENT:—It is moved and seconded that paragraph A of Section third of the committee's report be amended to read "When not less than fifteen members" in place of "when not less than twenty members."

[The amendment was lost.]

A MEMBER:—I move that the recommendation A be adopted as read.

[The motion was carried.]

THE PRESIDENT [Reading]:—"B. The duties of the local "honorary secretary shall be in general to serve as a channel of "communication between local members and the general body of "members through the Secretary and Council. But no member "in any locality shall be debarred from direct communication "with the INSTITUTE."

A MEMBER :—I move the adoption of that as read.

[The motion was carried.]

THE PRESIDENT [Reading]:—"C. In any locality where a "local honorary secretary shall have been appointed, local meet- "ings may be held, the local members to elect their own chairman. "Said meetings to be known as the meeting of THE ——— MEM- "BERS OF THE AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS. "Such meetings shall be for the purpose of reading, by the "authors or by proxy, and of discussing papers accepted by the "INSTITUTE, and such papers only. Such local meetings shall be "held simultaneously with the INSTITUTE meetings or subse- "quently. That is to say no paper before the INSTITUTE shall be "read or discussed at a local meeting in advance of its reading "at the INSTITUTE meeting."

DR. CHARLES E. EMERY :—Mr. Chairman, I move the adoption of that section, and in connection therewith beg permission to present my views on the general subject very briefly. The plan proposed I consider an admirable one as a tentative movement. Action of this nature is necessarily experimental. You cannot at first say what will be necessary or what the outgrowth will be. I am much obliged to the committee for mentioning so prominently the plan I outlined at the previous meeting, and I venture to say, I think that something of that kind will eventually grow out of the present movement. At the same time I am moving to have the present matter tried, because it is simpler and it has in some respects an advantage in keeping the matter entirely within the hands of the INSTITUTE, which I can see the members would be pleased to have arranged. The difficulty about the membership of other societies is not brought up in any way so long as it is kept entirely within this society, and if the pressure for this additional class of members or a class of members having certain privileges, which were provided for in the other paper, comes up strongly enough here, some provision can be made for such distinction. I am very much pleased with the report of the committee as a whole, and would have no hesitation in moving that the rest be adopted. But I think we will do better to adopt it by sections, and I take pleasure in moving the adoption of this particular section.

MR. MARTIN :—Might I ask for the reading of the clause that relates to the presiding officers?

THE PRESIDENT [Reading]:—"The local members to elect their own chairman."

MR. MARTIN :—The only point I wanted to get at was, whether it was the intention of the committee that the chairman should be chosen month by month or whether it should be an annual office; because it seems they are creating two offices at once, within the scope of the report.

MR. WEBB :—The intention was that the chairman might differ from meeting to meeting. Each meeting would elect its chairman.

Mr. Hammer:—The only local officer the Institute recognizes is the honorary secretary.

Mr. Phelps:—I suppose that under the rules suggested by the committee it would be open to the local meeting to elect a chairman at each meeting or to take one for three months or six months as they pleased. It is their affair entirely. Mr. Hammer has covered it by reference to the fact that the local honorary secretary provided for, is the only official person created by the action of the Institute.

The President:—Does that answer your inquiry, Mr. Martin?

Mr. Martin:—Yes. But I think it would be a little more business-like to have some one whose duty it is to be there and attend to the running of these meetings.

The Secretary:—I would like to inquire what would be the status for instance of a Vice-President in one of those cities where a meeting of this kind was held; whether his official capacity as an officer of the Institute would not give him a natural precedence as the presiding officer and cover the point Mr. Martin suggests.

Dr. Emery:—I would be glad to accept an amendment to my motion that a Vice-President present at the meeting shall preside, or that the presiding officer shall be selected by the meeting.

Mr. Hammer:—When this was before the committee I think I had the honor of recommending that if these local departments were organized in Chicago and other cities, thereafter the Institute could make their presiding officer a Vice-President of the Institute, or if there was a Vice-President located in that section that he would be recognized as the local officer. But the committee decided, and I think wisely so, that this ought to be left entirely to the judgment of the local members; that the Institute does not desire to dictate to them, and outside of desiring to endorse their nomination for an honorary secretary, that the government of the meeting was left entirely to the local members.

Mr. Phelps:—The chances are, I should say, about 100 to 1 that in any locality desiring local meetings and possessing among its members a Vice-President—the chances are about 100 to 1 that he will be selected as the local chairman, and that would seem to cover the matter entirely. Of course, so far as his status is concerned it is to be the same as that of any other member.

The President:—If you will pardon the Chair for taking part in the discussion, isn't there just a little bit of difficulty here? That if you have a Vice-President of the Institute to preside always at a local meeting that that local meeting will come to assume a little different grade than some other local meetings?

Mr. Phelps:—I do not think so, sir.

The President:—It is somewhat an unfortunate thing that in our discussions here everything is put down and printed. Still I will not hesitate to express myself freely. The question of local meetings is a very difficult question properly to handle. On the one hand we have a large membership that finds difficulty in getting to New York and naturally wants to get all the advantages out of the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS that it can. On the other hand we do not want to set up rival meetings which may and probably will in the near future raise the question as to which is the governing body and which is the offshoot. I would rather see, personally, these local meetings presided over by any officer that they may choose to select, and to let the plan as formulated by the committee that the local honorary secretary be the only official that the main body recognizes. There is no objection, of course, to the local body having a Vice-President as their presiding officer. They probably would select him if he was in the place. But I think it would be preferable not to say that they must have him. Else I fear this question would arise in the near future as to whether there was a difference in the grade between that kind of a local meeting and a local meeting in which they had only a presiding officer who was not a regularly elected officer of the INSTITUTE. Of course this is for you to decide. I think it is an important matter.

MR. PHELPS:—Precisely, Mr. President. A Vice-President residing in a given locality, if elected, will preside at those meetings, not as a Vice-President of the INSTITUTE, but as chairman of the meeting.

THE PRESIDENT:—It is moved and seconded that Section C be adopted as read.

[The motion was carried.]

THE PRESIDENT [Reading]:—" D. Wherever a local meeting " shall have been provided for as in the foregoing section, the " local honorary secretary shall be supplied by the Secretary of " the INSTITUTE with advance copies of papers to be read before " the INSTITUTE, which copies he shall distribute to the local " members before their regular meetings. The local honorary " secretary shall transmit to the Secretary of the INSTITUTE a re- " port of the discussion or comments on papers that he may " receive from members in his locality."

THE SECRETARY:—I wish to apologize for speaking too frequently on this subject, but I understand that this is the sole business of the evening, and the report is being read for the purpose of being discussed—

THE PRESIDENT:—Always glad to hear from you, Mr. Pope.

THE SECRETARY:—The Committee in recommending that advance copies be sent to the local secretary for distribution has perhaps failed to note one of the provisions of the postal regulations, and the conditions under which these papers are mailed now, and also the fact that it should be our purpose to relieve the local secretary as much as possible from the drudgery of an honorary office. I would therefore submit to the consideration of the INSTITUTE the fact that under ordinary circumstances the papers as now printed are mailed in advance direct to the members and would probably reach them from one to two days earlier than if they were sent to the local secretary and distributed by him. By the plan of the committee the local secretary would not only have to go to the trouble of redistributing them, but of paying two cents local postage. We mail them from New York City at one cent per pound. When the regular mail edition did not go out, the general secretary even then could mail from this office direct to the members and save a day's time. That appears to me a technical objection to the plan suggested.

Dr. Emery :—I second the amendment.

Mr. Phelps :—I think Mr. Pope has presented an extremely valuable point and has corrected the committee in an important matter. As a member of the committee I can say that I believe the impression of the committee was—it was my own impression—that at present the advance copies of papers were only sent out within a restricted district. I was not aware that they went to the entire membership of the Institute. If that is the case I can readily see that some amendment to this section is desirable.

The Secretary :—I would say further, Mr. President, that ordinarily it is my purpose to get them in the mail on Saturday night. In that case they reach Chicago, for instance, on Monday morning, at the same time that they are distributed in New York City, which is perhaps two or three days in advance of the meeting.

Mr. Phelps : —Then they are, as a matter of fact, sent to the entire list of members?

The Secretary :—They are all mailed at the same time.

Mr. Hammer :—A change of a few words here would meet the difficulty, I think. "Wherever local meetings shall have been provided for, as in the foregoing section, the local honorary secretary shall be supplied by the Secretary of the Institute with a number of advance copies of papers to be read before the Institute, which copies of papers he shall distribute to the members 'at' their local meeting", using the word "at" instead of "before". It seems to me that the insertion of those three words would cover that point, and not at all interfere with Mr. Pope's suggestion.

The President :—Mr. Hammer moves to amend as follows: To insert after the word "with" "a number of", making it read "with a number of advance copies"; and to strike out the word "before" and substitute the word "at", making the clauses read "which copies he shall distribute to the local members at their local meetings"; making the whole clause read as follows:

"Wherever local meetings shall have been provided for as in "the foregoing section, the local honorary secretary shall be sup-"plied by the Secretary of the Institute with a number of ad-"vance copies of papers to be read before the Institute, which "copies he shall distribute to the local members at their local "meetings".

Mr. Phelps:—I suggest the insertion of the word "suitable" —a suitable number.

The President:—Do you accept that?

Mr. Hammer:—Yes, sir.

Secretary Pope:—I suggest the substitution of the word "may" for "shall".

The President:—Do you accept that?

Mr. Hammer:—Yes, sir.

The President:—Gentlemen, are you ready to vote on the amendment?

Those in favor of amending Paragraph D of Section 3, as read, will signify their assent by saying aye—contrary minded, no.

[The motion was carried.]

The President:—Now we will take a vote on the article as amended.

Mr. Martin:—I would like to ask for information, Mr. President, about this very important point as it seems to me. The local honorary secretary is burdened with transmitting to the Secretary of the Institute the discussion at the local meetings. I would like to understand how that is to be done and what it would lead up to, and where we shall come out.

Dr. Emery:—There is a further point—who is to discuss the papers? That raises the question at once of local membership of this society. It was attempted at one time to get some revenue from such discussions, but it is not well to bring that question up now, but only to see that it must be met sometime. The question is whether we shall say the discussions shall be by members of the Institute only.

The President:—We will take these two points up separately. Mr. Martin wishes to ask how the discussions shall be obtained by the local honorary secretary. Shall they be stenographic reports?

Mr. Martin:—We have already decided, I think that is the understanding, that this Rule VI is not to have a great deal of influence upon us in the discussion of this matter, but it is provided in the rule that any member or associate may introduce a stranger in the meeting and that the latter shall not take part in the discussion except with the consent of the meeting. If we apply that rule to such meetings it seems to me we have protected ourselves with a safeguard against any such contingency.

The President:—I think it would be well to put a clause in that no stranger shall take part in the discussions or proceedings of such meetings. Would you add without the consent of the meeting?

Mr. Martin:—My idea is that as far as possible, without in any way limiting the liberty of the meeting, but in order to get harmonious and similar action everywhere, the conditions should be the same everywhere, so that the paper is discussed at each meeting under like conditions. Now we make a provision here in this rule in regard to the people who should be allowed to take part in the proceedings, and such a limitation, it seems to me, would be just as pertinent anywhere else as it is here in New York. That is the point I am making. It seems to me that we do not want to dictate to any local meeting any more than is necessary, but it would be very awkward indeed to have a lot of discussion tumbling in upon the Secretary from people who have been invited to speak and then after their remarks have been accepted to have them ruled out at some subsequent time. Even chairmen of committees come under that ordeal occasionally, and it is not at all pleasant.

The President:—Do you make a motion to that effect?

Mr. Martin:—I would make a motion that that clause be added—that any member or associate may introduce a stranger to the meeting, but the latter shall not take part in the proceedings without the consent of the meeting.

The President:—I understand Mr. Martin to have made this motion. That there be added to Section D of Article 3 of this report the following: "Any member or associate may intro-"duce a stranger to any meeting, but the latter shall not take "part in the proceedings without the consent of the meeting."

[Mr. Martin's amendment was adopted.]

Mr. Martin:—There is a further question I want to raise here, and that is about getting that report. As I understand it, these local secretaries will do this work out of love for the Institute, and in one way or another there will be plenty of work for the secretary. Do we expect him to make us a *precis* of what takes place, or do we expect him to get a stenographer and send in a full verbatim report of whatever takes place there? Or what do we want?

The President:—I think that would be left to them.

Mr. Phelps:—If I may be permitted, I think the committee was very desirous of not prescribing any very particular rule or method for these local meetings. It was desired to leave them as far as possible to their own devices. Now, it would be obviously to the interest of those local meetings to take their own meassures for having accurate reports of what they do, and send them here. The publication being under the same restrictions as the publication of proceedings here. There would be very little difficulty on the part of the editing committee, to dispose of them. I think it would be best to leave them to their own devices in that regard.

Mr. Martin:—As it stands, the remarks made in these meetings here in New York, are taken down in full and that is done at the expense of the Institute. We may consider ourselves a meeting of the Institute, and practically are, for all purposes, at the present minute. But whether we like it or whether we do not, we are simply the New York section of the Institute sitting here to-night, as a matter of fact, in the constitution of the meeting to-night. Now I think we want to get these things as nearly as possible on an equal footing with equal liberty all around.

THE PRESIDENT:—Were it not for the fact that we are not discussing that, I would like to ask Mr. Martin's reasons for that statement.

MR. MARTIN:—Excuse me, sir. We have probably coming before us, at an early date, a paper by a Chicago member, and the discussion very likely may take place largely in Chicago. Now I think that we would like to have that discussion as full as possible, and I think we want to have a pretty clear understanding how that is going to be done; at our expense, or whether the local members who are to take part in that, having already paid their subscriptions to the INSTITUTE, are to make an additional subscription for the sake of getting their remarks into print, I really think it is a very material and essential point.

THE PRESIDENT:—I understood you to say that whether we like it or not, we are sitting here—will the stenographer read it?

[The stenographer read Mr. Martin's remarks.]

THE PRESIDENT:—That I take exception to, as President of the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS. We are not the New York section, but we are the INSTITUTE.

MR. MARTIN:—Well, Mr. President, I like to hear such sentiments from yourself, being a Philadelphian. [Laughter.] But I am glad to stand up here to-night and say I believe that that feeling I expressed is largely the feeling of the members throughout the country, and in every word I say, and in every remark I make upon the subject, I am trying to consider the interest of these men. If there is anything we can do to-night that would give us a larger membership in any of those places than we have to-day, if instead of having thirty members in Chicago we can get three hundred, I think that, as members of the INSTITUTE, seeking its best and its largest interest, we should take just that action which will secure us that number of members.

THE PRESIDENT:—I am rather sorry that the discussion has taken this phase. Yet I never object to taking the bull by the horns when I meet him. It only shows that what I said early in the meeting, as to the danger I apprehended in the near future coming from this tendency to divide into sections, has come somewhat sooner than I thought it would. But that is the question that we have to meet. Now I question whether any very great majority of us here would at all like to be considered as the New York section, and I question whether the truest interests of the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS would be at all subserved by any such feeling. There must be some ruling, governing body, and we are as competent to be that here—I do not say that out of any local pride—we are more competent to be that here, because the INSTITUTE had its birthplace practically here, and has had its home here for some time, and I do think, especially at the present time, when the INSTITUTE appears to be entering on a career of usefulness, that we ought to be exceedingly careful in our legislation how we permit anything to enter into our ranks which will tend to separation rather than concentration, to dilution rather than concentration.

MR. MARTIN:—I have had the pleasure of spending a month recently in Chicago, where I have spoken with many of our distant members. The one desire of those men is to have no division or separation, but to remain in the INSTITUTE and to get the most they can out of it, and they think they can get the most out of it, that they can help to increase its influence, its reputation, and its glory by having some such arrangement as we are working for to-night. What that may lead us to, I do not know. This a country where the federal system has won its greatest triumphs, and I think we can adopt that system in the INSTITUTE just as well as in our national government.

THE PRESIDENT:—I am very glad to hear Mr. Martin say that. For the point that struck considerable alarm in my heart was the statement which still rankles in my mind that we are the New York section.

MR. MARTIN:—I will say the New York and Philadelphia section, if that will please you, sir.

MR. A. E. KENNELLY:—Concerning the question raised by Mr. Martin, suppose that a paper from a section is brought to New York to be read, that by reason of the attendant circumstances most of the discussion of that paper will take place at the locality where its author resides; under the arrangement that is suggested in this report there is no provision for a stenographic report except at the expense of the local members. It would be impossible to make arrangements generally for stenographic reports. When all the local sections that meet under such a régime come into existence, that matter would surely have to be undertaken at the outset at least, by the local sections themselves, and if any such assemblage finds that it cannot undertake a stenographic report, surely the individual speakers are capable of writing their own reports and transmitting them through the secretary.

THE PRESIDENT:—Do I understand Mr. Martin wishes to add any further amendment to D?

MR. MARTIN:—I simply threw out those remarks for discussion. This committee's report, as a whole, is very wise and is so liberal, so broad in its scope, that there is really very little to improve upon. But at the same time I think we want to understand clearly and fully what we are doing. When we see these bulls we want to take them by the horns and throw them. If we simply let these points slide now, I think we will regret by and by that we passed them over in this graceful and elusive manner.

MR. R. O. HEINRICH:—The amendment of Mr. Martin is covered in the Rules of the INSTITUTE, and I beg to ask the question how far the Rules of the INSTITUTE apply to these intended meetings, whether there is anything specified in the report of the committee how far the general rules, with all their amendments, apply to the meetings of the local sections. I refer to the amendment of the rule; the part of Rule VI which refers to the taking part in the discussion by a stranger. As I understand, it is contained in Mr. Martin's amendment.

THE PRESIDENT:—That has all been passed.

MR. HEINRICH:—I simply asked the question,—since this is contained in the Rules of the INSTITUTE, why is it put in there again? That would rather indicate that the general rules of the INSTITUTE would not apply to the local meetings if we have to put it in again here.

THE PRESIDENT:—I think it is perfectly proper to put it in. As I understand it, these local meetings are not actual meetings of the INSTITUTE. I do not see how they can be. We cannot have more than one INSTITUTE. They are meetings of members who cannot attend at the fountain head. We can get a repetition to a certain extent. You cannot regard these as absolute meetings of the INSTITUTE. As soon as you do you must have anarchy. There can be but one head-centre. I think it is perfectly proper to put that in. Now, gentlemen, these very discussions bring to my mind more clearly than anything else possibly can, at the very outset when we are trying to arrange for meetings which shall not absolutely be meetings of the INSTITUTE—somebody gets up and asks how are these going to be meetings of the INSTITUTE? They cannot be really meetings of the INSTITUTE unless you acknowledge that you have separated your INSTITUTE up into as many parts as there are separate meetings. Therefore you have something superadded, not so much as a part of the thing but as a place where the members can get an early account of what is going on at the INSTITUTE. Therefore I think it is perfectly proper to prescribe rules and very necessary to prescribe rules.

MR. WOLCOTT:—Then it is the ruling of the Chair that these Rules we have here do not apply to those local meetings unless they are adopted again for the local meeting.

THE PRESIDENT:—That would be my idea exactly.

MR. WOLCOTT:—It would seem to me that the way to keep the INSTITUTE whole would be to have those same Rules for all those meetings. Now as I understand it the meetings will be without rules, and they will go on without rules or will adopt rules for themselves.

THE PRESIDENT:—Gentlemen, you will pardon me. I must ask the members to have some consideration of rules of order. We are now discussing D. We stopped on Rule VI. some time ago. Those in favor of Section D standing as amended, will signify their assent by saying aye—contrary minded, no.

[The motion was carried.]

THE PRESIDENT [Reading]:—"E. The local honorary secretary "shall transmit to the Secretary of the INSTITUTE all papers "offered by local members, but any member may send papers "directly to the Secretary of the INSTITUTE."

MR. HAMMER:—I move that it be adopted.

[The motion was carried.]

THE PRESIDENT:—"F. The publication of the discussion at "the local meetings in the TRANSACTIONS of the INSTITUTE shall "be subject to the same regulations and restrictions as govern "the publication of discussions at the regular meetings of the "INSTITUTE. No publications of papers or discussions at local "meetings in local or other journals or newspapers is to be per-"mitted without the sanction of the Council, or the Secretary "of the INSTITUTE."

DR. EMERY:—I move its adoption.

[The motion was carried.]

THE PRESIDENT [Reading]:—"G. The expense of local meet-"ings shall be borne by the local members and shall not become "a charge upon the funds of the INSTITUTE." Will you consider that in connection with the note? I will read the note: "Re-"ferring to Section G, above, relating to the expense of local "meetings, the committee believes that when gatherings of local "members shall assume large proportions, say to the number of "one hundred, as they may well do in some localities, it will "then be advisable to consider charging some portion of the ex-"pense of local meetings upon the funds of the INSTITUTE."

DR. EMERY:—I would suggest that the words "in general" be put before the section, and that a clause be added modifying the previous words, at the discretion of the Council. I do not recollect the words well enough to put the words I propose in there. But I would like to have the discretion left to the Council to pay some expenses.

THE PRESIDENT:—I will read so that you can put in: "In "general the expense of local meetings should be borne by the "local members and not become a charge upon the funds of the "INSTITUTE unless otherwise ordered by Council."

DR. EMERY:—Except as specially authorized by Council."

THE PRESIDENT:—Except it may be authorized by Council.

MR. PHELPS:—I fear the effects of that amendment. I fear it would lead to invidious distinctions. If we were to have special local meetings and the considering of bearing the expense was left to the Council, they might elect to pay them in one locality on one particular occasion and not in another locality and on another occasion. And I fear that a general rule would be the only way in which we could meet that thing.

THE PRESIDENT:—Is Dr. Emery's motion seconded?

MR. HAMMER:—In Chicago I believe the Armour Institute has extended privileges to local members if they organize there, and I think the same thing would be done in Boston, and I think the Franklin Institute will offer similar facilities in Philadelphia, and the very important expense there will be assumed until the local membership becomes so large that they feel they want to have some special headquarters. The view of the Committee was to show the disposition of the INSTITUTE to meet that point, so that when these meetings grew to be an important factor or an important portion of the INSTITUTE in point of numbers that the INSTITUTE would then take some steps to assume a portion of their expense.

MR. MARTIN :—I would like to say that while in no wise do I take back what I said before about sharing the expense of the reports, Mr. Phelps' remarks strike me as admirable and weighty in this respect. We are going to leave upon these men locally the extra expenses, as I understand it, as they crop up, unless they happen to run up over a hundred. As a general thing it seems to me it is a better policy to help the weak rather than the strong, and perhaps a little handful of fifteen or thirty men would need and deserve our assistance more than a body that happened to be a hundred strong. Take, for instance, the little handful of thirty at Lynn at the present time. We have read a piece of poetry that tells us that even the Thomson Scientific Club cannot pay its rent. I think we should extend the light of our countenance to our friends at Lynn, and if they have a rattling good discussion, to chip in and pay part of the expense. But as Mr. Phelps thinks it is hard to choose in that way, perhaps they will tax themselves two or three dollars a head. I think in the long, perhaps in the short run, it will lead to a general toning up of the subscriptions. I know as a New York member I should not feel satisfied with paying ten dollars while some fellow member down South or out West is taxing himself an extra five dollars to get what I am getting in New York at his expense.

THE PRESIDENT :—I do not find that Dr. Emery's motion is seconded. Am I wrong? Will you take action then on Section G?

MR. PHELPS :—Although one of the committee who made this report, I can perceive a great deal of force in what has been said with respect to these financial questions, and if the other members of the committee think as I do, I would be willing to recommend that in all local meetings the charge for stenographic reports of discussions should be borne by the INSTITUTE. That would not be a very formidable charge. In respect to the place of meeting, as has already been said, in all places where we are at all likely to have any local meetings, there will be free quarters provided somewhere. As has been said, they are offered in Chicago, and they are offered in Philadelphia, and I believe in Boston. I can see the justice of the point, especially as Mr. Martin has presented it. We are trying to offer an opportunity to members to take part as we do here. I do not know that it would be fair to ask them to pay any more a year for the privilege of doing that than we do.

MR. CHARLES CUTTRISS :—I must say that I cannot agree perfectly with Mr. Phelps in saying that it will be a small item although I allow that he has more idea of the expense of a stenographer than I have. But if any body of twenty members can get together and have their stenographer and get talking wildly as they very often do, that man will have considerable work, and we will find bills coming in monthly or yearly, as the case may be, that will amount up into the fifties, and I do not think that the INSTITUTE wants to burden itself blindly with any unknown expense of that kind.

MR. MARTIN:—That is not half so good a point as it sounds. If, as Mr. Cuttriss says, we did allow the stenographer to run away with the report, I do not doubt that we would have such bills. But, as a general rule, in most cities you can get a meeting reported for a flat rate sum—$10 or $15. If we are going to have six meetings, say $10 or even $15, that would be $90. Suppose we had eight meetings a year—$720 for stenographic reports is not such an awful load as to break this camel's back.

THE PRESIDENT:—You will pardon your Chair, sir, if he is a little obtuse. The discussion appears to me to take a direction as if it were a foregone conclusion that those members were never going to come here and meet any more; that they had absolutely gone out from us and that we were subjecting them to a very great hardship by asking them to pay more for what they get than for what we get. As I understand it, that is not the position at all. They willingly join this society because there is no other such in this country. They have asked in addition for some extra privileges. It is quite right in equity that they should pay for them. I think it would be very dangerous for this society to assume the expense of stenographic reports.

MR. W. B. VANSIZE:—In regard to this subject I should like to say that I heartily agree with the expressions and the opinions of the President.

MR. MARTIN:—It is, of course, very unfortunate that those men cannot be with us, men who have been in our fellowship and who can really appreciate and enjoy what they know as an INSTITUTE meeting. I cannot blame those men for desiring to establish with the old fire a new altar and a new hearth somewhere else. I have spoken with men who come from far Western States in some of the largest cities there, and I have no doubt they would like to be here and enjoy the fleshpots of Egypt. But they have taken their fleshpots in their hands and gone West. But I think we ought to want to be with them and wish them all the Godspeed we can, and simply because we happen to stay behind not to want to keep to ourselves all that happens to be left behind. These men are willing not only to stand up and be counted, but are willing to stand the expense that will come from this new régime. I think the reaction may be such as Mr. Phelps has said as to the New York members, that when we find these men are paying more we will not sit down quietly and allow them to do it.

PROF. FRANCIS B. CROCKER:—I think the idea of having stenographic reports of local meetings paid by the INSTITUTE at the present time, at the beginning of this experimental matter, is injudicious. In the first place it would be very expensive. I have had some little experience with stenographic reports during the last summer. How can you give the man who lives in a place where he is the only member perhaps, how can you give him the same for his money as the man who lives in New York? It is an impossibility. That is his misfortune. When I went to Chicago to the Fair, I was not paid my railroad fare by the citizens of Chicago. It was simply my misfortune that I lived in New York and had to go to Chicago in order to see the Fair. It seems to me that the geographical distribution of cities in the United States is not the fault of the INSTITUTE and cannot be rectified by it. [Laughter.] A thing like that is so self-evident, so palpable even to local members, that I think no one could possibly expect, especially a resident of a small town, that he could have equal advantages. The INSTITUTE now has all it can do to pay the necessary expenses of carrying on the main body. If in any way it can help out of town members, I should be heartily in favor of doing it. But when it comes to paying just as much in every place where they can raise twenty members as the main body pays here, it seems to me entirely unreasonable.

MR. PHELPS:—We have all of us talked to-night on the assumption that the moment we pass this legislation we shall be surrounded with a swarm of local meetings in all parts of the country and that they are likely to overwhelm us and ride over us and squelch us. As a matter of fact, I think we are talking about a man of straw. I do not think there will be, in a year from now, more than two local meetings, even if there be one, and the purpose of the committee was to offer a tentative scheme which could not be objected to by the people in two or three regions who have been talking of local meetings. This subject was first brought up last spring in respect to California. Every opportunity was given to the people out there to tell us what they wanted. I believe we have heard nothing definite from them since. At Chicago there has been some movement toward local meetings, which is likely to take some active shape after some action of our own. But any apprehension that we have of a multitude of meetings in small towns all over the country is a bugbear.

MR. NELSON W. PERRY:—I have been in Chicago about six months, and I think, so far as the feeling there is crystallized, the members out there do not want us to go to any extra expense. They are willing to stand whatever extra expense there may be. It seems to me that it is perfectly absurd for us to pay the local expenses there, because we cannot tell what they may be. We are not asked to do that. I endorse the recommendation of the committee thoroughly from beginning to end.

THE PRESIDENT:—The question now is on Section G: "The expense of local meetings shall be borne by the local members and not become a charge upon the funds of the INSTITUTE." Will you take a vote on the section as it stands? Those in favor of passing it as it stands will signify their assent by saying aye—contrary minded, no.

[The motion was carried.]

THE PRESIDENT [reading "H."]:—The title, name and address of each local honorary secretary shall be printed in the publications of the INSTITUTE.

A MEMBER:—I move that it be adopted as read.

[The motion was carried.]

THE PRESIDENT:—Will you now move to adopt the report as a whole?

A MEMBER:—I move that the report be adopted as a whole.

[The motion was carried.]

REPORT OF COMMITTEE ON LOCAL MEETINGS.

TO THE COUNCIL.

Your Committee, appointed at the meeting of the Institute of October 18th, to consider the subject of Local Meetings, beg to report as follows:—

1st. It is the opinion of the Committee that provision should be made for local meetings of the members at points inconveniently distant from New York for the reading and discussion of papers accepted by the Institute.

2d. The Committee is of opinion that the provision in Section VI. of the Rules of the Institute, authorizing the Secretary to "call a special meeting on a requisition signed by fifteen or more members" was not intended to and does not cover local meetings of the character contemplated in this report.

3d. The Committee presents the following plan for the holding of local meetings of members:—

A. When not less than twenty members in any stated locality shall in writing notify the Secretary of the Institute of their desire to hold local meetings, such request shall be presented to the Council at its first meeting thereafter. The Council shall then, upon the recommendation and nomination of the signers of the request for local meetings, appoint a Local Honorary Secretary, who shall be a member, or associate member of the Institute residing in the specified locality.

B. The duties of a Local Honorary Secretary shall be in general to serve as a channel of communication between local members and the general body of members through the Secretary and Council. But no member in any locality shall be debarred from direct communication with the Institute.

C. In any locality where a Local Honorary Secretary shall have been appointed, local meetings may be held, the local members to elect their own chairman, such meetings to be known as MEETING OF THE ——— MEMBERS OF THE AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS, and such meetings shall be for the purpose of reading, by the authors or by proxy, and of discussing, papers accepted by the Institute and such papers only. Such local meetings shall be held simultaneously with the Institute meetings or subsequently; that is to say, no paper before the Institute shall be read or discussed at a local meeting in advance of its reading at the Institute meeting.

D. Wherever local meetings shall have been provided for, as in the foregoing section, the Local Honorary Secretary shall be supplied, by the Secretary of the Institute, with a suitable number of advance copies of papers to be read before the Institute, which copies he may distribute to the local members at their local meetings. The Local Honorary Secretary shall transmit to the Secretary of the Institute a report of the discussions at each local meeting, together with any written discussions or comments on papers that he may receive from members in his locality. Any member or associate may introduce a stranger to any meeting, but the latter shall not take part in the proceedings without the consent of the meeting.

E. The Local Honorary Secretary shall transmit to the Secretary of the Institute all papers offered by local members, but any member may send papers directly to the Secretary of the Institute.

F. The publication of the discussion at local meetings in the Transactions of the Institute shall be subject to the same regulations and restrictions as govern the publication of discussions at the regular meetings of the Institute. No publication of papers or discussions at local meetings in local or other journals or newspapers is to be permitted without the sanction of the Council or the Secretary of the Institute.

G. The expense of local meetings shall be borne by the local members and shall not become a charge upon the funds of the Institute.

H. The title, name and address of each Local Honorary Secretary shall be printed in the publications of the Institute.

In dealing with the subject before it, and in preparing its report, your Committee has carefully considered the documents and correspondence placed in its hands; particularly the paper of the Secretary on "Monthly Meetings" and the plan for the establishment of chapters of the Institute drawn up by Dr. Emery, both of which were read at the October meeting.

The scheme for local meetings suggested in this report has been designed with the purpose of affording to members, distant from the headquarters of the Institute, opportunity to participate in its work on substantially the same footing as members whose location permits them to attend the meetings held in New York. The Institute is a national body, having members in every section of the country, but with a large preponderance of membership relatively near headquarters. Its work and its publications have a national significance. Its subdivision into sections or chapters would detract from the unity and strength of its efforts to serve the best interests of electrical science and industry.

Your Committee, therefore, is of opinion that it would not be for the welfare of the Institute to create any separate local organizations or any class or classes of members not now provided for in its Rules.

It is believed that the plan for Local Honorary Secretaryships and for local meetings herewith submitted would meet fully the desire of members in diverse parts of the country to participate in the work of the Institute and that it

would avoid the disadvantage and disintegrating tendency that might arise from the formal establishment of a number of separate and distinct sub-societies.

HERBERT LAWS WEBB, *Chairman,*
A. E. KENNELLY,
M. I. PUPIN,
WM. J. HAMMER,
GEO. M. PHELPS.
} *Committee.*

New York, November 10, 1893.

THE PRESIDENT:—I wish to report, in addition to this, on behalf of the committee appointed on Congress work that the committee has had meetings and has considered the questions involved, and that the following gentlemen have agreed to undertake the work of determining the standard of light and of illumination: Professor Edward L. Nichols, of Cornell University; Prof. Charles R. Cross, Massachusetts Institute of Technology; Thomas A. Edison, of the Edison Laboratory; Dr. Frederic A. C. Perrine, of the Leland Stanford Jr. University; Dr. Louis Duncan, of the Johns Hopkins University; Prof. F. B. Crocker, of Columbia College; Prof. Reginald A. Fessenden, of the Western University of Pennsylvania.

I think the INSTITUTE is to be congratulated on the very high standing of the gentlemen who have kindly agreed to undertake the work in behalf of the INSTITUTE. It is the intention of the committee as soon as it can do so, to call a conference of the chairmen of the local sub-committees in some central place, probably New York, to discuss and agree on methods and plans. Does Mr. Kennelly wish to submit to the INSTITUTE in this connection the report of the Committee on Units and Standards or does he wish it to be printed and submitted afterwards?

MR. KENNELLY:—I think, sir, it would be better to print it first and take it up afterwards.

THE PRESIDENT:—Gentlemen, I do not see anything else on the card.

THE SECRETARY:—Mr. President, what I was going to ask was in regard to the Committee on Revision of the Rules which was re-appointed at the annual meeting. This is a committee appointed at the annual meeeting 1892 and re-appointed at the annual meeting 1893 to consider a revision of the rules, and I thought it well to call attention to it—the secretary of the committee being present—he could report at some future meeting so that we could have that thoroughly discussed and in shape in the course of two or three months. And Mr. Martin has also suggested that it might be well to have a committee prepare the substance of the report of this committee on local meetings for incorporation in the rules also, and if it will be satisfactory to the meeting, I will suggest that this same committee be given this matter in charge.

MR. HAMMER:—I would like to ask, as this has been adopted by the INSTITUTE, whether this does not become part and parcel of the Rules of the INSTITUTE pertaining to these meetings.

THE PRESIDENT:—Certainly, I suppose so.

THE SECRETARY:—I do not understand it so.

THE PRESIDENT:—It would not become an amendment to our by-laws since that would require an action of a different character. But as I understand Mr. Hammer's question, it is this—whether it needs any revision to become a matter of our by-laws. For my own part, I think that the committee on rules could very well put that in shorter phraseology. In other words, I do not suppose that it can possibly become a part of our rules, because that would be an amendment to them, and our rules require a written notice of a proposed amendment to be given at the previous meeting. We have agreed to do a certain thing. We need not make it a part of our by-laws, or we can, just as we choose. If it is the desire of the INSTITUTE then it would be proper to refer it to the committee on revision of rules. Then if they report it, it will be necessary to report it as an amendment of our rules and it would require the regular two months to go through.

THE SECRETARY:—I do not wish to insist at all that it should become a part of our established rules, especially as it is well understood to be an experimental matter, and to avoid the further revision of the rules in the future I think it would be well to stand merely as an order of the INSTITUTE or Council to be carried out. If eventually it proves entirely satisfactory it could be incorporated in the Rules.

THE PRESIDENT:—Do you wish to press that motion?

MR. HAMMER:—No, sir, I think that covers it.

Adjourned.

AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

NEW YORK, December 20, 1893.

The eighty-second 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 PRESIDENT:—The Secretary will read the minutes of the last regular meeting.

It was voted that the reading of the minutes be dispensed with.

THE SECRETARY:—The following associate members were elected at the meeting of Council this afternoon:

Name.	Address.	Endorsed by.
ETHERIDGE, E. L.	Inspector, Electrical Engineering Dept., World's Columbian Exposition, 66 No. Oxford Street, Brooklyn, N. Y.	L. S. Boggs. O. G. Dodge. Wm. H. Cothren.
LINDNER, CHAS. T.	Inspector, Electrical Engineering Dept., World's Columbian Exposition, Tacoma, Washington.	L. S. Boggs. O. G. Dodge. W. H. Cothren.
WILLIAMS, FRANK A.	Safety Insulated Wire and Cable Co., 25 Washington Avenue, Newark, N J.	W. J. Hammer F. R. Upton. W. T. M. Mottram.
MOORE, D. MCFARLAN,	Electrical Engineer, General Electric Co., 44 Broad Street, New York City	T. C. Martin. Jos. Wetzler. Edwin J. Houston.
MANSFIELD, ARTHUR NEWHALL	Assistant Electrician, American Telephone and Teleg. Co., 153 Cedar St., New York City.	F. A. Pickernell. Chas. R. Cross. G. A. Hamilton.
STURTEVANT, CHARLES L.	Patent Attorney, Atlantic Building, Washington, D. C.	W. A. Rosenbaum. Townsend Wolcott. F. L. Freeman.
PUFFER, WM. L.	Assistant Professor of Electrical Engineering, Mass. Institute of Technology, Boston, Mass.	Chas. R. Cross. H. V. Hayes. Geo. W. Blodgett.
COMSTOCK, LOUIS K.	Contracting and Consulting Engineer, Monadnock Building, Chicago, Ill.	Frank J. Sprague. C. T. Hutchinson. Geo. P. Low.

MUSTIN, HERBERT S.	Assistant Electrician, City of Hoboken, Police Headquarters, Hoboken, N. J.	Edw. Durant. James Hamblet. J. P. Wintringham.
WARNER, CHAS. H.	Consulting Electrical Engineer, 50 Broadway, New York City.	W. J. Hammer. H. A. Foster. Edwin J Houston.
McCLURG, W. A.	Manager, Electrical Dept., Plainfield Gas and Electric Light Co., 25 Madison Ave. Plainfield, N.J.	R. W. Pope. E. A. Merrill. W. M. Miner.
ROBERSON, OLIVER R.	Electrician, Western Uuion Telegraph Co., 195 Broadway, P. O. Box 856, New York City.	James Hamblet. G. W. Gardanier. Alfred S. Brown.
WASON, LEONARD C.	Head Draughtsman with F. S. Pearson, 199 Harvard Street, Brookline, Mass.	J. P. B. Fiske. Chas. R. Cross. F. S. Pearson.
SAHULKA, DR. JOHANN	Docent of Electrotechnics, Technische Hochschule, Vienna, Austria.	Ralph W. Pope. N. S. Keith. Townsend Wolcott.
STEVENS, W. LE CONTE,	Professor of Physics. Rensselaer Polytechnic Institute, Troy, N. Y.	Samuel Sheldon. Edw. L. Nichols. James Hamblet.
SHEA, DANIEL W.	Assistant Professor of Electrical Engineering and Physics, University of Ill., Champaign, Ill.	Samuel Sheldon. H. V. Hayes. Dugald C. Jackson.
McKAY, C. R.	Consulting Engineer, 140 South Main Street, Salt Lake City, Utah.	Louis Duncan. Samuel Reber. H. S. Hering.
CAPUCCIO, MARIO	Electrical Engineer, Piazza Statuto 15, Corino, Italy.	T. C. Martin. Ralph W. Pope. G. S. Albanese.
HUDSON, JOHN E.	President, The American Bell Telephone Co., 125 Milk Street, Boston, Mass.	H. V. Hayes. Francis Blake. I. H. Farnham.
SAGE, HENRY JUDSON	Electrical Engineer, Telephone Dept., Western Electric Co., 227 S. Clinton St., Chicago, Ill.	Chas. R. Cross. E. M. Barton. P. H. Alexander.
FROST, FRANCIS R.	Assistant in Electrical Testing, Bureau of Awards, World's Fair, Ithaca, N. Y.	R. B. Owens. B. F. Thomas. Chas. E. Emery.
SERVA, A A.	Assistant, Bureau of Awards, World's Fair, North Industry, Ohio.	R. B. Owens. B. F. Thomas. Chas. E. Emery.
REQUIER, A. MARCEL	Electrical Engineer, Westinghouse Electric and Manufacturing Co. Pittsburg, Pa.	Chas. F. Scott. F. Stuart Smith. R. W. Pope.
JAEGER, CHARLES L.	Inventor, Maywood, N. J.	J. N. Johnson. Ralph W. Pope. Chas. E. Dressler.
McCROSKY, JAMES W.	Graduate Student, Johns Hopkins University, 1104 McCulloh St., Baltimore, Md.	R. B. Owens. W. E. Shepard. H. A. Foster.
COREY, FRED. B.	Electrical Engineer, A. B. See Manufacturing Co. 442 Henry St., Brooklyn, N. Y.	H. H. Eustis. Geo. D. Shepardson. Alonzo B. See.
NORTON, ELBERT F.,	Inspector, City Electrical Inspection, 15 City Hall, Chicago, Ill.	C. C. Haskins. C. G. Armstrong. Alex. Dow.

Total 27.

THE PRESIDENT:—Have you any further business, Mr. Secretary?

THE SECRETARY:—Nothing further, Mr. President.

THE PRESIDENT:—If not, the regular order of the business of the evening as announced on the card will be taken up. I take pleasure in introducing to you Mr. Albert Stetson who will read a paper on the Practicability of Electric Conduit Railways.

Mr. Stetson read the following paper:

A paper presented at the 82nd Meeting of the American Institute of Electrical Engineers, New York, December 20th, 1893, President Houston in the Chair.

THE PRACTICABILITY OF ELECTRIC CONDUIT RAILWAYS.

BY ALBERT STETSON.

Two years ago, if an electrical engineer had been asked as to the practicability of an electric conduit railway, he would probably have shrugged his shoulders and expressed great scepticism as to ultimate success. The writer hopes to be able to show that some real progress has been made since then, and that the way out of the difficulty is in sight.

Electric traction for street railways has come to stay, and we must choose between the trolley, secondary battery and a conduit construction (either slotted or closed). The trolley has been, and still is the most important factor in electric traction. It has done, and still is doing excellent work in educating the public into the beauties of rapid transit in cities. There is much talk in our sensational daily press about the "deadly trolley," but much of it is ridiculous, and most of it undeserved. Surely no member of this INSTITUTE has joined in this outcry, and it would be a move in the right direction if some of our daily papers would have attached to their staffs, men who are capable of judging of technical subjects, so that they might be able to impart correct ideas as to the march of human ingenuity and progress. Our daily newspapers have specialists to report society scandals, dog-fights, pugilism and horse-racing. But, is it not strange that none of them employs trained technical experts? The progress of the arts and sciences is certainly as interesting to the average man and woman, as is what ladies do and say in a Turkish bath! It is, perhaps, useless to undertake to reform the press in this respect, but until we can, we must expect from time to time to hear of the "deadly trolley." There is a field, and a wide one, for the trolley, and the men who have devoted their talents and their money to advancing electric traction by the trolley, deserve well of the race. Many men have worked in this field, but I believe that Mr. Frank Sprague, with his twelve miles of road in Richmond, working successfully, sent electric railways forward twenty years. All honor then to the trolley men! They have and will hold for a long time the field in smaller cities and towns, and for inter-town communication. Railroad men as a rule, are very conservative, and until a few months ago there was nothing in sight that gave promise of our being able to do away with the trolley in cities. Certainly they could not be expected to wait until something practicable appeared, nor could they afford to stop their cars as soon as they heard of fairly successful experiments with a conduit railroad. Large vested interests cannot be overthrown nor changed in a moment, and it will be interesting to see what position trolley men will take when the conduit has demonstrated its utility and practicability. I expect to find them more than ready to adopt it wherever the traffic will warrant them in so doing.

We have admitted the claims of the trolley people, and given them due credit. But there are also rights of the public that must be respected. The streets of our cities belong to the public, and not to corporations. So long as nothing better was in sight, it was to the advantage of the *public* to have rapid transit, even though the trolley were the medium. But the structure is unsightly, and the poles and wires are a constant menace to the public. A trolley current will not, probably, kill a man who has no organic disease, but it will kill an animal, it will terribly burn any one who comes in contact with it, and would probably cause the death of a man troubled with heart disease. The feed wires are a constant source of danger. A trolley wire may get crossed with an electric light wire carrying an arc current, and it then becomes a dangerous thing. If the streets are at all wide, very large poles, taking up too much sidewalk space, are used, and these invade public rights. We have a right to beautiful streets, and no corporation, for the sake of private gain, has a right to disfigure them. We have a right to safe streets, and no corporation has a right to contribute anything towards rendering them less safe. In our cities and large towns, trolley as well as other current conveying wires must go underground so soon as any economical and practicable method is demonstrated.

Secondary batteries have been used in nearly every large city of the civilized world, to supply the demands for electric traction, but have been given up as too costly. I know there are still people who are willing to spend their money on this *ignis fatuus*, but the number is growing beautifully less year by year. The writer has spent much time in investigating the subject here and abroad, has examined everything in this line that Europe has offered, and he asserts, without fear of successful contradiction, that not one instance can be found in the world where a traction secondary battery has paid an honest dividend. The cars (if supplied with sufficient power) are too heavy for ordinary track construction, the heaviest ballasted steam track being scarcely good enough to insure them a commercial life. The conditions essential to the life of a secondary battery, are large, thick and heavy plates. Such a battery can probably be commercially employed in lighting stations, but those are diametrically opposed to the conditions for a traction battery. In traction, the battery must be small (on account of the limited space at our disposal), and it must be light; for every 150 pounds of lead carried, means the cost of transporting a passenger. To run successfully, we must be able to ascend such grades as exist in railway work, and when sufficient battery power for this is carried, the car becomes unwieldy. A sudden call for power from a small battery may tear it all to pieces, and the "self-contained" car then becomes anything but an "ideal" motive power. When, in 1881, Sir Wm. Thomson carried across the channel, Faure's little box containing 1,200,000 foot-pounds of electrical energy, great hopes were excited, and those million foot-pounds of energy were soon changed into millions of shares, and that changing process has been going on ever since! The average investor did not, of course, know that those large figures represented about the energy of 1½ ozs. of coal, and that, if Sir William had brought his pockets full of good cannel coal, the supply of energy in Old England would have been much increased, but investors did believe that there was the "ideal" system for electric traction. The best electrical, chemical, mechanical and engineering skill was employed, improvements were made on the original cell, millions upon millions of dollars were spent, and the results have been financially disastrous. Remember, please, that, to any but the electrical crank, the treasurer's books are the court of last appeal. The experience has been costly, and the shores of Old England are strewn with the wreckage of secondary battery ventures, and the bones of many a victim lie bleaching in the sun! But has nothing been done since then that gives promise of better things? Judged by my standard (the dividends paid), I answer, "No!" and to one who understands the principles involved in a successful traction battery, there seems to be nothing in sight to bid us hope for a successful solution of the difficulty from this direction. Should future metallurgists make us acquainted with some new metals, or should a cheap supply of palladium become available, the problem may be changed. But the writer believes the experiments of the E. P. S. company in England, and those of the Julien company in this country exhausted the possibilities under commercial conditions, and that we have seen no better battery than theirs. It is often said that the patent complications stopped the secondary battery experiments in this country (they did not in Europe), but I am unable to understand how. The Brush company has the patent rights, and the Julien company, or its successors, the exclusive license, and I have not heard of any extraordinary exertions on the part of the Brush company to reduce their surplus by secondary battery experiments—and I do know that the cars used in the Julien company's Fourth Avenue trials are for sale at "genuine bargain" prices. I admit that I may be mistaken, but universal experience is a pretty safe guide, and any one trying to work out a system of accumulator traction has my sympathy, and my prayers that he may early see the error of his way and return to the fold before the last fatted calf has been killed and eaten!

In a paper read before the American Street Railway Association, at Pittsburg, on October 21st, 1891, Mr. George W. Mansfield said:

> "Our rival is the cable. It certainly does look as if for the "enormous sums they expend in making their system feasible, "we ought, for an equal sum, to make ours perfect. Mechanic-"ally it is an assured success, but electrically it has not so "proved."

In large cities, the cable is a real rival of electric traction, and in many places has proved commercially successful. But there are some things that may give the electrician hope. The cable roads of St. Louis have to a very great extent given up their cable and are using the trolley, while those of Kansas City and Denver are said to be in a bad condition financially. City railroads in the future will, no doubt, be electric, and it is questionable whether any more cable roads will be built. The cost of the powerful machinery required, the wear and tear upon the cable and the grip, the large amount of real estate required for their buildings constitute serious "first charges" upon the earnings of cable companies, and confine their operations to very large cities where traffic is enormous. Electric traction demands only a portion of the expense necessary for a cable road, and capital is seeking this method for investment, while "fighting shy" of cable roads. When a car is on the track, ready to be moved, it is a question of veracity between the electrician and the cable man as to which motive power can move it more economically. Perhaps the advantage lies a little in favor of the cable man, but when interest on capital invested in buildings, real estate, machinery and the natural deterioration are taken into account, the electrician appears to have much the better of the argument. It is, of course, true that the cable traction has greatly increased the revenue of the Broadway line, but so would electricity have done. It is a well known fact that the better the facilities offered for travel, the more people will avail themselves of them, to the increased profit of the company supplying the need. There is nothing in the cable success here in New York to discourage the electrician, and I hope to see in a few years a conduit system in operation on Broadway and Third Avenue.

There is one other class that may be mentioned in passing, where mechanical motors are employed on the separate cars. Many different motors have been proposed, such as carbonic acid gas, ammonia, gas engines, hydraulic motors, and compressed air. None of these have, to my knowledge, demonstrated their commercial utility, though I think the future outlook in this direction is promising. Some years ago, in company with Mr. Stauffer of this city, I rode over the road from Vincennes to Paris, on a line operated by the McClosky compressed air system. Its operation was all that could be desired from a spectator's standpoint, but I mistrust it had the same trouble as many others; good enough to spend money on for experiments, but unprofitable from a financial point of view.

We now come to a consideration of conduit roads, their excellencies and their defects.

For years, electricians have worked to develop a practicable conduit system of electric railway, and many attempts have come near to success. The most extensive and, until recently, the most successful was the Bentley-Knight system. An immense amount of money was spent, the best talent was employed, and extensive lines were built at Cleveland and Allegheny City. Experiments were made in New York and Boston, and with the withdrawal from the field of the Bentley-Knight system, disappeared the last hope of our being able to place bare wires in a slotted conduit exposed to the severe conditions of our American climate. There is not a city in America to-day, where a bare conductor laid in a conduit can earn a dividend on the capital invested. No matter what the system may be, no matter how carefully the insulators may be protected, unless the conduit is made air-tight and water-tight (which, of course, no slotted conduit can be), mud and dirt will get into the conduit and settle on the conductors. Leakage takes place from a conductor in proportion to the length exposed, and glass, porcelain, ebonite or any other of the so-called insulators, when covered with dirt, conduct the current as well as a similar layer of dirt elsewhere would do. Over the path in this direction, Nature seems to have posted the warning, "No thoroughfare!"

Mr. F. L. Pope, in an article quoted by Carl Hering[1], says, in speaking of conduit experiments: "Hundreds of patents have "been taken out and more than a million dollars have been dis-"bursed in paying for tuition in the costly school of experience. "More than once, and in more than one direction, success has at "times seemed almost certain; yet the truth compels me to say "that from the hard practical standpoint of dollars and cents, by "which every invention must first or last be tried, the net outcome "of all this vast expenditure of labor, time and money has, up to "the present moment, been almost insignificant. The reward "which awaits the fortunate person who succeeds in completely "solving this problem, may well be regarded as a potentiality of "wealth beyond the dreams of avarice. The problem of the un-"derground circuit does not at first sight appear to be a very dif-"ficult one. It renders necessary, in the first place, a construction "which will effectually resist the action of forces tending to dis-"turb the condition of the wires, and with the heavy traffic on the "streets this involves a very strong structure. It is absolutely

1. Recent Progress in Electric Railways, pp. 168–170.

"necessary that the conductors shall remain insulated from each "other and from the ground under all conditions of weather. The "exigencies of heavy rains and snows necessitate a construction "which shall permit of a thorough insulation of the conductors "and a drainage of the entire system. There are other minor "points which require to be taken into consideration. Without "going into details, it is sufficient to say that the conduit system "has been tried on an extensive scale in Denver, Cleveland, and "Boston, and to a lesser extent in several other places, but in "every case the continual interference consequent upon its use "has exhausted the patience of the traveling public and compelled "its abandonment."

Mr. Mansfield, in the paper quoted above (also Hering, p. 170), says:

"In spite, however, of all this refinement and study, prac- "tically nothing has been accomplished; and I have no hesita- "tion in saying that the continuous live conductor in an open "slotted conduit is to-day a failure, and that it cannot be made a "success throughout our cities of to-day, its fatal weakness being "our inability to prevent the conduit from becoming filled with "water, mud, etc. The time may come when our sewerage sys- "tems will be perfect enough to enable us to overcome this fatal "weakness. To-day, however, they are not, and even an opti- "mistic view puts this time a long way distant."

Mr. F. H. Monks, of Boston, says: "The conduit system has "been thoroughly tested, and has been shown to possess no com- "mercial value for the propulsion of street cars to date." The standard work, "The Electric Railway in Theory and Practice, by Oscar T. Crosby and Louis Bell, Ph. D.," says, p. 255:

*   *   *   *

"The fundamental difficulty with all slotted-conduit electric "roads is the enormous difficulty of proper insulation. This "arises from the very nature of the case, for the conductors are "placed in a tube of limited diameter in free communication "with the open air through the slot. Water, dirt and mud in- "evitably find their way in, and sooner or later the result has been "either a positive short-circuit at a single point, or general leak- "age along the line in sufficient quantity to paralyze its oper- "ation."

The Bentley-Knight System.

We have already referred to this ill-fated method. They failed, but they made a noble fight. They had talent, experience and money, and they did thoroughly demonstrate that a conduit road using a bare wire cannot operate commercially in our American cities. I do not say that it is impossible to find men who will still put their money into such systems, nor do I maintain that men cannot be found to spend shareholders' money in such experiments, but parties working in that direction can well learn from the sad experience of the investors in the Bentley-Knight road. The laws of electricity have not changed since then; no great improvements have been made in insulating bare conductors; and, although I have examined a number of conduits with bare conductors, none of them have shown better construction, such expert technical skill, such regard for the laws of electricity, and none came nearer success. It is the same old story, too great expense and the impossibility of insulation.

In this system there is a conduit between the rails, provided with a slot, through which passes a brush for making contact with the bare wires laid in the conduit. They use a constant

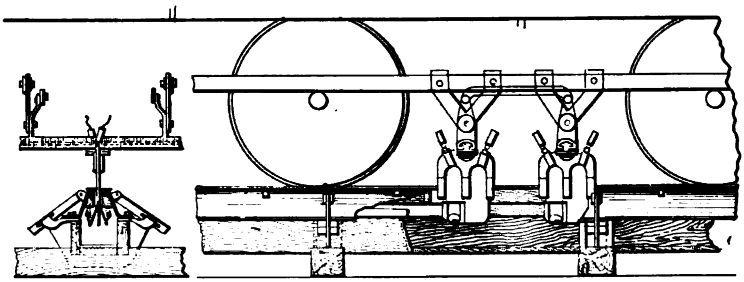

FIG. 1.—Bentley-Knight, Truck Equipments, Conduit and Plow.

potential, about 500 volts being the normal voltage. The current strength is about 7.5 amperes, and the motor used will stand 60 amperes for half-an-hour. The electrical equipment is mounted on the truck, entirely independent of the car body, and the car is started and controlled precisely as the trolley car is. The cut Fig. 1 shows the essential features of their construction, and at the left is shown a diagram of the conduit and the "plow." The "contact plow" is shown in the next cut Fig. 2. "It consists "of a flat frame hung from the car by transverse guides, on "which it is free to slide the whole width of the car, and extend-"ing thence down through the slot of the conduit. It is provided "with a swivel joint, so as to adjust itself to all inequalities of "road or conduit. The frame carries two flat insulated conduc-"tor cores to the lower ends of which are attached, by a spring "hinge, small contact shoes of chilled cast-iron that slide along in "contact with the two main conductors. At the upper ends are "attached flexible connections to prevent flashing at the contact." This road was worked in the winter time during the season of 1884-5.

The insulating support is "of vitreous material having support-"ing pins sealed in it for connecting it to the conductor and to the "wall of the conduit inclosing the conductor, and having a flange "at its base for protecting the conductor from the moisture that "may accumulate upon it or the walls of the conduit. To pre-"vent the water coming in through the slot from causing a leak "from the conductor to the metallic conduit, the insulator is pro-"vided with a water-shed flange at its base, which is preferably "curved outward on the form of a saucer, though any suitably-"shaped flange may be employed, and the insulator extends out

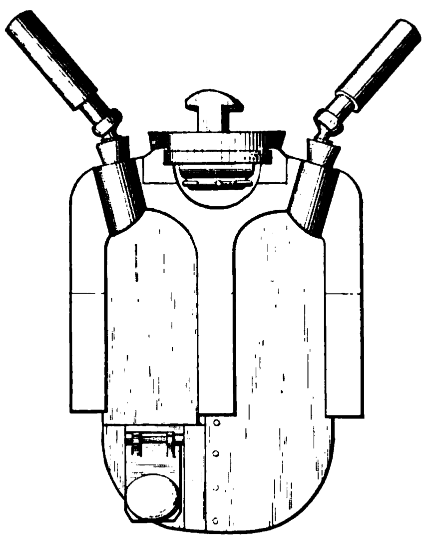

FIG. 2.—Contact Plow.

"horizontally from the conduit, so that the flange occupies an up-"right position." This is taken from U. S. Patent No. 455,339, of July 7, 1891, to Walter H. Knight, and further on in the same patent Mr. Knight sounds the key-note of the entire situation, and shows the rock upon which he and others using the same system have wrecked their barks. He says: "It has been found "that upon the insulators there is apt to be an excessive conden-"sation of moisture, owing to the fact that the conduit is beneath "the surface of the street, and that at certain times an excessive "leakage is apt to occur at certain ones of the insulators," and he goes on to say: "This leakage has a tendency to correct itself, "that is, the heat engendered by the passage of the leaking cur-"rent dries up the moisture which has given rise to it."

True, but that is rather an expensive drying process. Electric energy costs money to produce, and it loses as its distance from the prime motor increases, and then to use it for keeping the streets of cities dry (for it really amounts to that) is hard on the coal pile and the anticipated dividends! The Bentley-Knight conduit system was the harbinger of better things. Splendidly constructed and well managed, but working on the wrong principle, it stands to the commercially practicable conduit road of the present and the future, in about the relation of the telephone of Philip Reis to the finished product of Professor Bell's genius.

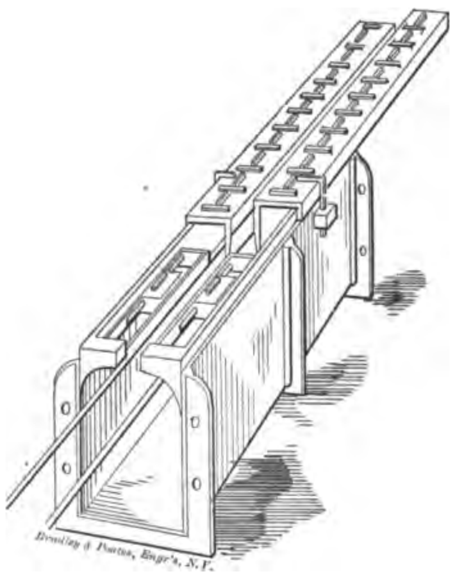

FIG. 3.—Love Conduit.

Of the Love system, now working in Washington, I have not been able to get any accurate or reliable information, and I have heard no favorable reports thereon from impartial authorities. Their conduit is well built, their road is level, and their wires are protected in the same manner as failed to protect the Bentley-Knight conductors. The details do not appear to have been worked out so completely as they were in the Bentley-Knight system. About Thanksgiving of last year I was in Chicago, and took occasion to visit and inspect the Love railway. It was not running at the time, had not been, I was informed, for a couple of months, and it was exceedingly doubtful when it would start again. I saw a plain, level piece of track, one-half to three-fourths of a mile in length, over which two cars were to be run, and at the power house a generator of 125 horse power! Now, gentlemen, that is not practical electrical engineering! There is the same bare wire in the conduit, and, until the laws of Nature are changed, there must be the same difficulties that have frustrated other attempts in the same direction. Of the Chicago conduit road Crosby and Bell say, p. 256: "The general character does not insure immunity from the difficulties that have caused the abandonment of similar attempts in the past."

THE BUDA-PESTH TRAMWAY.

At Buda-Pesth, in Hungary, is the only conduit road employing a bare conductor that has ever achieved measurable commercial success. The best description I have been able to obtain is found in the book of Mr. Hering: "Recent Progress in Electric Railways," pages 175 *et seq.*:

"The conduit, as seen in Fig. 25" (see Fig. 4), "is placed under one rail. It consists of castings having flanges of 18 centimetres (7 inches) placed every 1.2 metres (about 4 feet), the space between being a conduit of concrete. The oval shaped conduit has a width, clear, of 28 centimetres (11 inches), and a height of 33 centimetres (13 inches).

"The slot consists of two beam rails having no inside lower flange, and fastened to the conduit frames by wrought-iron angle pieces. The width of the slot is 33 millimetres ($1\frac{5}{16}$ inches), the total depth of the foundation below the rail top is 70 centimetres ($27\frac{1}{2}$ inches). The conductors, both positive and negative, are made of angle irons, secured, as seen in the figure, by means of insulators fastened to the castings. They are sufficiently high above the floor of the conduit to be protected from the water which may collect in the conduits. They are, furthermore, under the top of the oval, so that they cannot be touched from the outside. It should be noticed that there is no earth return used with this system, as both leads are insulated. The water which runs into the conduits is collected at the lowest points, and passes through settling boxes to the sewers. The second track may be of any desirable form, even only a flat rail. An objection to having the conduit under one rail instead of in the middle arises in cases where, by the nature of the course of the track and the curves, the cars become reversed in their positions on the track; such cases can probably be avoided by proper laying out of the road, and in the worst case by a second conduit under the outer rail for parts where it cannot be avoided. On a one-track line the cars must have their front and back platforms alike, as they cannot be turned around."

"The car truck and the motor are the same as those used in "connection with their overhead system;" .... "Only "one axle is driven; the other is flexibly connected to the third "point of support of the motor frame. The starting, stopping "and reversing of the motor is done as usual by a crank at either "end of the car operating the same switches. Controlling resis-"tances are placed under the body of the car."

" .... In a public address, one of the engineers "of the company stated that the latest figures (July and August,

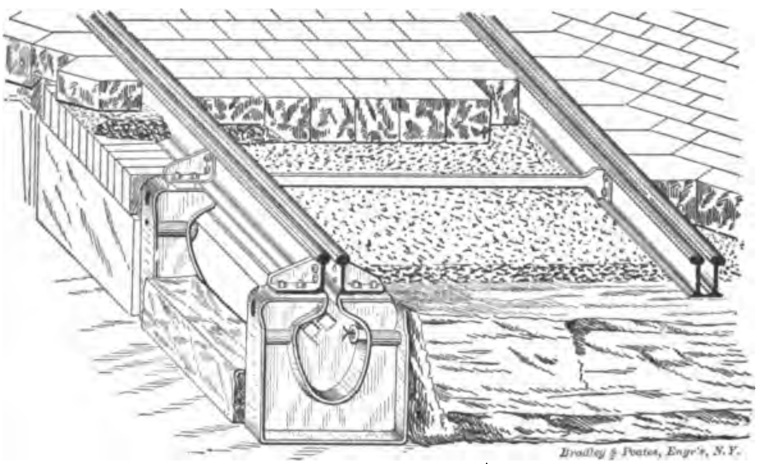

Fig. 4.—Buda-Pesth Conduit.

"1891), of the cost of running were only 37 per cent of the in-"come, which certainly is remarkably low." ....

In this place I wish to call attention to two facts—first, that the width of the slot is greater than could be allowed in any American city, since it would let into the conduit and wrench the wheels off two-thirds of our carriages. This system must have that width, since their conduit uses one of its slot-rails as a rail for the wheels of the car, and they must have the width to give room for the flange of the car wheel. The second point I wish to call attention to is the fallacy of giving the ratio of operating expenses to the net receipts. That can be manipulated in such a way that it is impossible to draw any sure conclusions therefrom. Only a short time since, a gentleman connected with the electric railway in the city where they claim to have the best roads in the country told me that their operating expenses were only 45 per cent. of their receipts. Any fair way of reckoning "operating expenses," means that, in this case, salaries and cost of running were

45 per cent. of their total expenses. If that is so, what becomes of the other 55 per cent. of income? Does it go into the pockets of the shareholders, or do the officials make way with the difference, between a "tight squeeze," 7 per cent. dividend, and the large margin between 55 per cent. and 7 per cent? I am sure that no member of this INSTITUTE will be deceived by any exceedingly small "operating expenses." Another point to be noticed above is, that they use only 300 volts potential. Most of us believe that Mr. Daft, in his experiments years ago, exhausted the practicabilities of electric traction at 300 volts. But, perhaps, the volts are individually more powerful in Buda-Pesth than the "North American volts" used in New York.

Crosby and Bell's book, quoted frequently before, says (page 258): "As will be seen from the description just given, the con-"duit itself possesses no very extraordinary features; it is simply "well made, substantial and carefully drained. This latter fact "is probably the most important factor in the very good results "obtained. The concrete conduit is of itself a fair insulator, and "serves as an additional security against grounds."

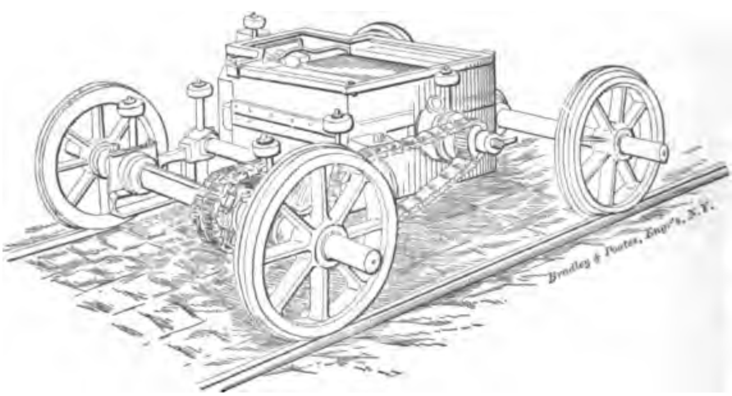

FIG. 5.—Motor Truck, Buda-Pesth System.

The Buda-Pesth record is certainly remarkable, standing alone as successful among all the failures that have attended conduit experiments. It has not, however, been all plain-sailing, and they have had some interruptions in their traffic. What success has attended the Buda-Pesth road has been due to several causes, first of all coming the excellent workmanship that goes with everything done by the world-renowned firm of Siemens and Halske. Next come the beautiful streets of Buda-Pesth, no city in the world excelling it in finely kept and well drained streets. Labor is very cheap there; large numbers of men can be employed to keep the streets clean and the conduit clear, while our dear American labor would make "operating expenses" tell a sad tale. Third, an equable climate, enjoyed in none of our cities. Snow seldom falls there, and the rain-fall is not great. Such a state of affairs, so many favorable conditions, would be found nowhere in America, and until they are found combined, there is no good reason for thinking that the Siemens and Halske system, employing the same bare conductor and the same construction that has failed here, would do any more than add one more wreck on the shore of conduit ventures. You must remove that bare conductor from the conduit, change the climatic conditions of our country, change the laws of Nature and of electricity, or you cannot have a commercially practicable conduit system of that type. We must have a system that does not require us to change our streets and our climate, but that will take the rough and tumble conditions as they exist in all our towns, overcome all climatic difficulties, pay no attention to flooded conduits and poor drainage, and work summer or winter, whether the sun shines or not! If a road has a grade, and a heavy shower suddenly comes up, the water will collect in the hollows faster than any practicable drainage system can carry it away, and the successful road must work right along, undisturbed by such trifles. Any bare wire laid in a conduit, call the system by whatever name you will, must inevitably break down under such conditions. But an electric system, like any other traction system, should never break down, more especially in bad weather, when the public most need traveling accommodations.

There remains still one class of conduits to be considered before passing to the system that has shown a solution of our troubles, or as near it as we can reasonably hope to come in the immediate future.

Closed Conduits.

The idea of a closed conduit has had charms for many inventors, and much midnight oil has been consumed over this problem. The same difficulty is met with in all of them, viz., the continuous grounding of the live wire. Where magnets are used to switch in short sections of the line, they have proved a constant source of trouble. Such things often show up well in the office model, but railroads do not run in offices! All of us who have had any experience with automatic electro-magnetic switching devices know that the less number we have of them the better they work, and that we avoid them wherever it is possible. In the closed conduit, or surface contact systems, there is necessarily a momentary grounding. On a dry day, with one car, the loss might be hardly noticeable. But, wet and muddy days will come in spite of the prayers of the electrician, and I always picture to myself Broadway on a muddy winter's day, with a jam of cars reaching from the Battery to the Post-Office. Perhaps, there might be 50 or 75 cars in the line, and any system grounded in 75 places will certainly break down.

Among those who have worked most in this field may be mentioned—Pollak, Lineff, Gordon, Wheless, Schuckert, Edison and Van Depoele. Their systems have all very much in common, and the fundamental idea is the same.

THE GORDON AND THE LINEFF SYSTEMS.

A small conduit two or three inches square is closed at the top by an iron cover laid in concrete, and this iron top serves as the current conductor. It is divided up into short lengths, about one-third the length of the car, and these sections are successively alive as the car passes along, and are cut out of circuit when the car has passed.

POLLAK SYSTEM.

"In this system there is a middle contact rail in short insu-"lated sections. The conductor carrying the current lies directly "beneath this rail, and by means of flexible contacts momentary "connections are made from it to that particular section over "which the car is at that moment. These temporary connections "are made by the aid of a magnet on the bottom of the car, "which, in passing over, attracts iron blocks under the rails, "making contact with that section."[1] . . . .

Schuckert proposes a modification of this, by having iron filings, when influenced by a magnet carried on the car, bridge over the space between the main and the sectional rail. The holes containing the iron filings were made widest at the bottom, and it was claimed the sparking would do no harm, as the filings always present plenty of new surfaces of contact. This system is not in practical operation anywhere.

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1. Hering, p. 182.

All will remember the announcement some time ago that Mr. Edison had invented a new system of electric traction destined to relegate the horses to the bone-yard, and cable and elevated roads to the junk shop! Such announcements from time to time coming from so eminent a source, have done immense harm to electrical enterprises, and they would utterly annihilate the reputation of any man less favorably known than Mr. Edison. Hence there were a few doubters when the pronunciamento came, and the great results promised, have after all failed to materialize. The overhead system, we are told, is entirely dispensed with, cars, trucks, tracks, and roadbed, such as are now in use, are retained, certain changes being made in the joints and cross-ties. The power, furnished by 1,000-volt generators, is distributed to reducing apparatus placed in boiler-plate manholes at intervals varying in accordance with the number of cars required to be operated. At various reducing points the current is transformed from 1,000 volts to a pressure of 20 volts, and put in direct communication with the tracks. This limit of 20 volts is fixed in order to prevent horses from being affected by the current. The economy of current is about the same as with the present system of trolley.

The chances are that that road has reached its full growth, and will never extend beyond Mr. Edison's laboratory.

Mr. Van Depoele proposed a conduit closed by flexible walls. When the shoe passed, the slot was to be pushed open, and the elasticity of the sides of the slot was to close the opening when the car had passed. All right, in theory, but suppose a layer of mud rests on the top of the slot when the car comes along. It opens, lets the mud into the conduit and keeps it there! A freezing temperature would soon convince the inventor that a closed conduit was an expensive ice-making machine! Such makeshifts will not do! We must work with our streets as we find them. The operative device must be simple and of few parts, and we must keep carefully in mind that a conduit at its best is a dirty place, and that iron and steel will rust, and when rusted may refuse to work.

Mr. Mansfield, in the paper already quoted, says: "There is "one more system, which practically combines all these methods, "and which obviates many of the objections. The inventor has "a slot between his rails and boxes, with contact device within, "placed at proper intervals. Upon his car is a plow which passes "along through the slot, and also a long contact plate or arrange-

"ment extending its entire length. The operation of the invention is as follows: The plow as it passes through the slot strikes "a lever in connection with each box, which lifts for a distance "of six or eight inches above the ground a piston carrying the "contact piece proper. This is made in the shape of a right angle "hook placed in a vertically moving piston, and thoroughly insulated from it. As this is raised up, the long contact plate under "the car passes beneath the hook and holds it up. The current "is taken into the car as the plate slides under the live hook. "Naturally, as the car passes along, the hook slides off from the "end of the contact bar and drops back into place. To protect "this hook, or contact piece proper, it is covered with an extension of the cylinder, so that, as far as the street is concerned, "the surface is perfectly smooth, and one sees nothing but a "small round cover in the center of each of these boxes. The "contact hook is alive only when it is resting on the contact plate "of the car. Certainly, in-so-far as getting rid of all troubles "due to the street being covered with water, this is successful. "The fatal weakness whereby the contact points remain permanently on the street surface is obviated here by the contact "point being practically lifted six or eight inches above the "surface."

Thus saith Mr. Mansfield, but there are some things to be looked at in this device. 1st. There are too many working parts to rust and get out of order. 2d. On a dry day, dirt would get into the hole out of which the contact plate is lifted, and on a muddy day the hole would be filled up immediately, so that the plate would not go back into place. It would therefore be necessary to have a man at each contact plate, or about every six or eight feet. The plate must go back into place, in order to leave the street level. 3d. The contact plate must be made very strong, in order that it may withstand the heaviest traffic of our streets. Hence the problem of lifting and dropping such a plate, as would be neccessary, with the car going 6 or 8 miles an hour, is not a simple one.

I do not wish to be rash in my judgment, but I see nothing especially promising in any of these closed conduit or surface contact devices. As Mr. Mansfield says: "Summing up the "general results of the underground and surface methods, it certainly does not look as if we could expect very much from them "in the immediate future."

Such was the state of the art, and such the general outlook for success, when over a year ago I became engaged in some attempts to solve the problem at Coney Island. This system was not a sudden idea, but had been a gradual development. For nearly two years it has been being gotten into shape, and a number of inventors have contributed their share towards its completion and success. I believe this is a good place to tell the real history of the invention. There have been many incorrect statements in the newspapers in reference to this device. No one would object to certain parties getting credit, even if it did not belong to them. But, when, by giving this credit, a great injustice is done to those who did the real work, it becomes a matter of duty to set the affair forth in its true light. The original idea of the box used down there, and the working of it out to its completed form belongs, I may say modestly, to me.

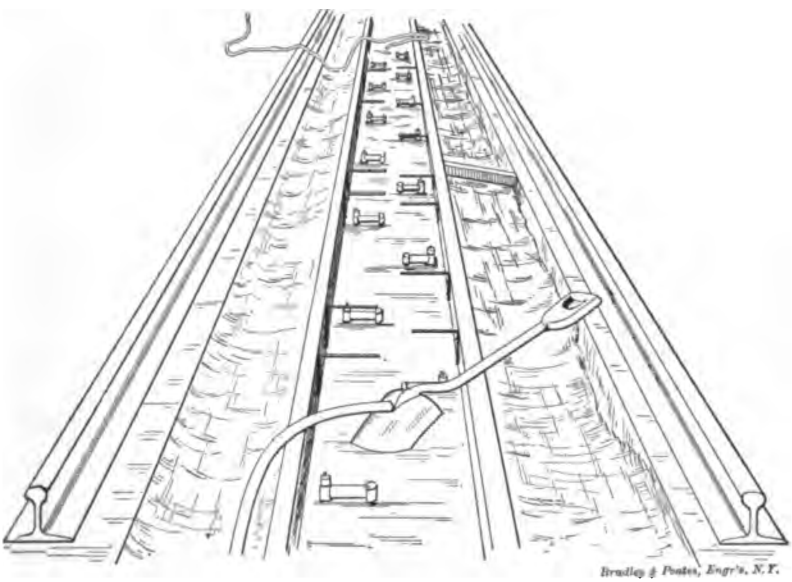

FIG. 6.—The Mud Test.

Among those who have worked upon the details of the invention, I may mention Mr. G. T. Woods, Mr. C. S. Van Nuis, and Mr. John H. Dale; but whatever success it has attained has been due to the suggestions and inventions of Mr. John J. Green, who brought to the work the mind of the inventor, and the skill of the trained mechanical and electrical engineer.

Such being the state of the art, the outlook being thus unpromising, knowing what had been done, having spent four months in the Patent Office, having examined the traction question on the whole Continent of Europe, we set to work to construct a system that should avoid the defects inherent in other devices. To strengthen their weak points, to make an all-the-year-round system, was our object. I assert with the utmost confidence that we have succeeded. To have gone over the same old ground, in the same old way, would have been folly for any

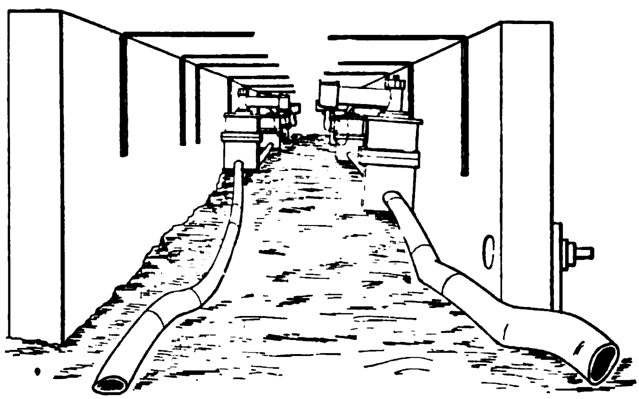

FIG. 7.—Conduit, showing Cables and Boxes.

one knowing of previous attempts and failures. Perfect insulation, no live wires in the conduit, fewest possible working parts, with nothing to get out of order. These were our cardinal points. We have not attempted to make a road operative in fine weather, and under ideal conditions; but have known that to ensure success we must take the streets as we find them. They

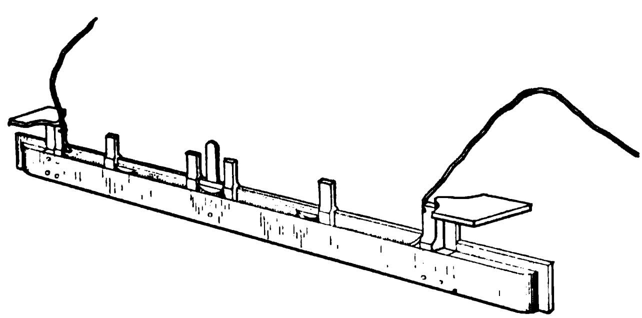

FIG. 8.—The Shoe.

will get muddy, rain will fall and snow will come, and if a slotted conduit road is to succeed, it must work undisturbed by them. We have, on three occasions, filled our conduit to the top with mud and water, burying out of sight conductors, contact boxes and all the devices. (The illustration, Fig. 6, shows our conduit in process of being filled.) Mud of the consistency of paste was used, and the report of a disinterested electrician, not known to me personally, brought there by the editor of one of our leading electrical journals, shows that the system was in no manner affected. With the shoe of the car in the mud, with boxes entirely submerged, the ordinary measuring instruments failed to show anything but the best insulation, and no disturbance of traffic would arise with 50 cars in a conduit filled with mud and water. No such condition would ever present itself in railway work, but a space of several hundred feet might get full of mud, snow or water, and the successful system must not be disturbed by such trifles. We made our mud test, not once, but three times. When the last test was made, our boxes had been under the mud four days; yet it is to those boxes that Mr. Stevenson's report refers. Four days' submergence is a terribly hard test, but if the line then works perfectly, there is absolutely nothing to apprehend from an occasional filling up during a shower.

Description.

A conduit is used, similar to that of cable roads, but nothing like so large or expensive. With our improved shoe, a conduit two or three inches wide and seven inches deep will answer perfectly. In the slot of the conduit, attached to the car, runs a shoe extending nearly the length of the car.

Fig. 8 shows the shoe detached from the car; the upwardly projecting pieces are the guides for making the shoe follow the conduit and conform to the curves.

The shoe is so supported that should the car leave the track it detaches itself immediately and remains on the top of the conduit. It cannot get caught, as the cable grip sometimes does, and drag the car along through crowded streets at a dangerous speed. As seen in the illustration, the shoe is somewhat like a cigar in shape, so as not to give any sharp blow to the contact arms. The sides of the shoe are formed of conducting material, insulated from each other, and perfectly flexible, so as to follow any curves. The shoe can be attached to and detached from a car in five to ten seconds.

In the conduit are placed two insulated cables, a metallic return being used. This I regard as necessary to ensure perfectly safe working. These cables run into and through contact boxes placed at intervals of ten feet on each side, they being arranged diagonally opposite, so that the distance between the boxes is about five feet. The construction of the boxes and the functions of their various parts will be readily understood from the illustrations. One shows the box closed and the cable running into the box, the other the box opened and the method of securing contact with the cable (see Figs. 9 and 10).

The main is carried into the box and, as can be seen, the lead and insulation removed for about two inches. Onto this bared

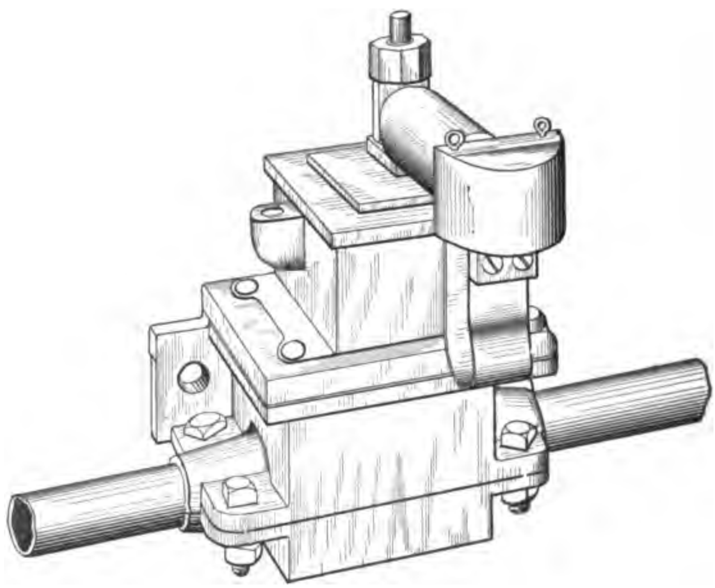

FIG. 9.—Box Closed.

cable there is placed a brass clamp with two upwardly projecting tongues.

The contact-making portion of the device is a vertical rod, having at its upper end an arm projecting into the conduit, and at its lower end a brush adapted to be brought into contact with the tongues attached to the cable. This rod and its bearings must, of course, be properly insulated from the contact box. When no car is passing, the brush is out of all electrical connection with the current conductor, but when the shoe on the car reaches the arm projecting into the conduit, it swings it to one side, bringing the brush on the rod into contact with the tongues fastened to the conductor, and the current passes to the motor on the car. As soon as the shoe on the car has passed the box, a torsion spring forces the rod back into its normal position, and that box is dead. The box is filled as far as it can be without interfering with the movement of the brush with solid paraffin, and above that with "transil oil." This makes excellent insulation. Of course the spindle, which at times becomes "alive," and its bearings are thoroughly insulated from all parts of the box and cable. Nothing can be better than an insulated cable, covered

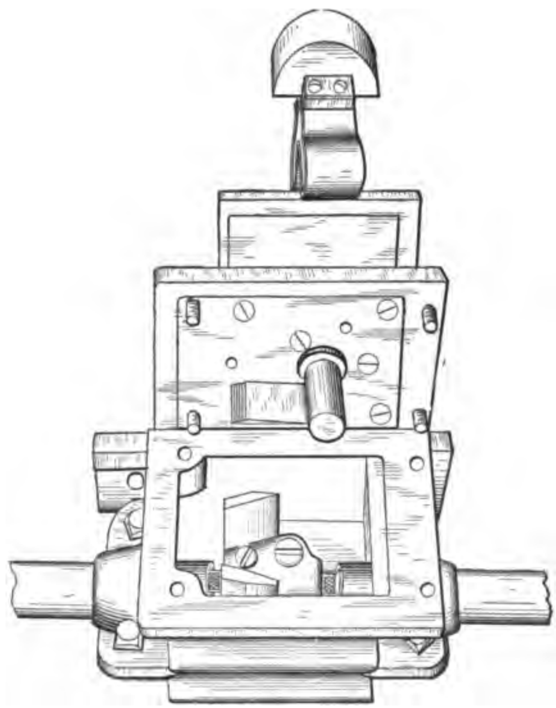

FIG. 10.—Box Opened.

throughout its entire length with a highly resisting material, and at the points where current is supplied to the car an insulation practically infinite.

No accident to persons or animals appears possible with this system. To avoid sparking within the box, the ends of the shoe are made of insulating material, thereby establishing mechanical contact within the box before the metallic portion comes into contact with the arm, and also holding the inside contact after the current ceases to flow to the motor. This is an important feature, for without such an arrangement the brushes would soon burn out, the oil become carbonized and the boxes useless.

Thus far I have described the box that we used at the Island, but it has long since been consigned to the usual receptacle for the rejected productions of progressive inventors. It did its work and did it well: it pointed the way to better things, and we have several forms of box, entirely obviating the defects in the first. The number of working parts has been reduced, and we now have the device brought down to its lowest terms. I regret that it is not possible for me to show these improvements, but patents have not been obtained on some, and on others not yet applied for.

As a guide for future investigators, I will enumerate the best features and the defects of the old box, as I understand them. 1st. Its insulation was excellent, that is, the theory of its insulation was. But oil insulation is in many cases a fraud, a deception and a snare. The best oil insulation is not so good as that of air containing 80% of moisture. Oil and water, perhaps, won't mix, but oil will attract moisture, and the two will make an emulsion that will cause the investigator much sorrow and mental anguish. Ordinary paraffin oil, even the highest grades obtainable, contains a great deal of water, which, in a device like this, will cause disastrous short-circuiting. Transil oil seems the best for the purpose. At Coney Island, before we adopted transil oil, our boxes would go wrong, in spite of all we could do, and we only discovered the trouble by putting some of our oil in a bottle to settle over night, and in the morning found an abundance of water in the bottom. Solid paraffin is the only substance that we found capable of withstanding the action of paraffin oil, and it must be the refined variety, such as is sold in hard, white cakes. Its use even was discarded, for when the water would work down the spindle through our imperfect stuffing box, the solid layer formed a bed for the moisture to rest upon, and being close to our brush, it short-circuited the box. It is better to let the moisture settle to the bottom of the box, out of harm's way. I don't think oil should be used on account of its insulating properties, but for keeping the working parts from rusting. 2d. Our old box was made in several parts (a mistake, remedied in our new one-piece box), the joints required planing, an expensive operation. Paraffin oil is difficult stuff to hold, going through almost everything. In the joints we used oil paper, screwed them together as tightly as possible, but the oil softened the paper and the boxes leaked. This we remedied by painting the joints with "Insullac," which withstood the oil excellently. 3d. Our stuffing box was a crude device, and did not, in many cases, keep out the water; but this is a simple mechanical affair remedied in our new boxes. 4th. A torsion spring is not so good as a straight one, nor so reliable in its action. 5th. We had a short brush in our box, when a long one should be used. Flexibility and sure contact are thereby secured. 6th. It was a mistake to have the two upwardly projecting tongues; one contact is the thing, and the brush always resting against it when the shoe touches the arm. Otherwise there will be a break on reversing and consequent "frilling" of the brushes, with carbonization of the oil. 7th. To make an oil-tight joint between the cable and the box was almost an impossibility. We taped the cable where it entered the box, but when it was tightened up there was often contact between the lead covering and the box. 8th. The box is too large. 9th. Great care must be be used in selecting and laying the cable. A slight defect gives leakage; this decomposes the alkaline earths; chlorine and caustic soda are perhaps produced, and the cable eaten away. Every joint must be carefully made and carefully sealed. Some one may say that it is impossible, but I answer it is the duty of the supervising engineer to see that it is properly done. If any one thinks this cannot be done, he is not my ideal engineer, and certainly does not want to meddle with electric conduit railways. These are the defects, but we have cured them all in our new system. The excellent principle of the old box has remained untouched by long experiment, the laterally yielding arm being the only form likely to answer the purpose.

I add the report of Mr. Stevenson upon our "mud test."

THE BRUSH ILLUMINATING CO., OF NEW YORK.

ALBERT STETSON, Esq.,
50 Broadway, New York City.

New York, September 21, '93.

Dear Sir:

In accordance with your request of yesterday, *in re* tests made by me on the new Electric Conduit and Car at Coney Island, I beg to submit the following report:

The insulation resistance of the whole line, when car is not in circuit, equals 63,500 ohms.

The insulation resistance of line, with shoe of car and adjacent switch boxes in contact with shoe submerged in water, equals 880 ohms.

E. M. F. of power circuit equals 353 volts; 353+380=.926 amp. loss.

Seventy-five feet of conduit were pumped up full of water previous to the test.

With Weston ammeter the reading was hardly discernable.

The loss on boxes tested by Weston voltmeter showed nothing.

The resistance test was made with Thomson tripod galvanometer, and by comparison with standard resistances

From the above figures the following result is arrived at:

As seventy-five feet of the conduit were submerged, this would include twelve boxes. Therefore, it is safe to assume that the loss was only on those boxes, as several of the dry boxes tested showed no leakage.

Then, as the loss on the circuit, with boxes all open (that is electrically) was .0058 amperes, equalling .0027 H. P., the loss on each box equals $\frac{1}{12}$, which amounts to .000483 amperes, and which equals .000223 H. P. It, therefore, would take 4,484 boxes entirely submerged to give a loss of 2.113 amperes or 1 H. P., and, as the boxes are six feet apart, this equals 5.092 miles.

The car standing with shoe entirely submerged, and the two adjacent boxes with the switches turned on, showed a loss of .926 amperes, equalling .438 H. P. And, as the car takes about 12 amperes, this equals 5.68 H. P. on ordinary track. This shows that it loses 7.96 per cent. of its power when so placed; or, expressed differently, thirteen cars placed in the same condition would lose about the same amount of power which it takes to drive one car.

(Signed), E. W. STEVENSON,
Electrician, Brush Illuminating Company.

[Affidavit as to correctness attached.]

These are submitted as very remarkable and satisfactory figures, but I mistrust that we are entitled to more, as there were five lamps burning on our car, and their consumption of energy must have been charged up as leakage on the line.

Now, although our conduit has withstood such tests, it must not be supposed that mud should be allowed to accumulate there. It should not, and the cleaner the conduit can be kept the better. At frequent intervals mud-holes should be placed, and each car should carry a stiff brush to sweep into these any accumulations of mud, snow and water.

The question has often been raised as to whether snow and ice would clog up the conduit and stop the car. The experience with the Cleveland Bentley-Knight road shows that snow did not stop a road with a bare conductor in the conduit, and it certainly could not affect ours. You must drain the conduit, and during such weather as the conduit might get, if left to itself, frozen up, it may be necessary to run a car over the track at intervals all night. But this would only be required once, say, in a winter.

I believe that our road has solved the question of conduit traction. I have not meant in my review of other systems to be in the least unjust to other workers in the same field. I only take my stand on other well-established principles, keep strictly to the laws of nature and electricity, take our climate as it is and our streets as we find them, and I assert, without fear of successful contradiction, that any road to succeed must be laid down practically on these lines.

To sum up, then, the objects of such a system are: To remove from the streets of our cities and large towns the unsightly structure that the trolley construction demands; to dispel all chance of injury to persons and animals; to present no such lightning conductor as the trolley line forms; to offer no obstacle to the work of the firemen, in case of a conflagration; to remove all risk of fires being set by the electric current, as frequently takes place with the trolley; to avoid corroding action on water and gas pipes, and the iron foundations of buildings. Finally, to furnish all the advantages of electric traction, without any of the present attendant evils.

The main advantages of such a system are evident, but there are others. The car runs backwards as well as forwards, and handles as nicely and easily as a locomotive. There is no trolley wheel to slip off the wire, depriving the motorman of his power at the very time he needs it most to prevent accidents.

I have endeavored to present our system to you as it is. I have not in any manner exaggerated its advantages, nor do I speak as one who is led to be enthusiastic over something that is new. I am too experienced in such matters and have seen too many new things spring into momentary notoriety, only to sink into oblivion. I believe in the practicability of the electric conduit, and believe that you can profitably devote your attention to the forwarding of that method of electric traction.

Discussion.

The President:—Discussion of this very interesting paper of Mr. Stetson is now in order.

Mr. Nelson W. Perry:—I would like to ask Mr. Stetson what provision is made for rounding curves, both as to the shoe itself in its entirety, and as to the insulated portion of the shoe; if there is provision made so that it will go into those guides on a curve of a given radius, how do you provide for its going into those same guides if the curve is of a different radius, and also in getting around any curve with your shoe nine feet in length.

Mr. Stetson:—I will try to make that plain. Suppose this [illustrating] to be a platform, such as I tried to show on the screen. At the end of our shoe we have a flat piece which rests on the platform. It may shove back and forth. When your car takes a curve, if it is going one way, the shoe will glide along on this platform; if the other way, on the other platform. The shoe is so flexible that it will bend up double. At the end of the shoe, in the insulated portion, there is a slot cut, and in that there is a nut, which allows it play either way. The nut working in the insulated portion permits flexibility. If it was not for that, if it was a straight piece, the metallic portions would bend in the middle and would meet, but you allow a slipping motion of the nut in the slot. On the end of this shoe there is a rod projecting upward and fastening onto the platform; then this slides in transversely with the bottom of the car. That is all the support that it has. It cannot get out because it must follow the conduit-slot. It is supported at both ends. When the car takes a curve the support of your shoe moves along either to the one side or the other of the platform.

Mr. Perry:—Supposing you have a shoe like that [referring to a sketch on the blackboard]. Your shoe has to go into these two contacts. Now the shoe comes along here. How is it diverted from the tangent to the curve so as to properly make contact with the next switch?

Mr. Stetson:—I see. We had a number of guides so that it must take very nearly this circle and follow it. When your shoe is going along it would naturally continue to move on a tangent; but the guide forces it around the curve.

Mr. Perry:—Supposing you had provided for a curve of a certain radius and you come to a curve of another radius. How would the different diversion be accomplished?

Mr. Stetson:—It would be just the same. The shoe will bend right up in the middle on your guides, and thus confine it absolutely to the conduit slot. The course of the shoe is absolutely controlled by the conduit slot.

Mr. Hewitt:—I suppose it is in order to make some remarks on the general statements in this paper, not only as applying to the main subject of Mr. Stetson's article, but the previous part. Referring to page 635, he says, "With the withdrawal from the field of the Bentley-Knight system, disappeared the last hope of our being able to place bare wires in a slotted conduit exposed to the severe conditions of our American climate." On the next page he says, in regard to the Bentley-Knight system, "They failed, but they made a noble fight, and they did thoroughly demonstrate that a conduit road using a bare wire cannot operate commercially in our American cities." I will not take any issue with him on the success of the Bentley-Knight system, but I do think these statements are too broad. The Bentley-Knight system demonstrated, no doubt, that bare wires placed in a conduit, as they were placed, will not operate successfully. Those two wires were placed directly beneath the slot and very close together, and anything falling in the slot tended to short-circuit them and interfere with the operation, but certainly we cannot argue from that, such a strong statement as Mr. Stetson makes.

With regard to the Love conduit, which he takes up next, I have had the good fortune to examine the only two roads operated on this system, the road in Chicago and the road in Washington. I had a little better success in Chicago than he had. The road there is 1⅝ miles long. It goes around several blocks. I made several trips around there and on three trips we were shut down, from one cause and another, twenty-three times. That I do not consider a success. It absolutely cannot run in winter when there is any frost and snow, and the railroad company, I believe, have decided that it is not a success. In Washington, however, the road is vastly improved and is run on a street where the conditions are far better than in Chicago. It is a beautifully paved street—paved with asphalt. It is a clean street. The road started on the 2d of last March, after all the snow had gone, and has worked successfully, so far as I can find out, through the spring and summer and up to date. But how it will operate this winter is a question which we can hardly answer. It has not been all plane sailing with them there. They have had trouble, largely from mechanical difficulties, so far as I can see. They were limited by an ordinance of the city which restricted the passage way for the trolley wheel to a very small space. The slot rail, as you noticed in that picture, turns down like this. The city limited them to the width of this passage way. The consequence is that the rims of the trolley wheels come very close to the sides of the iron and there is evidence there that sparks pass from the wheels to the iron. I found that trolley wheels there lasted about ten days. I do not claim that the Love conduit is a success by any means, but in my judgment it is to-day the nearest approach to a successful conduit system of any that has been tried. (See Fig. 11.)

With regard to Mr. Stetson's system, I have had the opportunity of visiting that at Mr. Stetson's kind request, and I hope he will pardon an honest opinion, although it may differ from his own. He proposes to place in an underground conduit, switches —you may call them switches or you may call them something else. They are working devices placed every ten feet on each side of the conduit, alternating, making one for every five feet of track. That would be over 1,000 working devices—working switches. I claim that that of itself is a very strong objection. I do not believe that any electrical switching devices can be placed underground a thousand to a mile and be operated in a road like Broadway, New York, with any success. I may be wrong and may underrate the abilities of the inventors, but that is the way I feel about it. I believe that a successful conduit road must be of the utmost simplicity. Now Mr. Stetson's road is 1,200 feet long, and in the middle of the road there is a curve. I do not know what speed has been attained.

MR. STETSON:—Twelve or fifteen miles an hour.

MR. HEWITT:—In a good many tests that have been made with

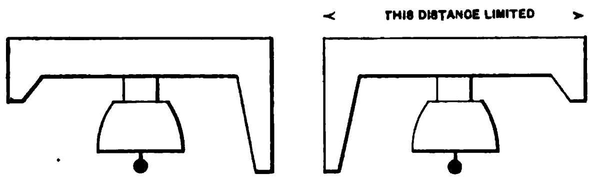

FIG. 11.

the recording speed indicators I have found that it takes some 200 to 300 feet to get up a speed of twelve or fifteen miles. Granting, however, that the car did make twelve or fifteen miles down there, imagine the continuous effect, with a car weighing twelve tons, striking those contact pieces in the street constantly. Take a street like Broadway, where the cars I believe run on a minute headway. That would be sixty times an hour that those contact blocks are going to be struck a pretty hard blow, and it seems to me it is almost impossible to put any working device in a conduit that is going to be permanent and that will withstand the hammering effect. I do not say that it is impossible, but I say that it has not been demonstrated. Practical demonstration is the best proof of all, and I do not think that a little road like the one at Coney Island, where passengers are not carried and where the cars are only run experimentally, is a demonstration of the lasting properties of such an apparatus.

With regard to the mud test which he refers to, I do not attach as much importance to that as Mr. Stetson does, perhaps. It simply shows the resistance of mud made with fresh water. We have a conductor on one side and a conductor on the other, one positive and the other negative. They are immersed in mud made with fresh water and it is simply a matter of pressure and resistance. That is all there is to it. Such conditions never would be met with in a street railway. It is an exaggerated case of course. I believe, though, that if that test were made with such mud as we get in our streets, which is very salty and more or less alkaline, the results would be considerably different.

MR. STETSON:—We had more salty mud than you could probably find elsewhere.

MR. HEWITT:—The day I was there the water was running from the city water supply.

MR. STETSON:—But it was Coney Island sand and dirt.

MR. HEWITT:—I did not taste it, and I presumed it was fresh water. Anyhow, that does not contradict the statement that I made that it simply shows the resistance of that mud. That was all that it did demonstrate.

With regard to the statement of Mr. Stevenson, I might take exception perhaps to the insulation resistance, but this I must acknowledge that the conduit which they have built there is more or less temporary and perhaps a working conduit put up for regular use would show higher resistance. But 63,500 ohms for a quarter of a mile of line would be considered very low. On a ten-mile track that would not amount to anything.

I must compliment Mr. Stetson on the very strong attitude he has taken in regard to this conduit and nobody would be more pleased than I to see it made a success. But from experience I have had in railroad work I do not anticipate the same success for such an innumerable array of switches underground that he does.

MR. STETSON:—As to the number of switches that there must be per mile with the system built as it is, I admit that that is the real fact; it will take about a thousand switches to the mile. But whether there are a thousand or ten thousand switches to the mile, it all depends on the construction of the device you have, as to whether you are likely to have a break-down. If I had the new box here I could show you a device in which the working parts are reduced to about one-third of what they are in this box. The blow is reduced to next to nothing. There is nothing but a flat spring and a flat brush. If you have got to have a device, I claim that you would hardly be able to get it simpler than two parts. As to the blow struck, I was talking with an electrician connected with one of the big companies, and he said: "It has been tried and you will find it won't work." I found nothing of the sort. In the first place there is not such a blow as you would get in striking with a hammer. That stroke is sharp. This is simply a pushing aside.

MR. HEWITT:—Don't you retain in your new box the spring device on which that plug operates?

MR. STETSON:—No, we do not retain that. That is not good.

MR. HEWITT—You make that solid then.

MR. STETSON:—Yes. As you work with things like this, you proceed rather from the complex to the simple than from the simple to the complex. You must be able to go backward or forward. If the arm were rigid and you were going in one direction and wanted to come back, you would either tear the spindle off or damage your shoe. So we made the arm yielding. That old box had three parts. The objections that you brought up were good ones. I will bring up some more. The principal objection to this old box is the expense. This box would cost $1.50; the new box will not cost $1. In the old box there were five surfaces that were planed on a planing machine. That costs money and takes time. It would seem, after you had planed it and used oil paper, as though you might get a joint that would hold paraffin oil. But we couldn't do it. You do not want any joints. The new box is one solid piece of iron. I admit that there are working parts in the box that we have, but we have reduced them to two, one being a long brush and the other a good stiff flat spring.

I will state in reference to the Love conduit system that I was not able to get reliable information in reference thereto. The information I wanted I could not get. I did talk with some good men, and was guided a great deal, by their opinion of the Love conduit in what I said about it. I would state about Chicago that I estimated the length as well as I could from Crosby and Bell's book. The road was not running when I was there. You must know that Bentley and Knight had a number of patents, and means of protecting their wires as the Love system has.

Mr. Joseph Sachs:—Mr. President, in reference to the mud test, when the shoe was entirely submerged, I fully agree with Mr. Hewitt in saying that that only showed the resistance of that type of mud. Mr. Stetson has stated, I believe, in his paper that the trouble with the bare continuous wire conduit system has been that the mud would get down through the slot and settle on the wires and cause leakage. There is nothing at all to hinder the mud from collecting on the shoe, while it is in contact with the switch boxes, causing leakage between the two sides thereof, particularly when the brush passes through a section of conduit that is entirely submerged, as in the test.

Mr. Stetson has also forgotten a type of system with which I had a little experience. I had occasion to construct a short line of this system, also, at Coney Island. It is a system which has contact surfaces, either on the surface of the road or in a conduit, but the mechanism for controlling these sections, or plates, or contact wires, is entirely out of the road bed, and put in such position that it can readily be gotten at, repaired, changed, or tested, at any time. As Mr. Hewitt has said, any mechanism placed in the roadbed is always apt to become disarranged and there is always a question whether anything of the kind would prove a practical success. Numerous small electrical devices located in a conduit or underground are apt to give a great deal of trouble. In my mind, a system which would operate as well, and perhaps even better, would be one in which contact sections or contact points are placed along a conduit and these contacts actuated one after another from some source exterior to the roadbed or so located to be accessible.

Mr. Stetson seems to place particular stress on the failure of magnetic cut-outs to work satisfactorily, but I will assure Mr. Stetson that I devised a form of magnetic cut-outs which were used on this particular road that I have in mind which worked very well. It was not a question of the failure of magnetic cut-outs, but was a question of the type of track construction that was used. We used iron plates, about 6x9 inches, placed in the centre of the track upon wooden planks, not painted or treated at all, but just rough wooden planks, and these were in the form of a square, closed, though, with the plates on top. This conduit was about 4x4 and contained the wires which were connected with different heads or contact plates. The contact heads were placed ten feet apart and at every 240 feet was a distributing box located on a post adjacent to the track. A wire from the different plates ran to the distributing box, and within this box magnetic cut-outs were located. A tap from the main feeder in the conduit was also brought to the distributing box, and this wire was at various times thrown into connection with the different heads as the car proceeded by the magnetic cut-outs. The current was taken from the plates by a brush underneath the car which was long enough to touch two plates. The rails were used as a return. We had what was called a surface contact system. The construction was the very crudest, but the installation showed that the placing of the contact controlling mechanism out of the roadbed was entirely feasible. From experiments I made at that time I do not believe that a surface contact system, that is, where the contacts feeding the current to the car are located on the surface of the street, is adapted to our large cities. But if these contacts were put into a conduit and proper drainage used, and the conduit made of the same size as Mr. Stetson proposes, or the same size as the Love conduit, I do not believe there can be any question as to the operation of such a system, and such a system would undoubtedly operate in perhaps as efficient a manner, and perhaps a better manner, than any system in which the contact device and mechanism was located in the duct.

There is another point about Mr. Stetson's system which I would like to know, and that is this: What provision is there made for preventing the arms from sticking, and what provision is there also made for getting at the boxes to repair and renew them or refill them with oil if the oil should leak out.

Mr. Stetson:—That has all been worked out in the new device. In the new system the switch device is not in the conduit. It is in the road-bed right where it ought to be. All that is necessary in the new system is simply to lift up a square iron plate, take out the whole inside mechanism and put it back. I know the system that you speak of. I examined it very carefully and made a report on it.

MR. SACHS:—I believe there was a method devised and tried sometime ago by Messrs. McElroy and Nicholson. This system contemplated putting in the road-bed a number of boxes, similar to the contact heads which I had reference to. In these contact heads were located electro-magnets and instead of the contact heads or arms, as they would be called in Mr. Stetson's system, being actuated mechanically, they would be thrown in connection with the feeder from the dynamo by the magnetic cutouts located therein. It was found in a short time from the sparking and the moisture that the mechanism became entirely disarranged.

Mr. Hewitt's remark about the number of devices used I think is a very correct one. No matter how simply the devices are made they certainly do get a great deal of wear and tear. The action of this shoe upon the arm does certainly amount to something, even though it is only a push, and I should think that the noise, on a system like the Broadway road, where as Mr. Hewitt says there is about a car a minute, would be quite noticeable and objectionable.

MR. STETSON:—Why didn't you come down and see it during the summer? You would have found an entire absence of anything of the sort. There is no noise from those rappings at all.

MR. SACHS:—There is another reason perhaps for no noise being made. I merely studied the system from various articles that have appeared in the journals and know from my previous experience, having studed that particular class of electric railroads to some extent, just what the requirements and conditions are. As I understand the system down at Coney Island the conduit you have, is not a conduit but an open trough and is not enclosed at all. As I understand it, your contact boxes are located upon the same level with your tracks. Actually you have not got a conduit system there but a surface contact system. No such system with such contact boxes could be used on Broadway.

MR. STETSON:—I do not think we ought to go quite so wide from the subject as that. That was simply a matter of avoiding splitting the cross ties of the track. We preferred to have it on top as a means of avoiding expense.

MR. SACHS:—I made that same test myself with the system we had at Coney Island. Our plates were located right on the surface and I took this same Coney Island mud and I tried to make the same test. I got some peculiar results. I got about one-half an ampere or so on each head when the mud was right up flush with the rail on about 400 volts pressure. Upon thinking the matter over and examining the mud I found I was wrong and that this mud was rather different from ordinary city mud and there was not very much path for the current to go through wet sand. The water we used was fresh water, as I supposed yours was. I suppose the effect you got was very much like the effect I got. As regards putting the heads on the surface and getting the poorest condition you can get, I do not agree with you, as I believe a submerged conduit is much worse than a surface contact system under ordinary conditions. I do not want to say that the system is not operative, because I believe it is. I think the system will work. But I am merely giving my experience and views in the matter, and I think when you put these boxes in the conduit with the rattling all along it, you will find there is quite a good deal of noise and you will find other difficulties not met with in your experimental installation.

MR. STETSON:—They are not in the conduit.

MR. SACHS:—The arms are in the conduit; that is where the noise comes from—not from the box. There is another point, when you get your arms in the conduit how about the mud collecting on your box and contact arms? I believe that a practical conduit system can be operated, something of the class that Wheless or Hunter and several others have proposed, viz.: a conduit not very large, with sections therein, or contact plates therein and each contact plate being put into action one after the other by a series of magnetic cut-outs. Now the preferable form would certainly be to locate these cut-outs exterior to the road-bed and by so locating them, all the dangers that Mr. Stetson has talked about would be eliminated. Such would certainly be an efficient system if properly installed, and the leakage would be brought down to the minimum.

MR. STETSON:—In reference to that, I prefer a mechanical action to any electro-magnetic action.

MR. F. A. SCHEFFLER:—The author has made such rabid statements in reference to the use of closed conduits that I feel disposed to offer some suggestions to contradict such inferences as he makes. I believe, firmly, that the future will show that a successful closed conduit can be maintained, which can be manufactured and constructed for much less money in first cost per mile than any system heretofore offered for open conduits.

A closed conduit will also have many more advantages, especially those of operating expenses, than an open conduit. If a conduit filled with water can show such elegant results as to leakage measurements as are set forth in the test of Mr. Stetson's system, it seems to me that a closed conduit which cannot be filled with water even to make a test for leakage would be far superior for actual operation to the open conduit. The results of Mr. Stetson's tests are remarkable, and while I do not question their accuracy (although it is somewhat out of the general rule to correct matters which have been stated and an affidavit attached certifying to their correctness) it seems almost impossible to believe that it would take 4484 boxes entirely submerged to give a loss of 2.113 amperes or through a distance of over five miles.

If you can secure such results with a conduit full of water and five miles long, why not throw away the conduit and use the surface where such an accumulation of water is impossible, as to quantity. A surface accumulation would be of higher resistance than a body of water six or seven inches in diameter, hence the leakage would be much less on a closed conduit. Mr. Stetson proves by his test that his theory of extreme leakage in enclosed conduit can be overcome, for if it can be done underground in an open system why should it be not easier accomplished by a closed system—when one learns how to do it.

The Metropolitan Traction Co. has offered fifty thousand dollars as a prize for the best system which shall do away with the "deadly" trolley. I am very anxious to own that prize and would feel pretty sure of getting it were it not for the fact that I may only take the chances with a thousand others. I venture to make the broad statement that the prize will be given—if it is for an electrical system—for a closed conduit system which shall operate in the same every day stay-with-you-manner that Mr. Stetson has so intelligently outlined.

MR. STETSON:—I want to ask the gentleman if he thought about this question of closed conduits. I have given quite a little thought to them.

MR. SCHEFFLER:—I have looked at it, sir.

MR. SACHS:—Pardon me for taking up so much of your time, but permit me to say another word in reference to closed conduits. I tried to do a little in that direction, and I think that Mr. Scheffler is quite correct in his assertions that the leakage in a surface contact system would certainly be less than that in a conduit system which was entirely submerged, that is one in which the contact devices were entirely submerged. In a surface contact system it is usual to put the contacts in the centre of the track at the highest point. Now being brought to that highest point between the rails, the moisture, or mud, or dirt, has a tendency to settle down on the rails, and the quantity of dirt and moisture that would get between your rails, which would perhaps form your return conductor, the plate or contact would be of much higher resistance than that collected in the submerged conduit, as Mr. Stetson puts it. I think that, taking the submerged conduit on the one hand and a surface contact system properly constructed and in a similar location on the other, the advantage would be with the latter in reference to a minimum leakage.

MR. W. T. M. MOTTRAM:—There is one point that has not been shown up very fully here, and that is this: Wherever the car is in service the current is exposed, is it not? Now if the contact strip is standing in water, it is bringing the current right into the water. Now it is quite conceivable to me that there would be a piece of track on some ordinary line, of a thousand feet in length, in which the conduit would be full of water. It is also conceivable that there might be twenty live cars standing on that piece of track. If you turn 500 volts loose at twenty different points with metallic strips each about nine feet long, your short-circuit will simply close you down.

[Adjourned.]

OBITUARY.

Dr. Norvin Green, first President of the American Institute of Electrical Engineers, was born in New Albany, Ind., April 17, 1818. His early life was spent in Kentucky, to which State his parents removed their home. There he was educated for the medical profession, graduating with high honors at the University of Louisville in 1840. He soon after received the appointment of physician for the Western Military Academy at Drennon Springs, Ky., became interested in politics, and served several terms as a member of the Kentucky legislature. In 1853 he was appointed a Commissioner of the United States in charge of the construction of government buildings in Louisville. It was while serving in this capacity that he became one of the lessees of the United Morse and Peoples Telegraph lines between Louisville and New Orleans, and was made president of the consolidated company which grew out of those interests, under the name of the Southwestern Telegraph Co. Most of the remainder of his life was devoted in an administrative capacity to telegraph affairs. When the American, the United States and the Western Union companies were consolidated in 1866, Dr. Green was elected Vice-President of the Western Union Co., which office he held up to 1878 with the exception of three years, during which period he was President of the Louisville, Cincinnati and Lexington R. R. Once more he entered politics and barely lost the nomination for U. S. Senator from Kentucky. Returning to the Western Union Telegraph service in 1873 he continued as Vice-President until April 23, 1878, when he was elected President to fill the vacancy caused by the death of William Orton. This office he held until his death, which occurred at Louisville, Ky., February 12, 1893.

Dr. Green was of judicial mind and imposing presence. He was not only a democrat in politics, but democratic in his relations with his associates, or with those who came in contact with him upon business or personal matters. He was of a philosophical temperament; calm and dignified as became a gentleman of his standing. He was one of the founders of the American Institute of Electrical Engineers and sanguine in his prediction of its future usefulness. His acceptance of the office of president in 1884 was not as might have been supposed, a mere incident in his busy life, to be neglected as a matter of no consequence. He set the example of conscientious attention to duty, and his advice was of great value at a time when it was most needed. In view of his distinguished career, the Council on December 3d, 1889, made him an Honorary Member. Dr. Green left a family consisting of a wife, four sons and two daughters.

George Bartlett Prescott, Jr., was born at Dubuque, Iowa, in 1858. He was the eldest son of Alphonso and Harriet Prescott. He entered the service of the Western Union Telegraph Co. in 1875 as a Morse telegraph operator at Lindell Hotel office in St. Louis, Mo. In 1876 he was transferred to the electrician's office of the same company in New York City, and was promoted to the position of assistant electrician in 1879. He remained with the Western Union Telegraph Co. until 1882, when he resigned to accept the position of chief assistant with Edward Weston, then the electrician of the United States Electric Lighting Co. at Newark, N. J. He continued with Mr. Weston until 1889, when he was appointed electrician of the Electric Accumulator Co. He had already given much attention to the development of the storage battery, and fitted up an experimental car with accumulators for the West End Co. of Boston in 1887-8. On the first of April, 1890, he accepted the general agency of Day's Kerite Wire and Cables in New York City, but was obliged to relinquish the position by reason of failing health in the fall of 1891. A temporary improvement in his physical condition permitted him to fill a brief engagement in the Stanley Laboratory at Pittsfield, Mass., but he was again compelled to abandon all business engagements, and seek to repair his shattered constitution by a trip to Bermuda. There was no hope of recovery, however, and he returned to Northampton, Mass., in January, 1893, completely prostrated, where he died on February 12th, 1893.

Mr. Prescott was elected an associate member of the American Institute of Electrical Engineers, July 12, 1887, and transferred to full membership November 1st, 1887. At the annual election in May, 1888, he was elected a manager, serving a full term of three years. He was not only active in his duties as an officer and a member, but served with credit as one of the Committee on Units and Standards, where his experience in electrical research was of special value. In 1888 he married Miss Frances R. Cooley, of Pittsfield, Mass., who survives him.

At a meeting of Council June 6th, 1893, the following tribute of respect to his memory was adopted:

The Council of the American Institute of Electrical Engineers, desiring to express its sense of the loss to the Institute in the death of George B. Prescott, Jr., it is hereby

Resolved, that Mr. Prescott's untimely departure from the life of this world has deprived the American Institute of Electrical Engineers of a highly valued member, one whose active mind, quick perceptions and intellectual ability have long been at the service of the Institute at large and of the Council. He had acquired the respect of his fellow members for his ability and his attainments as an electrician, and, not less, their personal esteem for his broad and generous qualities of heart. Youthful in years, he had acquired an enviable reputation for the thoroughness and accuracy of his work, which he was ever trying to excel by further study. As a fellow member and as a personal friend, his loss will long be felt by his associates.

ANTHONY RECKENZAUN, [Associate Member, November 1, 1887; Member, December 6, 1887]; died in London, November 11, 1893. Mr. Reckenzaun was born in 1850, at Gratz, in Austria, where he received a thorough technical education. He went to England in 1872, and obtained employment with a firm of marine engineers. In connection with their works he established evening classes for the benefit of employes, and gave lectures during three years on Machine Construction and Drawing, and on Steam; but in order to qualify himself according to the rules of the South Kensington Science and Art Department, he first underwent examination in those subjects, and passed with first-class honors. Subsequently he attended the courses of lectures given to qualify science teachers at the Royal School of Mines in 1877 and 1879, when he again obtained "first-class" at the final examination in steam and mechanics. On visiting the Paris Exposition of 1878, he resolved to devote his time to the study of electrical engineering. He then applied himself to electrical work, and later joined the Faure Accumulator Co. Soon afterwards he accepted the post of engineer to the Electrical Power Storage Co. He designed the first electric launch, "Electricity," driven by storage batteries in 1882; and soon afterwards he built an electric tramcar which was publicly exhibited on the West Metropolitan Tramway, London, in March, 1883. Mr. Reckenzaun gave numerous lectures and read papers on "Storage Batteries and Electric Locomotion" before scientific societies, notably the British Association, the Society of Arts, the American National Electric Light Association (meeting in Boston, Mass., 1887), the AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS,[1] and the Vienna Electro-Technical Society. The Society of Arts awarded him its silver medal for his paper on "Electric Locomotion." He has also written numerous articles for scientific journals. The English *Electrical Review* in particular has published many valuable contributions from his pen. He started business on his own account in 1885 for the purpose of building electric boats, cars and motors for various purposes. The most noteworthy of the boats was the "Volta," which made the celebrated double voyage between Dover and Calais on the 13th of September, 1886, also the more difficult voyage between London and Dover. Mr. Reckenzaun spent nearly a year in the United States, when he introduced his electric cars on several street railways, and where he built the "Magnet," the first electric launch in American waters. As an enthusiastic advocate of the storage battery for traction and navigation, Mr. Reckenzaun has probably contributed more than any one else to bring the possibilities of the accumulator to the attention of the public, both technical and lay, and much of the practice obtaining in these fields to-day is due to him. Among other work may be mentioned the Reckenzaun-Pentz meter. He came to America again last summer to inspect the electrical exhibits at the World's Fair, and to survey the general technical situation. Before leaving New York, and after his return from Chicago, he was urged to give up work and visit some of the resorts in Southern Europe or the West Indies, but declined attempting to defeat fate, for he regarded himself as foredoomed. His death has deprived the electrical engineering profession of a pure and noble spirit, and his friends of a manly, tender companion.

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ENGINEERS.

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AMERICAN INSTITUTE OF ELECTRICAL ENGINEERS.

CATALOGUE OF MEMBERS.

FEBRUARY 1ST, 1894.

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.

ACHESON, EDW. G.	President The Monongahela Electric Light Co., Monongahela City, Pa.	A Jan. 3, 1888 M May 1, 1888
ADAMS, ALTON D.	Engineer and Manager, Adams Electric Co., Worcester, 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
ANTHONY, PROF. W. A.	(*Past President.*) Consulting Electrician, Vineland, N. J.	A Dec. 9, 1884 M Jan. 6, 1885
ARNOLD, BION J.	Consulting and Contracting Engineer, 565 The Rookery and 4128 Prairie Ave., Chicago, Ill.	A Oct. 25, 1892 M Nov. 15, 1893
AYER, JAMES I.	James I. Ayer & Co., 511 North 4th St., St. Louis, Mo.	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
BARBERIE, E. T.	Electrician, 395 St. Nicholas Ave., 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.	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, 1886
BAUER, A. H.	Electrical Engineer, Pullman Palace Car Co., Room 310, Pullman Building, Chicago, Ill.	A Feb. 7, 1890 M Apr. 21, 1891
BAYLIS, ROBERT N.	Electrical Engineer, 81 Fulton St., New York City.	A Oct. 1, 1889 M May 17, 1892
BELL, PROF. A. GRAHAM	(*Past President.*) Baddeck, Cape Breton, N. S.	A April 15, 1884 M Oct. 21, 1884
BELL, DR LOUIS	(*Manager.*) Electrical Engineer, General Electric Co., Boston, Mass.	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, E. G. Bernard & Co., 43 4th St., Troy, N. Y.	A. Jan. 5, 1886 M July 12, 1887
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.	Electrician Calumet and Hecla Mining Co., Calumet, Mich.	A May 17, 1892 M Feb. 21, 1893
BOURNE, FRANK	Electrical Engineer and Vice-President, The Field Engineering Co., 143 Liberty St., New York City.	A April 21, 1891 M Nov. 15, 1892
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.	Electrical Engineer, 102 Spring St., Rochester, N. Y.	A May 24, 1887 M Dec. 6, 1887
BROOKS, MORGAN	Treasurer and General Manager, The Electrical Engineering and Supply Co., 249 Second Ave. South, 2948 Park Ave., Minneapolis, Minn.	A May 20, 1890 M June 17, 1890
BROWN, ALFRED S.	Electrical Engineer, P. O. Box 856, Western Union Telegraph Co., 195 Broadway, New York City.	A Mar. 18, 1890 M Feb. 21, 1893

BROWN, J. STANFORD, *E.E.*,	Consulting and Constructing Electrical Engineer, Neftel, O'Connor & Co., Inc., 126 Liberty St., New York City.	A Sept. 6, 1887 M Nov. 1, 1887
BRUSH, CHAS. F.	Electrical Engineer, 956 Euclid Ave., 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., 68 Baker St., Lynn, Mass.	A June 17, 1890 M May 19, 1891
CARROLL, LEIGH	President and General Manager, The Edison Electric Illuminating Co., 2021½ First Ave., Birmingham, Ala.	A Oct. 1, 1889 M Nov. 12, 1889
CHAMBERLAIN, J. C.	Electrical Engineer, 135 East 18th St., New York City.	A Dec. 6, 1887 M Jan. 3, 1888
CHANDLER, CHARLES F.	Professor of Chemistry, Columbia College, and Prof. of Chemistry and Physics, College of Physicians and Surgeons, New York City.	A Jan. 20, 1891 M June 7, 1892
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
CLARKE, CHAS. L.	Mechanical and Electrical Engineer, 55 Liberty St., New York City.	A April 15, 1884 M Jan. 6, 1885
COLBY, EDWARD A.	Lock Box 313, Newark, N. J.	A April 2, 1889 M May 7, 1889
CONDICT, G. HERBERT	General Manager, the Electric Car Co. of America, 4720 Green St., Germantown, Pa.	A July 12, 1887 M Sept. 6, 1887
COTHREN, WM. H.	173 South Oxford St., Brooklyn, N. Y.	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
CROCKER, FRANCIS B. [Life Member.]	Professor of Electrical Engineering, School of Mines, Columbia College, and 26 W. 22d St., New York.	A May 24, 1887 M April 2, 1889
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., 180 Summer St., Boston, Mass.	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, Burke Building, Seattle, Wash.	A Dec. 9, 1884 M Jan. 6, 1885
DANVERS, ALAN	Managing Director and Chief Electrician, The Anglo-Portuguese Telephone Co., L't'd, 60 Travessa Santa Justa, Lisbon, Portugal.	A Nov. 1, 1887 M Sept. 3, 1889
DANVERS, ERNESTO	[Address unknown.]	A Jan. 3, 1888 M May 1, 1888
DAVIES, JOHN E.	Professor of Physics, University of Wisconsin, 523 Carroll St., Madison, Wis.	A Jan. 7, 1890 M Mar. 18, 1890
DAVIS, CHARLES H., *C. E.*,	Consulting and Constructing Engineer, 120 Broadway, New York City, and 308 Walnut St., Philadelphia, Pa.	A Mar. 18, 1890 M June 17, 1890
DAVIS, MINOR M.	Ass't Electrician, Postal Telegraph Cable Co., 5 Dey St., 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 15, 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.	Supt. and Electrician, Chaudiere Electric Light and Power Co., Ltd., 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.	Care of Otis Bros. & Co., Yonkers, N. Y., and 126 East 22nd St., New York City.	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	(*Vice President.*) Johns Hopkins University, Baltimore, Md. Firm of Sprague, Duncan & Hutchinson, 15 Wall St., New York City.	A July 12, 1887 M Sept. 6, 1887
DUNSTON, ROBT. EDWARD	Room 86, Foster Block, Hartford, Conn.	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
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., 44 Broad St., New York City.	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
EVEREST, AUGUSTINE R.	Electrical Engineer, Thomson Electric Welding Co., Lynn, Mass.	A May 19, 1891 M Dec. 20, 1893
FARNHAM, ISAIAH H.	Electrician for the 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.	President and General Manager Field Engineering Co., 143 Liberty 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
FLEMING, WILFRID H.	Trask Ave., Bayonne City, N. J.	A Dec. 6, 1887 M Jan. 3, 1888
FOSTER, HORATIO A.	Electrical Engineer, Chief Assistant, Prof. George Forbes, Room 24, 7th Floor, 35 Wall St., and 548 East 164th St.	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
GRAY, DR. ELISHA	Electrician and Inventor, Highland Park, Ill.	A Feb. 16, 1892 M May 17, 1892
GRISCOM, WM. W., *M.A.*	Electrical Engineer, 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

HAMBLET, JAMES	(*Manager*) Manager Time Service, W. U. Tel. Co., 195 Broadway, P. O. Box 856, New York City.	A Nov. 1, 1887 M Dec. 6, 1887
HAMILTON, GEO. A.	Electrician, Western Electric Co., 22 Thames, cor. Greenwich St., New York, and 532 Morris Ave., Elizabeth, N. J.	A April 15, 1884 M Oct. 21, 1884
HAMMER, W. J.	(*Manager.*) Consulting and Supervising Electrical Engineer, 1301 and 1302 Havemeyer Building, 26 Cortlandt St; Residence, 220 W. 44th St., N. Y.	A June 8, 1887 M July 12, 1887
HASKINS, CHARLES H.	Electrician, 80 Broadway, New York City.	A April 15, 1884 M Oct. 21, 1884
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, 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.	Manager and Electrical Engineer, Wright Electrical Engineering Co., 196 Sumner St., Boston, 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.*) 44 Broad St., Box 3067, New York City.	A Sept. 16, 1890 M May 17, 1892
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
HOUSTON, PROF. EDWIN J. [Life Member.]	(*President.*) Prof. of Physics, Franklin Inst., Prof. of Physics and Physical Geography, Central High School, 1809 Spring Garden St., Philadelphia, Pa.	A April 15, 1884 M Oct. 21, 1884
HOWELL, JOHN W.	Electrician, Edison Lamp Works, Harrison, N. J.	A July 12, 1887 M June 5, 1888

HOWELL, WILSON S.	Incandescent Lamp Department, General Electric Co., Harrison, N. J., and 19 Webster Place, Orange, N. J.	A Sept. 3, 1889 M Mar. 18, 1890
HUNTER, RUDOLPH M.	Mechanical and Electrical Engineer, 926 Walnut St., Philadelphia, Pa.	A July 13, 1886 M May 17, 1887
HUNTING, FRED S.	Electrical 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, 56 West 25th St., Firm of Sprague, Duncan & Hutchinson, 15 Wall 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
JACKSON, DUGALD C.	Professor of Electrical Engineering, University of Wisconsin, Madison, Wis.	A May 3, 1887 M June 17, 1890
JACKSON, FRANCIS E.	With Edison Lamp Works, Harrison, N. J.	A Jan. 3, 1888 M June 17, 1890
JACKSON, J. P.	Assistant Professor of Electrical Engineering, Penn. State College, State College, Pa.	A Sept. 27, 1892 M Jan. 17, 1894
JANNUS, FRANKLAND	Attorney-at-Law, 928-30 F. St., 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
JONES, FRANCIS W. [Life Member.]	Assistant Gen'l-Manager and Electrician, Postal Telegraph-Cable Co., 5 Dey St., New York City.	A April 15, 1884 M Oct. 21, 1884
KEITH, DR. NATHANIEL S.	Care Low's Exchange, 57 Charing Cross, London, Eng.	A Sept. 20, 1893 M Jan. 17, 1894
KINSMAN, FRANK E.	Electrical Engineer and President Kinsman Block Signal Co., 96 Broadway, 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.	Phillips Insulated Wire Co., 39 Cortlandt St., New York City.	A Dec. 6, 1887 M Jan. 3, 1888
LANGE, PHILIP A.	Assistant Superintendent Westinghouse Electric and Manufacturing Co., Newark, N. J.	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 Lines, Lehigh Valley R. R. Co., So. Bethlehem, Pa.	A June 8, 1887 M July 12, 1887

LAWSON, A. J.	Electrical Engineer and Contractor, Grantown, Scotland.	A Mar. 18, 1890 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.*) President, The H. Ward Leonard Co., 136 Liberty 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.	22 Chestnut St., Newark, N. J.	A Sept. 6, 1887 M Nov. 1, 1887
LLOYD, ROBERT MCA.	Electrician, 2 W. 36th St., New York City.	A Oct. 21, 1890 M Nov. 15, 1893
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
MACFARLANE, ALEXANDER, *D. Sc., LL.D.*	Professor of Physics, University of Texas, Austin, Tex	A Jan. 19, 1892 M May 17, 1892
MAILLOUX, C. O.	Consulting Electrical Engineer, 45 William St., New York City.	A April 15, 1884 M Oct. 21, 1884
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, 31 Nassau St., New York City.	A July 12, 1887 M April 21, 1891
MAYNARD, GEO. C.	Electrical Engineer, 1409 New York Ave., 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.	Electrical Engineer, Editor *Electricity*, 6 Park Place, and 404 W. 22d St., New York City.	A April 19, 1892 M Nov. 15, 1892
MILLIS, JOHN	Captain, Corps of Engineers, U. S. Army, 1 Prytania St., New Orleans, La.	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, 40 California St., San Francisco, Cal.	A Jan. 16, 1892 M June 7, 1892
NICHOLS, DR. EDWARD L.	Professor of Physics, Cornell University, Ithaca, N. Y.	A Oct. 4, 1887 M Dec. 6, 1887
NOLL, AUGUSTUS	New York Electric Equipment Co., 59 Duane St., New York City.	A Sept. 27, 1892 M April 18, 1893

PAINE, SIDNEY B.	Power and Mining Department, General Electric Co., 180 Summer St., Boston, Mass.	A June 8, 1887 M Nov. 1, 1887
PAINE, F. B. H.	Engineer, Westinghouse Electric and Mfg. Co, 620 Atlantic Ave., Boston, Mass.	A Dec. 16, 1890 M Nov. 25, 1891
PARKHURST, CHARLES D.	Lieut. 1st Artillery, U. S. Army, Fort McHenry, Md.	A Dec. 21, 1892 M Nov. 15, 1893
PARKS, C. WELLMAN	U. S. Board of Education, Washington, D. C.	A July 12, 1887 M May 1, 1888
PARSHALL, H. F.	Electrical Engineer, Lynn, Mass.	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.	Chief Engineer West End Street Railway Co., 95 Milk Street, Boston, Mass.	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.*,	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., 18 Cortlandt St., 153 Cedar St., New York City.	A Feb. 7, 1890 M Mar. 18, 1890
PIERCE, RICHARD H.	Electrical Engineer, World's Columbian Exposition, 5434 Monroe Ave., Hyde Park, Ill.	A April 18, 1893 M Dec. 20, 1893
PIKE, CLAYTON W., *S. B.*	Electrical Engineer, Queen & Co., 1010 Chestnut St., Philadelphia, Pa.	A Dec. 16, 1891 M Oct. 25, 1892
POPE, FRANKLIN L.	(*Past President*). Consulting Electrical Engineer and Expert, 39 Cortlandt St., N. Y. Residence, Elizabeth, N. J.	A April 15, 1884 M Oct. 21, 1884
PORTER, J. F.	Representing J. G. White & Co., of New York, Heist Building, Kansas City, Mo.	A Sept. 6, 1887 M Nov. 1, 1887
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, 49 Wilcox St., Detroit, Mich.	A April 15, 1884 M Oct. 25, 1892
RAYMOND, CHAS. W.	Civil, Electrical and Mining Engineer, Monte Vista, Col.	A June 8, 1887 M May 17, 1887
RECKENZAUN, FREDERICK,	Electrical Engineer, Box 225, 58 Demott St., West Hoboken, N. J.	A Mar. 6, 1888 M June 5, 1888
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, Western Reserve Building, 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, ROBERT A.	Electrical Engineer, Royal Electric Co., Montreal, P. Q.	A Sept. 27, 1892 M April 18, 1893
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
SCHULZE-BERGE, FRANZ	44 Monroe Place, Brooklyn, N. Y.	A Nov. 12, 1889 M Mar. 18, 1890
SCOTT, CHARLES F.	Assistant Electrician, Westinghouse Electric and Mfg. Co., Pittsburg, Pa.	A April 19, 1892 M Jan. 17, 1893
SHALLENBERGER, O. B.	Electrician, Westinghouse Electric and Mfg. Co., Pittsburg, Pa.	A Sept. 7, 1888 M Dec. 4, 1888
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.	Lincoln St. Ry. Co., 9th and K Sts., Lincoln, Neb.	A Feb. 7, 1890 M Mar. 18, 1890
SLATER, HENRY B.	Manager and Sup't, 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
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.	Supt. of Construction N. W., General Electric Co., 403 Sibley St., St. Paul, Minn.	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
STIERINGER, LUTHER	Electrical Expert, 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
TERRY, CHARLES A.	Lawyer, Westinghouse Electric and M'f'g 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 Co., and Thomson Electric Welding Co., 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 Loudres, Paris, France.	A Oct. 14, 1887 M Dec. 6, 1887
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, 597 Bergen Ave., Jersey City, N. J.	A Feb. 7, 1890 M Nov. 15, 1893
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, Electrical, Mechanical Engineering and Trading Co., 44 Broad St., New York City.	A June 8, 1887 M Nov. 1, 1887
VANSIZE, WILLIAM B.	Solicitor of Patents, 44 Broad St., New York City.	A April 15, 1884 M Oct, 21, 1884
WADDELL, MONTGOMERY	Engineer, The Waddell-Entz Electric Co., Bridgeport, Conn.	A Feb. 7, 1888 M May 1, 1888
WALDO, DR. LEONARD	Electrical Engineer, Secretary, The Waldo Foundry, and Member Firm of Sprague, Duncan, & Hutchinson (Ltd.), 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
WEAVER, W. D.	32 W. 31st St., New York City.	A May 17, 1887 M May 17, 1887

WEBB, HERBERT LAWS	(*Manager.*) 18 Cortlandt 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.	General Manager, The United Columbian Electric Co., 280 Broadway, New York City.	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., Electrical Expert, Board of Electrical Control, New York City.	A June 2, 1885 M Sept. 1, 1885
WILKES, GILBERT	Consulting Electrical Engineer, 149 Griswold St., Detroit, Mich.	A Jan. 7, 1890 M Mar. 18, 1890
WILLYOUNG, ELMER G.	Electrician, James W. Queen & Co., 817 Filbert St., Philadelphia, and Ardmore, Pa.	A Nov. 24, 1891 M Dec. 20, 1893
WILSON, CHARLES H.	Firm of Wilson & Jackson, Times Building Chicago, Ill.	A Nov. 24, 1891 M Feb. 16, 1892
WILSON, FREMONT	Consulting Electrician, 132 Nassau St., and 293 Lenox (6) 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.	Designer of Steam Electrical Plants, Wilton, Conn.	A June 8, 1887 M Nov. 1, 1887
WOLCOTT, TOWNSEND	Electrician, care of W. A. Rosenbaum, Times Building, New York City.	A Mar. 6, 1888 M Dec. 16, 1890
WOODBRIDGE, J. L.	Secretary and Treasurer, Woodbridge & Turner Engineering Co., 47 Times Building, New York City.	A June 8, 1887 M Nov. 1, 1887
WURTS, ALEXANDER JAY	Electrical Expert, Westinghouse Electric & Mfg. Co., Pittsburg, Pa.	A April 19, 1892 M Nov. 15, 1892
YOUNG, C. GRIFFITH	Electrical Engineer, care J. G. White & Co., 29 Broadway, New York City ; 1425 Maryland, Ave., Baltimore, Md.	A Jan. 3, 1889 M April 21, 1891
ZETZSCHE, PROF. DR. CARL EDUARD,	Telegraph Engineer, Carlstrasse 13, Dresden, N., Saxony.	A Nov. 1, 1887 M Jan. 3, 1888

Members, - - - - - 223.

ASSOCIATE MEMBERS.

ABBOTT, ARTHUR V.	Chief Engineer, Chicago Telephone Co., 203 Washington St., Chicago, Ill.	Oct. 21, 1890
ADAMS, COMFORT A., JR.	Instructor in Electrical Engineering, Harvard University, 21 Stoughton Hall, Cambridge, Mass.	Jan. 17, 1894
ALBANESE, G. SACCO	Compagnie Internationale Electricite, Liege, Belgium.	Sept. 20, 1893
ALBERT, HENRY	Superintendent, Royal Light, Heat and Power Co., Front Royal, Va.	Feb. 21, 1893
ALBRIGHT, H. FLEETWOOD	Electrical Engineer, Western Electric Co., 227 S. Clinton St., Chicago, Ill.	Sept. 27, 1892
ALDEN, JAMES S.	Assistant Manager, with L. H. Alden, 486 River Drive, Passaic, N. J.	May 19, 1891
ALDRICH, WILLIAM S.	P. O. Box 256, Morgantown, W. Va.	Mar. 15, 1892
ALEXANDER, HARRY	Electrical Contractor, 126 Liberty, and 340 W. 145th St., New York City.	April 21, 1891
ALEXANDER, P. H.	Southern Electric Co., Hoen Building, Baltimore, Md.	Dec. 16, 1890
ALMON, G. H.	Electrical Engineer and Contractor, 136 Liberty St., New York City.	Sept. 20, 1893
ANDERSON, W. E.	Professor of Physics and Electrical Engineering, Virginia A and M College, Blacksburg, Va.	Sept. 20, 1893
ANDREWS, WM. S.	General Manager, Edison Electric Illuminating Co. of Lancaster, Williamsport, and Sunbury, Pa.	Mar. 5, 1889
ANTHONY, WATSON G.	Electrician, 32½ Webster St., Newark, N. J.	Feb. 24, 1891
ARMSTRONG, CHAS. G.	Electrical Expert and Electrical Architect, 1400 Auditorium Tower, Chicago, Ill.	Sept. 27, 1892
ARNOLD, CRAIG R.	Electrician and Treasurer, Arnold Electric Mfg. Co., Chester, Pa.	Nov. 15, 1892
ATWOOD, GEORGE F.	Orange, N. J.	Sept. 16, 1890
AUERBACHER, LOUIS J.	Electrician and Head Salesman, The E. S. Greeley & Co., 5 and 7 Dey St., New York City.	Sept. 20, 1893
BADT, LIEUT. FRANCIS B.	6506 Lafayette Ave., (Englewood), Chicago, Ill.	April 19, 1892
BAILLARD, E. V.	363 Manhattan Ave., New York City.	Dec. 3, 1889
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

BARTH-BARTOSHEVITCH, A.	Mechanical and Electrical Engineer, Westinghouse Electric and Mfg. Co , 160 Arch St., Allegheny City, Pa.	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., 89 Liberty Street, New York City.	June 8, 1887
BATES, FREDERICK C.	Electrical Engineer, with Julian Scholl & Co., 126 Liberty St. and 73 Madison Ave., New York City.	Jan. 20, 1891
BAUER, W. F.	Electrician, 62 Steuben St., East Orange, 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, FREDERICK, DR.	Assistant Professor in Physics, Cornell University, 117 E. Buffalo St., Ithaca, N. Y.	April 21, 1891
BENNETT, J. C.	Electrician, General Electric Co., Box 3067, 44 Broad St., New York City.	Mar. 18, 1890
BENTLEY, MERTON H.	Chicago Telephone Co., 221 Scoville Ave., Oak Park, Ill.	Oct. 18, 1893
BERHGOLTZ, HERMAN	Treasurer and General Manager, 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
BERTHOLD, VICTOR M.	Patent Department, American Bell Telephone Co., and Editor, *New England Courier*, Boston, 16 Upton St., Cambridgeport, Mass.	May 17, 1892
BETHELL, U. N.	Acting General Manager, The Metropolitan Telephone and Telegraph Co., 18 Cortlandt St., N. Y. City.	Jan. 17, 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.	The Street Railway Publishing Co., 26 Cortlandt St., New York City.	Nov. 13, 1888
BLAKE, THEODORE W.	Electrician, National India Rubber Co., Bristol, R. I.	Sept. 20, 1893
BLISS, DONALD M.	Electrician, Holtzer-Cabot Electric Co., 1 Davis Court, Washington St., Brookline, Mass.	Feb. 7, 1890

BLISS, WM. J. A.	820 Connecticut Ave., Washington, D. C.	Jan. 20, 1891
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	Electrical Engineering Department, World's Fair, 3012 Groveland Ave., Chicago, Ill.	Sept. 20, 1893
BOGUE, CHARLES J.	Manufacturer of and Dealer in Electric Supplies, 206 Centre St., New York City.	Dec. 3, 1889
BOHM, LUDWIG K., *Ph.D.*,	Consulting Electrical and Chemical Expert, 81 Nassau St., New York City.	Nov. 15, 1892
BOTTOME, TURNER D.	Electrician, Indianapolis, Ind.	April 21, 1891
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
BOWMAN, FRED. A.	Supt., New Glasgow Electric Co., New Glasgow, Nova Scotia.	May 19, 1891
BRACKETT, PROF. CYRUS F.	Princeton, N. J.	April 15, 1889
BRADDELL, ALFRED E.	Electrical Inspector, Underwriters' Association, Middle Department, 308 Walnut St., Philadelphia, Pa.	Sept. 1, 1890
BRADY, PAUL T.	Syracuse, N. Y.	July 12, 1887
BRAGG, CHARLES A.	Manager Phila. Agency, Westinghouse Electric and Mfg. Co., Girard Building, Philadelphia, Pa.	Sept. 20, 1893
BREITHAUPT, E. CARL	Electrical Engineer, Berlin, Ont.	June 6, 1893
BRENNER, WILLIAM H.	Electrical Engineer, Montreal Street Railway Co., Montreal, P. Q.	Sept. 20, 1893
BRIXEY, W. R.	Proprietor and Manufacturer, Day's Kerite Wire and Cables, 203 Broadway, New York City.	Sept. 20, 1893
BROADNOX, FRANCIS	Engineer, Safety Insulated Wire and Cable Co., 50 Broadway, New York City.	Jan. 17, 1894
BROICH, JOSEPH	Superintendent and Electrician, with F. Pearce, 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, 100 West 73d St., New York City.	Jan. 7, 1890
BROWN, CHARLES A.	Attorney, Firm of Barton & Brown, Attorneys and Counsellors, 1428–1430 Monadnock Building, Chicago, Ill.	July 12, 1887

BRYANT, WALDO C.	Manager and Treasurer, The Bryant Electric Co., Bridgeport, Conn.	May 16, 1893
BUBERT, J. F.	Electrical Engineer, The Mather Electric Co., 116 Bedford St., Boston, Mass.	June 7, 1892
BUCKINGHAM, CHAS. L.	Patent Attorney, Western Union Telegraph Co., 195 Broadwav, P. O. Box 856, New York City.	April 15, 1884
BUNCE, THEODORE D.	The Storage Battery Supply Co., cor. 23d St. and 1st Ave., New York City.	May 20, 1890
BURKE, JAMES	Electrical Engineer, General Electric Co,, 24 Front St., Schenectady, N. Y.	May 16, 1893
BURNETT, DOUGLAS, *B. S.*	42 Livingston, St., Brooklyn, N. Y.	Feb. 21, 1893
BURNS, ELMER Z.	Consulting Electrical Engineer, Niagara Falls, N. Y.	Feb. 7, 1890
BURTON, GEO. D.	Electrician and President, Electrical Forging Co., 194 Washington St., Boston, Mass.	April 21, 1891
BURTON, WILLIAM C.	Edison Illuminating Co., 238 Twenty-seventh St., Milwaukee, Wis.	Sept. 20, 1893
BUTLER, WILLIAM C.	Monte Cristo Mining Co., Everett, Washington.	Mar. 21, 1893
BUYS, ALBERT	Electrician, Rutherford B. S. & C. Elec. Co., Rutherford, N. J.	Feb. 7, 1890
CABOT, JOHN ALFRED	City Electrician, 123 W. 8th St., Cincinnati, O.	May 16, 1893
CALDWELL, EDWARD	Editor, *Street Railway Gazette*, 1432–3 Monadnock Block, Chicago, Ill.	Jan. 20, 1891
CALDWELL, FORDYCE S.	Proprietor Western Electric Construction Co., 503 Delaware St., Kansas City, Mo., and 151 Henry St., Brooklyn, N. Y.	Sept. 22, 1891
CALLENDER, ROMAINE	Electrician, Brantford Electrical Laboratory, Brantford, Canada.	Sept. 27, 1892
CANFIELD, MILTON C.	Electrical Engineer, 18 Clinton St., Cleveland, O.	Feb. 21, 1893
CAPUCCIO, MARIO	Electrical Engineer, Piazza Statuto 15, Corino, Italy.	Dec. 20, 1893
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*) Electrician, Metropolitan Telephone and Telegraph Co., cor. Spring and Wooster Sts., New York City.	April 15, 1890
CASE, WILLARD E.	6 Fort St., Auburn, N. Y.	Feb. 7, 1888
CASPER, LOUIS	Contracting Electrician, Sheridan, Wyoming.	April 21, 1891

CHAMBERLAIN, F. H.	Electrician, Metropolitan R. R. Co., 2411 P St., N. W. Washington, D. C.	June 17, 1890
CHENEY, FREDERICK A	Secretary, Treasurer, and General Manager, The Elmira Illuminating Co., Elmira, N. Y.	Oct. 1, 1889
CHENEY, W. C.	Superintendent and Electrical Engineer, Portland General Electric Co, Portland, Ore.	Sept. 22, 1891
CHERMONT, ANTONIO LEITE	7 Rue da Matriz, Rio Janeiro, U. S. Brazil.	Mar. 18, 1890
CHILDS, W. H.	Bookkeeper for The Estey Organ Co., Brattleboro, Vt.	Sept. 6, 1887
CHINNOCK, C. E.	137 Sixth Ave., Brooklyn, N. Y.	April 15, 1884
CHRISTMAS, ADOLPH FREDERICK	Manager Electrical Department, Johnson Co., Johnstown, Pa.	May 16, 1893
CHUBBUCK, H. EUGENE	Secretary and Treasurer, The New Omaha Thomson-Houston Electric Co., Omaha, Neb.	Dec. 4, 1888
CLAFLIN, ADAMS D.	General Manager, The Mather Electric Co., 116 Bedford St., Boston, Mass.	June 7, 1892
CLEMENT, LEWIS M.	1013 Central Ave., Oakland, Cal.	April 21, 1891
CLEVELAND, WM. B.	Electrical Engineer, 309 Perry-Payne Building, Cleveland, O.	April 15, 1884
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
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
COMPTON, ALFRED G.	(*Manager.*) Professor of Physics, College of the City of New York, 17 Lexington Ave., New York City.	Nov. 1, 1887
COMSTOCK, LOUIS K.	Contracting and Consulting Engineer, Monadnock Building, Chicago, Ill.	Dec. 20, 1893
COOLIDGE, CHARLES A.	Supt. and Electrician, Northern Improvement Co, Centralia, Washington.	April 19, 1892
COREY, FRED. B.	Electrical Engineer, A. B. See Manufacturing Co., 442 Henry St., Brooklyn, N. Y.	Dec. 20, 1893
CORNELL, CHAS. L.	Electrical Engineer, Cornell Engineering Co., Hamilton, O.	Feb. 7, 1890

CORSON, WILLIAM R. C.	Assistant Electrician, 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
COWLES, JOSEPH W.	Electrical Engineer, General Electric Co., 620 Atlantic Ave., Boston, Mass.	Feb. 21, 1893
CRAIG, J. HALLY	Cumner, Craig & Co., 69 Broad St., Boston, Mass.	May 16, 1893
CRAIGIN, HENRY A.	Engineer, Westinghouse Electric and Mfg. Co., 15 Charles St., Boston, Mass.	June 6, 1893
CRANDALL, CHESTER D.	Assistant Treasurer, Western Electric Co., 227 South Clinton St., Residence, 4438 Ellis Ave. Chicago, Ill.	Sept. 27, 1892
CRANDALL, JOSEPH EDWIN	Electrician, C. & P. Telephone Co., 619 Fourteenth St., N. W. Washington, D. C.	April 18, 1892
CRANE, W. F. D.	Manager Electrical Department H. W. Johns Manufacturing Co., 87 Maiden Lane, New York City, and 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
CROSBY, OSCAR T.	(*Vice-President*) General Manager Railway Dept. General Electric Co., 44 Broad St., New York City.	Mar. 18, 1890
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., Pacific Ave., Jersey City, N. J.	April 15, 1884
CUSHING, F. W.	General Western Agent, Day's Kerite Wire and Cables, 1106 The Rookery, Chicago, Ill.	Nov. 24, 1891
CUSHMAN, HOLBROOK	Instructor in Physics, Columbia College, 337 West 22d St., New York City.	June 5, 1888
DACUNHA, MANUEL IGNACIO	Manager of the Electrical Section, Emprera Industrial Gram-Para, Para, U. S. of Brazil.	May 16, 1893
DAME, FRANK L.	Engineer, The Northwest 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

DANIELL, FRANCIS G.	Electrical Engineer, State Street H. R. R. Co., 528 State St., New Haven, Conn.	Nov. 12, 1889
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.	Consulting Engineer, care of Phœnix Construction Co., 115 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,	Superintendent, Germania Electric Co., Marlboro, Mass.	Oct. 27, 1891
DELAND, FRED	Editor, *Electrical Engineering*, 565 The Rookery, Chicago, Ill.	Feb. 16, 1892
DEMING, EDWARD	General Manager, Deming Automatic Electric Safety System, 522 Hancock St., Brooklyn, N. Y.	April 18, 1893
DENTON, JAMES E.	Professor of Experimental Mechanics, Stevens Institute of Technology, Hoboken, N. J.	July 12, 1887
DESMOND, JERE. A.	Supt. Electrical Dept., Kingston Electric Light Co., Kingston, N. Y.	Jan. 19, 1892
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, 1014 Bergen Ave., Jersey City, N. J.	Feb. 5, 1889
DODGE, OMENZO G.	U. S. Navy, Washington, D. C.	Sept. 20, 1893
DOOLITTLE, THOMAS B.	Engineering Department, American Bell Telephone Co., 125 Milk St., Boston, Mass.	May 16, 1893
DOREMUS, CHARLES A.	*M.D. Ph.D.* Chemist and Physicist, Bellevue Hospital Medical College, College of the City of New York and American Veterinary College, 59 W. 51st St., New York City.	July 7, 1884
DOW, ALEXANDER	Engineer, Public Lighting Commission, 511 Hammond Bldg., Detroit, Mich.	Sept. 20, 1893
DRESSLER, CHARLES E.	Maker of Scientific and Electrical Apparatus, College of the City of New York, 17 Lexington Ave., New York City.	Dec 16, 1890
DUNN, GANO S.	Electrical Engineer, of the Crocker-Wheeler Electric Co., Ampere, E. Orange, N. J.; Residence, 223 Central Park, West, New York City.	April 21, 1891

DURANT, EDWARD	Electrician, 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
EDWARDS, JAMES P.	Electrical Engineer, 1569 Walton Way, Augusta, Ga.	April 19, 1892
EGGER, ERNST	Electrical Engineer care of B. Egger & Co, X, Simmeringstr, 187, Vienna, Austria.	Feb. 21, 1893
EICKEMEYER, RUDOLF	President, Eickemeyer & Osterheld Mfg, Co., Yonkers, N. Y.	Sept. 20, 1893
EKSTROM, AXEL	Electrician, 92 Hamilton Ave., Lynn, Mass.	June 17, 1890
ELEY, HARRIS H.	Electrical Workshop Supt. W. C. & S. W. Telephone Co., 88 Colston St., Bristol, Eng.	Jan. 7, 1890
ELMER, WILLIAM JR.	Under-graduate in Electrical Engineering, Princeton University, Princeton, N. J.	Mar. 18, 1890
ELY, WM. GROSVENOR, JR.	Edison General Electric Co., 226 Union St,. Schenectady, N. Y.	Mar. 21, 1893
EMMET, HERMAN L. R.	Publisher and Printer, 36 Cortlandt St., New York City.	April 15, 1884
ENDE, SIGFRIED H.	Colonnade Hotel, 39 Lafayette Place New York City.	Jan. 17, 1894
ENTZ, JUSTUS B.	Electrical Engineer, The Waddell-Entz Co., Bridgeport, Conn.	Jan. 7, 1890
ESSICK, SAMUEL V.	Electrician, The Essick Printing Tel. Co., Yonkers, N. Y.	May 19, 1891
ETHERIDGE, E. L.	66 No. Oxford St., Brooklyn, N. Y.	Dec. 20, 1893
FAY, THOMAS J.	"C. & C." Electric Motor Co., 402 Greenwich 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, GEORGE E.	Secretary and General Manager, Commercial Electric Co., 55–57 Gratiot Ave., Detroit, Mich.	Sept. 27, 1892
FISKE, HENRY G.	Electrician, Croton Magnetic Iron Mines, 45 E. 22d St., New York City.	Nov. 12, 1889
FISKE, J. P. B.	Electrical Engineer, General Electric Co., Lynn, Mass.	June 17, 1890

Fitzmaurice, James S.	Chief Engineer, The Electric Light Branch, 210 George St , Sydney, N. S. W.	Sept. 20, 1893
Flack, J. Day	Supt. of Equipment, General Electric Co., Lamp Works, Harrison, N. J.	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
Flegel, Geo. C.	Electrical Editor, The *Railroad Telegrapher*, Argos, Ind.	Sept. 20, 1893
Fleming, Richard	208 West 15th St., New York City.	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.	Supt Steubenville Street Railway Co., Steubenville, O.	Mar. 18, 1890
Floy, Henry	168 North Ave., Allegheny, 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.	110 W. 129th St., New York City.	April 21, 1891
Forbes, Francis	Lawyer, 137 Broadway, New York City.	Sept. 16, 1890
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
Freedman, William H.	Tutor in Electrical Engineering, School of Mines, Columbia College, 120 W. 125th St., New York City.	Mar. 18, 1890
French, E. L.	Stanley Laboratory Co., Pittsfield, Mass.	Dec. 16, 1890
French, Thomas, Jr.	Professor of Physics, University of Cincinnati, Avondale, Cincinnati, O.	Sept. 20, 1893
Frey, Charles P.	Electrician, The E. S. Greeley & Co. Laboratory, Greenbush, N. Y.	June 6, 1893
Frost, Francis R.	Ithaca, N. Y.	Dec. 20, 1893
Frye, Henry W.	Associate Editor, The *Electrical World*, Times Building, New York City.	May 16, 1893

FULLER, LEVI K.	Vice-President, Estey Organ Co., and Governor of Vermont, Brattleboro, Vt.	Mar. 5, 1889
GARDANIER, GEORGE W.	Electrician, Western Union Telegraph Co., 195 Broadway, New York City.	April 18, 1893
GERRY, M. H., JR.	Engineer, N. W. General Electric Co., 3333 Cedar Ave., Minneapolis, Minn.	April 18, 1893
GIFFORD, CLARENCE E.	Assistant Electrical Engineer, The Buffalo Railway Co., 878 Prospect Ave., Buffalo, N. Y.	May 16, 1893
GILES, WALTER A.	Engineer and Contractor, 416 Lewis Block, Pittsburg, Pa.	Nov. 1, 1887
GILLILAND, E. T.	Pelham Manor, N. Y.	April 15, 1884
GOLDMARK, CHAS. J.	Electrical Engineer, Field Engineering Co., 143 Liberty St , and 473 Park Ave., New York City.	June 5, 1888
GOLDSBOROUGH, WINDER E., *M.E.*,	Greensborough, Caroline Co., Md.	Mar. 21, 1893
GORTON, CHARLES	Civil Engineer, Belmont, N. Y.	Nov. 12, 1889
GORDON, REGINALD	Assistant in Physics, Columbia College, Residence, 76 Park Ave., New York City.	Feb. 24, 1891
GRAY, W. N.	Electrical Engineer, 200 Neave Building, Cincinnati, O.	Oct. 1, 1889
GREENE, S. DANA	Assistant General Manager, General Electric Co., Schenectady, N. Y.	Sept. 20, 1893
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
GRIFFIN, CAPT. EUGENE	General Electric Co., 44 Broad St., New York City.	Feb. 7, 1890
GUY, GEORGE HELI	Secretary, The New York Electrical Society, 131 West 34th St., New York City.	May 16, 1893
HADLEY, WARREN, B.	Supt., Wiring Dept., Edison Electric Illuminating Co., 431 Fifth Ave., 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., 80 Broadway, New York City.	Sept. 16, 1890
HALSEY, WILLIAM B.	Inspector, Postal Telegraph-Cable Co., 96 Broadway, New York City.	Mar. 18, 1890

HAMMATT, CLARENCE S.	Supt, Jacksonville Electric Light Co., Jacksonville, Fla.	Sept. 20, 1893
HANCOCK, L. M.	Supt. of Construction, Western Electric Co., 225 Fair Oaks St., San Francisco, Cal.	May 19, 1891
HANDLEY, ARTHUR	Electrical Engineer, Electrical Engineering Co. of Ireland, Limited, 61 Dawson St, Dublin, Ireland.	Dec. 16, 1890
HARDING, H. McL.	1014 Havemeyer Building, 26 Cortlandt St., New York City.	May 24, 1887
HARRINGTON, WALTER E.	Electric Railway Engineer, 307 Market St., Camden, N. J.	Mar. 17, 1891
HART, FRANCIS R.	Supt. of Construction, Mass. Electrical Engineering Co., 4 P.O. Square, Boston, Mass.	April 21, 1891
HARTMAN, HERBERT T.	Assistant Engineer, Canadian General Electric Co., Peterborough, Ont.	Mar. 21, 1893
HASKINS, CARYL D.	General Electric Co., Schenectady, N. Y.	Mar, 18, 1890
HASKINS, CLARK CARYL	City Electric Light Inspector, 582 West Congress St., Chicago, Ill.	Sept. 20, 1893
HASSON, W. F. C.	Consulting Engineer, 104 Sutter St., San Francisco, Cal.	Mar. 18, 1890
HATZEL, J. C.	Electrical Engineer and Contractor, 114 Fifth Ave., New York City.	Sept. 3, 1889
HEATH, HARRY E.	Chief Draughtsman, Eddy Electric Mfg. Co., Box 189, Windsor, Conn.	Mar. 21. 1893
HEALY, LOUIS W.	The Wightman Electric Co., 1205 Marion St., Scranton, Pa.	June 26, 1891
HENSHAW, FREDERICK V.	Electrical Engineer, The "C. & C." Electric Motor Co., 402 Greenwich St., New York City.	Feb. 5, 1889
HEWLETT, EDWARD M.	Electrical Engineer, Railway Dept. General Electric Co., 128 Johnson St., Lynn, Mass	May 19, 1891
HIGGINS, EUGENE	Electrical Engineer, Racine, Wis.	April 19, 1892
HILL, GEORGE, *C.E.*	Consulting Engineer, 44 Broadway, New York City.	April 19, 1892
HOCHHAUSEN, WILLIAM	Electrician, The Excelsior Electric Co., 196 Willoughby St., 74 Hanson Pl., Brooklyn, N. Y.	April 15, 1884
HOLCOMB, EUGENE R.	Engineering Dept., Edison General Electric Co., Portland, Ore.	June 17, 1890
HOLMES, FRANKLIN S.	Electrical Engineer, with C. H. Davis, 120 Broadway, N. Y., 445a Macon St., Brooklyn, N. Y.	April 21, 1891
HOLT, MARMADUKE BURRELL,	Mining and Electrical Engineer, The Colorado Smelting Co., Pueblo, Col.	April 15, 1890
HOOPES, ARTHUR	West Chester, Penn.	April 19, 1892

HOWSON, HUBERT	Patent Lawyer, 38 Park Row, New York City.	June 8, 1887
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
HUFF, S. W.	General Manager, The Raleigh Street Railway Co., Raleigh, N. C.	Nov. 24, 1891
HUMPHREYS, C. J. R.	Manager, Lawrence Gas Co., Lawrence, Mass.	Sept. 6, 1887
HUMPHREYS, PROF. WM. J.	Washington College, Chestertown, Md.	April 18, 1893
IDELL, FRANK E.	Mechanical Engineer, 616 Havemeyer Building, New York City.	July 12, 1887
IHLDER, JOHN D.	Electrical Engineer, with Osterheld, & Eickemeyer, Manuf'rs of Dynamos and Motors, Yonkers, N. Y.	Oct. 2, 1888
INSULL, SAMUEL	President, Chicago Edison Co., The Rookery, Chicago, Ill.	Dec. 7, 1886
IVES, EDWARD B.	Lieutenant U. S. A., Electrical Engineer, 11th and Colona Sts., Philadelphia, Pa.	April 2, 1889
IWADARE, KUNIHIKO	Electrician, Osaka Electric Light Co., Osaka, Japan.	Sept. 20, 1893
IZARD, E. M.	Electrical Engineer, Chicago, Ill.	Mar. 5, 1889
JAEGER, CHARLES L.	Inventor, Maywood, N. J.	Dec. 20, 1893
JAMES, JOHN N.	Electrician, Naval Observatory, Washington, D. C.	Feb. 16, 1892
JOHNSON, J. N.	Electrician in Cable Dept., Western Union Tel. Co., 16 Broad St., New York City.	Sept. 20, 1893
JOHNSTON, A. LANGSTAFF	Consulting Engineer, Hestonville, Mantua and Fairmount Passenger R.R. Co, 4300 Lancaster Ave., Philadelphia, Pa.	April 21, 1891
JOHNSTON, W. J.	President, The W. J. Johnston Co., Ltd., Times Building, New York City.	April 15, 1884
JONES, F. R.	Professor of Machine Design, University of Wisconsin, Madison, Wis.	May 20, 1890
JUDSON, WM. PIERSON	U. S. Civil Engineer, Oswego, N. Y.	June 8, 1887
KEEN, WILLIAM M. B., *M.D.*	85 Varick St., New York City.	Sept. 16, 1890
KEILHOLTZ, P. O.	Secretary and Superintendent, U. S. Electric Power and Light Co., Baltimore, Md.	Mar. 21, 1893
KELLER, E. E.	Manager and General Supt. of Exposition Work, Westinghouse Electric and Mfg. Co., 5506 Monroe Ave., Chicago, Ill.	Sept. 20, 1893
KELLOGG, JAMES W., *M.E.*	General Electric Co., 140 E. 27th St., New York City.	June 26, 1891

KENNELLY, A. E.	(*Vice-President.*) Electrician, Edison Laboratory, Orange, N. J.	May 1, 1888
KIMBALL, A. S.	Professor of Physics, Worcester Polytechnic Institute, Worcester, Mass.	Sept. 3, 1889
KINNEY, HARRY A.	Electrical Engineer, 634 West Warren Ave., Detroit, Mich.	Mar. 18, 1890
KIRKEGAARD, J. GEORG	General Incandescent Arc Light Co., 329 Union St., Brooklyn, N. Y.	Sept. 20, 1893
KNOX, JAMES MASON	Student in Electrical Engineering. Columbia College, School of Mines, New York City.	Jan. 17, 1894
KREIDLER, W. A.	Editor and Publisher, *Western Electrician*, 6 Lakeside Building, Chicago, Ill.	Oct. 4, 1887
LAIN, DAVID E., *B. S.*	Electrical Engineer, Yonkers, N. Y.	Nov. 13, 1888
LAND, FRANK	69 Park St., Lynn, Mass.	Sept. 22, 1891
LANMAN, WILLIAM H.	Patent Dept., General Electric Co., 44 Broad St., New York City.	June 6, 1893
LAWTON, W. C.	Electrician in Motor Dept., Edison Electric Illuminating Co., 808 Greene Ave., Brooklyn, N. Y.	June 6, 1893
LEDOUX, A. R., *M. S.*, *Ph.D.*,	Chemical Expert, 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	2625 Wabash Ave., Chicago, Ill.	Mar. 15, 1892
LEVIS, MINFORD	Superintendent, 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
LIEB, CHARLES A.	44 Broad St., New York City.	May 16, 1893
LIEBIG, GUSTAV A., JR.	Electrical Testing Bureau, Johns Hopkins University, Baltimore, Md.	Mar. 6, 1888
LINDNER, CHAS. T.	Electrical Engineer, Dwight Way and Tel. Ave., Berkeley, Cal.	Dec. 20, 1893
LITTLE, FRANKLIN P.	Manager, F. P. Little & Co., 141 East Seneca St., Buffalo, N. Y.	May 17, 1892
LOGAN, CHARLES H.	[Address unknown.]	Nov. 18, 1890
LOOMIS, OSBORN P.	Electrical Engineer, Bound Brook, N. J.	Sept. 16, 1890
LORRAIN, JAMES GRIEVE	Norfolk House, Norfolk St., London, W. C., England.	May 16, 1893
LOVEJOY, J. R.	General Manager, Supply Dept., General Electric Co., Schenectady, N. Y.	April 21, 189
LOW, GEORGE P.	Electrical Inspector, Pacific Insurance Union, 303 California St., San Francisco, Cal.	Jan. 17, 1893

LOZIER, ROBERT T. E.	Electrical Expert, The General Electric Co., Schenectady, N. Y.	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	Electrical Engineer, Union Electric Co. 45 Broadway, New York City, and Bedford, N. Y.	June 26, 1891
MACFADDEN, CARL K.	Bartholomew, Stow & Co., 57 Michigan Ave., 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,	General Manager, Brush Electric Light Co., Rock Island, Ill.	Sept. 22, 1891
MACQUESTEN, W, D.	Electrical Engineer and Contractor, 15 Cortlandt St., New York City.	April 15, 1890
MADDEN, O. E.	136 Liberty 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
MAGENIS, JAMES P.	Editor. The *Adams Freeman*, Adams, Mass.	Sept. 27, 1892
MALCOLM, PHILIP S.	Western Agent, Gibson Electric Co., Portland, Ore.	Mar. 18, 1890
MANN, FRANCIS P.	Westinghouse Electric and Mfg. Co., Pittsburg, Pa.	June 6, 1893
MANSFIELD, ARTHUR NEWHALL	Assistant Electrician, American Telephone and Telegraph Co., 153 Cedar St., New York City.	Dec. 20, 1893
MANSFIELD, GEO. W.	Electrical Engineer, 620 Atlantic Ave., Boston, Mass.	June 2, 1885
MARKS, LOUIS B.	Chief Electrician, Royal Arc Electric Co., Residence, 51 East 67th St., New York City.	May 20, 1890
MARTIN, A. J.	General Supt., West End Electric Co., 1308 North 29th St., Philadelphia, Pa.	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., and Equipment Dept., Navy Yard, N. Y.	Oct. 21, 1890
MARTIN, T. COMMERFORD	(*Past-President.*) Editor, *The Electrical Engineer*, 203 Broadway, New York City.	April 15, 1884
MASON, JAMES H.	Electrician, 1391 Fulton St., 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, GEORGE M.	411½ California St., Room 6, San Francisco, Cal.	Dec. 16, 1890
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., 25 Madison Ave., Plainfield, N. J	Dec. 20, 1893
MCCROSKY, JAMES W.	Graduate Student, Johns Hopkins University, 1104 McCulloh St., Baltimore, Md	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, U. T.	Dec. 20, 1893
MCKIBBIN, GEORGE N.	Chemist and Electrician, Reed & Mc-Kibbin, Consulting Electrical Engineers and 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, Missouri School of Mines, Rolla, Mo.	May 17, 1892
MERCER, ANDREW G.	Electrician, Waterloo Electric Co., Waterloo, N. Y.	Sept. 3, 1889
MEREDITH, WYNN	Asst. Superintendent Operating, Electrical Dept. Midwinter Fair, 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, Room 600, 7 Beekman 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. 45 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.	With F. B. Crocker, 236 W 22d St., New York City.	Oct. 27, 1891
MOORE, D. MCFARLAN	Electrical Engineer, General Electric Co., 44 Broad St., New York City.	Dec. 20, 1893
MOORE, JOHN J.	Rider Ave., New York City.	Nov. 12, 1889
MORDEY, WM. MORRIS	Electrician, Brush Electrical Engineering Co., 34 Montserrat Road, Putney, London, Eng.	Sept. 22, 1891
MORRISON, J. FRANK	15 South St., Baltimore, Md.	April 15, 1884
MORSS, EVERETT	Vice-President, Simplex Electric Co., 297 Beacon St., Boston, Mass.	Sept. 22, 1891
MORROW, JOHN THOMAS	Supt. Electrolytic Plant, Boston and Montana Consolidated Copper and Silver Mining Co., Great Falls, Mont.	Dec. 21, 1892
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
MOSS, GEO. W.	Manager, The Mexican Telegraph Co., Vera Cruz, Mexico.	Jan. 7, 1890
MOSSCROP, WM. A., *M.E*	Electrical Engineer, 128 Oliver St., Boston, Mass.	May 7, 1889
MOTT, S. D.	Electrical Engineer, Passaic, N. J.	Sept. 20, 1893
MOTTRAM, WILLIAM T. M.	Representative Curtis Electric Mfg. Co, Pacific Ave., Jersey City, N. J.	Mar. 21, 1893
MUSTIN, HERBERT S.	Assistant Electrician, City of Hoboken, Police Headquarters, Hoboken, N. J.	Dec. 20, 1893
MYERS, GEO. FRANCIS	Electrical and Mining Engineer, Penn Building, Pittsburgh, Pa.	June 17, 1890
NESMITH, S. D.	[Address unknown.]	Sept. 16, 1890
NEWELL, ARTHUR J.	Electrical Engineer, R. T. Oakes & Co., 366 High St., Holyoke, Mass.	Mar. 18, 1890
NORTON, ELBERT F.	Chief Inspector, City Electrical Inspection, 15 City Hall, Chicago, Ill.	Dec. 20, 1893
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

Ockershausen, H. A.	Electrical Engineer, 65 Madison Ave., Jersey City, N. J.	Sept. 6, 1887
O'Dea, M	Electrician. University of Notre Dame, Notre Dame, Ind.	June 8, 1887
Olan, Theodor, J. W.	Civil and Electrical Engineer, 113 West 34th St., New York City.	May 16, 1893
Osborne, Loyall Allen	Engineer and Electrician, Westinghouse Electric and Mfg. Co., 29 Plane St., Newark, N. J.	Oct. 18, 1893
Osterberg, Max	Student in Electrical Engineering, Columbia College. 232 East 62nd St., New York City,	Jan. 17, 1894
Otten, Dr. Jan D.	Engineer, Thomson-Houston International Electric Co., Kleinbeerenstrasse 21, Berlin. S. W. Germany.	Nov. 18, 1890
Owens, R. B.	Professor of Electrical Engineering, University of Nebraska, Lincoln, Neb.	June 17, 1890
Page, A. D.	Assistant Manager Edison General Electric Co. Lamp Works, Harrison, N. J.	Jan. 19, 1892
Palmer, G. W., Jr.	[Address unknown.]	April 15, 1890
Parcelle, Albert L.	Electrician and Inventor, 157 Washington St., Boston, Mass.	Dec. 16, 1891
Parker, Herschel C.	Assistant in Physics, Columbia College, 21 Fort Green Pl., Brooklyn, N. Y.	April 19, 1892
Parmly C. Howard	Tutor in Mathematics, College of the City of New York, 344 W. 29th St., New York City,	Feb. 21, 1893
Parsell, Henry V., Jr.	Supt of Construction, Edison Decorative and Miniature Lamp Dept, Harrison, N. J., and 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 694, City of Mexico, Mexico.	Sept. 6, 1887
Pedersen, Frederick Malling	Assistant Electrical Engineer, Crocker-Wheeler Electric Co., 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.	629 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.	W. M. Petty & Co.. General Electrical Supplies, Rutherford, N. J., and New York City.	May 16, 1893
PFUND, RICHARD	Electrical Apparatus and Supplies, 153 Broadway, Brooklyn, N. Y.	April 18, 1893
PHILLIPS, EUGENE F.	Manufacturer Insulated Electric Wire, Providence, R. I.	July 13, 1889
POOLE, CECIL P.	Contracting Electrical Engineer, 108 Eighth St., Lynchburg, Va.	Jan. 3, 1888
POPE, RALPH W.	Secretary to the American Institute of Electrical Engineers, 12 W. 31st St., New York City; Residence, 570 Cherry St., Elizabeth, N. J.	June 2, 1885
POWELL, WILLIAM H.	Manchester, Conn.	June 17, 1890
PUFFER, WM. L.	Assistant Professor of Electrical Engineering, Mass. Institute of Technology, Boston, Mass.	Dec. 20, 1893
PUPIN, DR. MICHAEL I.	(*Manager.*) Instructor in Mathematical Physics, Columbia College, 46 W. 72d St., New York City.	Mar. 18, 1890
RANDALL, JOHN E.	Columbia Incandescent Lamp Co., 1912 Olive St., St. Louis, Mo.	May 7, 1889
RANDOLPH, L. S.	Electrician, Blacksburg, Va.	Feb. 21, 1893
RAY, WILLIAM D.	Electrical Engineer, 124 Seeley Ave., Chicago, Ill.	Sept. 27, 1892
READ, ROBERT H.	Firm of Pope, Read & Rogers, 39 Cortlandt St., New York City.	Jan. 19, 1892
REBER, SAMUEL	Lieut. U. S. Army, Johns Hopkins University, Baltimore, Md.	Sept. 20, 1893
REED, CHAS. J.	Electrician, 609 Norris St., Philadelphia, Pa.	Mar. 5, 1889
REED, HENRY A.	Secretary and Manager, Bishop Gutta-Percha Co., 422 E. 25th St., New York City.	June 4, 1889
REID, THORBURN	Electrical Engineer, 24 Chase St., Lynn, Mass.	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.	Electrician, 45 Linden St., Brooklyn. N. Y.	June 8, 1887
REIST, H. G.	Electrical Engineer, General Electric Co., Schenectady, N. Y.	June 17, 1890
REQUIER, A. MARCEL	Electrical Engineer, Westinghouse Electric and Manufacturing Co., Pittsburg, Pa.	Dec. 20, 1893
RIKER, ANDREW L. [Life Member.]	Electrical Engineer, 737 Madison Ave., New York City.	Nov. 1, 1887
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
ROBINSON, ALMON	Draughtsman, Expert in Methods of Gearing, P. O. Box 943, Lewiston, Me.	Sept. 6, 1887
RODMAN, LIEUT. SAMUEL, JR.	Electrician and Expert in High Explosives, 17 Quincy St., 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
ROPER, DENNEY W.	414 Langdon St., Alton, Ill.	June 6, 1893
ROSEBRUGH, THOMAS REEVE	Lecturer in Electrical Engineering, School of Practical Science, 107 Mutual St., 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
ROSS, NORMAN N.	Electrical Engineer, Canadian General Electric Co., 350 Main St., Winnipeg, Manitoba.	Sept. 20, 1893
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., 63 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	Electrician, 137 Broadway, New York City.	Mar. 15, 1892
SAGE, HENRY JUDSON	Electrical Engineer, Telephone Dept., Western Electric Co., 227 So. Clinton St., Chicago, Ill	Dec. 20, 1893
SAHULKA, DR. JOHANN	Docent of Electrotechnics, Technische Hochschule, Vienna, Austria.	Dec. 20, 1893
SANBORN, FRANCIS N.	Wilkesbarre, Pa.	Nov. 24, 1891
SANDS. H. S.	Consulting and Constructing Electrical Engineer, Peabody Building, Wheeling, W. Va.	Feb. 21, 1893
SARGENT, W. D.	General Manager, N. Y. & N. J. Tel. Co., 16 Smith St., Brooklyn, N. Y.	April 15, 1884
SAXELBY, FREDERICK	Electrical Engineer, 288 Summer Ave., Newark, N. J.	June 5, 1888

SCHEFFLER, FRED.	Agent, Stirling Boiler Co., 74 Cortlandt St., New York City.	May 16, 1893
SCHLOSSER, FRED. G.	Superintendent of Electric Dept., Laclede Gas Light Co., 1038 Leffingweil 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, Atlanta, Ga.	Sept. 20, 1893
SCHREITER, HEINR, *C. E.*	Editor, *Der Techniker*, 11 Chambers St, New York City.	Jan. 17, 1893
SEARING, LEWIS	Shepard & Searing, Mechanical and Electrical Engineers, 513 and 514 Mining Exchange, Denver, Col.	April 3, 1888
SEE, A. B.	A. B. See Manufacturing Co., 966½ Bergen St., and 116 Front St., 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, Wilkesbarre, Pa.	Sept. 20, 1893
SELDEN, R. L., JR.	Deep River, Conn.	Jan. 17, 1893
SERRELL, LEMUEL WM.	Mechanical and Electrical Engineer, 10 Wall St., New York City.	Nov. 1, 1887
SEVER, GEORGE F.	Instructor in Electrical Engineering, Columbia College, 121 East 30th St., New York City.	Jan. 17, 1894
SERVA, A. A.	North Industry, Ohio.	Dec. 20, 1893
SHAIN, CHARLES D.	136 Liberty St., New York City.	June 7, 1892
SHAW, EDWIN C.	Manager, Akron General Electric Co., Akron, O.	May 17, 1892
SHAW, GEORGE B.	General Manager, National Electric Mfg. Co., Eau Claire, Wis.	April 15, 1890
SHEA, DANIEL W.	Assistant Professor of Electrical Engineering and Physics, University of Ill., Champaign, Ill.	Dec. 20, 1893
SHEBLE, FRANKLIN	Electrical Engineer, General Electric Co., Lynn, Mass.	Oct. 21, 1890
SHEEHY, ROBERT J.	Electrical Engineer, 101 W. 76th St., New York City.	April 21, 1891
SHEPARDSON, GEORGE D.	Dept. of Electrical Engineering, University of Minnesota, Minneapolis, Minn.	April 21, 1891
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

SMITH, CHARLES HENRY	Assistant Electrician, South Eastern Tariff Association, Atlanta, Ga.	Jan. 17, 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. ELLIOT	Superintendent Fire Alarm Telegraph, 122 W. 73d St., New York City.	April 15, 1884
SMITH, OBERLIN	President and Mechanical Engineer, Ferracute Machine Co., Lockwold, 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	Rio de Janeiro, Brazil.	Sept. 6, 1887
SPAULDING, HOLLON C.	P. O. Box 454 Exeter, N. H.	April 21, 1891
SPENCER, THEODORE	Assistant in Mechanical Department, American Bell Telephone Co., 2 Cragie St., Cambridge, Mass.	Mar. 21, 1893
SPICER, CHAS. W.	Central Power Station, W. and G. R. R., Washington. D. C.	Nov. 12, 1889
SPIKE, CLARENCE J.	Halifax, N. S.	Mar. 18, 1890
SPRAGUE, FRANK J.	(*Past-President.*) Electrical Engineer and Inventor, 182 West End Ave., Vice-Prest. Sprague Electric Elevator Co.. Postal Telegraph Bldg., Firm of Sprague, Duncan & Hutchinson, 15 Wall St., New York City.	May 24, 1887
SPROUT, SIDNEY	Electrical Department, Midwinter Fair, San Francisco, Cal.	Jan. 17, 1894
SPRUSON, WILFRED J.	Member of the firm of Hepburn & Spru on, Consulting Engineers and Electricians, 169 King St , Sydney, Australia.	Dec. 16, 1890
SQUIER, LIEUT. GEORGE O.	Johns Hopkins University, Baltimore, Md.	May 19, 1891
STADELMAN, WM. A.	Vice Prest, and Chief Engineer, Equitable Engineering and Construction Co., Drexel Building, Philadelphia, Pa	Feb. 7, 1890
STAHL, TH.	Creusot Works, Creusot, France.	Nov. 15, 1892
STANLEY, WILLIAM	Electrician, Pittsfield, Mass.	Dec. 6, 1887
STEVENS, W. LE CONTE	Professor of Physics. Rensselaer Polytechnic Institute, Troy, N. Y.	Dec. 20, 1893
STOCKBRIDGE, GEO. H.	Patent Attorney, Room 114. 39 Cortlandt St . New York City.	May 24, 1887
STOCKLY, GEO. W.	President, Brush Electric Co., Cleveland, O. Lakewood, N. J.	April 15, 1884
STONE, CHARLES A.	Manager, with E. S. Webster, Mass. Electrical Engineering Co., 4 P. O. Sq. Boston Mass.	May 19, 1891

Storrs, Prof. H. A.	111 E. 24th St., New York City.	Mar. 21, 1893
Stratton, Milton G.	Assistant Electrician, Lawrence Gas Co., 256 Essex Street, Lawrence, Mass.	Sept. 20, 1893
Strong, Frederick G.	116 Bedford St., Boston, Mass.	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
Sullivan, M. C.	Electrical Engineer, 136 Liberty St., New York City.	Dec. 16, 1890
Summers, Leland L.	Assistant Electrician, Postal Telegraph Co., 23 Imperial Building, Chicago, Ill.	Feb. 16, 1892
Sventorzetzky, Capt. Loudomir	Military Engineering Academy, St. Petersburg, Russia.	Sept. 20, 1893
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
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	(*Vice-President.*) Electrical Engineer and Inventor, 35 So. 5th Ave., and The Gerlach, 55 W. 27th St., New York City.	June 5, 1888
Thompson, William Geo. MacNeill	Resident Engineer, Sault Ste. Marie Canal, St. Catherines, Ont.	July 12, 1887
Tischendoerfer, Fred W.	Electrical Engineer, 77 Centre St., Lynn, Mass.	April 19, 1892
Tobey, William Boardman	The Stanley Laboratory Co., Pittsfield, Mass.	Sept. 16, 1890
Townsend, Henry C.	Attorney and Expert in Electrical Cases, 5 Beekman St., New York City.	July 10, 1888
Tregoning, John	32 Belmont Ave., Providence, R. I.	April 2, 1889
Trott, A. H. Hardy [Life Member.]	Electrical Expert, with General Electric Co., Lynn, Mass.	Jan. 20, 1891
Tuttle, George W.	Storekeeper, Sawyer-Man Electric Co., 510 W. 23d St., New York City.	Mar. 17, 1891
Uehling, T. A.	[Address unknown.]	May 19, 1891

UHLENHAUT, FRITZ, JR.	Philadelphia Traction Co., 4101 Haverford St., Philadelphia, Pa.	May 7, 1889
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, 5 Wilson St., Albany, N. Y.	Oct. 25, 1892
VANCE, A. ST. CLAIR	General Superintendent, The H. Ward Leonard Co., 136 Liberty St. New York City.	April 2, 1889
VANDERGRIFT, JAMES A.	Care of Westinghouse Electric and Mfg. Co., Robinson St., Allegheny City, Pa.	Nov. 24, 1891
VAN TRUMP, C. REGINALD	Engineer and Manager, Wilmington, City Electric Co , Wilmington, Del.	Feb. 5, 1886
VAN VALKENBURGH, F. S.	914 Fifth St., Seattle, Wash.	June 5, 1886
VAN VLECK, FRANK	President, Van Vleck Tramway Co., Wells Fargo Bldg., Los Angeles, Cal.	Nov. 16, 1886
VAN WYCK, PHILIP, V. R., JR.	127 W. 58th St., New York City.	April 21, 1891
VARLEY, RICHARD, JR.	General Manager, Varley Duplex Magnet Co., 62 Cortlandt St., New York City.	Mar. 18, 1890
VERLEY, HORACE S. L.	With Dr. Wm. E. Geyer, as Laboratory Assistant, Stevens Institute, Hoboken, N. J.	May 17, 1892
WACKER, GEORGE G.	Electric Organs, 3644 Third Ave., New York City.	Sept. 6, 1887
WAIT, HENRY H.	Assistant Electrical Engineer, Western Electric Co., 4919 Madison Ave., Chicago. Ill.	Sept. 20, 1893
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.	2054 Madison Ave., New York City.	Nov. 12, 1889
WARDLAW, GEORGE A.	Assistant Engineer, People's Light and Power Co., Doolittle House, Oswego, 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., Manchester, Conn.	Dec. 16, 1890
WARNER, CHAS. H.	Consulting Electrical Engineer, 50 Broadway, New York City.	Dec. 20, 1893
WARNER, ERNEST P.	Electrical Engineer, Western Electric Co., 227 So. Clinton St., Chicago, Ill.	Sept. 20, 1893

WASON, CHAS. W.	Electrical Engineer, East Cleveland R.R. Co., 1762 Euclid Ave., Cleveland, O.	May 19, 1891
WASON, LEONARD C.	Head Draughtsman with F. S. Pearson, 199 Harvard St., Brookline, Mass.	Dec. 20, 1893
WATERHOUSE FRANK G.	Room 6, No. 302 Asylum St., Hartford, Conn.	Sept. 6, 1887
WATERMAN, F. N.	Electrical Engineer, Westinghouse Electric and Mfg. Co., 120 Broadway, New York City.	Feb. 21, 1893
WATERS, EDWARD G.	Electrical Engineer, Pennsylvania General Electric Co., 425 Wood St., Pittsburg, Pa.	Mar. 18, 1890
WATSON, ROBERT	931 F. St., N. W., Washington, D. C.	Oct. 21, 1890
WATTS, H. FRANKLIN	Electrical Engineer, 121 Liberty St., New York City,	May 20, 1890
WAYLAND-SMITH, F.	Resident Agent, Syracuse Steel Foundry Co., 26 Cortlandt St., New York City.	June 6, 1893
WEAVER, NORMAN R.	Galveston City R.R. Co., Galveston, Texas.	Oct. 25, 1892
WEBSTER, DR. ARTHUR G.	Docent in Physics, Clark University, Worcester, Mass.	Jan. 19, 1892
WEBSTER, EDWIN S.	Mass. Electric Engineering Co., 4 P. O. Sq., Boston, Mass.	April 21, 1891
WEST, JULIUS HENRIK	Engineer, Handjery St., 58 Friedenau, Berlin, Germany.	Sept. 20, 1893
WELLES, FRANCIS R.	Manufacturer, Bell Telephone Manufacturing Co., Antwerp, Belgium.	Sept. 6, 1887
WHITE, GEO. MONTAGU-	Agent for West Indies Thomson-Houston International Electric Co., Kingston, Jamaica, W. I.	Sept. 22, 1891
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. 309 So. 13th St., Omaha, Neb.	Feb. 7, 1890
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.	Secretary and General Supt. Davis Electrical Works, Chicopee Falls, Mass.	Sept. 20, 1893
WIENER, ALFRED E.	Electrical and Mechanical Engineer, General Electric Co., 24 Yates St., Schenectady, N. Y.	May 16, 1893

WIGHTMAN, MERLE J.	Vice-President and Electrician. The Wightman Elec. Mfg. Co., 4 Commonwealth Bldg., Scranton, Pa.	Mar. 5, 1889
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
WILLIAMS, FRANK A.	Safety Insulated Wire and Cable Co., 25 Washington Ave., Newark, N. J.	Dec. 20, 1893
WILLIAMSON, G. DEWITT	Dobbs Ferry, N. Y.	April 18, 1893
WINSLOW, I. E.	Thomson-Houston International Electric Co., 27 Rue de Londres, Paris, France. Address, 457 Produce Exchange, New York City.	Nov. 12, 1889
WINTRINGHAM, J. P.	Theorist, 36 Pine St, New York City, and 153 Henry St., Brooklyn, N.Y.	May 7, 1889
WIRT, CHARLES	(*Manager.*) Consulting Engineer and Proprietor, The Wirt Laboratory, 56 Fifth Ave., Chicago, Ill,	Sept. 8, 1888
WIRT, HERBERT C.	Electrician, General Electric Co., Boston, 12 Millmount St., Roxbury, Mass	June 26, 1891
WOLVERTON, B. C.	Electrician, N. Y. & Pa. Telephone and Telegraph Co., Elmira, N. Y.	Mar. 18, 1890
WOOD, E. J.	Consulting Engineer and Contractor, 243 Broadway, New York City.	July 12, 1887
WOODRUFF, H. O.	Manager, Sioux City Electric Co., Sioux City, Ia.	Oct. 2, 1888
WOODWARD, FRANCKE L.	Undergraduate in Electrical Engineering, Harvard University, 13 Kirkland Pl., Cambridge, Mass.	June 26, 1891
WOOLF, ALBERT E.	Electrician and Inventor, Woolf Electrical Co., 104 Washington St., and 864 Lexington Ave., New York City.	Sept. 16, 1890
WORSWICK, A. E.	Electrical Engineer, Mutual Light and Power Co, Montgomery, Ala.	Sept. 20, 1893
WRAY, J. GLEN	Cable Tester, Chicago Telephone Co., 104 Linn St., Janesville, Wis.	Sept 20, 1893
WRIGHT, JOHN D.	[Address unknown.]	Oct. 21, 1890
WRIGHT, PETER	Inspector of Electrical Works, United Gas Improvement Co., Drexel Bldg, Philadelphia, Pa.	May 16, 1889
YARNALL, V. H.	Superintendent of Construction, J. G. White Co., 29 Broadway, New York City.	May 16, 1893
ZALINSKI, EDMUND L.	Captain of Artillery, U. S. A., care War Dept., Washington, D. C.	May 17, 1887

ZIMMERMAN, LAURENCE J. Electrical Engineer and Inventor,
604 W. 46th St., New York City. Mar. 21, 1893

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Chamberlain, J. C., 135 E. 18th St.
Chandler, Prof. C. F., Columbia Coll.
Clark, Ernest P., 192 Broadway.
Clarke, Charles L., 55 Liberty St.
Colgate, Geo. L., 136 Liberty St.
Compton, A. G., 17 Lexington Ave.
Crane, W. F. D., 87 Maiden Lane.
Crocker, Francis B., 26 W. 22nd St.
Crosby, O. T., 44 Broad St.
Cushman, Holbrook, 337 West 22d St.
Cuttriss, Chas., 1 Broad St.
Dana, R. K., 16 Cliff St.
Davidson, E. C., 179 Times Bldg.
Davis, Charles H., 120 Broadway.

New York City.—Continued.

Davis, Joseph P., 115 W. 38th St.
Davis, Minor M., 5 Dey St.
Decker, D. H., 5 Beekman St.
Dickerson, E. N., 15 Wall St.
d'Infreville, Georges, 10 Desbrosses St.
Donner, William H., 126 E. 22d 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, 115 E. 26th St
Dyer, R. N., 36 Wall St.
Emery, Dr. Charles E., 95 Nassau St.
Emmet, H. L. R., 36 Cortlandt St.
Emmet, W. L. R., 44 Broad St.
Ende, Sigfried H., 39 Lafayette Place.
Fay, Thomas J., 402 Greenwich St.
Field, C J., 143 Liberty St.
Fiske, Henry G., 45 E. 22nd St.
Fleming, R., 208 W. 15th St.
Flint, B. P., 120 Broadway.
Foote, Thos. H., 110 West 129th St.
Forbes, Francis, 137 Broadway.
Foster, Horatio A., 35 Wall St.
Freedman, Wm. H., 120 W. 125th St.
Frye, Henry W., Times Bldg.
Gardanier, George W., 195 Broadway.
Goldmark, Chas J., 473 Park Ave.
Gordon, Reginald, 76 Park Ave.
Griffin, Capt. Eugene, 44 Broad St.
Guy, Geo. H., 131 W. 34th St.
Hadley, Warren B., 431 Fifth Ave.
Hall, Edward J., 18 Cortlandt St.
Hall, William P., 80 Broadway.
Halsey, William B., 96 Broadway.
Hamblet, James, 195 Broadway.
Hamilton, Geo. A., 22 Thames St.
Hammer, W. J., 1301 Havemeyer Bldg.
Harding, H. McL., 1014 " "
Haskins, Charles H., 80 Broadway.
Hatzel, J. C., 114 Fifth Ave.
Hayes, Harry E., 153 Cedar St.
Henshaw, F. V., 402 Greenwich St.
Herzog, Dr. F. Benedict, 30 Broad St.
Higgins, Edward E., 26 Cortlandt St.
Hill, George, 44 Broadway.
Holmes, Franklin S., 120 Broadway.
Howson, Hubert, 38 Park Row.
Hutchinson, Dr. Cary T., 15 Wall St.
Idell, Frank E., 616 Havemeyer Bldg.
Jenks, W. J , 44 Broad St.
Jones, Francis W., 5 Dey St.
Johnson, J. N., 16 Broad St.
Johnston, W. J., Times Building.
Keen, William M. B., 85 Varick St.
Kellogg, James W., 140 East 27th St.
Knox, James M., School of Mines.
Knudson, A. A., 39 Cortlandt St.
Lanman, Wm. H., 44 Broad St.
Ledoux, A. R., 9 Cliff St.
Leonard, H. Ward, 136 Liberty St.
Levy, Arthur B., 810 Lexington Ave.
Lieb, Chas. A., 44 Broad St.

New York City.—Continued.
Lloyd, Robert McA., 2 W. 36th St.
Lufkin, Harvey L., 39 Cortlandt St.
Lundell, Robert, 44 Broad St.
Luquer, Thatcher T. P., 45 Broadway.
Mackie, C. P., 45 Broadway.
MacQuesten, W. D., 15 Cortlandt St.
Madden, O. E., 136 Liberty St.
Mailloux, C. O., 45 William St.
Mansfield, A. N., 153 Cedar St.
Marks, L. B., 51 E. 67 St.
Martin, F., Madison Square Garden.
Martin, T. Commerford, 203 Broadway.
Maver, William, Jr., 31 Nassau St.
McKibbin, George N., 80 Broadway.
Merrill, E. A., 42 Cortlandt St.
Metcalfe, George R., 6 Park Place.
Meyer, Julius, 7 Beekman St.
Mitchell, John Murray, 45 Wall St.
Monell, Joseph T., 236 W. 22d St.
Moore, D. McFarlan, 44 Broad St.
Moore, John J., Rider Ave.
Moses, Dr. Otto A., 1037 Fifth Ave.
Noll, Augustus, 59 Duane St.,
Olan, Theo. J. W., 113 W. 34th St.
Osterberg, Max, 232 E. 62d St.
Parmly, C Howard, 344 W. 29th St.
Pedersen, F. M., 327 W. 34th St.
Pattison, Frank A., 136 Liberty St.
Phelps, Geo. M. 203 Broadway.
Pickernell, F. A., 153 Cedar St.
Pope, Franklin L., 39 Cortlandt St.
Pope, Ralph W., 12 W. 31st St.
Pupin, Dr. Michael I., 46 W. 72d St.
Read, Robert H., 39 Cortlandt St.
Reed, H. A., 422 East Twenty-fifth St.
Riker, Andrew L., 737 Madison Ave.
Roberson, O. R., P. O. Box 856.
Rosenbaum, Wm. A., Times Building.
Rosenberg, E. M., 784 Lexington Ave.
Sachs, Joseph, 137 Broadway.
Scheffler, Fred., 74 Cortlandt St.
Schreiter, Heinr, 11 Chambers St.
Seely, J. A., 121 Liberty St.
Serrell, Lemuel Wm., 10 Wall St.
Sever, Geo. F., 121 E. 30th St.
Shain, Charles D., 136 Liberty St.
Sheehy, Robert J., 101 W. 76th St.
Smith, J Elliot, 122 W. 73rd St.
Smith, T. Jarrard, 7 Dey St.
Sprague, Frank J., 15 Wall St.
Stieringer, Luther, 1873 Lexington Ave.
Stockbridge, Geo. H., 39 Cortlandt St.
Storrs, Prof. H. A., 111 E. 24th St.
Stump, C. E., 26 Cortlandt St.
Sullivan, M. C., 136 Liberty St.
Taltavall, Thos R, World Building.
Terry, Chas. A., 120 Broadway.
Tesla, Nikola, 55 West 27th St.
Thompson, Edward P., 5 Beekman St.
Townsend, Henry C., 5 Beekman St.
Turner, Wm. S., 47 Times Building.
Tuttle, George W., 510 W. 23rd St.
Vail, J. H., 44 Broad St.

New York City.—Continued.
Vail, Theo. N., 18 Cortlandt St.
Vance, A. St. Clair, 136 Liberty St.
Vansize, William B., 44 Broad St.
Van Wyck, P. V. R. Jr., 127 W. 58th St.
Varley, Richard, Jr., 64 Cortlandt St.
Wacker, George G., 3644 Third Ave.
Wardell, Geo. P., 2054 Madison Ave.
Warner, Chas. H., 50 Broadway.
Waterman, F. N., 120 Broadway.
Watts, H. Franklin, 121 Liberty St.
Wayland-Smith, F., 26 Cortlandt St.
Weaver, W. D., 32 W. 31st St.
Webb, Herbert Laws, 18 Cortlandt St.
Weller, Harry W., 280 Broadway.
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, 132 Nassau St.
Wintringham, J. P., 36 Pine St
Wolcott, Townsend, Care of W. A. Rosenbaum, Times Bldg.
Wood, E. J., 243 Broadway.
Woodbridge J. L., 47 Times Building.
Woolf, Albert E., 864 Lexington Ave.
Yarnall, V. H., 29 Broadway.
Zimmerman, L. J., 604 West 46th St.
201

Official Stenographer.

Ryan, R. W., 300 Mulberry St.

Niagara Falls.

Burns, Elmer Z.
1

Oswego.

Judson, Wm. Pierson
Wardlaw, Geo. A., Doolittle House.
2

Pelham Manor.

Gilliland, E. T.
1

Rochester.

Bradley, C. S., 102 Spring St.
1

Schenectady.

Burke, James, 24 Front St.
Churchill, Arthur, 5 So. Church St.
Greene, S. Dana, General Electric Co.
Ely, Wm. Grosvenor, Jr., 226 Union St.
Lozier, R. T. E., General Electric Co.
Haskins, Caryl D. " "
Lovejoy, J. R. " "
Reist, H. G., 5 So. Church St.
Rice, E. Wilbur, Jr., General Electric Co.
Rohrer, Albert L., " "
Stebbins, Theodore " "
Steinmetz, C. P., " "
Wiener, A. E., 24 Yates St.
13

Syracuse.

Brady, Paul T., General Electric Co.
Marvin, H. N., 208 So. Geddes St.

2

Troy.

Bernard, Edgar G., 43 4th St.
Stevens, W. Le Conte.

2

Waterloo.

Mercer, A. G., Waterloo Electric Co.

1

Yonkers.

Eickemeyer, Rudolf
Essick, Samuel V.
Ihlder, John D
Lain, David E.,

4

NORTH CAROLINA.

Raleigh.

Huff, S. W., The Raleigh St. Ry. Co,

1

Wilmington.

Barnard, J. H., Wilmington St. Ry. Co.

1

OHIO.

Akron.

Shaw, E. C.

1

Cincinnati.

Cabot, John A., 123 West 8th St.
Creaghead, Thos. J., 296 Plum St.
French, Prof. Thos., Jr., Avondale.
Gray, W. N., 200 Neave Building.

4

Cleveland.

Black, Chas. N., Belden St.
Brush, Chas. F., 956 Euclid Ave.
Canfield, Milton C., 18 Clinton St.
Cleveland, W. B., 309 Perry-Payne Bldg.
Cowles, Alfred H., 656 Prospect St.
McKinstry, J. P., 316 Seneca St.
Roberts, E. P., Western Reserve Bldg.
Sperry, E. A., Mason and Belden Sts.
Wason, Chas. W., 1762 Euclid Ave.

9

Columbus.

Thomas, Prof. Benj. F., Ohio State University.

1

North Industry,

Serva, A. A.

1

Hamilton.

Cornell, Chas. L.

1

Salem.

Davis, Delamore L., 299 Lincoln Ave.

1

Steubenville.

Flood, J. F., Steubenville Street Ry. Co.

1

OREGON.

Portland.

Cheney, W. C., Portland General Electric Co.
Dame, F. L., The Northwest General Electric Co.
Holcomb, E. R., Edison Gen'l Elec. Co.
Malcolm, P. S., Gibson Electric Co.
Mitchell, Sidney Z., Fleischner B'ld'g.

5

PENNSYLVANIA,

Allegheny City.

Barth-Bartoshevitch, A., 160 Arch St.
Fessenden. Prof. R. A.
Floy, Henry, 168 North Ave.
Vandergrift, James A., Robinson St.

4

Altoona,

Dudley, C. B., 1219 Twelfth Ave

1

Chester.

Arnold, C. R., Arnold Elec. Mfg Co.

1

Erie.

Boynton, E. C., 156 W. 8th St.

1

Germantown.

Condict, G. Herbert, 4720 Green St.

1

Haverford College.

Griscom, Wm. W.

1

Johnstown.

Christmas, Adolph F.

1

Lancaster.

Andrews, Wm. S.

1

Monongahela City.

Acheson, E. G.

1

Philadelphia.

Braddell, Alfred E., 316 Walnut St.
Bragg, Chas. A., Girard Bldg.
Flagg, S.G., Jr., 19th St. and Penn. Ave.
Hering, Carl, 927 Chestnut St.
Houston, Prof. E. J., 1809 Spring Garden St.
Hunter, Rudolph M., 926 Walnut St.
Ives, Lieut. E. B., 11th and Colona Sts.
Johnston, A. L., 4300 Lancaster Ave.
Levis, Minford, 54 North 4th St.
Marks, Prof. W. D., Edison Elec. Lt. Co.
Martin, A. J., 1308 N. 29th St.
Perot, L. Knowles, 308 Walnut St.
Pike, Clayton W., 1010 Chestnut St.

Philadelphia.—Continued.

Reed, C. J., 609 Norris St.
Smith, T. Carpenter, 212 Drexel Bldg.
Stadelman, Wm. A., Drexel Bldg.
Uhlenhaut, F., Jr., 4101 Haverford St.
Willyoung, Elmer G., 817 Filbert St.
Wright, Peter, Drexel Bldg.,

19

Pittsburg.

Giles, Walter A., 416 Lewis Block.
Healy, L. W., Westinghouse Electric & Mfg. Co.
Mann, Francis P., Westinghouse Electric & Mfg. Co.
Myers, Geo. Francis, Penn Building.
Requier, A. M., Westinghouse Co.
Schmid, Albert, Westinghouse Electric and Mfg. Co.
Scott, Chas. F., Westinghouse Electric and Mfg. Co.
Shallenberger, O. B., Westinghouse Electric and Mfg Co.
Smith, F. S., Westinghouse Electric & Mfg. Co.
Stillwell L. B., Westinghouse Electric and Mfg. Co.
Temple, William Chase, P. O. Box 800.
Waring, R. S., 61 Westinghouse Bldg.
Waters, Edward G., 425 Wood St.
Wurts, Alex. J., Westinghouse Electric and Mfg Co.

14

Scranton.

Wightman, Merle J.

1

South Bethlehem.

Lattig, J. W.

1

State College.

Jackson, Prof. J. P.

1

West Chester.

Hoopes, Arthur.

1

Wilkesbarre.

Sanborn, F. N.
Seitzinger, H. M.

2

RHODE ISLAND.

Bristol.

Blake, Theo. W.

1

Providence.

Miller, Joseph A., 25 Butler Exchange.
Phillips, Eugene F., 67 Stewart St.
Tregoning, John, 32 Belmont Ave,

3

TENNESSEE.

Memphis.

Roessler, Capt. S. W., 99 Madison St.

1

TEXAS.

Austin.

Macfarlane, Prof. Alexander

1

Galveston.

Weaver, Norman R.

1

UTAH.

Salt Lake City.

McKay, C. R., 140 So. Main St.

1

VERMONT.

Brattleboro.

Childs, W. H.
Fuller, Hon. Levi K.

2

Rutland.

Francisco, M. J., Rutland Elec. Lt. Co.

1

VIRGINIA.

Blacksburg.

Anderson, Prof. W. E.
Randolph, L. S.

2

Front Royal.

Albert, Henry

1

Lynchburg.

Poole, Cecil P., 108 8th St.

1

Richmond.

Leonard, M. B., Sup't C. & O. R.R. Tel.
McCluer, C. E., So. Bell T. & T. Co.

2

WASHINGTON.

Centralia.

Coolidge, Charles A.

1

Everett.

Butler, William C.

1

Seattle.

Daft, Leo, Burke Building.
Van Valkenburgh, F. S., 914 5th St.

2

WEST VIRGINIA.

Huntington.

Wallace, Geo. S., Box 214.
1

Morgantown.

Aldrich, William S., P. O. Box 256.
1

Wheeling.

Sands, H. S., Peabody Bldg.
1

WISCONSIN.

Eau Claire.

Shaw, Geo. B., National Elec. Mfg Co.
Bates, M. E., National Elec. Mfg Co.
2

Janesville.

Wray, J. G., 104 Linn St.
1

Madison.

Davies, John E., 523 Carroll St.
Jackson, Prof. Dugald C.
Jones, Prof. F. R., University of Wis.
3

Milwaukee.

Boardman, H. B., 1530 Grand Ave.
Burton, W. C., 238 27th St.
2

Racine.

Higgins, Eugene
1

WYOMING.

Sheridan.

Casper, Louis
1

DOMINION OF CANADA.

MANITOBA.

Winnipeg.

Ross, Norman N., 350 Main St.

NOVA SCOTIA.

Baddeck.

Bell, Prof. A. Graham.
1

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. 1

ONTARIO.

Berlin.

Breithaupt, E. Carl
1

Brantford.

Callender, Romaine
1

Ottawa.

Ahearn, T.
Dion, Alfred A., 72 Sparks St.
2

Peterborough.

Hartman, Herbert T.
1

Rat Portage.

McCrossan, J. A.
1

St. Catherine.

Thompson, William G. M.
1

Toronto.

Langton, John, Canada Life Building.
Rosebrugh, Thomas R., 107 MutualSt.
Rutherford, W. M., 63 Front St
3

QUEBEC.

Montreal.

Brenner, Wm. H.
Ross, Robert A.
Sise, Charles F., P. O. Box 1918,
3

MEXICO.

City of Mexico.

Peck, S. C., Apartado 694.
1

Vera Cruz.

Moss, Geo. W.
1

SOUTH AMERICA.

UNITED STATES of BRAZIL.

Para.

da Cunha, Manoel Ignacio,
Empresa Industrial Gram-Para.
1

Rio Janeiro.

Souza, Carlos Monteiro,
Chermont, A. L., 7 Rue da Matriz.
2